Objectivity, power, and interests: a sociological analysis

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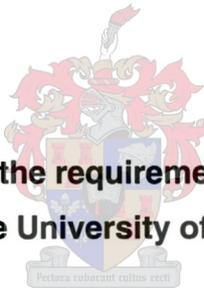
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Declaration

I, the undersigned, hereby declare that the work contained in this thesis is my own original work and has not previously in its entirety or in part been submitted at any university for a degree.

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Abstract

Discourse about the human world has, since Socrates, been structured around the assumption that one view of a given matter is better than competing views, and that argumentation, if carried out correctly and systematically, will favour the view which has the preponderance of reasons and evidence on its side. If this supposition were dropped, the nature of social scientific inquiry would change significantly.

For many commentators in the social sciences the ineliminable interpretative dimension of social inquiry and the standpoint-bound character of interpretation lead to the conclusion that we have to abandon any notion of objective truth in the social sciences. The central question raised in this thesis is whether this abandonment is inevitable or even plausible. Is it plausible to conflate objectivity and truth? Is objectivity a possible characteristic of the individual researcher or a characteristic of the scientific research process? Does the cultural environment of the researcher impact on the validity of research findings? If science is a social phenomenon, are scientific beliefs different from other beliefs? How do the interests of the individual researcher or the formal organisation of scientific practice impact on the validity of findings? What role does power play in the shaping of knowledge? These are the questions that will be addressed in the following thesis.

The methodology of Max Weber serves as a point of departure and divergences and similarities to the work of Weber are explored in the writings of Kuhn, the Edinburgh School, Latour, Foucault, Habermas, as well as contemporary postmodernist and feminist writers. The analysis of these various concepts and approaches is not presented chronologically, but rather as an exposition of the contributors of various commentators in the fields of both the sociology of science and knowledge, and the philosophy of science.

Opsomming

Diskoers oor die menslike wêreld is, sedert Socrates, gestuktureer rondom die aanname dat een siening van 'n gegewe saak beter is as mededingende sienings, en dat argumentasie, indien korrek en sistematies uitgevoer, ten voordeel sal wees van die siening wat gesteun word deur die oormaat van redes en bewyse. As ons hierdie aanname sou laat vaar, sal die stand van sosiaal wetenskaplike ondersoek ingrypend verander.

Vir menige kommentator in die sosiale wetenskappe lei die onafwendbare interpretatiewe dimensie van maatskaplike ondersoek, en die standpunt-gebonde aard van interpretasie, tot die gevolgtrekking dat ons enige opvatting van objektiwiteit in die sosiale wetenskappe moet laat vaar. Die kernvraag in hierdie tesis is of hierdie verskuiwing onvermydelik of selfs aanneemlik is. Is dit geldig om objektiwiteit en waarheid saam te snoer? Is objektiwiteit 'n moontlike eienskap van die individuele navorser, of 'n eienskap van die navorsingsproses? Watter impak het die kulturele omgewing van die navorser op die geldigheid van die navorsingsbevindinge? As wetenskap 'n sosiale fenomeen is, is wetenskaplike oortuigings enigsins anders as ander oortuigings? Watter impak het die belange van 'n individuele navorser, of die formele organsiasie van wetenskaplike praktyk, op die geldigheid van bevindings? Watter rol speel mag in die vorming en skepping van kennis? Hierdie is die vrae wat aangespreek word in dié tesis.

Die metodologie van Max Weber dien as vertrekpunt, en ooreenkomste tot en afwykings van die sienings van Weber word ondersoek in die werk van Kuhn, die "Edinburgh School", Latour, Foucault, Habermas, sowel as kontemporêre postmoderne en feministiese skrywers. Die analise van hierdie verskeie konsepte en benaderings word nie kronologies aangebied nie, maar eerder as 'n uiteensetting van die bydraes van verskeie kommentators op die gebied van die sosiologie van die wetenskap en van kennis, sowel as die filosofie van wetenskap.

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Introduction

The idea of disinterested, neutral, objective analysis, which arguably goes back to Plato, culminated in modern positivism, in the idea that science serves humanity by communicating morally neutral, objective information to improve the human condition. Under the influence of positivist thought, it was believed that statements that reported direct observations could provide neutral data against which theoretical explanations could be tested. Such statements, if true, were thought to mirror or represent the way the world is, to reflect in an unmediated way what is the case. The truth of such statements could be determined without recourse to metaphysical or theoretical beliefs. This notion that we can gain direct and immediate access to reality was famously coined by Derrida as "the metaphysics of presence" (Turner, 1996:402). Under the influence of the development of the methodology of natural sciences, objectivity was viewed as unproblematic, one could simply observe whether what was asserted was in fact the case or not. Alan Chalmers (see Mouton & Joubert (eds.), 1990: 40) refers to this as the "common-sense view of science". Inherent to this view of science is what Mouton refers to as the "naturalistic notion of objectivity", or as stated by Mouton, "the term naturalistic refers to the fact that this ideal of objectivity originated in the natural sciences, where the assumption that the distance between subject and object is desirable, is perhaps less problematic", though "this assumption is less plausible when applied to the social sciences" (Ibid. 40).

Rob Stones (1996: 1-39) argues that contemporary social theory has presented a much richer and more sophisticated view of the social world than the one with which modernist sociology worked. Stones contends that "this rich and complex social ontology (what sorts of things the social world is made up of) has not, however, been matched by a corresponding development in the sophistication of research guidelines" (Ibid. 1). It is in this sphere of epistemology and methodology that "the state of the art is trailing forlornly some way behind" (Ibid. 1).

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Though there is a long tradition of thought that has been suspicious of claims to have direct and objective access to the real world, as stated by Stones, "its pre-eminence only began to be significantly challenged in the late 1960s and 70s" (Stones, 1996:15). In the second half of the century, in an era of increasing multicultural cosmopolitanism, social theorists became conscious of not writing only for their traditional (mostly Western) audiences, but for increasingly sophisticated multi-cultural audiences. As stated by Thomas McCarthy, "the presence of this critical audience places enormous pressure on traditional ideas of social-scientific objectivity, which are closely linked to notions of intersubjective validation. The community of researchers that now has to be convinced includes a much greater variety of competent observers" (Hoy & McCarthy, 1994:86). The realities of writing in this increasingly decentred and multicultural public sphere have heightened self-consciousness about the assumptions that have historically structured social-scientific disciplines.

Discourse about the human world has, since Socrates, been structured around the assumption that one view of a given matter is better than competing views, and that argumentation, if carried out correctly and systematically, will favour the view which has the preponderance of reasons and evidence on its side. If this supposition were dropped, the nature of social scientific inquiry would change significantly. Thomas McCarthy writes, "we have gotten used to living with unresolved disagreements in all the 'human sciences', where what Kuhn calls the 'preparadigmatic' stage seems never to be superseded by 'normal science', as regularly happens in the natural sciences. We even have plausible explanations of why this should be the case, and they turn on the ineliminable interpretative dimension of social inquiry and the standpoint-bound character of interpretation" (Ibid. 241). For many commentators in the social sciences these same features lead to the conclusion that we have to abandon any notion of objective truth about the human world. The central question in the following thesis revolves around whether this abandonment is inevitable and whether there are no alternatives.

Inquiry in the social sciences has to do with phenomena and practices that are always already interpreted, and hence the aim of this form of inquiry is at interpretations of interpretations, or as Anthony Giddens famously coined it, the "double hermeneutic". As all interpretations are from a given point of view, and social reality gives rise to a multiplicity of points of view, one may well conclude that no interpretation is ever final. If there is every reason to expect the conflict of interpretations to be a permanent feature of social sciences research, is it still at all plausible to postulate objectivity as a methodological criteria or even as an ideal? In this regard the impact of postmodernist theory has been particularly pernicious, and the challenge presented in such a radical way, that no secure grounds could be claimed for comparative evaluation of research results. Despite this, research of the sort that is being challenged thrives, even where the challenges would seem on philosophical grounds to have been fatal. The beginning of an explanation for why this is so perhaps lies in the separation of "abstracted empiricism" from "grand theory" which C. Wright Mills critiqued some time ago.

The complex and diverse activities called "social science" have for a long time attempted to formulate predictive general laws in the manner of naturalistic knowledge. The critics of this approach have placed interpretation and meaning at the centre of the social sciences, and emphasised the normative and moral purposes of this endeavour, i.e. Robert Bellah's notion of sociology as moral inquiry (see Seidman, 1994: 283-293). In the history of social science there have also been numerous attempts to incorporate both of these orientations into one theoretical and methodological framework. Such a mixture of causal explanation and complex interpretation has perhaps been most persuasively attempted by Max Weber, and more recently, Jürgen Habermas, who in the words of James Bohman, "argued for this irreducible methodological complexity" (Bohman, 1991: 7).

Whereas the old logic of the social sciences sought idealised reconstructions, the new logic argues for an indeterminate and open-ended complexity, and rejects the "received view" in favour of tracing the actual practices of social scientists. As

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stated by Bohman, "the turn to history and to practices has placed the social sciences at the centre of contemporary epistemology and philosophy of science" (Ibid. 8).

The widespread rejection of the logical positivist view of social science in the 1960's has led to the emergence of various antimodernist and post-modernist views that stress the subjective aspects of social sciences practice. The main tenets of modern social science, that reason is the main instrument of scientific progress, that objectivity implies a value-neutral attitude, and that truth and certainty are the goals of science, have been criticised and rejected (Mouton & Joubert, 1990: 1 & 2). These critical viewpoints and frameworks, however, do not only lack a unifying paradigm, but "the boundaries between different points of view are constantly changing" (1990: 1). The post-modern condition has led to a fragmentation and cross-borrowing of innumerable perspectives and orientations, and any attempt to group them together, even under the term "postmodernist", poses the danger of oversimplification.

Useful for the purposes of this introduction is the work of Mouton and Joubert, who identify various current trends and issues in the methodology and philosophy of the social or human sciences[1]. As stated by Mouton and Joubert; "It is not surprising, that the large scale rejection of the logical positivist view of science in the sixties, would lead to the emergence of views which tended to stress various aspects of the 'subjective', whether the historical (Kuhn) or the sociological (Edinburgh school) or even the totally a-logical (Feyerabend's epistemological anarchism) variety" (Ibid. 2). Thomas Kuhn's concept of "paradigms" raised serious doubts regarding the rationality and objectivity of science, and led to the adoption of more pluralist and relativist conceptions of science. The rationality/relativism debates of the 1970's that were sparked by Peter Winch's work on cross-cultural understanding created a new interest in the concepts of rationality and objectivity. This inconclusive debate

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[1] It is worth noting that the use of the term social or human sciences is already considered a choice for or against the traditional methods of the social sciences.

was followed by what Mouton and Joubert refer to as "discourse fever", wherein Foucault's studies of historical discourses raised problems regarding the role of power relations and ideology in the history of science. This interest in power and ideology received a new impetus through the Critical Theory of Jürgen Habermas who has famously refused to reject the belief in reason and objectivity characteristic of the modernist project. Each of the aforementioned components of this diversified and fragmentary phase in the philosophy of social science will be addressed in this thesis. The analysis of these various concepts and approaches is not presented chronologically, but rather as an exposition of the contributions of various commentators in the fields of both the sociology of science and knowledge, and the philosophy of science. The methodology of Max Weber serves as a point of departure and divergences and similarities to the work of Weber are explored in the writings of Kuhn, the Edinburgh School, Latour, Foucault, Habermas, as well as contemporary postmodernist and feminist writers.

For many commentators in the social sciences the ineliminable interpretative dimension of social inquiry and the standpoint-bound character of interpretation lead to the conclusion that we have to abandon any notion of objective truth in the social sciences. The central question raised in this thesis is whether this abandonment is inevitable or even plausible. Is it plausible to conflate objectivity and truth? Is objectivity a possible characteristic of the individual researcher or a characteristic of the scientific research process? Does the cultural environment of the researcher impact on the validity of research findings? If science is a social phenomenon, are scientific beliefs different from other beliefs? How do the interests of the individual researcher or the formal organisation of scientific practice impact on the validity of findings? What role does power play in the shaping of knowledge? These are the questions that will be addressed in the thesis.

The four chapters of the thesis are arranged as follows:

1. The first chapter is concerned with Max Weber's position on the objectivity of the social sciences, and his attempt to bridge the gap between those who believe that the social sciences should attempt to follow the path of the natural sciences, and those who argue that the study of people as social beings is entirely distinct from other sciences. To this end an analysis is made of the key concepts in Weber's methodology, and the relation between Weber's conception of objectivity and the concepts 'verstehen', 'ideal-type', 'value-relevance' and value-freedom' is explored.

2. The second chapter is concerned with the social construction of scientific discourse. Contrary to the position adopted by Max Weber, the notion that science is or can be protected against intrusion of external influences is challenged by studies on the actual practice of science. The chapter traces various conceptions of science and knowledge through the work of Thomas Kuhn, the 'strong programme' in the sociology of science, the ethnographic fieldwork of Bruno Latour and Steve Woolgar, and the notion of knowledge/power in the work of Michel Foucault. Comparisons are drawn between these varying contributions to the study of scientific knowledge, and similarities and divergences are addressed.

3. The third chapter is concerned with the implications of postmodernist theorising and the deconstruction of objectivity. Attention is paid to the implications of a non-hierarchical conception of knowledge claims, specifically the methodological implications of standpoint theory in which the notion of a divergence of standpoints rooted in different experiences has become most developed.

4. The fourth and concluding chapter is concerned with the possibility of charting a new course between the modern faith in the rational individual and the dissolution of the rational subject in post-modern thought. A critical theory that

recognises the political nature of scientific research, and takes the problems of interpretation in research seriously, is proposed as a way to restore the connection between the traditional methodology of social science and the grounding of research in subject/subject epistemic relations and practical reason.

Chapter 1

Max Weber's key concepts and the problem of objectivity

In this chapter an analysis is made of the key concepts in Max Weber's methodology by briefly looking at Weber's own formulations and claims, as well as the contributions of various commentators on the meaning of the concepts `verstehen', `ideal-type', `value-relevance' and `value-freedom', and the relation between these concepts and the notion of objectivity.

Introduction

The problems and questions raised by the connection between theory and practice has repeatedly drawn the attention of philosophers and social scientists. It has led to the debate with whose commencement the name of Max Weber is particularly linked. It is a debate concerning the significance and possibility of value-freedom in science, and persists to this day.

The central theme of the Enlightenment, the struggle between rationality (science), and prejudice and dogmatism, gave rise to the notion that research can only be valid and objective if all subjective variables, values and preconceived assumptions were excluded from the process. The scientist must allow the facts to speak for themselves (nominally), prior to being spoken. The phenomenon of human subjectivity is treated on par with objects in the natural world. The sociologist, as observer of social reality, provides knowledge that is purely instrumental in form, knowledge which does not carry any logically given implications for practical policy or the pursuit of values. Like the natural sciences, sociology is neutral in respect to values (see Mouton in Mouton & Joubert, 1990:40-42).

The 20th century version of this relationship between values and objectivity

propounded by Max Weber, though more sophisticated in its explication, shares the assumption that "non-epistemic value judgements" can be excluded from the process of research (Ibid. 43). Weber's methodological writings form the classic point of reference for problems of value-interpretations. When he discussed these issues he was actually entering the later stages of a debate that had been going on for twenty years, the now famous *Methodenstreit*, the battle of methods.

By way of introduction it should also be noted that German intellectual life, not surprisingly, exercised a strong influence on the thought of Weber. One of the primary influences which demands mention is the epistemology of Immanuel Kant. Kant argued strongly that the external world, what he called the "thing in itself", can essentially never be known, for we always perceive the world through the screen of our subjective categories. Weber was a neo-Kantian, in the sense that he believed that one never knows anything apart from the categories one applies to it. This applies to history, social life, and the individual human actor. The social scientist is always selecting particular patterns to concentrate upon, and the patterns are framed by the categories of the observer's own analysis. For Kant there are two sources of human knowledge; sensibility and understanding - "Through the former objects are given to us; through the latter they are thought" (Tarnas, 1991:341). It is only through the workings of the understanding that sense experience comes to be ordered and classified into experience of the objective world. But here the similarity between Kant and Weber ends. For, in so far as the sociologist is concerned with knowledge in particular and not beliefs in general (regardless of whether these are or are not known to be true), he is not asking what knowledge essentially is. He is, rather, asking what social conditions promote or inhibit the acquisition of what sorts of knowledge (Turner (ed.), 1996:40-41).

Weber provided one possible solution to a well-known dilemma. If you are passionately religious, for instance, and interested in religion as the object of your research, you will be neither impartial nor objective; but if you regard

religion, as in this example, as a mere web of superstitions, there is the danger that you will never have a deep understanding of the religious life of men. Weber finds a solution by drawing a distinction between questions and answers: one must have a feeling for the importance of what men have experienced in order to understand them, but one must detach oneself from one's personal concern if one is to find a universally valid answer to a question which is inspired by a passionate interest.

Yet the perplexing question is precisely what the nature of this detachment is. This detachment, in the case of Weber, and hence the validity of research findings (answers), is founded in the methodological procedures which are applied after the research problem (question) has been formulated. This calls for an analysis of the central concepts in Max Weber's methodology, and will be conducted by briefly looking at Weber's formulations and the contributions of various commentators on the debate on the meaning of his concept "verstehen", and his use of the construct "ideal type".

Verstehen

Max Weber states that to "understand" action, we must "identify a concrete 'motive' or complex of motives 'reproducible in inner experience', a motive which we can attribute the conduct in question with a degree of precision" (Weber, 1949: 52).

Verstehen, the first technique here, is Weber's method of interpreting subjective meanings. This may be rendered into English as 'understanding', but many commentators on Weber retain the German word or add an adjective to the English one because there seems to be more involved in it than can be captured by simple translation. At the most basic level to use Verstehen we must observe a situation and ask why the people involved acted the way they did. But what is the nature of this method of understanding, and what is the content of Weber's

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concept of subjective meaning? Also, how is this method of interpretation applied? To begin with, it would seem that the central relation is between the meanings of the individual actor, the way the individual acts, and the circumstances in which he/she acts.

Both the supporters and the critics of Verstehen have seen it as a special method of gaining knowledge that is peculiar to the human disciplines. They have regarded it as essentially an intuitive mental process in which one tries to reproduce another's thoughts and emotions in one's own mind through a shared humanity and empathy. Since this process can never be a complete and reliable one, for to be so one would have to become the other person, they have stressed the essential incomprehensibility of other men or cultures committed to values differing from our own. In an ultimate sense this is of course true, and one could argue that we are all imprisoned in our own selfhood. In the case of the social sciences, however, the interpreter presupposes a basis for judgement that is shared by all actors.

L.M. Lachmann, in his book entitled "The legacy of Max Weber" (1970: 17-48), proffers an interesting notion of Verstehen which is worth mentioning. Whereas it has often been said that in propounding the method of Verstehen Weber was defending the heritage of German idealism against the onslaught of positivism, Lachmann argues that regardless of Weber's own attitude toward it, the interpretation of human utterance is much older than German Idealism. The whole hermeneutical tradition that goes back to Schleiermacher in the 1800's is nothing new. Lachmann argues that we may say that it is a "natural" way of rendering an intelligible account of human manifestations, as it is nothing less than the traditional method of classical scholarship. In broad sketches his thesis is that when people traditionally strove to understand a text they employed a number of procedures aimed at the greatest possible understanding of what the author wanted to say. If the text contained a generalisation such as a legal norm or religious precept a decision had to be made as to what kind of concrete

situation the text applied to. This textual interpretation, he argues, is the prototype of Verstehen. Before the rise of natural science this was the method of scholars, whether studying the Bible or translating Homer, and had little to do with intuition. The writing of history became more than a recording of mere facts, and historiography emerged as a scholarly discipline. It was only natural that historians would adopt this method. Not content to merely study ancient chronicles, they began to ask why people acted as they did. This pursuit for the actor's purposes and goals was done by the same means as those by which scholars for centuries attempted to ascertain what the author meant and intended (1970: 17-48). States Lachmann:

> "Once we have realised that the historical method of interpretation applied to overt action instead of to texts, a method aiming at identifying a human design, a `meaning' behind observable events, we shall have no difficulty in accepting that it can be just as well applied to human interaction as to individual actors" (1970: 20).

This does, however, not clarify the question as to how the method of Verstehen is to be put to the test of empirical verification.

In his analysis Dennis Wrong argues this objection, that Verstehen provides no procedure for verification, "no way of determining whether the intuited meaning of a action really corresponds to the actor's actual judgements and intentions" (Wrong, 1970: 18). The argument: to provide an intelligible or meaningful explanation of an action is at most to satisfy criteria of plausibility rather than empirically verified truth. Verstehen, then, is no more than an indispensable aid in developing hypotheses which can then be put to the test of empirical verification in accordance with the most basic method of science. Wrong, however, suggests that there is another way of viewing Verstehen. He states that, "if Verstehen is seen as a directive to look for certain substantive aspects of human conduct rather than as a special method of acquiring knowledge, it appears in a somewhat different light" (Ibid. 19). In this view our understanding of others is not limited to the scope of our own personal experience. Wrong

states that though it may take a thief to catch a thief, a thief's goals are nonetheless intelligible to an honest man (Ibid. 20). It is this ability which in Weber's view gives social science an interpretative advantage over the methods of the natural sciences.

This aspect of Weber's methodology is also raised by Coser, who argues that when the objection is raised that rational knowledge of causal sequences may be attained in the world of nature, but not in the unpredictable and irrational human world, "Weber counters by turning the tables" (Coser, 1971: 220). The well-known argument of Weber is that whereas knowledge of nature must always be from the outside, by the observation of external courses and events and by recording their uniformities, in the case of human action it is possible to impute motives by interpreting men's actions and words (Ibid. 220). The interpretation of human action in terms of motives can be constructed as a form of causal explanation precisely because we are able to analyse it in terms of chains of rationality, by linking `motives' or `purposes' to the `means' whereby the actor seeks to attain particular goals. In so far as conduct is rational, it is also highly predictable. Following Schopenhauer and Nietzsche, Weber saw society as the product of human wills. Individuals are not merely thinkers, they are also actors, and the freer an individual's action, the less his act is conditioned or coerced, the more clearly it can be analysed in terms of a means-ends rationality and hence predicted by an observer. The operation of "free will", which is realised in a given situation, thus presupposes that human behaviour conforms to ascertained regularities (Giddens, 1974: 7).

Thus interpretative understanding is possible by penetrating the subjective meanings which people themselves attach to their behaviour and that of others. His or her answer may well amount to little more than a rationalisation of his real motives, in which case we may well conclude that `actions speak louder than words' and construct an alternative interpretation of his conduct. The point, however, is that we can constantly test our interpretation against the actor's own

account, his conduct, and the situation in which he is acting. Dennis Wrong, in this regard, states that this very real possibility of testing our interpretations is the meaning of Weber's argument; "that interpretations which are `meaningfully adequate' must always be complemented by consideration of their `causal adequacy'" (Wrong, 1970: 21).

Weber's idea, reduced to its simplest terms, is that in the realm of natural phenomena we can understand only through the intermediatery of mathematical propositions, observed constants, and previously established laws. In other words, we must explain phenomena by propositions confirmed by experience in order to have the feeling that we understand. As stated by Raymond Aron, "Comprehension is therefore mediated; it occurs through the intermediatery of concepts or relationships" (Aron, 1967: 191). In the case of human behaviour, however, comprehension may be immediate. I understand why a driver stops in front of a red light; I do not need to observe how often drivers regularly stop before red lights in order to understand why they do it. But the notion of immediate intelligibility is not unequivocal. It does not refer to some mysterious faculty of intuition exterior to reason as in the case of, for instance, the aesthetic idealism of Keats, Coleridge and Shelley. Nor is it immediate in the sense that we can grasp at once, without previous investigation, the significance of the behaviour of others, for the behaviour of others is not without ambiguity. A man does not always know the motives for his actions, and an observer is still less capable of guessing them intuitively. It requires prior investigation to distinguish between what is probable and what is true.

When meanings are rational, we can often understand them by noticing how an actor works toward his goals, particularly if his goals are similar to our own. Moreover, empathy can often help us understand familiar non-rational motives like pride, envy, jealousy, loyalty, love, etc. However, it is very difficult for us to empathise with actors who display unfamiliar emotions, accept strange goals, or respond to foreign traditions. To answer such questions, Weber introduced his

ideal types.

> "Such constructions make it possible to determine the typological locus of a historical phenomenon. They enable us to see if, in particular traits or in their total character, the phenomena approximate one of our constructions: to determine the degree of approximation of the historical phenomenon to the theoretically constructed type. To this extent, the construction is merely a technical aid which facilitates a more lucid arrangement and terminology" (Weber in Gerth & Wright Mills (eds.), 1948: 324).

Even if we cannot understand individual meanings, he reasoned, we might be able to judge these meanings as departures from some defined norm. If we decide, for example, how a completely rational person would behave in a given context, we can compare real behaviour with the rational ideal type. For Weber the ideal type is the chief instrument of causal analysis in society, the fundamental concept of all social sciences. Says Coser, "any interpretative explanation must become a causal explanation if it is to reach the dignity of a scientific proposition. Verstehen and causal explanation are correlative rather than opposed principles of method in the social sciences" (1971: 221). Consequently, our immediate and `natural' intuitions of meaning only become valid social knowledge when incorporated into theoretical structures which aim at causal explanation. In the case of Weber the ideal type is the central construct in such theoretical structures and one of the most distinctive forms of the Verstehen mode of explanation.

The ideal type

"The kind of ideal-type model of social action which is constructed, for example,

for the purposes of economic theory is … 'unrealistic' insofar as it normally asks how men would act if they were being ideally rational in pursuit of purely economic goals. It does so in order (i) to be able to understand men's real actions, shaped as they are, at least in part, by traditional restraints, emotional impulses, errors and the influence of non-economic purposes and considerations, to the extent that they are also affected by the rational pursuit of economic goals; but also (ii) to facilitate knowledge of their real motives by making use of this very deviation of the actual course of events from the ideal type. An ideal-typical model of consistently mystical and other-worldly attitude to life … would have to proceed in exactly the same way. The more sharply and clearly constructed the ideal types are - in other words the more unrealistic they are in this sense - the better they perform their function, which is terminological and classifactory as well as heuristic.

From the methodological point of view, the only choice is often between a terminology which is not clear at all and one which is clear but unrealistic and 'ideal-typical'. In this situation, however, the latter sort of terminology is scientifically preferable" (Weber in Gordon, 1991: 474).

Weber argued that no scientific system is ever capable of reproducing the infinite diversity of particular phenomena in concrete reality, and that all science involved selection and abstraction. The social scientist in particular is faced with a dilemma in his choice of conceptual apparatus. Coser highlights this dilemma by stating that if the social scientist selects very general concepts he is likely to leave out what is most distinctive in the phenomena under study, whereas if he particularises the phenomena he allows no room for comparison with related phenomena (Ibid. 223). The ideal type was meant to provide a means of overcoming this dilemma by serving as a measuring rod, "to ascertain similarities as well as deviations in concrete cases. It provides the basic method for comparative study" (Ibid. 223). Thus `bureaucracy' is an ideal type, a form of organisation in which everything is done according to the rules, everyone has a

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strict position, there is a clear chain of command, and so forth. Of course most organisations never really fit this model, and Weber was quite aware of it. Hence he pairs bureaucracy with another ideal type, 'patrimonialism', which is a form of organisation that is distinctly unbureaucratic, centred around personal networks and cliques. He can then characterise various states as fluctuating somewhere between patrimonialism and bureaucracy, and show the conditions that pushes it toward one end of the continuum or the other. History, in short, is an endless flux of particulars that we can never grasp in entirety. By using the device of ideal types, we can pin it down between certain fixed reference points.

Thomas Burger, in his analysis of the ideal type, states that there are three interrelated aspects of ideal-typical constructs, which have to be taken into account. These are their logical character, their content, and their function in research. With regard to its logical character an ideal type is a concept of several relatively similar and complex phenomena. Burger cites the example of the concept "bureaucracy" which refers to a set of empirical phenomena, which to varying extents exhibit bureaucratic features. The conceptual content is thus abstracted from empirical reality in an exaggerated fashion. With regard to its substantive content, an ideal type describes certain kinds of norms and plans which individuals decide to follow, and the acts and thoughts, which follow from these decisions. Finally as to its function in research, Burger states that an ideal type is "a heuristic device for - among other things - finding out in a specific empirical case by what motives the actors in question were guided in their actions" (Burger, 1976: 155). All three these aspects must be taken into account when answering the question, "what is an ideal type?"

What is clear is that ideal types do not provide a description of any set of empirical phenomena but purely of phenomena, which would exist if people always decided to act in certain ways in certain kinds of situations. They are constructed in such a fashion that empirical occurrences similar to those in the model are possible, and frequently empirical situations exist which exhibit the

same features as the model. It is precisely such cases, which will suggest the construction of the model. So, the ideal type solves two empirical research problems. Firstly, it provides some way to sort out reality, to cut down the number of things we must notice in complex situations. Secondly, it provides a basis for judging whether certain important variables exist in the situation being studied. Ideal types are consequently not merely ends in themselves, simple labels for reality, but also form the variables we use in empirical studies. The main condition, however, is that such constructs should not be mistaken for accurate descriptive or explanatory accounts of empirical phenomena. It would seem that Weber managed to avoid this problem due to his acute awareness of the precarious methodological status of ideal-typical constructions.

In all instances the ideal type is a means rather than an end, and the end of a science of culture is always to understand subjective meanings; that is to understand the meaning people have given to their existence. These subjective meanings are by no means obvious. Aron (1967: 10), in his analysis, states that "in Weber, the aim is always to understand life as it is lived, and his orientation of scientific curiosity probably results from the relation that exists in Weber's thought, and particularly in his epistemological theory, between knowledge and action. One of the fundamental themes in Weberian thought is the antithesis between *Werturteil*, or value judgement, and *Wertbeziehung*, or value reference" (Ibid. 10). Men make value judgements, they create values, and historical existence in Weber's view is essentially a creation and affirmation of values. Hence sociology is a science of culture, a science of comprehension of the existences that are defined by the creation of values. Weberian science is defined, therefore, by an effort to understand and explain the values men have believed in, and to explain and understand the works produced by men. How can there be an objective science, one not distorted by our value judgements, of the value-charged productions of men? This is the central question Weber asked himself and tried to answer.

Weber's concept of value-relevance

Without a desire to serve some personal, cultural, moral or political interests over others, social scientists would have no reason to teach or write at all. "An attitude of moral indifference", Weber writes, "has no connection with scientific objectivity" (Weber, 1949: 60). Consequently values have to be implicated in the practice of the social sciences for the practice to be rational and purposeful. Though the social sciences should be value-free, they should also be value-relevant. Weber writes:

> "The problems of the empirical disciplines are, of course, to be solved `non-evaluatively'. They are not problems of evaluation. But the problems of the social sciences are selected by the value-relevance of the phenomena treated ....It should only be recalled that the expression `relevance to values' refers simply to the philosophical interpretation of that specifically scientific `interest' which determines the selection of a given subject matter and the problems of empirical analysis (Weber, 1949: 21).

> It is due to the evaluative ideas with which he unconsciously approaches his subject matter, that he has selected from an absolute infinity a tiny portion with the study of which he concerns himself....To be sure, without the investigator's evaluate ideas, there would be no principle of selection of subject-matter and no meaningful knowledge of the concrete reality. Just as without the investigator's conviction regarding the significance of particular cultural facts, every attempt to analyse concrete reality is absolutely meaningless, so the direction of his personal belief, the refraction of values in the prism of his mind, gives direction to his work" (Ibid. 82).

The phrase "relevance to value" refers to the values that lead social scientists to

select one question or study over another. This value orientation appears to operate at a number of levels, incorporating the values of a culture (what is deemed significant or otherwise), the values of a discipline such as sociology, and the values of the observer such as the sociologist. The implications of this position would seem to include the view that what is conceived of as historical reality changes as a result of cultural change, and that the field of knowledge in social science changes with the historical process itself. Weber's position departs radically from the form of positivism that views social science as a linear, cumulative process. Far from a value orientation creating a subjective barrier to the acquisition of valid historical knowledge, it is the indispensable means of acquiring any historical knowledge at all. As Michael Root (1993:36) states,

> "when Weber speaks of value-relevance, he has more in mind than the values that rationalise the choice of question asked. He is also thinking of the values that rationalise the way we ask the questions and the direction we take in answering them".

Values, then, not only give scientific work direction, they also give it content. Values do not only determine what social phenomena the social scientist chooses to study, they also determine the point of view from which he studies the phenomena, and hence how the phenomena is theorised and explained. How then is the social scientist to achieve universally valid statements that are not merely and purely subjective? Weber's answer seems to point in the direction of the procedures of an empiricist research strategy, to which I will return shortly. For now, suffice it to say that in Weber's view the social sciences must not only be value-relevant, but also be value-free. Yet how is it possible for studies in the social sciences to be both value-relevant and value-free?

Value-freedom

The psychological sense of `subjective' is not usually what people have in mind

when they say that social science is subjective and natural science is not. What is usually meant is that the findings of the social sciences are biased and unreliable. Closely connected to the claims about the subjectivity of the social sciences in this sense is the thesis that values influence the findings of social scientific inquiry. Using the adjective 'subjective' in the English rendering of Verstehen is legitimate, but it does not mean that Weber opposed the notion that the study of social phenomena can be objective. Though Weber presents a complex position on the objectivity of the social sciences, he embraced the criterion of objectivity without reserve and advocated it more passionately and at greater length in his writings than many other major figures in the history of social science. Freedom from value judgements - "Wertfreiheit" - was, for Weber, not merely a possibility but a necessity in social science. In consonance with this view, he embraced the fact-value dichotomy, which was a notable feature of the reorientation of Western thought during the era of the Enlightenment. (see Gordon, 1991: 489-493).

What, then, is the nature of the distinction between facts and values? As stated by Scott Gordon, "Weber held, as David Hume had, that this is a logical dichotomy, not an ontological one" (Ibid.490). Reality is not divided into two realms that cannot interact. Values can of course affect facts, and facts can affect values in the real world, but, as stated by Scott Gordon, "a statement in the subjunctive mood does not have the same semantic status as one in the indicative mood" (Ibid. 490). To say that a person holds certain values is not equivalent to saying that he is morally correct or incorrect in holding them. What Weber insisted on is that social scientists should maintain a clear distinction between *is* statements and *ought* statements.

Weber did not intend to exclude values from the subject matter of sociology. He rejected psychological explanations of social phenomena because they construe persons as responding more or less passively to stimuli, whereas, in his view, the fundamental character of human behaviour is that it is consciously directed at

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the attainment of specific goals, and these goals reflect value judgements. The values that men hold are consequently facts, which the sociologist must take into account in his analysis of social phenomena. Weber also does not claim that the sociologist makes no value judgements of his own. On the contrary, as we have seen, he argues that in choosing topics for research, or in offering advice on social policy, for that matter, the sociologist is unavoidably involved in making value judgements. How then, one may ask, is social scientific objectivity attained? For, an attitude of moral indifference has no connection with scientific objectivity in Weber's view. Objectivity in sociology consists in making it plain when one is speaking about facts and when one is resorting to value judgements of one's own.

But differentiating between factual statements and moral statements is a great deal easier to accomplish when one is speaking abstractly than when one is engaged in the practical work of social science. How then is one to differentiate between facts and values on the level of methodology? For if the instruments of empirical investigation themselves contain value judgements, the results they provide will necessarily be a mixture of facts and values that cannot be disentangled. Weber's contention in this regard is that his methodology has no normative implications, that it does not involve any commitment to a moral value or political philosophy. The concept of ideal-types is held to be devoid of any normative notion of 'ideal'.

The social scientist constructs the ideal-type for certain heuristic purposes. Their usefulness is not assessed on the basis of their fit with the cultural phenomena or empirical reality at hand, but on how well they make the significance of the phenomena clear, and how well they make the causal relations between phenomena clear. Like many other commentators on Weber, Michael Root (1993:36) makes the point that by recommending that social scientists employ ideal-types in their studies, Weber is opposing 'scientific naturalism'. The goal of both natural and social science for the naturalist is to reduce reality to laws, and

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to construct concepts in such a manner that the concepts should be a mirror to empirical reality. In this regard Weber does not fit the traditional conception of the positivist mould. He offers an alternative epistemology for the social sciences in which concepts are not merely convenient *names* which we coin in order to mirror social phenomena, but analytical instruments, or heuristic tools, for understanding the meaning of, and causal relations between, elements of social life. For, as postulated previously in this text, Weber conceives explanation as a matter of interpreting facts in light of their meaning for the subjects of science rather than subsuming facts under general laws.

In Weber's view then, the ideal-type is chosen on the basis of a judgement of value, but the choice is value-relevant, not value-laden. Ideal-types attain this status of value-neutrality because their use is not a claim that individuals or collectivities ought to conform to the type. They are concepts with which reality is compared, not ideals by which reality is judged. As succinctly stated by Weber: "There are ideal-types of brothels as well as religions" (1949: 69).

According to this old logic of the methodology of social science employed by Weber, evaluations are to be excluded from scientific explanations for two reasons: they introduce 'normative biases', and they cannot provide the basis for empirical-causal explanations. The exclusion of evaluation leads Weber to the seemingly anti-critical doctrine of the 'value-neutrality' of the social sciences. Yet value-neutrality in Weber's sense does not exclude the possibility of social criticism, but rather only limits its scope. In his methodological writings Weber repeatedly criticises the positivists of his day, yet even if in accordance with his methodology social scientists do not make value judgements, their analyses must still be value-related. It is often not clear how this is possible without making judgements.

Concluding remarks

Weber, perhaps the most influential social scientist of the twentieth century, presents a complex position on the objectivity of the social sciences. In many ways, Weber can be seen to have attempted to bridge the gap between those who believe that the social sciences should attempt to follow the path of the natural sciences, and those who argue that the study of man as a social being is entirely distinct from other sciences. Weber argued that in the case of the social sciences the facts do not speak for themselves. As cultural beings studying other cultural beings, the researcher poses other sorts of questions, and applies other methodological devices, and hopes to gain different sorts of knowledge from those characteristically associated with natural sciences. All knowledge of concrete social reality, Weber says, is from particular points of view. Yet, though the social scientist's values influence the construction of the conceptual scheme that is used in an investigation, they need not influence its objectivity. On the one hand, he insists upon the need to use theoretical concepts and empirical evidence the way the natural scientists do, but he viewed the social scientist as also engaged in a special process of Verstehen which supplements the mode of explanation of the natural sciences, but does not replace it. As stated by W.G. Runciman (see Gordon,1991: 468), "Weber's position can be construed as a self-conscious and deliberate attempt to have it both ways".

Weber was also willing to contemplate the possibility of conflicting explanations and theories for a given phenomenon or historical case, yet he was passionately concerned to establish sociology as an "objective" social science. He was outspoken against the practice of using the university lecture hall as a platform for political propaganda and was equally insistent that political commitments should not be allowed to intrude in scientific research. At the same time Weber was aware that attempts to understand an individual action or the historical development of a society could only be partial and based upon an incomplete state of knowledge. Because it is only possible to grasp the sum total of the causes of any unique event

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by replicating the whole of the reality which was antecedent to it, the social scientist must choose those factors which are deemed the most important.

Whatever method or theory we use can only impose an order or reality, not exhaust it. We can consequently not deduce reality from a set of *a priori* theories and concepts, but must apply an empirical approach to sociological research. Objectivity can be ensured only through the greatest possible precision in our research method. Paradoxically, the choice of factors to be given attention will be based on the theoretical problems of the social scientist, and hence the selection of a research agenda is value-laden. This tension prevented Weber from aligning himself wholly with either the nomothetic view (that the social sciences could simply apply the methods of the natural sciences), or with the idealist tradition in German philosophy (that the free will of individuals always introduced an element of unpredictability). While accepting that nomothetic propositions might be possible in sociology, they could never constitute a complete account of human agency. This called for a special approach to human behaviour; one that recognises that because people possess a free will, they can choose the goals they wish to pursue and social science cannot specify in advance what these goals will be.

Causal explanations will be incomplete unless we take this characteristic of goal-directness or value relevance into account. That is why general laws or nomothetic explanations in the social sciences will be incomplete. On the other hand, Weber did not agree with the idealists that the hallmark of the freedom of the human will is the unpredictability and irrationality in our behaviour. On the contrary, our actions have the greatest freedom when they are simultaneously at their most predictable, that is, when they are the result of calculated rational choice. We are of course under no compulsion to share this assumption. As stated by Ira Cohen (see Turner, 1996: 114)

> "more recent theorists of action, especially John Dewey and Anthony Giddens, virtually stand Weber's definition of action on its head by proposing that taken-for-granted forms of conduct provide a basis for

many ordinary forms of action, while existential meaning surfaces primarily during critical periods in which routines break down".

It should be noted, however, that in many instances of research it may be neither necessary nor feasible to follow Weber's path, but whatever strategy a scholar may want to adopt, it should be informed by a clear understanding of what Weber and others have tried to accomplish in the past. Such understanding may be useful for the clarification of our own purposes.

Weber's discussion of the nature of objectivity in the social sciences can, in conclusion, be seen as an attempt to dispel the confusions that, in his view, surround the relevance of value judgements. As discussed previously, Weber does not advocate "moral indifference", the elimination of ideals from scientific discussion, but that social scientists be as clear as possible about their own values and ideals and their relevance to their work. The crucial problem for methodology is to define the limits to the intrusion of ideals into scientific analysis so as to avoid the danger of total subjectivity.

These limits can be understood by reference to the term value-orientation that Weber borrowed from his contemporary Rickert. "Value-orientation" is both a limit on social science and the factor which makes it possible. Each new value-orientation can shed some light upon empirical reality, and when new value-orientations arise out of contemporary societal problems, fresh questions are asked of society. Sociological theory can therefore be regarded as the result of an ongoing dialectic between society and the sociologist. Our ideals determine the problems to be studied and act as the source of theories and explanations that can then be verified, like any other scientific analysis.

As stated by David Lee and Howard Newton (1989: 170),

> "Weber's methodological writings constitute attempts to reconcile apparent polar opposites: idealism and the scientific method;

political commitment and objectivity in the social sciences; individualism and structured social action. It is because these dilemmas remain with us today, rather than because of the success of Weber in resolving them, that Weber's work continues to receive so much attention".

There can, however, be no doubt that Weber provided essential clues as to how sociologists can access the substance of social life, and his example provides a substantive example to those who prefer an interpretative sociology to rigorous positivism.

James Bohman (1991: 189), in an essay on criticism and explanation, raises the point that much of social science is practical and value-related in quite a different sense. It is practical in the technical sense that its explanations supply better means to solve problems. From Marx forward many social theorists have attempted to develop a stronger notion of the critical and practical purposes of social science. Yet Weber argued for a much more limited conception on the grounds that criticism is theory-laden and theory-dependent, and that this limits what social science can do. The nature and limits of theoretical knowledge in the social sciences is of course an epistemological issue, and Bohman argues that "limits on criticism are often limits on theory, as is the case for Weber" (Ibid. 189).

Weber attempts to exclude certain types of values in his explanation of social phenomena, specifically those values he refers to as judgements. According to Weber reference to values is necessary in an explanation of social actions because such actions are end-directed. Bohman argues that, as a social action, criticism is itself also oriented to a particular end, that is, to change the attitudes and beliefs of members of a society (Ibid. 189). Weber would retort that no substantive value rationality can be achieved, and that values are ultimately a matter of mere choice or decision. Bohman, however, introduces an interesting alternative which he derives from his reading of Habermas, that "the concept of norms, not values, is the proper way to characterise the moral aspects of social

reality in that it points to the structuring of social encounters by shared knowledge, expectations, and rules" (Ibid. 190). Criticism in this view is not merely a relatively arbitrary choice between moral ends, but an act of communication. States Bohman, "The contrast between Weber and Habermas can clarify one main point - namely, that the distinction between facts and values is an outcome, and not a presupposition, of the commitments of a social theory" (Ibid. 190). Such a view points to the possibility that critical theory helps us reflect better on our situation and improve things not by controlling a domain of phenomena but by changing beliefs and attitudes. This notion will be further explicated in the final and concluding chapter.

Chapter 2

The social construction of scientific discourse

Weber emphasised the empirical nature of social scientific practice, and the objectivity of the knowledge that it furnishes. Though, as will be argued in this thesis, this is a defensible view of the ideal practice of science, it would be foolish to claim that scientists are detached from their particular cultural environments. Does the cultural environment of their times impact on the validity of their findings? If science is a social phenomenon, are scientific beliefs different from any other beliefs? These questions have been raised in an area of research called the 'sociology of science', and consequently this chapter will trace prominent contributions to this debate, through the various conceptions of science and knowledge in the work of Thomas Kuhn, the 'strong programme' in the sociology of science, and the ethnographic fieldwork of Bruno Latour and Steve Woolgar. The final section of the chapter will briefly address the relationship between science and power, and specifically the notion of knowledge/power in the work of Foucault. Throughout the chapter comparisons are drawn between these various contributions to the study of science, and similarities and divergences are addressed.

Introduction

The modern mind has demanded a specific type of interpretation of the world: its scientific method has required explanations of phenomena that are concretely predictive, and therefore impersonal, mechanistic and structural. To this end, explanations have been systematically "cleansed" of all human and subjective qualities. In Ernest Gellner's words, "it was Kant's merit to see that this compulsion for mechanistic impersonal explanations is in us, not in things" (Gellner, 1975: 206-207). And "it was Weber's to see that it is historically a

specific kind of mind, not human mind as such, that is subject to this compulsion" (Ibid. 207).

The historical specificity of knowledge is the fundamental problem raised by Kuhn - the problem of explaining why in the history of science one paradigm is chosen over another if paradigms are ultimately incommensurable, that is, if they cannot be rigorously compared. As Kuhn has pointed out, each paradigm tends to create its own data and its own way of interpreting those data in a manner that is so comprehensive and self-validating that scientists operating within different paradigms seem to exist in altogether different worlds. Nor does any consensus exist among scientists concerning a common measure or value that could be used as a universal standard of comparison. Kuhn has argued that ultimately the decision regarding the validity of knowledge lies with the ongoing scientific community, which provides the final basis of justification. Yet, as many scientists have pointed out, this answer seems to undercut the very foundation of the scientific enterprise, leaving it to the mercy of sociological and personal factors that subjectively distorts the scientific judgement.

Inspired by Kuhn a flood of detailed historical and sociological studies of the practice of natural science have come to light. These studies borrowed methodologies from a variety of sources, including the sociology of knowledge (Barry Barnes and David Bloor), and ethnographic fieldwork (Bruno Latour and Steve Woolgar). In contrast to the traditional epistemological debates about science in which science is idealised and rooted in rational foundations, these studies describe what actually takes place in science laboratories. Such descriptions have an empirical and historical orientation, rather than a philosophical one. It is claimed that the ignorance of most social scientists regarding the basic features of natural scientific activity is no greater than that which is routinely faced by sociologists studying other forms of culture. It is further claimed that any analysis of knowledge must incorporate its inherently theoretical and constructed character. There is no basic pattern or structure

inherent in the natural world. Barry Barnes and David Edge, the editors of a book entitled "Science in context", argue that "nature can be patterned in different ways: it will tolerate many different orderings without protest", and that none of these orderings is self-sustaining, and therefore, "specific orderings are constructed not revealed, invented rather than discovered" (Barnes & Edge, 1982: 5). Hence knowledge has a conventional character, and science is a sub-culture or set of sub-cultures. More importantly, "science cannot simply be assumed to be an independent, external agency, pumping expertise into the social order" (Ibid. 8). Contrary to the position adopted by Max Weber, the notion that science is or can be protected against the intrusion of external influences is emphatically challenged. The barrier which protects science from such influences, whether a rational commitment to scientific method, or a strongly sanctioned normative order which is definitive of science, is rejected. According to Barnes and Edge, "the evidence presently available suggest that 'external' influences upon scientific judgement are neither unusual or necessarily pathological, and that the barrier which such influences have to penetrate is not fundamentally different from the boundaries surrounding other sub-cultures (Ibid. 9).

Sociologists of scientific knowledge, it is argued, should use the same procedures to explain "true" and "false" scientific beliefs, practising an ethnographic indifference to the content of the scientific claims made by the researcher they studied (Turner (ed.), 1996: 91). It can of course still be claimed, as it was by Popper in response to Kuhn, that such accounts of the messy practice of science had no implications for the prescriptions of the philosophy of good science.

Thomas Kuhn

In recent years, historians and philosophers of science have paid increasing

attention to the social context of science, a field previously traversed by only a few sociologists, such as Robert Merton. During these years no theory of the nature of science has received more attention than Kuhn's. Kuhn's "The Structure of Scientific Revolutions" was a bold attempt to unite the history of science, the philosophy of science, and the sociology of science into a theory of scientific development. Kuhn's well-known thesis is that the history of science reveals two alternating phases, a period of 'normal science' and a period of 'revolution'. During the first of these scientists proceed with their work within the frame of the established basic conceptions or 'paradigm' of the peer group. But, as scientific investigation proceeds, bits of empirical information come forward that are not consistent with the accepted paradigm. Initially, scientists do not worry about such apparent falsifications of the basic conceptual framework with which they are working, but as the anomalies accumulate the established paradigm becomes increasingly untenable. Eventually, it is cast out by a 'revolution' in scientific thinking, a new paradigm is adopted, and the `normal' work of science resumes. Historians of science have been very critical of the empirical value of Kuhn's central notions of 'paradigm' and 'revolution', and are not inclined to accept his model as a satisfactory depiction of the actual history of science. As stated by Scott Gordon, "in effect, Kuhn was attempting to state a universal 'law of history', and his thesis, like other similar propositions about history, is more speculative than empirical" (Gordon, 1991: 616).

Kuhn conceptualises a paradigm shift as akin to a religious conversion, that scientists are 'born again' and look at the world through new eyes. As different paradigms are incommensurable there are no general criteria or common measures that can be used to determine whether one paradigm is better than another is. Consequently Kuhn diverges radically from Popper regarding the question of progress in scientific knowledge, or even the possibility of differentiating scientific from non-scientific propositions. Convention becomes the sole criteria of validity. Good science is consequently conceived as being in accord with the paradigm convention of the scientific peer group, and when that

convention changes, it becomes bad science.

In response to his theory, a deluge of criticism was brought down on Kuhn. The main thrust of this criticism pointed out that his conception deprives science of any claim to being an empirically controlled method of objective inquiry, or even one that is rational. Scott Gordon points out that, as a consequence, Kuhn significantly modified his original position, arguing that a paradigm is not such an autonomous ontological conception that it is immune from empirical tests that scientists routinely apply, nor that paradigms are absolutely incommensurable and immune to the usual epistemic criteria of theory choice. States Gordon,

> "with such admissions, however, Kuhn's theory of science falls to the ground. A 'paradigm' becomes merely a theoretical hypothesis, perhaps one that is more central to a field of science than others, but not differing from them in any fundamental way. A 'revolution' in science becomes simply a period of exceptionally rapid advance, initiated by discoveries that prove to be unusually fruitful in the investigation of old problems or in opening up new lines of scientific inquiry" (Ibid. 617).

Despite Kuhn's partial retractions and qualifications, the notion of a paradigm became extremely popular amongst social scientists and is still popular today. Though Kuhn may have overstated the ontological autonomy of paradigmatic propositions, he did usefully point to the fact that in some fields of science core propositions are adhered to despite evidence to the contrary. The science of economics is a case in point, for its adherence to a conception of consumers and producers as rational utilitarian agents, in spite of conflicting empirical experience and psychological theories such as Freud's regarding the non-rational foundations of human actions. More incisive perhaps is the fact that Kuhn drew attention to the social nature of science, more especially the role of peer groups as the basis of established authority. It is this aspect of Kuhn's work that became the impetus behind the Edinburgh School's strong programme in

the sociology of science.

The 'Strong Programme' in the Sociology of Science

It is no longer just that there are doubts as to whether the social sciences can be more like the natural sciences, it is widely recognised that the natural sciences are much more deeply culturally and theoretically constructed than traditional and textbook accounts of science have suggested.

It would be foolish to claim that scientists are totally detached from their particular cultural environments. As such the 'sociology of science' explores a part of our culture and history that is of great importance, indeed of growing importance, as the role of science in all societies widens and deepens. This form of research has recently received considerable attention. In its boldest form this area of research claims that the attempt of philosophers to establish the epistemological foundations of science are misguided. Science is a social phenomenon that should be studied by sociological methods just as any other social phenomena, be it mores, religion, deviance, etc.

The notion that external factors, such as political, social, or economic environment play a significant role in science has a long history, especially with respect to the social sciences. The leading figures of the 'Edinburg School', David Bloor and Barry Barnes, carry this notion a step further. In their view scientists are dominated by their cultural ambience in all aspects of their work and thought. It determines not only their choices of problems to investigate, but their philosophical conception of the nature of science and the criteria of warrantablility that they use in evaluating beliefs. All beliefs are considered epistemically 'symmetrical', whether they are beliefs about observable phenomena, philosophical principles, or the power of witchcraft. As stated by Barnes and Bloor (see Hollis & Lukes (eds.), 1982: 23), "it is not that all beliefs

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are equally true or equally false, but that regardless of truth and falsity the fact of their credibility is to be seen as equally problematic". In a book entitled Interests and the growth of knowledge (1977: 25), Barnes states that "what matters is that we recognise the sociological equivalence of different knowledge claims". Consequently science, which are sets of beliefs like any other, should not make pretensions to rational detachment, and is construed as a purely empirical social phenomenon. The empirical nature of this form of inquiry thus leads to the claim by Bloor and Barnes that they are practising a "science of science".

David Bloor, in an essay entitled "A Sociological Theory of Objectivity" (see Brown, 1984: 229-245), proposes the theory that objectivity is social. The impersonal and stable character of some of our beliefs is seen to derive from the fact that beliefs are social institutions. In Durkheimian fashion beliefs that are objective do not belong to any individual. They have an external thing-like aspect to them, and a theory of objectivity must address the object-like stability of the things we believe in, "the external, compelling character of the standards, rules and procedures that we use" (Ibid. 229). The specifications that something must meet in order to merit the title 'objective' is accounted for by reference to social institutions. Taken-for-granted practices are sanctioned by a group, they are shared, and have the quality of being external to the individual. This leads to the identification of the objective with the social, not as a deduction, but as a conjecture with 'suggestive power' and 'problem-solving capacity' (Ibid. 229).

In a comparison with Popper's treatment of the same subject, Bloor interprets Popper's 'world three', the objective world of intelligibles, as the social world. This world for which Popper claims 'relative autonomy', is really the social world, though in Bloor's opinion, Popper's metaphysical terminology obscures this fact. Bloor emphasises that his reading of Popper is not meant to be true to Popper's intentions, and ironically, Popper himself could obviously not defend his theory in terms of subjective intentions and remain internally consistent. Be that as it may, Popper's claim is that objective knowledge is 'knowledge without a knowing

subject'. Bloor takes this to mean that "objective knowledge refers to something like the state of a discipline, or the state of culture, at any given time" (Ibid. 232). The discipline of physics, for instance, does not refer to what any individual physicist knows, but to the "entire corpus of standards, conventions, paradigms, accepted results and procedures", and consequently, physics is the "property of the collectivity and the role" (Ibid. 232). Thus Popper's claim that the activity of understanding consists in operating with third world objects "becomes the claim that our intellectual operations proceed by the use of socially given categories and socially shared meanings" (Ibid. 233).

A defender of Popper's theory would argue that our beliefs and assertions have logical implications, which is a vitally important feature of knowledge, and that this feature can consequently not be explained by a social theory. Bloor claims to do so is to postulate a quasi-autonomous world of objective knowledge, which exists independently of our constructions and which can be discovered. Just as "the eye can literally see a physical object, he says, so the mind can similarly 'see' intellectual objects" (Ibid. 234). Bloor's contention is that this does not solve the problem of how to know whether we are seeing the right object, and that the talk of discovery conceals the act of <u>creation</u>.

Bloor is of course drawing on the work of Wittgenstein, and more pertinently the well-known work of Peter Winch, as a resource. Wittgenstein's analysis of meaning and rule-following, and his observation that "no course of action could be determined by a rule, because every course of action can be made to accord with the rule", leads to Bloor's assertion that "ultimately it is action which determines meaning, not meaning which determines action" (Ibid. 235). Consequently, differences in social organisation ought to be connected with the differences in how knowledge is organised. If something is objective by virtue of its being a social institution, then variations in objectivity should be reducible to, and located in, variations in the institutions that give rise to knowledge. The idea that objectivity resides in experience and that objective knowledge can be known

directly and immediately, raises the question of <u>who</u> has believed that reality is directly given in experience, and <u>why</u> they have believed it. Bloor contends that these questions reveal that empiricism has functioned as the ideology of scientific professionalism due to two explanatory principles, namely, the social use of nature, and the idea of group interests, on which I will elaborate in the following section.

If the causes of the variation in objectivity are social, then they are whatever moves people to try to alter their institutions. According to Bloor "the manner and character of its variation are, accordingly, defined and limited by the range of options that we have in organising our social life. If these are endless, then the forms of knowledge will be endless. If they are limited and revolve around a small range of basic possibilities, then so will the forms of objectivity. These are matters for further study" (Ibid. 245). This theme has a lot in common with the postmodernist conception of local knowledges, yet surprisingly, and contrary to postmodernism, the Edinburgh school is unashamedly naturalistic. As stated by James Brown, "the main point is that the sociologist is a scientist too, and ought to act as scientists do; he or she should try to characterise knowledge in a scientific fashion. 'If sociology could not be applied in a thorough-going way to scientific knowledge it would mean' concludes Bloor, "that science could not scientifically know itself" (Ibid. 10 -11).

In this regard Barry Barnes, in a text entitled "Interests and the growth of knowledge", distinguishes the orientation of the philosopher or epistemologist from that of the sociologist. The sociologist is concerned with "the naturalistic understanding of what people take to be knowledge, and not with the evaluative assessment of what deserves so to be taken" (Barnes, 1977: 7). Knowledge here is understood 'naturalistically' in terms of causes, and not evaluatively in terms of good or bad, appropriate or inappropriate reasons. All supposedly internal, normative questions cannot be distinguished from external causal ones. This conception of explanation is surprisingly empiricist, considering that Kuhn's

account of scientific revolutions is taken as a guide. Barnes and Bloor repeatedly state that the sociology of knowledge should give strictly causal explanations of beliefs which are "concerned with the conditions which bring about beliefs or states of knowledge" (Bloor, 1976: 4-5). Bloor elaborates four tenets of a causal, anti-normative approach:

"1. CAUSALITY. It would be causal, that is concerned with the conditions, which bring about belief or states of knowledge. Naturally there will be other types of causes apart from social ones which will cooperate in bringing about belief.

2. IMPARTIALITY. It would be impartial with respect to truth and falsity, rationality or irrationality, success or failure. Both sides of these dichotomies will require explanation.

3. SYMMETRY. It would be symmetrical in its style of explanation. The same types of cause would explain, say, true and false beliefs.

4. REFLEXIVITY. It would be reflexive. In principle its patterns of explanation would have to be applicable to sociology itself. Like the requirement of symmetry this is a response to the need to seek for general explanations. It is an obvious requirement of principle; otherwise sociology would be a standing refutation of its own theories" (Bloor, 1984: 10).

The conditions which bring about beliefs are conceived by Bloor as primarily, though not exclusively, social, having to do with what is called 'interests', rather than internalised norms as in the case of Talcott Parsons. In this view the problem with past sociologies of knowledge is that they exempted science from proper analysis by claiming that it is interest-free. Even the sociology of knowledge itself should, reflexively, be explained causally. Their naturalistic and empiricist orientation leads to a view that science is the best way to explain both

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the social and the natural sciences, and that good science explains things causally.

James Bohman, in his analysis of the 'strong programme', argues that Barnes' and Bloor's appeal to the empirical successes of their studies, in order to silence philosophical critics, provide the opposite effect of what they desire to indicate. Says Bohman, these empiricist studies "provide good empirical reasons to reject the programme's causal approach, insofar as they fail not only to explain the necessary and sufficient conditions for scientific beliefs, but even to show clear explanatory connections between natural knowledge and social contexts" (Bohman, 1991: 41).

Barnes and Bloor may well answer that all theories are underdetermined by the evidence, and that theories are empirically equivalent, that the choice between them must be determined on grounds other than rational ones, for reasons other than the appeal to evidence. According to Bloor, rationally justified beliefs must still be explained, and the same processes cause true and false beliefs. Causal explanations of the practice of science are required not because of the internalisation of norms, but because of what Barnes and Bloor call the 'symmetry principle'. As stated by Bohman, "the symmetry referred to is between rational and irrational belief formation: there is no significant difference between explanations of true and false beliefs (Ibid. 41). Causal explanation is not confined to 'bad science', tainted by external factors which lead to wrong conclusions, because the same sort of causes generate both good and bad science. Consequently, since rational evaluation is indeterminate in all cases, the `real reason' that scientific beliefs are adopted must be explained by sociological causes such as interests, pressures and forces, which are naturalistically described by Barnes and Bloor. What is, however, not clear, is why the unmasking of interests is necessary if evaluative criteria are insignificant.

Bohman comments that the 'strong programme' is methodologically doomed to failure, that "its explanations will always be indeterminate and fall short of giving the required necessary and sufficient conditions for belief formation", that social interests or forces "cannot form the basis of an adequate account of determining causes for intentional actions in scientific practices, even granting the symmetry principle" (Ibid. 43).   What is clear is that the explanations of the strong programme rest on the assumption of a strong tendency to conformity in human behaviour. They have, however, not formulated the process of socialisation or the formation of class identity, which might account for how many factors work determinately enough to be explanations. Their conception of science seems to be that of a coercive, controlling institution, both through force and authority. By rejecting the internal perspective of practitioners in science as being inadequate for explanations, the strong programme overlooks an important source, the explanations of reflective participants in practices.

We are left with a theoretical hypothesis which does not consistently apply its own empiricism. The criteria of theory choice are presented as no more than social conventions that scientists have be enculturated to accept. As Scott Gordon points out, "it does not demonstrate that the Newtonian theory of the planetary system is merely a social convention of Western-educated astronomers" (Gordon, 1991: 622). "The contention that beliefs have causes does not mean that all beliefs have the same causes, much less that `social factors' are the only causes that operate in the domain of human mentation" (Ibid. 622).

The ethnography of science: Bruno Latour and Steve Woolgar

Bruno Latour's early collaboration with Steve Woolgar, "Laboratory Life", was the

first significant laboratory field study of scientific practice. Woolgar and Latour describe their analysis as an "ethnography of the sciences", using methods of participant observation in an actual laboratory setting. The term 'anthropology' or 'ethnography' is defined as taking a particular perspective, that of a stranger. Because of its phenomenological background, ethnomethodology pursues a methodological ideal of pure 'documentary' description, and in common with the 'strong programme', emphasises the active character of social judgements. What is considered important is what members of a setting actually do, not what they think they do. Detailed descriptions of the actual practice of science reveal explanations of unnoticed aspects of everyday activities. The details of the description often contradict what scientists themselves believe about their own activities, and as such description itself can become critical.

In an interview with T. Hugh Crawford, Latour points out what he considers to be a serious flaw in the 'strong programme', the proposed symmetrical approach to the study of science and society. He acknowledges his debt to Barnes and Bloor for showing that "we should not explain false belief and accepted truth in different ways", "but it is only halfway done, because the symmetry is obtained by giving a social explanation of both true knowledge and false belief" (File: ///A/Latour.HTM).

Latour's argument is that though Bloor has remedied the asymmetry which explains true science with nature and false science with society, "his remedy is also an asymmetrical argument because he explains both in terms of the social" (Ibid. 6). In Bloor the social is only one half of the explanation, but the other half is not clear. Latour suggests that we follow what Michel Callon refers to as "the generalised principle of symmetry", that we treat society and nature symmetrically. Says Latour, "this new symmetry principle is much different from Bloor because Bloor is a radical Durkheimian thinker, which is to say that society 'up there' should be able to explain true and false belief in the same terms .... the inputs of nature being necessary to anchor our beliefs, but not to shape them"

(Ibid. 6).

It is Latour's contention that the social sciences have no 'out there' or 'up there' that serves as a foundation, but that the notions of 'up there' and 'out there' are themselves created in the "laboratory", a "special type of laboratory where things like the notion of force and social groups are located and then re-extended outside" (Ibid. 7). Says Latour,

> "once you have this dichotomy between the representation of humans in the political sciences and the representation of the non-human which is basically taken over by science, there are two critical tasks: one of them is defined by the first enlightenment which is to say that natural science now sees through the obscurantism of the past. The second task comes from the knowledge of the emerging social sciences, so that we can now see through the mistakes, aberrations, and arrogance of the natural sciences. Both of these tasks are to see through the naturalisation of discourse, and are grounded in our belief in the social sciences" (Ibid. 7).

There is a striking similarity between Latour's notion of the naturalization of discourse and Marx's theory of ideology in Capital. There phenomenal forms are seen to correspond to natural forms due to the opacity of reality. Ideological illusions are seen to have their origin in the phenomenal forms of reality itself, rather than due to the intention to deceive others or from self-deception. In Marx's words, phenomenal forms appear "directly and spontaneously as current modes of thought" (Mepham & Ruben (eds.), 1979: 149).

For Latour there is a complete similarity between the internal divide between the "representation" of the human and non-human, and the external divide between the cultures of the natural and social sciences. More particularly, Latour's ethnomethodology aims to show the constructed character of what scientists

take to be natural or eternal facts. His description has an alienating, defamiliarising effect, like the view of an ethnographer from another culture on our own practices. As stated by Bohman in his analysis of ethnomethodology as a form of social criticism, "what is immediate now becomes mediated, what is familiar becomes dependent on a web of shared expectations" (Bohman, 1991: 204). In this way the method of ethnographic description, turned inward, provides critical distance. Reality is not conceived as an objective fact, but as a construct, showing the socially constructed character of the objectivities of everyday life. In applying this method to scientific practices Latour provides a new, non-causal basis for the sociology of science, and consequently avoids the difficulties of the interest-based explanations of the strong programme. Though claiming to be "materialist", this approach is not concerned with macro-structural causation as in the case of the strong programme's "interests", but with how facts are produced locally in their micro-settings. Tracing their production through a purely descriptive approach paradoxically has the critical effect of changing our false beliefs about science.

In "Laboratory Life" Latour and Woolgar contend that science in its finished and polished form conceals the process of making scientific knowledge. Just as in the case of ethnomethodological studies of how gender is constructed in everyday interaction, their studies find the process of construction of facts in the laboratory to be a "contingent and negotiated order", in no way different from everyday practical activities. This contingent order is obscured by "finished science", as opposed to "science in the making". The observers of "science in the making" look beyond the self-descriptions of scientists just as anthropologists do who "refuse to bow before the knowledge of a primitive sorcerer" (Latour & Woolgar, 1979: 29).

The model of anthropology, which is employed here, is not that of interpretative anthropologists, but must claim to know more than the scientists themselves, "secluded" as they are by naïve beliefs about the "authority of science". Instead of the taken-for-granted picture we have of science as being organised and

coherent, science in the making "in fact consists of a disordered array of observations with which scientists struggle to produce order" (Latour, 1987: 36). Once the ethnographic gaze sees through the sociological micro-foundations of scientific order, we can "deconstruct" the very idea of "hard facts" and "objects", which now is "nothing but inscriptions". Says Latour, "the object now emerging is a completely new object that doesn't have the classical features of objectivity. As Serres says, it is a 'quasi-object' because it does not have the characteristics of a naturalistic object. For example, what is anthropogenic heat? Of course it is human: it is socially constructed, because it is our heat produced by our pollution; but it has the scale of the planet, so it is a natural phenomenon" (Latour, HTM: 11). Introducing the notion of quasi-objects is seen as a way of addressing the realist/constructivist debate. Redefining both nature and society as agents in association creates a way out of the dilemma of treating constructions as causes. Also, changes in society are seen to affect the natural environment, and those changes, in turn, affect society. Thus, as argued by Ulrich Beck, today "nature is society and society is also nature", with the result that nature has been politicised, and natural scientists, like social scientists, have had their work politicised (Beck in Ritzer, 1996: 576).

Surprisingly, and in stark contrast to post-modern theory, Latour assumes that we live in a non-modern world, that is, "the retrospective realisation that from the beginning of the scientific revolution, we have never been modern. These revolutions have never happened. We have never been cut off from our past; we have never been different" (Latour, HTM: 11). The notion of quasi-objects is used to cross the boundary between people and things, a concept which Latour says we have to invent and use, and ultimately drop, in order to "trace and define a social relation that is not social, and a natural relation that is not naturalised" (Ibid.12). The notion of concepts as heuristic devices is proposed because, contrary to postmodernism, we should not use culture, the content of science, or discourse as the cause of phenomenon. The idea is to avoid a "metalanguage" and trace an "infralanguage". The challenge is to give an explanation where

discourse, society and nature are the consequences and not the causes. This accords with the first rule of method in "Science in Action", that you follow the scientists as they are working and do not wait until the object or the meaning is completely constructed. The question that this raises is, of course, what is it that is following the scientists? Is the infralanguage' being used coming from the field which is studied, and not from the ethnomethodologist? If done successfully, this would mean that the terms of the explanation would make criticism of them impossible.

In common with the Edinburgh School, the "norms of inquiry" are seen to play little role in explaining actual research. The operative norms are prestige, honour, and status within a particular social network, not the disinterested search for truth. Consequently norms like objectivity are conceived more as rhetorical devices than determinants of scientific activity. Moreover, a recurrent theme in both "Laboratory Life" and "Science in Action" is the description of the political character of truth claims in science, of the process by which some claims become authoritative and immune from criticism (Bohman, 1991: 207).

Latour treats the truth claims of science in a similar fashion to the way Marx demystifies the claims of capitalism. The taken-for-granted notions of capitalism, that the economy is self-equilibrating, or that profit comes from exchange, is exposed through detailed documentary descriptions of how commodity production in capitalist societies actually works. Latour similarly tries to show descriptively that an average person would be unable to refute the claims of a scientist who has expensive instruments to produce facts, and hence raises the costs of making claims to the contrary. A close analysis of the `textual process' by which truth claims come to be accepted in the sciences, demystifies the `accepted' view of scientific discourse. Science is not viewed as a democratic and consensual endeavour, but as a highly competitive and antagonistic one, as illustrated by the defensive methods of writing and presenting a scientific paper to be objection-proof (Ibid. 208).

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Reality, or "out-there-ness", is a consequence of scientific work rather than its cause. As stated by Bohman, "reality is produced through having a claim accepted and embedded in other claims in other scientific papers; it is a reflexive accountability of social action, in the scientific community rather than any `fit' with the facts" (Ibid. 208). In intertextual fashion particular facts come to be accepted by being cited by others, they become a "black box", an unquestioned assumption, and the "black box" becomes increasingly less likely to be reopened. Moreover, resources and instruments are accumulated to defend the claim, making argument and debate more and more costly.

Clearly, laboratory studies do present a potentially powerful criticism of our accepted beliefs of scientific authority and knowledge. What is not clear is how we should change our beliefs about science and its practice. Do we conclude that science is a part of our web of beliefs, like any other belief, or does this analysis have more radical implications? The traditional conception of the research process treats the techniques, methodologies and theories of research as essentially separate from its political, organisational and administrative context. Likewise, almost all sociological methods texts assume that researchers work on their own, make their own decisions and otherwise proceed in isolation of the demands of their colleagues, their institutions and discipline. Both the Edinburgh School and Latour and Woolgar, criticise the distinction between research and its social and cultural context.

But what does it mean to talk about understanding the research process in its social and cultural context? Does it suggest that we just need to juxtapose a description of researchers at work with another description of the prevailing social circumstances? It would seem that this is decidedly not what these authors are claiming. To place the technical details of research alongside a description of social circumstance, you would still imply that the two domains are basically discrete, that they are separate from, though in some way connected

to, each other. What they explore are more profound senses in which research is social. Latour states that "you can't alternate between social realism and naturalism, and then throw in some semiotics or discourse analysis. You must do the three together; then immediately you realise that in science studies we have, all along, been studying phenomena that have the characteristics of being narrative, collective, and outside. They are quasi-objects, they are not of our own making. We build them collectively, and they are narrated. That is it: real, narrated, social" (Latour, HTM: 11).

Clearly Latour avoids the thoroughgoing naturalism of the Edinburgh School with a far more reflexive approach. What I know about an object is always shaped by how I reflexively envision it. There is no pure objectivity, nor, for that matter, is there any pure subjectivity. Everything is impure. As reflected in the post-modern concept of 'intertextuality', everything is contaminated by everything else, and in relationship to everything else. "It is merely a universe of discourse, a rhetoric based on action that is itself only discourse" (Latour in Rosenau, 1992: 111). States Rosenau,

> "this discourse takes the form of a power game, a struggle, a war with verbal negotiation, pressure, lobbying, and other elements designed to gain support, to enrol, to mobilise resources, that in the end assures an intellectual monopoly for the product" (Ibid. 111).

The claim is that no external reality exists which can serve as the ultimate arbitrator, and that in employing social science research techniques, the process of conforming to the rules leads to the scientist, in turn, being "constructed" by the research process. Rosenau cites Latour's well-known claim that "even scientific procedures such as pasteurization are said to have re-created social and intellectual life when bacteria, microbes, became social actors outside the laboratory and moved into society" (Ibid. 113). This raises the suspicion that Latour, like self-proclaimed postmodernists, can only argue about the existence of an independent reality because they are "insulated from reality, never

personally experience the violence, terror, and degradation prevalent in modern society" (Ibid. 111).

By introducing the notion of quasi-objects Latour attempts to avoid the pitfalls of a thoroughgoing constructivist argument, yet makes claims in line with a social constructivist framework by conceptualising knowledge claims, especially scientific ones, as power moves and not moves towards truth. As in the case of Foucault's work on the human sciences, Latour conceptualises natural science as a product of power and knowledge, of a politics of truth.

Yet, as will be explored in the following section, in contrast to Latour and Woolgar, Foucault's explanations make no reference to the intentions of scientists. As stated by Bohman, "the creation of ever-extending networks of power and knowledge work not because scientists do not follow the norms and ideals of their practice, but precisely because they in fact do so" (Bohman, 1991: 209). Such networks of knowledge and power, in Foucault's view, are revealed only by adopting an external perspective, as in the case of Foucault's use of power to explain the human sciences. For Latour and Woolgar, on the other hand, the test of the validity of their explanations is that the observers' and the participants' accounts must be identical, not different and external. Their claim is, however, not supported by their studies themselves, which often emphasise aspects of scientific practice which are dismissed by natural scientists as trivial or unimportant. States Bohman, this

> "puts Latour and Woolgar's criticism on the horns of an irresolvable dilemma: either they go the way of Foucault and Durkheim and deny any import to intentions and beliefs, or they must really produce an identical and internalist account, in which case there is no demystification or deconstruction of beliefs about facts" (Ibid. 210).

In fact, since the norms of inquiry are not addressed, no adequate criticisms are

advanced that could change anyone's belief about such norms. In contrast Foucault's analysis of power in the human sciences does not deny the role of norms in the explanation of research practices, on the contrary, the political nature of the norms of inquiry are produced through these very same norms. Latour and Woolgar can only offer a criticism of the practice of natural science by being selective about the aspects of participants' knowledge to which they appeal, and more importantly, by limiting their descriptions to the level of everyday knowledge. This leaves the ethnography of science, as in the case of postmodernism, open to the possibility of competing descriptions, which raises the question of the explanatory importance of the very normative elements which they do not address.

Power/Knowledge: Foucault

The analytical and empirical study of power forms a central area of social investigation. For this reason, as has often been noted, `power' has become one of the most disputed and contested of all sociological concepts. Paradoxically everyone knows what the word `power' means, until asked to formulate a precise definition. No definition of power has been able to attain universal support amongst social scientists. The reason for this is that the various conceptions of power do not simply depend on the 'facts' of the case, but on disciplinary conventions, theoretical conceptions, and political values.

It has been argued by Steven Lukes (1986. 26) that power, at the most fundamental level, can be said to refer to the idea of "bringing about consequences". That is to say, power must be seen as involving the production of causal effects; "the absolutely basic core to, or primitive notion lying behind, all talk of power is the notion that A in some way affects B" (Ibid. 26). In this generic sense of power as 'cause', power can, of course, be seen as a feature of all human action. Power is, indeed, integral to the very concept of human agency, that is, the transformative capacity of people. Lukes argues that two

distinct concepts of power have emerged in the history of social thought. The first of these concepts sees power as arising wherever A affects B in a manner that is contrary to B's interests. Such an approach sees power relations as asymmetrical, as 'zero-sum' relations which involve actions in which there is potential resistance or conflict between agents. Power is, from this point of view, an element in a conflict of sectional interests. This sectional, interest-based concept can be contrasted to the second concept of power that Lukes identifies, the non-sectional or non-zero concept, which sees power as existing only in and through processes of legitimation. Power is seen as a collective capacity that arises from structures of consensual communal organisation.

Choices between the interest-based or sectional concept, and the legitimation-based and non-sectional concept, is not a simple matter. Any choice of theoretical concept raises questions of values. The choice between definitions is however not purely a matter of value judgements. There can be good theoretical or empirical grounds for preferring one concept of power over another. Thus Lukes has suggested that the non-sectional concept of power is less valuable than the sectional concept because it systematically ignores the central issues which have always prompted scholars to study power. The proponents of the non-sectional concept "focus on the locution 'power to', ignoring 'power over'. Thus power indicates a 'capacity', a 'facility', and 'ability', not a relationship. Accordingly, the conflictual aspect of power - the fact that is exercised over people - disappears altogether from view" (Ibid. 9-64).

In this chapter, however, the focus is not upon the analytical penetration of differing political conceptions of power and interests, but upon the socio-historical process of their formation. The relationship between power and interests is also not simple. Different conceptions of what interests are, are associated with different moral and political positions. The point is that any view of power rests on some normatively specific conception of interests. Is it legitimate, for example, to go beyond the subjective, conscious interests of

agents and attempt to identify 'objective interests' which people may deny or of which they may be unaware? Power, then, stands in a complex relationship to intentions, wants, knowledge, and interests. The outcomes of power

> "will be related to the desires and beliefs of the powerful, or those they present, either (directly) through their intentions or (indirectly or directly) via their interests, and, unless otherwise specified, they will involve effects on welfare, limits on freedom, distributive advantage or the securing of collective goods, or some combination of these" (Lukes, 1986: 13).

Power, then, according to Lukes, should be understood in sectional, interest-based terms as strategic action within structural constraints. The question is how this view is to be developed and made useful for the construction of research strategies? One of the most fruitful attempts to do this can be found in the work of Denis Wrong (1979). Wrong has argued that power is a capacity or disposition that may or may not be realised in action. Force, manipulation and persuasion are, according to Wrong, the most general mechanisms that are involved in power relations of all kinds. These mechanisms of power can operate in interpersonal contexts, but they can also be involved in the establishment of stable institutional structures of power that have been the principal concerns of those who have undertaken research on social power. To describe these institutional relations we may use the Weberian term 'domination'. An individual or group exercises domination when it can issue a command to others and can be certain that this will result in obedience. Wrong consequently recognises the concept of legitimate domination or 'authority', and suggests, following Weber, that 'traditional' or 'rational' legitimisation have been the most common basis of legitimate domination. What Wrong has shown in the consideration of power, domination and legitimacy, is the fact that power can be properly understood only if it is connected to the larger cultural context of legitimisation and to the material distribution of resources. More importantly, for the purposes of this thesis, is the distinction which Wrong makes between "latent

power" and "possible power". Wrong argues that to ignore the difference between possible power and realised power, is to "mistakenly attribute power to groups whose power is merely possible and where a long process of social mobilisation and indoctrination would have to take place before it can become a reality" (Ibid. 9). In Wrong's view to locate the ultimate seats of power in the group structure of power is to "ignore Weber's insistence that society is a system of meanings as well as of interacting persons and confines us to the appearance and surface of social life" (Ibid. 9).

One of the earliest writers to explore this was, of course, Max Weber, who saw the social distribution of power as expressed in the relations of class, status, and party. In an article entitled "The Disciplinary Society: from Weber to Foucault", John O'Neill argues that Foucault is thereby seen to complement Weber's formal-rational concept of bureaucracy and legal domination with "a physiology of bureaucracy and power" which is the definitive feature of a disciplinary society (O'Neill, 1987: 42-60). Although Foucault does not study the bureaucratic process in the Weberian mode, his studies of the prison, hospital and school "go beyond Weber in grounding the legal-rational accounting process in techniques for the administration of corporeal, attitudinal and behavioural discipline" (Ibid. 45).

O'Neill views the works of Weber and Foucault in terms of a convergence upon a simple question, namely, what are the techniques by which man has subjected himself to the rational discipline of the applied human sciences? O'Neill does not consider it far-fetched to view Weber as an archaeologist of the power man exerts over himself, and thus as a precursor of Foucault's conception of the disciplinary society. In the case of both writers, history is not conceived as containing some rational essence, as in the case of Marx, even though it is understood as a process of increasing rationalisation. Of course, neither thinker is entirely "intelligible apart from Marx's analytic concerns", but both are closer to Nietzsche than to Marx in their grasp of "the radical finitude of human rationality"

(Ibid. 43).

What distinguishes Foucault from Weber, however, is his interest in how forms of rationality inscribe themselves in practices or systems of practices, and what role they play within them. His primary unit of analysis is that of 'discourse'. As stated by Mark Philip, "a discourse is best understood as a system of possibility for knowledge" (my emphasis) (Skinner (ed.), 1985: 69). In similar fashion to Wittgenstein's conceptions of 'modes of social life' and 'language games', Foucault's method is to try to identify the rules that permit certain statements to be made, and how these rules lead us to identify statements as true or false. These rules become more apparent when an "object of discourse is modified or transformed, as when homicidal monomania becomes viewed as moral degeneration or paranoid schizophrenia" (Ibid. 69). Consequently statements become true or false by virtue of our having ways to reason about them. A discourse is a way of reasoning, one that makes certain forms of knowledge possible, and excludes other ways of reasoning. But these rules "are not rules which individuals consciously follow; a discourse is not a method or a canon of inquiry", rather, "these rules provide the necessary preconditions for the formation of statements, and as such operate `behind the backs' of speakers of a discourse" (Ibid. 69). As a result our classificatory systems do not simply mirror enduring features of the natural world, and "the relationship between words and things is always partial and rooted in discursive rules and commitments which cannot themselves be rationally justified" (Ibid. 70). In fact, it is precisely because rationality does not play a role in the foundations for discourse that Foucault emphasises rapture and discontinuity in the history of ideas.

What is more pertinent to the theme of this thesis is Foucault's emphasis on the constitutive role that power plays in knowledge. As stated by Sarup, "Foucault inverts, following Nietzsche, the common-sense view of the relation between power and knowledge. Whereas we might normally regard knowledge as providing us with power to do things that without it we could not do, Foucault argues that knowledge is power over others, the power to define others" (Sarup,

1988: 73). Modern societies are seen to pay ever-increasing attention to the psychology of the individual, and in law, for instance, the intentions of the individual in the act of transgression becomes the central criterion of culpability. In contrast to the feudal and monarchial systems of the past, modern societies are characterised by the exercise of disciplinary power which is interiorised through prescriptions of what is considered `normal', and as a consequence each person becomes his or her own policeman. These "disciplinary technologies" are both a form of power and a mode of knowledge. Power and knowledge are not separate entities. Contrary to the notion of knowledge as liberating, knowledge takes the form of technical control, and contrary to the conception of power as merely repressive, power is seen as productive. Power both enables and constrains us, this is what makes power a transformative social force. States Carlo Frigerio, "for Foucault the reduction of power to repression is not only inadequate, it is dangerous. It makes us think that the liberation struggle can be won by demarking 'truths' that have been made invisible by the mechanisms of power" (Frigerio in Mouton (ed.), 1990: 328). As was seen previously in the case of Latour, "beliefs cannot be cut off from the social practices in which they are embodied, nor can beliefs be measured against a given reality existing outside those practices" (Ibid. 328).

Foucault states that "it is not power, but the subject, which is the general theme of my research" (Douglas and Rabinow (eds.), 1982: 208). This notion of the "subject" refers to the "double character" of man, that he is both the subject who produces knowledge, while at the same time being the object of that knowledge through the practices of the human sciences. More specifically, it is the positivist tradition in the human sciences which "inverts" truths about man. As Frigerio (see Mouton & Joubert, 1990: 323-330) relates, defining norms, deviances and pathologies, and prescribing treatments for them, "a plethora of experts have emerged who dictate how human beings have to be measured against the norm, and these classified and disciplined accordingly". This is what Foucault means by "made subjects" (Ibid. 324). In common with both the `Edinburgh School' and

Latour, questions of epistemology are treated as questions of social order, or as stated by Frigerio, "Foucault's analyses of the genealogy of modern medicine, legal systems, psychology and criminology reveal that epistemology and politics are ineluctably intertwined in power/knowledge technologies and in apparatuses (dispositifs) by which human beings are made subjects and treated as objects" (Ibid. 324).

Foucault does not view power as a possession or a capacity. It is conceptualised as having the pervasive character of a network that extends everywhere. Consequently, as stated by Sarup,

> "Foucault suggests that an analysis of power should concentrate not on the level of conscious intention but on the point of application of power. In other words, he wants to shift attention from questions such as 'who has power?' or 'what intentions or aims do power holders have?' to processes by which subjects are constituted as effects of power" (Sarup, 1988: 82).

Power itself creates and causes the emergence of new objects of knowledge, and ultimately how we observe and talk about what we observe. How precisely power influences how and what it is possible to know is however not clear. As stated by Latour, "he slashes knowledge/power, but adding the slash does not solve the problem" (Latour, HTM). Latour argues that the slash conceals the asymmetrical nature of the analyses, for though we need knowledge in order to exert power, power tends to overshadow knowledge in Foucault's work.

We are left in the dark as to how specifically power affects the knowledge which production it facilitates. By reducing what we can know to convention and avoiding questions of epistemology, truth comes to play no part in the transformation of knowledge. Truth is always relative to discourse. A discourse embodies knowledge (or, rather, what it defines as knowledge) and therefore it embodies power. There are rules within a discourse concerning who can make

statements and in what context, and these rules exclude some and include others. Those who have knowledge have the power to fix the flow of meaning and define others. The world is thus made up of a myriad of power relations and each power is seen to generate a resistance. Numerous authors have commented that though Foucault postulates that power produces resistance, no explanation is offered as to why this is so. If power cannot be associated with repression, as in Lukes' view, the question remains why people would resist. Sarup, for instance, argues that "Foucault is trapped within a logical 'impasse'", that "given his conception of power, there can be no escape, no locus of opposition or resistance, because power itself has no specific basis or ground" (Ibid. 93).

Foucault leads us into a rarefied philosophical atmosphere in which the world is seen as created by language and the self is only a "position in language" a mere "effect of discourse". With attention thus deflected from the search for practical ways in which to achieve communication, the path is open for a view that all ways of knowing are exercises in power. Power is "decentered", not the property of any subject, and because it is normalised, it is routinely practised by subjects upon themselves insofar as they re-enact the premises of their culture. Although this grasps a dimension of the modern experience of power, it also obscures the fact that people can often distinguish between what power is and what it ought to be. The criteria for distinguishing between legitimate and illegitimate power is made internally impossible, and we are left without a base from which to make critical judgements. Linda Nicholson, in her analysis of postmodernism, states this objection, and argues persuasively that "from the correct observation that truth contains a dimension of power cannot be deduced the claim that truth is power" (Nicholson in Seidman & Wagner, 1992: 86). Her argument is that power takes on a variety of forms and "to claim that knowledge is a kind of power is not to claim that it is identical to that power" (Ibid. 86).

Foucault, consequently, can advocate only resistance, not emancipation. Also,

the implication that the mutuality of power and knowledge is universal, not just distinctive to modernity, and that similar analyses can be developed for all cultures and historical periods, contradicts his theory of historical ruptures. We need not agree with Foucault that structures of knowledge, epistemes or paradigms, simply change, succeeding each other without gradual transitions or the possibility of comparative evaluation. Practical activities bring such structures into simultaneous use and under some circumstances force comparative evaluations. Relatedly, we must be careful to avoid the presumption that discourses are inherently unitary. Competing interpretations and evaluations may be present within a particular discourse, and these internal criticisms can be of help to the would-be interpreter. In fact, these internal criticisms point to the fact that the would-be objects of understanding cannot be presumed to remain stable and unchanged, a theme that is further explored in the concluding chapter.

Concluding remarks

In this chapter it has been argued that the Edinburgh School, in their commitment to naturalistic causal explanation, fails to explain the necessary and sufficient conditions for scientific beliefs, nor do they present clear explanatory connections between natural knowledge and social contexts. They argue that because rational evaluation is indeterminate in all cases, the real causes of scientific beliefs must be explained in terms of sociological causes such as interests and forces. The nature of these interests and forces however remains vague. They do not adequately explain or theorise the process of socialisation by which scientists supposedly form a common and uniform identity, and by rejecting the internal perspective of practitioners in science, they overlook an important source of explanation of scientific practice. Also, the contention that all beliefs have social causes, and that objectivity is therefore inherently social, does not hold up to scrutiny. The notion that all belief is caused by social factors

is self-refuting, and implies a thoroughgoing relativism which is not addressed by merely postulating the principle of reflexivity.

In the case of Latour and Woolgar, it has been argued that they avoid the thoroughgoing naturalism of the Edinburgh School with a far more reflexive approach. Their detailed description of the actual practice of science reveals explanations of unnoticed aspects of everyday scientific activities which often contradict what scientists themselves believe about their activities. Latour contends that the Edinburgh School presents an asymmetrical argument because both society and nature are explained in terms of the social, and proposes a 'generalised principle of symmetry' which treats both society and nature symmetrically. Contrary to the Edinburgh School nature is also seen to shape our beliefs. Though the objectivities of everyday life are seen to be socially constructed, a non-causal basis for the sociology of science is proposed, thereby avoiding the interest-based explanations of the 'strong programme'. By redefining both nature and society as agents in association, and introducing the concept of quasi-objects, Latour avoids the dilemma of treating constructions as causes.

Though laboratory studies do present a potentially powerful criticism of our accepted beliefs regarding scientific knowledge and authority, it is not clear how we should change our beliefs about science and its practice. More significantly, in following scientists as they are working, it is not clear what it is that is following these actors. It has been argued that if the language being used is that of the researcher, the description then becomes a second-order interpretation of scientists' constructions, whereas if the description is couched in the language of the scientists themselves, it loses its critical import. Consequently, it is argued that Latour and Woolgar can only offer criticism of the practice of natural science by being selective about the aspects of participants' knowledge to which they appeal. Though Latour avoids a thoroughgoing constructivist argument, he nonetheless makes claims along the lines of such an argument by

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conceptualising knowledge claims as power moves.

In the following section a brief explication of liberal and Marxist conceptions of power is presented, contrasted to Foucault's view of the constitutive role which power plays in knowledge. Power is seen to both enable and constrain us, and is thereby a potential transformative social force. The analysis of power is, however, not done on the level of conscious intention, but on the point of application of power. This analysis is informed by the notion of the "subject", that man has a "double character", he both produces knowledge, while simultaneously being the object of that knowledge through the practices of the human sciences. Because man is the object of knowledge, questions of epistemology are treated as questions of social order, as in the case of the `Edinburgh School' and Latour.

The argument has been that Foucault leaves us in the dark as to how knowledge is specifically affected by the power which facilitates its production. The slash, power/knowledge, does not solve the problem. This discrepancy is perhaps best explained by Steven Seidman, who argues that Foucault emphasises language and discourse as the agents of knowledge, and consequently, there is an absence of "any attention to the institutional contexts or social effects of discourse - in a word, to the interconnection of knowledge and power" (Seidman, 1994: 216). Also obscured in Foucault's analysis, and related to Seidman's critique, is the fact that people can distinguish between what power is and what it ought to be, that is, between legitimate and illegitimate power.

Finally, it has been argued that Foucault deflects our attention from the practical ways in which people achieve communication, and thus conceals the reflexive manner in which ways of knowing need not exclusively be exercises in power. One could of course still argue that attentiveness to the effects of power in shaping all claims to knowledge, is overlooked by people who are not privileged by power. Steven Phohl states that, regardless of the research methods we

choose to investigate a given phenomena, "all critical researchers are faced with an additional task, the challenge of reflexively situating our claims to knowledge within and against the dominant structures of power, which constitute our present history" (Phohl, 1994: 470). Phohl thus proposes a power-reflexive epistemology, "to work within and against the grain of ideological distortions, rather than pretending that one can float free of ideology altogether" (Ibid. 471). This power-reflexive epistemology also recognises that knowledge is not a simple servant of power, that though knowledge may itself become a form of power, knowledge may also subvert power (Ibid. 409). Power need not necessarily distort and contaminate our research, for as argued previously, reciprocal relations of power can be participatory and based on mutual respect. Respect entails openness and a willingness to listen, which is part of a power-reflexive approach to research practice. This theme is taken up and further elaborated in the concluding chapter.

Chapter 3

The deconstruction of objectivity

The notion of the epistemic symmetry of beliefs introduced by the Edinburgh School became a central theme of postmodern theory in which the non-hierarchical nature of knowledge claims is developed into its most radical and relativistic form. Consequently, in this chapter a brief overview of postmodernist thought will be presented, followed by a case study of feminist standpoint theory, in which the methodological implications of a divergence of standpoints rooted in different experiences are investigated.

Introduction

As noted in the previous chapters, the orthodox consensus and basic assumptions of a positivist and empiricist model of modern science has come under attack from a variety of directions. In this regard the impact of postmodernist theory has been particularly pernicious. Pauline Rosenau, in her analysis of the impact of postmodernism on the social sciences, lists the apparent inadequacies of modern science which have been raised by these critics (Rosenau, 1992: 10). These inadequacies include the failure to produce the dramatic results promised by enthusiasts; the abuse of modern science by legitimating the preferences of the powerful; the discrepancy between modern sciences' theoretical claims and actual practice; the discrepancy between modern sciences' claim as a solution to all problems and its inability to cope with the problems of our century; its disregard for the mystical and metaphysical dimensions of human existence; and lastly, its silence on the ethical and normative purposes of scientific knowledge (Ibid. 10). As stated by Rosenau, "in short, postmodernism in the social sciences is, at least in part, a response to the perceived inadequacies of scientific social science" (Ibid. 10). According to

Rosenau further points out, postmodernism, like most theoretical strategies, is not entirely original, and borrows elements from an array of orientations. "It appropriates, transforms, and transcends French structuralism, romanticism, phenomenology, nihilism, populism, existentialism, hermeneutics, Western Marxism, Critical Theory, and anarchism" (Ibid. 13). Out of this array of borrowings and orientations two methodological approaches can be identified, "introspective, anti-objectivist interpretation, and deconstruction" (Ibid. 118).

From the foregoing it should be clear that "postmodernism" is an extremely illusory concept. The evasiveness of postmodernism can perhaps be better understood by taking note of the postmodernist imperative regarding the finality of a definition or classification. One of the most important objectives of postmodernism is the avoidance of final definition or classification through a continual increase in, and fragmentation and diversification of, discourse. Of some importance when it comes to understanding postmodernists' antipathy toward the practice of final definition, is Foucault's conception of power in terms of the formulation of discourse. What is feared is that the establishment of fixed and final discursive formations serve to exclude (and victimise) alternative discursive formations and thereby repress difference.

Another idea that can help one to understand more clearly the postmodernist dislike of final definition and delineation, is Derrida's notion of "closure", the act of bringing something to an end. A central theme in the work of Derrida is the prescription that the attempt at completion, the act of bringing to an end, should never be allowed to succeed in its goal. Consequently, one can recognise, in contrast to a strategy of definition or placing within boundaries, a strategy of continual interpretation. As stated by Ritzer, unlike the structuralists, who saw order and stability in the language system, "Derrida sees language as disorderly and unstable. Different contexts give words different meanings" (Ritzer, 1996: 97). It is impossible for scientists to search for the underlying laws of language, which is the product of 'logocentrism', the search for a universal system of

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thought that reveals what is true, and which is the dominant form of Western social thought. States Ritzer, "logocentrism has led to the closure of not only philosophy, but also of the human sciences", and hence an apt way to describe Derrida's focus is the "deconstruction of logocentrism" (Ibid. 597).

Derrida and Foucault, however, never proclaimed themselves postmodernists. As argued by Calhoun, "the structuralist background to poststructuralist theory is widely forgotten or misunderstood", and "we need to avoid exaggerating any emphasis on the prefix "post" and recognise the extent to which Derrida and especially Foucault write as inheritors of structuralism" (Calhoun, 1995: 99). For the purposes of this chapter, however, Derrida and Foucault are treated as precursors to post-modern theory due to the pervasive influence of the concepts 'discourse' and 'differánce' on this mode of inquiry.

A further difficulty is that `postmodernism' and 'poststructuralism' are often conflated and treated as equivalent. The difference between the two is, according to Rosenau, "one of emphasis more than substance: postmodernists are more oriented toward cultural critique while the poststructuralists emphasise method and epistemological matters" (Rosenau, 1992: 3). Due to the constraints of a single chapter this useful distinction is not maintained in this exposition. Lastly, it is also worth noting that the difficulties of the postmodern concept are further confounded by the fact that the arguments of more conventional critics of modern social science are interwoven with those of postmodern theorists, which make clear demarcations problematic (Ibid. 5).

Postmodernism

For our purposes it is enough to note that this emphasis on diversification and fragmentation, the focus on difference, and the concomitant deferral of final meaning, are all meant to refute the so called metanarratives which are

perceived to be an integral part of modernism. As is well known, the concept of metanarratives was introduced by Jean-Francois Lyotard who defined postmodernism as "incredulity toward metanarratives" (Tarnas, 1991: 401). Metanarratives can be described as discourses in terms of which it is claimed that truth, knowledge and objectivity exist independently of historical social practices and other contextual factors. As in the case of the sociology of science, the attack on metanarratives therefore translates into a rejection of the idea that the scientific method is instrumental to the discovery of truths independent of the scientist and the scientific method.

In common with Foucault, postmodernists react to the modernist epistemological position by arguing that truth flows from discourse. Each discourse contains rules and procedures which themselves demarcate what can be said to be meaningful and true. From a postmodernist perspective one can argue that the different discourses are not regulated by certain over-arching (transcendent) rules and that, as a result of this lack of over-arching authority, the truth claims made by opposing discourses are in principle beyond adjudication, or incommensurable in the language of Peter Winch. The settling of disputes are possible only through prior agreement on the rules, and cannot be enforced by a specifically positioned group or person purporting to have unlocked certain objective and universal truths.

Postmodernism, then, requires all attempts at the legitimation of knowledge claims to take a very local and plural form (Fraser & Nicholson, 1990: 23). This idea of local and plural legitimation of knowledge is, however, criticised by authors who argue that these local legitimations are already informed by larger inequalities and that these local legitimations can, therefore, lead to the exacerbation and further entrenchment of larger inequalities.

It is, on the one hand, possible to think of postmodernism in terms of a new social reality, an epochal change, which has the effect of displacing (even

replacing) the previously predominant modern reality. Claims along these lines identify central themes of modern social reality such as communication by means of the printed word, colonialism, capitalism, an emphasis on work etc. In contrast, post-modern reality is characterised by electronic means of communication, relaxation, the consumption of resources, an emphasis on human relationships etc. From the local vantagepoint of someone living in rural Africa, I want to question the notion of dramatic social and cultural changes that are a radical departure from previous trends. Though, as Calhoun argues, these changes are real and major, they do not appear to amount to an epochal break (Calhoun, 1995: 99). The two basic organising forces in modernity, capitalism and bureaucratic power, have hardly began to dissolve. Also, as stated by Calhoun, "the problems of self and agency are neither new to the post-modern era nor obsolete because superseded either historically or theoretically; these problems continue to shape our lives and thought as they have shaped them throughout modernity" (Ibid. 99). I would support Calhoun's contention that to use the prefix 'post' is to narrow our notion of the modern. Rather, the insights of postmodernist thinkers could be incorporated into a richer sociological approach to the entire modern era (Ibid. 99).

The focus of this thesis is, however, not primarily on theories of society or social reality but on theories of knowledge and method. Accordingly I will briefly compare what has come to be known as modern and postmodern forms of knowledge. Although presented as dichotomies, the differences often fall along a continuum. A considerable amount of literature from those who are committed to the modernist approach is of a defensive sort when confronted with the epistemological directions advocated by postmodernist analysis. The tactic is generally to dismiss postmodern claims regarding knowledge as old wine in new bottles, followed by an incorporation of the postmodern concepts within the discourse of modernist thought.

Taking their cue from Derrida, postmodernists argue that modernist thought has

its roots in the Enlightenment and tended toward a totalising truth centred on an ostensibly discoverable logos. Driven by uniform and formal rational methods, one dominant and globalising thought emerges, and narrative knowledge is seen to be usurped by scientific knowledge. The search for truth by the modernists is guided by the ideal of establishing absolute postulates from which all other `facts' can be explained by linear, deductive logic. Narrative knowledge, on the other hand, is based on myth, legend, tales, stories etc., which provide the wherewithal of being in society. Whereas scientific knowledge tends toward closure, narrative knowledge embraces imaginary free play. For postmodernists, knowledge is always both relational and positional. Accordingly, standpoints are always situated in social relations and within ideologies. In line with Foucault, power and knowledge are intricately connected and hierarchically arranged. To enter a 'discursive formation' (whether legal, medical, scientific, political etc.) is to enter the logic and rationality embedded within it, or, as Foucault would reiterate, truth is always discourse specific.

Useful in this regard is the analysis of Seidman and Wagner (1992: 1-14), who trace the development of social theory, and how questions regarding the epistemological, political, and moral status of the social sciences have been contested since the inception of the social sciences. During the 1960's and 1970's the dominant positivism of the social sciences came under severe criticism. The critics argued that

> "the natural and social sciences involve an interpretative ordering of social reality; that philosophical, aesthetic, and moral considerations play a role in all empirical inquiry; and that in its resistance to empirical verification science resembles literary interpretation" (Ibid. 1).

These critics of positivism, though agreeing on certain points, disagreed on other fundamental issues. Some critics, though rejecting the positivist model of the social sciences, nonetheless did not challenge the `epistemic privileging' of

science. Other critics, especially poststructuralists, challenged both the scientism of positivists and the scientism of their critics. These critics emphasised the social and historical embeddedness of all inquiry, as well as the undesirability of a unified social scientific paradigm. Consequently a new division developed between those who "defended the possibility and desirability of achieving secure analytical foundations - 'modernists' - and critics of this project" (Ibid. 2). The central concern of the critics of this project, the postmodernists, is the consideration of "the relationship between scientific knowledge, power, and society as well as the relation between science, critique, and narrative" (Ibid. 2).

Postmodernism, in common with Critical Theory, criticises the Weberian notion that science is or should be value-neutral, and emphasises the practical and moral meaning of science. Steven Seidman (1992: 47-81) argues for a radical decentering of science and proposes that "social scientific knowledge bears the imprint not only of the broad civilisational and national culture of which it is a part but typically of the more specific class, race, gender, or sexual orientation of its producers" (Ibid. 6). The attempt of any author to advance a general theoretical standpoint is deconstructed, that is, reveals the particular standpoint and interests of its author in its basic concepts and explanations. Seidman, for example, argues that the conceptual framework of Marxism exhibits the standpoint of, and bias of, a white European, middle-class heterosexual male. "A general theory of society and history that is centred on economically based class dynamics neglects and marginalises social and political dynamics that revolve around gender, ethnicity, race, sexuality, or age" (Ibid. 7). In the tradition of Foucault, what Marx does not say therefore takes on as much, if not greater importance, than what he does in fact say.

Charles Lemert (see Seidman, 1992: 17-46), argues that the distinction between "a social theory and a sociological theory is that between a readiness to place politics ahead of epistemology as the foundation for thinking and an insistence

that knowledge (including a theory of knowledge) always necessarily precedes and informs statement about the social world" (Ibid. 34). Social theory is therefore seen to begin with a political position of some sort rather than an epistemology. Lemert asserts that the concept of "difference" is "the most powerful social theoretical concept in the poststructuralist vocabulary", and one that is "the most explicitly political" (Ibid. 35). As in the case of Foucault he advocates a decentering of the intellectual and political world in order "to experience and understand the differences of social reality - differences experienced most acutely by those in the excluded positions in Western society: women, homosexuals, the poor, the working class, nonwhites, and third world" (Ibid. 39). Consequently this critique becomes a critique of essentialism, that if there is an essential truth then there is only one truth, and that truth is the controlling truth of the dominant class. The claim to truth, as in the case of Foucault, becomes an act of power, that is, the will to form humanity. As stated by Seidman, "this epistemic suspicion is at the core of postmodernism" (Ibid. 68). Seidman concedes that postmodernism leaves many questions unanswered, in particular the specter of relativism, but argues that this course is preferable to "the repression of difference and diversity that is implied in the quest for foundations and disciplinary order" (Ibid. 75). This choice is unashamedly political and partial, and implies a choice between polar opposites. In common with various other commentators I would like to argue for an intermediate position between modernism and postmodernism, which will form part of the concluding chapter of this thesis. First I want to address the methodological implications of the recognition of a divergence of standpoints rooted in different experiences.

One of the implications of postmodernist theory and the emphasis on the plurality of different perspectives on the world is that instead of evaluating knowledge we should explore its social origins. As stated by Jeffrey Alexander in his analysis of the postmodernism of Richard Rorty, "rather than criticising society in light of universalist norms, we should criticise universalist norms in light of their social

base (Alexander in Seidman & Wagner, 1992:343). It is in feminist theory that a version of standpoint theory has become most developed, and is therefore most conducive as a case study and explication of methodological considerations. Feminism shares with postmodernism a critique of universalism through its exposure or deconstruction of universal thinking as male-centred. It is, however, worth noting, and as stated by Pauline Rosenau, "that feminist postmodernists are ambivalent about postmodern relativism and anti-objectivism, especially when discussion turns to their own particular group" (Rosenau, 1992: 15).

A methodological investigation of standpoint theory

The great Enlightenment thinkers praised universalism as one of the premier intellectual virtues. But they did not claim to have achieved perfect universalism, rather, they claimed to move in that direction. This was the direction of progress, and meant a continual overcoming of partiality. Within this logic of increasing universality, one could not say with pride that one was partial (see Calhoun (1995: 162-166). Calhoun identifies two primary origins from which the critique of a universal standpoint developed. One direction was in relation to Marx, who sought to show that the man taken as universal by philosophers of individual rights was only bourgeois man, man as citizen, but not man as worker. One feature of Marx's proletariat, however, was that, though it claimed to be all encompassing, it seemed in fact to be primarily male. Like the category of citizen, it was a false universal, a genderless term which hid the gender bias of its construction.

The second lineage of standpoint theory identified by Calhoun descended from Hegel who challenged the ahistorical character of Kant's philosophy. This is illustrated by Hegel's famous dialogue of master and slave. Hegel suggested that while the master's standpoint offered an illusory autonomy, it was in fact

both distorted and dependent on the recognition of the slave. Some philosophers, following Hegel, argued that "a greater perspicacity, though not quite an objectivity, was offered by the standpoint of the oppressed subordinate" (Ibid. 164). The slave is given a special claim to universality by the very nature of the slave's experience of oppression. A variety of other thinkers have taken up Hegel's dialectic of master and slave in a similar radical spirit, but it is in feminist theory that this version of standpoint theory has become most developed.

Feminism shares with postmodernism a critique of the universalism inherent in modern social and political thought through its exposure of universal thinking as male-centred. From the late 1970's, however, an internal debate arose within feminist thought, when black feminists challenged the universal pretensions inherent in feminist's own notions of sisterhood. The black feminist critique of white feminism emerged to accuse white middle-class feminists of ethnocentrism, racism, and of the selfsame errors of which they accused men, that is, that they generalised and theorised from their own situation. As stated by Terry Lovell (see Turner, 1996: 307), "this critique has reverberated through feminism in the past 15 years. Conjoined with post-feminist 'gender-scepticism', it has threatened to deconstruct the concepts on which contemporary feminism seemed to depend: 'woman', 'women', 'gender'." As a result a shift took place in feminist thinking, and it was proposed that the basis of women's political identity was not so much rooted in women's shared oppression by men but in women's shared identity as different from men. It marked, in other words, a shift towards political identity rooted in difference. The main point is that black feminists raised the need to acknowledge that the category 'woman' is itself differentiated by class, race, ethnicity, disability, sexuality, age, etc. As stated by Craig Calhoun (1995: 168), "the very construction of a women's standpoint, thus, not only relies on the distinctive experience of women but on the creation of an abstract category of women that is not given by that experience but by theory."

This critique from the diversity of women's positions in society and representations in culture has meant that the feminist analysis of women and gender must be different, and that this is not a simple matter of adding on a consideration of race or other dimensions of difference between women. Terry Lovell states that "the point is well taken that gender, class, and race are not discrete and cumulative forms of oppression, but that gender is constructed in and through differences of race and class, and vice versa" (Ibid. 310). This challenge from diversity has taken contemporary feminist theory into the heart of the most intractable questions on the nature of knowledge itself.

The basic argument has to do with the relationship between epistemological standpoint and experience. The gulf between male and female experiences is seen to be unbridgeable, and the differences incommensurable. Increasingly, the term standpoint came to denote not the search for a standpoint capable of offering universal understanding, but recognition of the divergence of standpoints rooted in different experiences.

The origins of standpoint theory thus lie within the critical theory tradition and its recognition that all research is inherently political. What has influenced the methodology of feminists most directly is critical theory's emphasis on problematising the view of the researcher as objective and value-free. As Harding emphasises, "there is no such thing as a problem without a person (or groups of them) who have a problem: a problem is always a problem for someone or other" (Ibid. 7). The requirement that social research be `objective' and 'value-free' has had the effect of shifting the spotlight away from the researchers themselves and their own subjective and value-laden perspectives, and has focused it, instead, exclusively upon the adequacy and accuracy of the methods used by the researcher, from the 'context of discovery', as Harding terms it, to the 'context of justification' (Ibid. 7).

The central, agenda-setting role of researchers in their ability to decide what is,

and is not, worthy of research has thus been largely ignored and defined away as an issue. Concepts such as 'objectivity' and 'value-neutrality' provided the necessary ideological apparatus to ensure that not only is such a patriarchal and bourgeois agenda unchallengeable, but also, if an alternative perspective was offered - whether anti-racist or feminist, for instance - then it could be simply dismissed as politically biased, subjective, and thus, ultimately, unscientific. If all research is inherently political by nature then, it has been argued, this needs to be fully acknowledged by researchers who would place their own subjectivity and value assumptions, as Harding contends, in the "same critical plane as the overt subject matter" so as to recover the "entire research process for scrutiny in the results of the research" (Ibid. 9).

The contention is further that it is impossible to eliminate the male-dominated or androcentric biases from social research simply by use of the scientific method, especially when, as Harding maintains, "androcentrism arrives in the inquiry process through the identification and definition of the research problems" (Ibid. 184). The only way forward in the circumstances is thus to found (root) this process of identification and definition in women's experiences. Moreover, it is argued that this privileging of women's experiences over men's should not be regarded as a move towards relativism. Men and women's experiences do not provide equal bases for developing scientific problems. This is because the activities of men, as Harding explains, "shape the horizons of their knowledge and support interests in ignorance of the misery generated by the domination of women" (Ibid. 186).

Very similar arguments privileging the experiences and epistemological standpoint of black people have also been put forward by writers within the anti-racist tradition (Hooks, 1982). It is, however, important to point to the difference of emphasis between feminist and anti-racist methodology. It is a difference that can be understood in the different ways in which women and black people have been treated in the research process. Women have been rendered invisible,

whereas black people have been rendered problematic, via comparison with the white middle-class norm, in terms of for instance, their 'deviant' family structures. While anti-racist writers have therefore been largely concerned with challenging the dominant mode of race research, feminist writers have been more concerned with rendering women visible. Not surprisingly, therefore, whilst anti-racists have been raising questions more about what is being researched and why, a number of feminist writers have been more concerned with who should be doing the research.

As pointed out by Paul Connolly in an essay on standpoint epistemology, the importance of this work in challenging our very claims to knowledge cannot be underestimated (Connolly in Lyon & Busfield, 1996: 185-197). Indeed, they have raised a number of central epistemological questions that are pertinent to the topic of this thesis. However, as has been pointed out by a number of commentators, such arguments also encounter a number of important and fundamental problems. The first relates to the simple division between women and men and the essentialist notion of women's shared experience. Connolly argues that the problems emerging from this can be illustrated through the use of a rather simple question: who would be best able to research black women, a white woman or a black man? Would one opt for a white woman due to their common experience as women, or would a black male researcher have an equal understanding of her experiences by virtue of a shared experience of racism. My experience would suggest that gender is a more basic category than race, but this assumption does not alter the fact that both the white woman's and black man's prior insight and shared experience can only ever be partial. This would imply on a practical level that the goal of 'symmetry' between researcher and participants would render large areas of research impossible. In similar vein, some feminist commentators have argued that men should focus on masculinity and, whilst they should support and ensure that their work is informed by feminist theory, they are neither in a position to, nor should be critical of, or engage with, feminist theory. This argument of course raises more problems than it solves. It

makes little sense to study masculinity without reference to women. Also, as pointed out by Connolly, there is an inherent contradiction within the logic that men should support feminist theory and ensure that it informs their work whilst not critically engaging with it. Feminism contains a wide range of competing, and at times incompatible, critical thought. It requires that an assessment and value-judgement be made regarding which strand of feminist thought is to be supported, which in turn demands a critical engagement with feminism (Ibid. 193).

At the heart of these problems, therefore, are essentialist notions of experience. Such a claim, however, involves a strong assertion regarding the uniformity of members of a collectivity. States Calhoun, "to base this claim on what is shared in experience is to court refutation from an infinitely ramifying range of experiences" (Calhoun, 1995: 166). Or, as stated by Susan Bordo (see Turner, 1996: 336) in her criticism of post-modernist feminism, postmodernism has substituted "the view from nowhere of traditional objectivist science" with "an equally imaginary view from everywhere". Other feminist writers who advocate a standpoint epistemology have consequently focused more closely on what counts as 'experience', and have moved toward the anti-racist focus on the what and why of research rather than simply on who is doing the research.

It should be clear from the foregoing discussion that standpoint epistemology has raised fundamental questions about the value-neutrality of social research and it's associated claims to objectivity. There can be no doubt that a researcher's social identity will affect the way in which people relate to her or him in the field. On a practical level though, it is no more than a simple truism that certain social settings are more accessible to certain researchers than others are. For standpoint theorists this is, however, not the issue at hand. For them the question is whether men should engage in critical social research on gender at all. In line with Connolly it is my contention that as reflexive beings we are capable of empathy and critical reflection, and are able to learn from and

incorporate the experiences of others into our research. This leads to the simple conclusion that it is more important to be concerned with what a researcher is doing and why, than with whom they are. Our actions rather than our social identity are what is of most importance (Connolly in Lyon & Busfield, 1996: 185-197).

A further branching in what Calhoun refers to as the `standpoint family tree', produces the argument that it is meaningless to give priority to any particular standpoint. Says Calhoun, "does not human life admit to, indeed necessarily produce, innumerable standpoints?" (Ibid.166). This postmodernist-influenced line of argument is that it is necessary to forego the attempt at universalisation, even such partial universalisations as the standpoint of women. Charles Lemert summarises this succinctly by stating that those "who dispute the feminist standpoint theory do so because it remains essentialist, simply substituting for the critiqued cultural models of masculinity equally universal ideals for knowledge, social relations, and moral development" (Seidman & Wagner, 1992: 39).

In this view the essentialist, centred, and universal model of standpoint theory remains modernist. Its approach does not take the very difference that it cites as the rationale for its methodology seriously. Feminist standpoint theory has attempted to address this critique from diversity, but, as argued by Terry Lovell (see Turner, 1996: 336), "although the term most often used is 'feminist standpoint' there is an elision in much of this writing between 'feminist' and 'women', a gap which is papered over by the assumption that to be a feminist is precisely 'to take the standpoint of women'. But the critique from diversity instantly forces us to ask '*which* women?' Women do not all share the same interests; indeed sometimes their interests may be directly opposed." Can groups whose political identities are rooted in their difference move beyond their own specific interests? As Lovell (Ibid. 337) writes,

> "what is to stop the slide from 'standpoints' to a familiar

individualistic liberal pluralism, which is where much postmodernist thought has ended? Even postmodernists continue, *de facto*, to take soundings in terms of 'race', class, gender, and ethnicity. Does this not suggest that, after all, these *are* the major *structuring* dimensions of the contemporary world? Does this discredit 'poststructuralism'?"

Concluding remarks

Contrary to this post-modernist influenced feminist position, with its emphasis on difference, and which equates science with discourse, one could argue that the contribution of female scientists was that, though they see different things, they do not see them on fundamentally different foundations of reason and observation. As argued in the previous chapter, recognising the extent to which science is a collective enterprise pursued largely through discourse, need not mean that science be reduced to just discourse, or just empirical observation and rational deduction. Interpersonal relationships are not only sources of bias, but also sources of common understanding.

According to Calhoun, "there is not necessarily a sharp and mutually exclusive opposition between taking seriously the notion of science as conversation and recognising that reason and observation have a role to play in knowledge" (Cahoun, 1995: 177). Calhoun contends that the postmodern view of discourse is presented as though it proves that the notion of truth based on a foundational observation and deduction are false, whereas it should rather be seen as a qualification and relativisation of such claims. The notion of conversation as arbitrary, as more or less interesting, rather than as a practical means of gaining consensus on the validation of knowledge, "recognises no history of concrete social practices that encourage mutual understanding and even epistemic gain" (Ibid. 178).

A feminist voice that reminds us of the social nature of scientific practice is of course itself an interested voice within science, not just a reversal of the interestedness of male-dominated science, yet this does not mean that science must cease to seek better ways of understanding because it admits to partiality and interest. As stated by Calhoun, "it misses the possibilities opened by Gadamer's rejection of both finalistic and historicist hermeneutics in favour of an account of judgement enabling us to move from a worse to a better position (within some practical frame of reference) without claiming essential truth" (Ibid. 180). It is this practical dimension which encourages a more historically situated approach to knowledge without yielding to a debilitating relativism. As stated by John Mandalios (see Turner, 1996: 280),

> "historical understanding enables individuals to reflect upon the historicity, as against naturalism, of a given concrete practice or institution. To understand that the order of things differs according to place (culture) and time (historical juncture) is to adopt a more reflexive stance toward the social world and its investigation. Self-reflexivity and historical consciousness, it might be said, go hand in hand".

The challenge, therefore, is to chart a new course between the classical and modern overdetermination of rationality, and the dissolution of the rational subject in postmodern thought. This line of investigation will be explicated in the following and final chapter. First I would like to make some general remarks about the paradoxical position adopted by "postmodernists".

Postmodernism has generated a new object, namely "modernism". Under this label the whole vast sweep of Western thought since the Enlightenment has been compressed. As Richard Tarnas (1991: 401) writes,

> "postmodernism in this sense is an antinomian movement that assumes a vast unmasking in the Western mind ....

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Deconstruction, decentering, disappearance, dissemination, demystification, discontinuity, difference, dispersion, etc. Such terms express an epistemological obsession with fragments or fractures, and a corresponding ideological commitment to minorities in politics, sex and language".

To assert a general truth is at best to claim a temporarily useful fiction and at worst an oppressive fiction masking relationships of power, violence and subordination. In consequence there is no postmodern worldview, nor the possibility of one. Postmodernism, as implied by Lyotard's famous "incredulity towards metanarratives", is fundamentally subversive of all paradigms, except, paradoxically, the superiority of its own perspective.

According to Tarnas (1991: 401), "the postmodern mind's sense of superiority derives from its special awareness of how little knowledge can be claimed by any mind, itself included". Ironically postmodernism cannot justify itself on its own terms and must be regarded as but one more perspective having no necessarily universal value. Tarnas aptly points out that "implicitly, the one postmodern absolute is critical consciousness, which, by deconstructing all, seems compelled by its own logic to do so to itself as well" (Ibid. 402).

While postmodernism contributes to our knowledge through its emphasis on the plurality of different perspectives on the world, and the diversity of that world, it moves far too easily from a recognition of the various obstacles to obtaining accurate knowledge to the claim that we cannot make comparative judgements about the quality of different knowledge claims. One could argue that the problems involved in the verification of social knowledge are not as insurmountable as postmodernists would have us believe, but that they draw our attention to how "modernists" have underestimated these problems. In this regard Rob Stones (1996: 2) states that "defeatist postmodernists decry any notion of realism, while sociological modernists work, implicitly or explicitly, with

a crude form of realism in which the reality of the social world is all too unproblematically apparent to the favoured theoretical framework, whether this be Marxism, functionalism, modernisation theory, systems theory or whatever. In these latter cases, far too much of the burden of proof is placed upon the theoretical framework, to the detriment of an adequate stock of empirical knowledge". The point that Stones is making is that whereas modernists typically present "facts" as if they speak for themselves, postmodernists too easily move from the recognition that reality cannot be directly represented to the invalid claim that reality cannot therefore be represented at all.

Chapter 4

Critical social science as dialogue with practical intent

In this chapter an attempt is made to chart a course between inflexible positivism and debilitating relativism by drawing on the postpositivist theorising of Jeffrey Alexander and the Critical Theory of Thomas McCarthy and Craig Calhoun, with specific reference to their interpretation of the work of Jürgen Habermas. Particular attention is paid to the problem of cultural difference and the political dimension of social sciences research, and tentative conclusions regarding the status of objectivity in the social sciences are drawn.

Introduction

Critical theory has attempted to bridge the gap that has been central to conventional positivism, namely between facts and values. Bryan S. Turner (1996: 12) argues that "it has been a significant but tragic and misguided reading of Weber's philosophy of social science to suggest that value freedom implies that the social theorist can have no political engagement. In fact Weber's notion of value freedom was primarily a warning against the abuse of office and privilege; it was a criticism of university professors, namely that they should not preach from the lecture hall as though their political opinions were neutral facts about the world". In this interpretation the work of Weber can then be seen as a precursor to the Critical Theory of the Frankfurt theorists. It is important to state that though these theorists played a crucial role in the development of Critical Theory, this mode of analysis, as noted by Craig Calhoun (see Turner (ed.), 1996: 462), "is carried on not just by Habermas and his associates, but by a wide range of others working in varying approaches: feminist theorists, poststructuralists, theorists of practice, etc."

In the previous chapters it has been argued that the empiricist approaches to epistemology such as that of Kuhn, the Edinburgh School, and Latour, impoverish their analyses by ignoring normative prescriptions for the conduct of science. Likewise the discourse analyses of post-modern theorists impoverish their explanations through an external critique, failing to recognise that discourses are not unitary, and that internal criticisms are part of the normative practice of science and raise issues among scientists of the empirical adequacy of theories. However, as Max Weber pointed out, empirical evidence must be framed in terms of meaning, which in turn raises the problem of the interpretation of meaning.

Interpretation has been understood by positivists as implying purely subjective intuition or empathy, that is, a practice of acquiring knowledge without controls or correctives. Critics of the post-modern persuasion have, on the other hand, denied that objectivity of any sort is possible, or that it is at best an arbitrary convention. Contrary to these two extremes Max Weber argues for an intermediary position, where objectivity does not mean that the social scientist reports on facts that are 'out there', but adopts a conceptual scheme which is in a dialectical relationship with empirical data. To cite Weber's social, psychological and other extra-scientific 'interests' is to fail to recognise that his concepts have generated a vast assemblage of hypotheses used by scientists who did not share Weber's extra-scientific concerns. As stated by Berger and Kellner, "the question of whether the famous 'Protestant ethic thesis' is or is not objectively valid as an interpretation of certain facets of modern history cannot be decided as a result of any amount of delving into Weber's biography or psyche" (1981: 54).

Sociology of course cannot escape from the historicity of social forms in which meanings are embodied. Nor can the sociologist 'suspend' the values that are part and parcel of this historical situatedness. Such 'suspension' can also not be guaranteed by rigorous research methods, which can themselves be influenced

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by values. Consequently critics of the social sciences have often maintained that social science is by its very nature subjective, and whereas the natural sciences produce objective knowledge, the social sciences are biased and unreliable. This argument is based on the thesis that values and interests uniquely influence social scientific inquiry. In this regard Weber presents a classical point of reference on the objectivity of the social sciences, attempting to bridge the divide between those who believe we should attempt to follow the path of the natural sciences, and those who conceive the study of people as being an entirely distinct mode of inquiry. Weber concedes that all knowledge of concrete social reality is from particular points of view, yet argues that this need not render objective knowledge impossible.

We have also seen how the Edinburgh School, in their commitment to naturalistic causal explanation, tries to explain the necessary and sufficient conditions for scientific belief. They argue that rational evaluation is indeterminate in all cases, and that the real causes of scientific beliefs must hence be explained in terms of sociological causes such as interests and forces. It has been argued that because they do not formulate the process of socialisation by which scientists supposedly form a common and uniform identity, the nature of these interests and forces remains vague. By overlooking the internal normative perspective of practitioners in science, they overlook an important source of explanation of scientific practice.

Latour, in turn, contends that the Edinburgh School presents an asymmetrical argument because both society and nature are explained in terms of the social, and proposes that we treat both society and nature symmetrically. Contrary to the Edinburgh School nature is also seen to shape our beliefs. Latour proposes a non-causal basis for the sociology of science, avoiding the problems of interest-based explanations, and the dilemma of treating constructions as causes.

It has been argued that Latour's work is illustrative of how the techniques of anthropological observation are being used more and more by modern science on itself. This research shows that people are often unaware of their behaviour and motivation, as they are themselves frequently surprised by this behaviour when they are questioned about why they said or did certain things. States David Hoy, "the genealogist thus tries to see as strange what the culture takes to be familiar" (Hoy and McCarthy, 1995: 174). This method of observation is a fragile combination of experiential nearness and analytical distance, and though laboratory studies do present a potentially powerful criticism of our accepted beliefs regarding scientific knowledge and authority, in the case of ethnographic laboratory studies, it is not clear how we should change our beliefs about science and its practice. More significantly, in following scientists as they are working, it is not clear what it is that is following these actors. If the language used is that of the researcher, the description becomes a second-order interpretation, whereas if the language used is that of the scientists themselves, it loses its critical import. Also, as argued by Thomas McCarthy, "detailed descriptions need not undermine what they depict, they may as well show that the respect accorded certain reputedly rational practices is in fact deserved" (Ibid. 82). McCarthy proposes that such studies have a dual potential for corroboration and critique, and that

> "reconstructed rule systems can turn out, as they do in Foucault's archaeological investigations, to be contingent, arbitrary, transformable singularities; or they can turn out, as they do in Habermas' pragmatic analyses of communication, to be enabling conditions of basic practices to which there are no good alternatives" (Ibid. 83).

The point McCarthy is making is that there is nothing in the methods of ethnographic description, as such, that dictates from the start whether descriptions of lived practices will lead to their rational reconstruction or their critical deconstruction. Consequently, Latour and Woolgar can only offer

criticism of the practice of natural science by being selective about the aspects of participants' knowledge to which they appeal. States McCarthy, "one would expect that the most convincing accounts will sometimes be stories of gain, sometimes stories of loss, but mostly stories of both – as were, indeed the classical social-theoretical accounts of the emergence of the modern world as a whole" (Ibid. 83).

By treating society and nature symmetrically, and introducing the concept of quasi-objects, Latour avoids a thoroughgoing constructivist argument, though he nonetheless makes claims along the lines of such an argument by conceptualising knowledge claims as power moves. As in the case of Foucault, questions of epistemology are treated as questions of social order, and hence point to a politics of truth. This leads to an argument in which the individual is represented as thoroughly submerged in some whole, whether language, culture, history, or power. Understanding knowledge becomes a matter of understanding the social practices in which we justify beliefs.

Donna Haraway states that "the temptations of a social constructionist framework lie in its contention that all knowledge claims, most certainly and especially scientific ones, are to be theorised as power moves, not moves towards truth" (Phohl, 1994: 388). While sympathetic to this temptation Haraway is concerned that this perspective offers no "objectively defendable or ethically scientific position from which to question existing structures of power" (Ibid. 388). Without denying the situated character of knowledge, which links constructionism to other critical viewpoints, Haraway supplements its theoretical claims with those of "feminist critical empiricism". As seen in the previous chapter this is a claim to "embodied objectivity" or the "experiential standpoint" of women and other disadvantaged groups. The claim is not that the standpoint of the oppressed is without distortions, but that an attentiveness to the effects of power in shaping all claims to knowledge, is overlooked by people who are most privileged by power. This is what gives the constructions of the oppressed their

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partial objectivity, the awareness of the interdependence between power and knowledge. As Stephen Phohl writes, situated objectivity is "a methodological move away from all purely scientific doctrines", and "demands that constructionists be as reflexive about the contexts of their own theoretical activities as they are about the claims of the people they study" (Ibid. 389).

Researchers, like anyone else, are bound to the social context in which their studies occur. In interpreting the interpretations of others, researchers must rely on the same interpretative practices as everyday people. How, then, can they claim objectivity for their analyses? Constructionists acknowledge that objectivity is partial, at best, but attempt to approximate objectivity by providing a detailed account of the natural history of the research, taking the social context and context-bound interpretative decisions into account. The strength of this approach is that it does not retreat into the scientific haven of survey analysis or forced-choice questionnaires. In this regard Phohl states, "these methods may be useful, but they also depend upon interpretative inferences and context-bound judgements about what is or isn't a meaningful answer to a pre-packaged question" (Ibid. 365). Phohl's argument is that the use of such allegedly objective research instruments presents as many interpretative problems as those raised in the course of ethnographic fieldwork. The suggestion is made that in order to approximate objectivity, attempts should be made to partially replicate a particular study. By repeating a particular investigation in some other context, we can approach research in a comparative fashion, highlighting similarities and differences in the findings, and may better understand and transcend the social factors that limit objectivity.

Constructionist and deconstructionist arguments rest on the assumption that the practices of cultures or discourses are inherently unitary, and not subject to competing interpretations and internal critiques. As argued previously, recognising the extent to which science is a collective enterprise pursued largely through discourse, need not mean that science be reduced to just discourse.

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Interpersonal relationships are not only a source of bias, but also sources of common understanding. States Thomas McCarthy, "the role of warranting and contesting reasons is in turn tied to the ability of competent subjects to accept and reject them, to weigh and revise them, to originate and criticise them" (Ibid. 37). McCarthy argues that it is this ability which is lost from sight, and that "accountable agents get transformed into cultural dopes, nodal points in grids of power, effects of the play of difference" (Ibid. 37). In line with Habermas he argues that the potential for uncoerced agreement on the basis of reasons open to intersubjective assessment gets downgraded. Consequently exclusively deconstructionist approaches surprisingly fail to acknowledge that knowledge has both the potential to be dogmatic and subversive. McCarthy argues that if we adopt a more pragmatic approach, "we can appreciate both aspects of ideas of reason: their potential for misuse and their irreplaceable function in co-operative social interaction" (Ibid. 37).

It is, however, no longer just that there are doubts as to whether the social sciences can be more like the traditional conception of natural science, it is widely recognised that the natural sciences are more deeply culturally, theoretically, and institutionally constructed than traditional and textbook accounts of science have suggested. Consequently, the social sciences can be conceived as practical and value-related in quite a different sense than argued by Weber, that is, practical in the technical sense that its explanations supply better means to solve problems. This stronger notion of the critical and practical purposes of social science goes beyond the much more limited conception of Weber who conceived criticism as theory-laden and theory-dependent, and thus as a limit on what social science can do. It is now acknowledged that natural science must confront the very same problems posed by the cultural and theoretical embeddedness of scientific practice. In line with Habermas, the concept of norms, not values, is consequently proposed as the proper way to characterise the practices of both social and natural scientists, as "it points to the structuring of social encounters by shared knowledge, expectations, and rules"

(Bohman, 1991: 190). Criticism, in this view, becomes an act of communication, a way of improving things not by controlling a domain of phenomena, but by changing beliefs and attitudes.

The problem of cultural difference

The modern/post-modern debate echoes the inconclusive rationality/relativism debate that centred on Peter Winch's interpretation of Wittgenstein and the problems of cross-cultural understanding. One of the virtues of the work of Foucault, Derrida, and a number of postmodernist theorists, has been to introduce the importance of difference. Craig Calhoun, in a chapter entitled "Cultural Difference and Historical Specificity" develops the argument that we need to be attentive to problems of cultural difference in a way that social theorists seldom have been (Calhoun, 1995: 70-96). By this Calhoun does not mean simply seeing theories as embedded in cultural traditions, or indulging in an easy relativism that does not take difference seriously, but the recognition, contrary to Habermas, that some differences among social actors amount to conflicting or incompatible claims. Calhoun acknowledges that relativism of some sort is a necessary starting point in the project of taking difference seriously, but that it is not a satisfactory endpoint. The very scientistic attempt to separate empirical theory from normative theory, as in the case of Weber, "has contributed to normative theory's problematic over-commitment to a culturally insensitive Enlightenment universalism" (Ibid. 73). This Western ethnocentrism is often couched in the language of liberal individualism, that human beings are essentially interchangeable individuals, and that the similarities of individuals is more important than the apparent cultural and other differences among them. Calhoun states that "there are even cases where extreme relativism and strong universalism actually meet in shared individualism", and that "in this sense, both that branch of modernity which has lately travelled under the name of postmodernism and the explicit Enlightenment modernism proclaimed for

example by Habermas, suffer from weaknesses of cross-cultural sensibility" (Ibid. 73). Calhoun's contention is that postmodernism is apt to make cultural difference an insurmountable barrier to both general discourse and normative critique, whereas Habermas' line of thinking leads to cultural difference being reduced to mere positions in a developmental scheme, or to not being granted any theoretical significance whatsoever.

Calhoun further contends that universalist thought tends towards a position where there can be only one set of fundamental values and that for "Habermas, famously, these are held to be implicit in the validity claims of all speech" (Ibid. 74). Habermas thus claims an empirical basis for his normative theory, but "the relevant catch comes with his decontextualized treatment of the giving of reasons" (Ibid. 74). One of the implications of taking difference seriously is that theory "must be contentful, not purely and exclusively formal" (Ibid. 76). The point is that understanding cultural difference is not a mere act of translation, but a process in which the researcher and informant engage each other in a process of gradually improving understanding through dialogue, and where each is changed by it. Understanding is achieved because they, researcher and informant, gradually change into people who can understand each other. Says Calhoun, "Since knowing is an activity constitutive of the person, not a mechanical storing up of data, gaining in knowledge always means changing somewhat" (Ibid. 82). The implications of this are that though Habermas conceives human beings intersubjectively, he is criticised for not considering that the best judgement might begin with relationships rather than decontextualized individuals. Under the best of circumstances communication does not take place in an 'ideal speech situation', and as Derrida has stressed, "language itself produces and makes inevitable the potential for infinitely ramifying interpretations and plays of difference" (Ibid. 87). Consequently a theory claiming objective clarity and certainty can do so only by presupposing a foundation in the habitus and culture of the practitioner, by presupposing tacit assumptions – "that which can be left unsaid"(Ibid. 88). Calhoun suggests that the answer to this lies in

increasingly grounding theories in the self-reflexivity of theorists, "in cultural sensitivity and historical specificity, not in suggesting that because theoretical discourse cannot live up to its own ideals we must forfeit those ideals as regulative constructs" (Ibid. 88).

On this view theory must be a 'polyphonic' discourse, not a monological statement. Ethnographic accounts become dialogical or 'polyphonic' texts that permit those being represented to speak in their own voices, tell their own stories, and challenge the ethnographers' views. The reflexive researcher no longer conceals the complex, situated, ambiguous, and conflictual interactions behind the smooth objectivity of a realist description. The objectivity of the researcher's account cannot be warranted independently of the people who are being researched. As stated by McCarthy, in defence of Habermas, "such representations are in principle contestable by the subjects whose beliefs and practices are in question. This is one of the features of subject/subject epistemic relations that distinguish them from epistemic relations between subjects and objects-pure-and-simple" (Hoy and McCarthy, 1994: 87). The contention is that the one-way descriptions of classical realism implied that the subjects were disqualified as competent partners in dialogue in the very act of representing them. In McCarthy's words, "assymetrics of representation thus enacted and reproduced assymetrics of power" (Ibid. 88). The solution is thus practical, and in line with Habermas, demands reciprocal understanding and symmetrical discussion of differences to overcome the partial nature of differences, and hence, the partial nature of objectivity.

Whatever the merits of subject/subject epistemic relations as a normative ideal, this is obviously not the situation in the world today. The contemporary world is hierarchically structured along lines of race, class, gender and ethnicity, amongst others, and interpersonal and intercultural encounters are infused with potentially distorting inequalities of power. In a situation of structural inequality assymetrics of power cannot be resolved solely by new methods of representation, and

raises questions regarding the practical aims and political nature of social sciences research.

The political dimension of social sciences research

In two separate chapters, one on values and objectivity in the social sciences, and the other on participatory research as a new paradigm, Johann Mouton investigates the implications of a politics of truth, and how this "refers to the fact that social science is per definition value laden" (Mouton & Joubert (eds.), 1990: 39-54), (Mouton in Coetzee (ed.), 1989: 387-403). Being value-laden, and therefore political, truth is enmeshed in power play, and varies according to the theoretical strategies employed in the social sciences. States Mouton, "truth is not revealed, but is constructed by and through the process of practising science" (Ibid. 39). Contrary to the traditional notion of objective research, that is, the "naturalistic" notion of objectivity which originated in the natural sciences during the seventeenth and eighteenth century, Mouton argues that power is "in fact an intrinsic dimension of all social sciences research" (Ibid. 43). This contention is based on the premises that, unlike theories in the natural sciences, social theories are themselves social practices and "constitute an integral part of the social practices that are studied" (Ibid. 43). Social theorising is reflexive, interacts with the reality being studied, and is thus "influenced by other existing practices" (Ibid. 44). It does not leave the people whom it investigates unaffected, and the question is therefore not whether social sciences have the ability to bring about social change, or "whether the social sciences have an interest in power, but what is the nature and content of this interest in power" (Ibid. 45).

Mouton identifies three dominant epistemological traditions in the twentieth century philosophy of social science, namely; positivism, the enlightenment ideal of rationality, and critical theory. Each of these epistemological traditions

employs their own metaphor of power and emphasises a different methodology. In the case of positivism an interest in power is expressed in the therapeutic ideal of healing society, which is conceived as analogous to an organism, of its evils and diseases. Effective treatment is based on the accuracy of the diagnosis and hence this theoretical strategy is closely linked to a quantitative methodology in social sciences research.

In the case of the enlightenment ideal of rationality the interest in power is expressed in the power of reason. Unlike the organic metaphor employed in the case of positivism, the human mind or consciousness "forms the basis for the presumed analogy between the study of human beings and the study of society" (Mouton in Coetzee, 1989: 393). Man is conceived as "a conscious, self-directing, rational human being and not a biological organism" (Ibid. 393). The aim of the social sciences is consequently primarily directed at understanding, to produce knowledge which will free people of ignorance, superstition and prejudice, and hence closely aligned to the ideals of the Enlightenment. This theoretical strategy, which embraces the phenomenological ideal of understanding, is associated with an anti-positivist epistemology and methodology, and is closely linked to a qualitative methodology in social sciences research.

Lastly, in the case of critical theory, an interest in power is expressed in a concern for the ideological distortions which affect people through either "the process of self-deception (individual) or because of false consciousness (society)" (Ibid. 397). The central assumption of this theoretical strategy "is that man is essentially in a state of alienation" (Mouton & Joubert, 1990: 49). Hence political power is seen as the ultimate goal, in order to transform the conditions that constitute people's alienation and oppression, the core concept being the idea of transformation. This theoretical strategy embraces both the positivist ideal of causal theories based on objective (quantitative) observation, and interpretative descriptions based on intersubjective (qualitative) understanding.

As stated by Mouton, "Theories are therefore deemed to be successful to the degree that they help social actors to overcome their alienation. The ultimate epistemological criterion is therefore pragmatic" (Kotze, 1989: 397). This interest in the humanisation and emancipation of the research subject is associated with the methodology of participatory research and an emphasis on dialogue.

Each of these theoretical strategies, or paradigms, is thus seen to be aligned to a particular metaphor of power that is, in turn, associated with a particular research methodology. This raises the question of which paradigm one is meant to follow and which is most correct. Mouton argues that "the only answer to these questions is that the deciding factor will be the nature of the phenomenon being studied and the way in which the research problem is formulated" (Ibid. 403). Mouton's approach is thus contextual and pragmatic, and points to the type of approach proposed by Habermas. Mouton's model corresponds to that of Habermas in many respects. Habermas too conceded that human interests structure knowledge, yet did not think of interests in relativistic terms, and asserted the existence of three universal interests that produce three general forms of knowledge, which correspond to Mouton's three paradigms.

More importantly, in the context of Mouton's argument, is the question whether one can still argue that social research can be objective if it is conceived as intrinsically political and value-laden. In answer to this Mouton states that "the ideal of objectivity and the notion of the political dimension of social science are in fact compatible" (Mouton & Joubert, 1990: 49). Mouton proposes that the solution to this dilemma lies in how one defines objectivity, and that objectivity should not be seen as a characteristic of the researcher but as a characteristic of the research process and methodology followed, and revolves around the question of whether this methodology meets the criteria of validity. Contrary to a social constructionist argument, it is not because we agree that we hold a claim to be valid, rather, we agree because we have grounds for granting its validity. In similar vein to Habermas, our agreements concerning validity claims are made

on the basis of reasons that are open to intersubjective assessment. As stated by Thomas McCarthy, "this is a side of social interaction to which post-Nietzsheans who treat social relations as so many expressions of the will to power are peculiarly blind: the 'will' to non-violent, consensual co-operation" (Hoy & McCarthy, 1994: 39).

Habermas' theory of communicative rationality focuses on the pragmatic presuppositions of mutual understanding, and in my view is one of the most fruitful attempts to steer a course between the extremes of positivism and relativism. This does not, however, imply that Habermas' theory is not open to criticism. In this regard it is useful to introduce the criticisms of Brian Fay, who writes, "the form of the mistake in Habermas' argument is the unstated assumption that to understand a speech act is to agree with it" (Fay, 1987: 188). Fay's argument is that there need not be agreement about the validity claims of any particular speech act in order to understand it, and that it is possible to think that people are justified in making the claims they do without at the same time agreeing that the claims are true. Hence Habermas is seen to move too easily from talk of justification to talk of agreement, or as stated by Fay, "to equate rational warrant with agreement is to beg the question which is at issue" (Ibid. 189). One need not, of course, agree with all aspects of Habermas' theories to concede that legitimate scientific practice cannot also be politically engaged. A social science can be at once scientific, practical, and critical. As stated by Seidman, "Habermas' intent should not be obscured in the detail of his theory of communicative action. He wished to provide a defence of critical theory as a form of reason, not ideology" (Seidman, 1994: 178). The commitment of critical theory to social justice is in this view not a culture-specific value, but a reflection of the orientation to rational consensus that one finds in everyday life. It is on the grounds of this assumption, that reason is an integral part of everyday life, that Habermas claimed a rational basis for critical theory (Ibid. 176-179).

Habermas criticises postmodernists for having hidden normative sentiments

which are concealed from the reader, and which prevent them from developing a self-conscious praxis aimed at overcoming the problems they find in the world. In contrast, Habermas' normative sentiments of free and open communication make the source of his critiques of society clear, while also providing the base for political praxis. More importantly, for the purposes of this thesis, Habermas accuses postmodernists of ignoring everyday life and practices, which are central to his idea of communicative rationality, which is in turn grounded in the rational potential that exists in everyday life (Ritzer, 1996: 586-591).

In most practical contexts some idea of being 'true to the facts' will be of decisive importance in assessing the validity of claims. Says McCarthy, "the stark opposition between knowledge and interest, which traditionally meant ignoring the latter the better to pursue the former and recently seems to mean the converse, also has to be superseded by critical theory" (Ibid. 223). As pointed out by Mouton, critical theory is not unique in its pursuit of interests, and hence critical theory is no more utopian than other theoretical strategies. Such interests are not meant to replace critical inquiry, but to inform them. Whether individuals and societies are committed to the idea of uncoerced agreements, arrived at in free and equal exchanges, by considering reasons pro and con, makes an enormous difference. As stated by McCarthy, "critical theory could do worse than retaining that practical faith and the utopian impulse that animates it" (Ibid. 224).

towards a social science with pragmatic intent

In the case of Foucault, and postmodern theory generally, an interpretation can only illuminate our understanding of a given phenomenon at the cost of shading over or leaving out, and social reality is more complex than any single interpretation can possibly be. Thomas McCarthy, in a comparative analysis of Foucault and Habermas, concedes that this depiction of interpretation is true, but

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argues that if one adds the regulative constraints of consistency and coherence, "then the space for an irreducible plurality of equally good interpretations considerably narrows" (Hoy & McCarthy, 1994: 238-245). McCarthy's argument is that an interpretation not only has to comply with the agreed upon facts and the accepted methods of determining them, but will also

> "have to be internally consistent and cohere with everything else we think we know about the domain under consideration, or it will have to defend its failure to do so by challenging elements (including purported facts) of the established consensus"
> (Ibid. 241).

Hence conflict of interpretations in the social sciences is carried out under constraints, and make possible a debate on the best interpretation of a given phenomenon. That such a debate frequently fails to end in consensus is not problematic for McCarthy, who stresses the pragmatic importance that discourse need not lead to rational agreement, but that it should be carried out as if rational agreement about the best interpretation is possible. If this approach is followed says McCarthy, "the pluralism that then remains is not that of self-encapsulated, incommensurable points of view, but of different voices in an ongoing discursive consideration of the reasons for and against competing views" (Ibid. 242).

McCarthy further rejects the suggestion that the claim to a right or better view necessarily reveals intolerance, ethnocentrism, and disrespect. He contends that all parties enter a discussion thinking that they have good reasons for holding the beliefs they do, and respect for opposing views does not imply granting that they are as good as one's own prior to any discussion. Respect entails openness to listen, weighing the grounds on which views are held, and being open to the possibility that they may prove to be superior to owns own. McCarthy concedes that there is every reason to expect the conflict of interpretations to be a permanent feature of human studies, but that the consequences of this need not lead us in the direction taken by Foucault and

postmodernist theory. Interpretations can be assessed in accordance with criteria of truth, consistency, and coherence as compared to other bodies of knowledge and interpretation, and this conflict then takes the form of "critical-reflective discourse". McCarthy argues that whereas dialogue is set "in opposition to the regulative ideas of universality and consensus; in reality, those ideas are often its driving force" (Ibid. 244). Persuading others of the validity of one's claims is often the reason why dialogue in the form of discourse exists. Says McCarthy, "we have to see the vindication of universal validity claims through the reasoned agreement of a universal audience as something that is always only ongoingly accomplished in ever-changing circumstances, and for all practical purposes" (Ibid. 244).

The core criteria are therefore pragmatic. It is not possible to separate the evaluation of any theory or research from the range of possible alternatives to it. As stated by Craig Calhoun, "choices are made with regard to epistemic gain, not absolute truth" (Calhoun, 1995: 87). Because theoretical communication takes place within constraints, it can rise above the ordinary problems of communication. This is not only a claim to greater clarity and precision, but "in Habermas' terms to offer readier redemption of validity claims" (Ibid. 87). Hence Calhoun contends, contrary to Peter Winch and the postmodernists who followed in his wake, that theoretical discourses enable communication across lines of cultural difference, "because even where theory does no thematise reflexivity it nonetheless involves it" (Ibid. 87).

General theory in the Postpositivist Mode

In an essay entitled "General Theory in the Postpositivist Mode: The Epistemological Dilemma and the search for Present Reason", Jeffrey Alexander (see Seidman & Wagner, 1992: 322-368) argues that the choice between scientific theory and anti-theoretical relativism is both a false and dangerous

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dichotomy. The notion that the only alternative to positivist theory is resigned relativism gives rise to a false

> "epistemological dilemma", the notion that "either knowledge of the world is unrelated to the social position and intellectual interests of the knower, in which case general theory and universal knowledge are viable, or knowledge is affected by its relation to the knower, in which case relativistic and particularistic knowledge can be the only result" (Ibid. 323).

Alexander states that the social and historical character of theoretical knowledge "does not negate the possibility for developing either generalised categories or increasingly disciplined, impersonal, and critical modes of analysis"(Ibid. 323). The argument which Alexander presents is that we are bound to standards that are rooted within groups and social institutions, that traditions "have a distinctively rational, impersonal bent" (Ibid. 323). Part of these standards and the cultural history that produced them, is that actors understand that the world does not revolve around them, and that this understanding makes impersonal study of the world possible. Alexander states that "acknowledging representational subjectivity does not mean abandoning the possibility of differentiating our representations from objects in the outside world. The possibility for so comparing 'objective' and 'subjective' is produced by the development of human culture itself" (Ibid. 343). The ability to differentiate between our theoretical frameworks and reality is "the first criterion of whether universality, and some conditional conception of objectivity, can be achieved" (Ibid. 349). This criterion, in Alexander's view, has been met by both natural and social science, and "is inscribed in, and sustained by, practical prohibitions against contaminating empirical data", so that "empirical variations can be compared with the experimenter's personal expectations" (Ibid. 350).

In other words, by adhering to scientifically sound methods, we can test our preconceptions against the empirical data, and this is part of the very practice of

science. These methods make it possible for individuals to "share conceptions of their impersonal worlds", and "the more individual practice can be subject to extra-personal control, the more it submits itself to universal criteria of evaluation" (Ibid. 350). It is this possibility of reaching consensus, that makes shared ground possible, and "the more neutral this ground not only seems but is in fact" (Ibid. 350). The criteria of objectivity can be met to the extent that social science succeeds in developing the conditions for consensus, which Alexander suggests, is a part of everyday communication. "Because experience is personal, mutual understanding becomes problematic and hence of ultimate importance" (Ibid. 352). It is because we are always striving to understand others and not only ourselves that we "strive for common knowledge" and "construct categories" (Ibid. 352). What we have in common makes it possible for us to distinguish between the particular and the general, and hence, "universal, depersonalised norms are possible – in life as well as in method" (Ibid. 353). To the extent that we are open and willing to listen to others we have a choice of mutual understanding, and this understanding includes "acknowledging the decentredness of human reality and accepting some at least of its impersonal claims" (Ibid. 354). Like Habermas, Alexander is rooting the possibility of mutual understanding and social science in the rational potential of open and self-critical communication, hence "interpretative understanding is not simply personal and empathic; it necessarily involves an impersonal reference that allows a critical and universalistic response" (Ibid. 355).

Concluding remarks

"Philosophy of science without history of science is empty; history of science without philosophy of science is blind" (Gordon, 1991: 589). To which the sociology of science might well add that an understanding of science must entail recognition of the cultural and political embeddedness of scientific practice. These matters have received a great deal of attention during the past century,

yet has settled few of the epistemic problems of social or natural science. While scientists claim to be making ever rapid progress, social theorists and philosophers have raised doubts about the basic foundations of their knowledge and practice, doubt which has been largely disregarded by practising scientists, but cannot be ignored if the blindness which Kant spoke of is to be avoided.

With the fall of positivism it has, however, become widely accepted, though not uncontested, that all observations are 'theory-laden', with the effect that we cannot rely on information supplied by sense-data. In his comprehensive study of the history and philosophy of social science, Scott Gordon addresses this issue, and argues "that the word 'theory' in the phrase 'theory-laden' is used imprecisely, failing to differentiate between a number of quite different controls that may impose themselves on factual observations" (Ibid. 605). Gordon identifies five distinct contentions that have been advanced in relation to the 'theory-ladeness' of observations (Ibid. 604-607).

The first is that observations are concept-laden, that is, that we make use of concepts to order the sensations that we receive. Gordon proceeds by drawing a distinction between the concepts used in a theory and the theory itself. In his view "concepts are like nouns in a sentence; they assert nothing in themselves" (Ibid. 605). Though conceding the obvious claim that observations are concept-laden, he refutes the claim that this should lead to the conclusion that theories cannot be subjected to and evaluated by the use of empirical evidence. As previously argued, in the language of 'discourse', concepts are not this innocent, yet difference is not so absolute as to exclude commonalties in use. It is precisely these shared meanings in everyday practical usage that makes communication and shared observation possible. Truth is therefore defined, not as direct correspondence to reality, but as the result of a process of agreement by a community of investigators and based on explanatory and practical success.

The second contention, that observations are hypothesis-laden, is the claim that observations are so controlled by the hypothesis itself that contradictory observations are not possible. This is, according to Gordon, simply bad science. It does not pose a fundamental epistemic difficulty, rather, it is a practical problem of honesty in scientific work, of not manipulating data to support a theoretical hypothesis.

The third contention is that observations are value-laden, that ideological, moral, and aesthetic values contaminate the empirical process. Gordon concedes the incontrovertible fact that values may and do "contaminate" the observation of empirical data, but denies that this poses "an insurmountable epistemic difficulty", for though all scientific work has social policy implications, especially that of the social sciences, it is not so deeply embedded in the methodology of scientific investigation as to be insurmountable.

The fourth contention, that observations are interest-laden, is dismissed by Gordon because it fails the test of self-reference. Proponents of this view, such as the Edinburgh School, will be seen to expose themselves to the parallel contention that their observation reflects their interests. Such epistemic tit-for-tat does, however, not solve the problem. The test of a theory is of course whether it can serve as a useful foundation for successful empirical research programmes. As noted by Johann Mouton, a particular research methodolgy is linked to a particular paradigm or theoretical strategy that is aligned to a particular metaphor of power. This raises the question of which paradigms one is meant to follow and which is most correct. As argued previously the answer to these questions will be determined by the nature of the phenomena being studied and the manner in which the research problem is formulated. The approach which is being proposed is thus contextual and pragmatic, that objectivity is not a characteristic of the researcher, but of the research process and methodology being followed, and revolves around the question of validity. It is proposed that it is not because we agree that we hold a claim to be valid, but

that we seek agreement by critically investigating the grounds for granting its validity. Our arguments concerning validity claims are ongoingly made on the basis of reasons that are open to intersubjective assessment.

The fifth, and most general contention, is that observations are laden with culture-specific ontologies. This contention recognises that people are the product of enculturation, and that cultures differ. Consequently scientific knowledge is seen as a reflection of the metaphysical beliefs of the smaller part of humankind. Gordon refutes this claim by citing the example of a rain dance, which "does not mean that a rain dance does indeed cause rain to fall when it is performed by believers" (Ibid. 607). The conclusion drawn is that though the world is perceived differently by different cultures, this does not mean that there are many worlds. Or, as stated by Jeffrey Alexander (Seidman & Wagner, 1992:343), "acknowledging representational subjectivity does not mean abandoning the possibility of differentiating our representations from objects in the outside world". Contrary to Latour's notion of quasi-objects, it is plausible to postulate a world external from ourselves. This physical world is of course a less problematic object of study, and cultural embeddedness poses greater difficulties for the social sciences as part of the culture(s) which are themselves the object of study. As has been previously suggested, in order to approximate objectivity, attempts should be made to replicate a particular study. By repeating a particular investigation in some other context, we can approach research in a comparative fashion, highlighting similarities and differences in the findings, and may better understand and transcend the cultural factors that limit objectivity. Social science can be at once scientific, practical and critical. The commitment of critical theory to social justice is not a culture-specific value, but a reflection of the fact that reason is an integral part of everyday life, and that people can and do draw distinctions between what power is and should be, and between telling stories and telling the truth.

Gordon concludes that "like the positivists themselves, their critics went too far,

claiming in effect that if scientific theories cannot be certain they cannot be objective, and that objectivity must therefore be abandoned, even as an ideal" (Ibid. 608).

The choice between competing theories rests on their usefulness as instruments of investigation. As such scientific investigation is an exercise in pragmatics. The radical critics of scientific practice cite that it is possible to postulate more than one theory to account for observed phenomena, but, as Gordon contends, we are not called upon to prove a scientific theory empirically true, but only to illustrate that it is empirically adequate. To conflate truth and adequacy is to problematise research beyond usefulness.

The replacement of authority and tradition by reason and empirical observation as the means to validate knowledge is a central theme of the Enlightenment, and a central part of the story of modern science. Postmodernist critics point out that authority and tradition play a greater role than scientists' narratives about themselves acknowledge. We also know from the sociology of science the extent to which traditions, paradigms, material scientific cultures, etc. shape the understanding of scientists with regard to the problems they identify, and the possible ways to approach these problems. Science has diversified into so many specialisations that it is no longer possible for a single scientist to know everything about his or her work on the basis of observation and rational deduction alone. Much has to be necessarily accepted on the authority of others and out of tradition. The conventional understanding of science, which places the experiment on centre stage, ignores this crucial matter of dialogue among scientists. Consequently the validation of knowledge, in the conventional understanding of science, is dependent on the procedures of reason and observation, which are applied by a decontextualised individual scientist. Says Calhoun, "but wouldn't we grasp better the nature of scientific revolution (and of the practice, though not the rhetoric, of contemporary science) by focusing on the nature of the public discourse among scientists by which putative new

knowledge is subjected to the critical examination of a range of interrogations?" (Ibid.175).

Calhoun, like other critical analysts of science, wants to refocus our attention away from the individual knower in order to recognise the collective practice by which knowledge is constituted and modified. Says Calhoun, "one does not have to stretch Kant too far in a Habermasian direction to see the notion of universal communicability as an indication that knowledge would only be complete when all different (but clearly commensurable) standpoints were included in the conversation of knowledge. Kant explicitly saw taking the standpoints of others, and ideally of everyone else, as the way to escape the illusion that arises from the ease of mistaking subjective and private conditions for objective ones" (Ibid. 175). Calhoun contends that what is however not clearly recognised, is that this dialogue among scientists must not be conceptualised as simply an error-correction mechanism or a method for after-the-fact verification, "but as one of the actual bases for knowledge" (Ibid. 175).

As has been argued previously, science is, however, not just discourse, but also entails empirical observation and rational deduction. Hence institutional arrangements and interpersonal relationships are not just sources of bias, but also sources of common understandings. Contrary to the centrality of the experiment and its ideal of control, this conception of research is inherently intersubjective, and recognises that observation constitutes a social relationship which introduces change into what is observed, and hence challenges the notion of an objective reality that can be discovered "out there". A further implication of this intersubjective and dialogical view of science is that taking seriously the notion of science as conversation does not necessarily imply a rejection of the role that deductive reason and empirical observation play in knowledge. In this regard Cahoun states that "the postmodernist 'discovery' of discourse if often presented as though it proves the notion of truth based on foundational observation and deduction to be false, rather than as a qualification and

relativisation of such truth claims" (Ibid. 177). This is to treat conversation as essentially arbitrary, as more or less interesting, and not as a means toward practical consensus as a validation of knowledge. In the manner of Peter Winch years ago, and the epistemic symmetry of the Edinburgh School, postmodernists treat different standpoints as incommensurable, and ignore a history of concrete social practices that encourage mutual understanding and even epistemic gain.

Ironically, postmodernists, as the term suggests, can hardly refute epistemic gain in their own terms. As pointed out by McCarthy, they must surely ascribe to some notion that we now know "more and better about the historical variability and cultural diversity of forms of life, about the linguistically mediated character of thought and action, about the contingency and contextuality of rational practices, and the like" (Hoy & McCarthy, 1994: 227). Without assuming that we have learned something in these respects, postmodernist critics would not be able to make sense of their own critical arguments. Says McCarthy, "Looking at the trajectory of critical social theory since its appropriation of Max Weber, we find that its basic direction is quite similar to Foucault's in an important respect: it aims to understand the ways in which reason and rationality have been socially constructed, as a means of achieving a critical self-understanding with implications for practice" (Ibid. 225). The real difference between critical theorists and postmodernists, in this view, has to do with whether there is at all a positive side, or an emancipatory dimension, to the story of the enlightenment. Although postmodernism encourages us to live without an enemy, it stops short of offering constructive bases for mutual understanding and trust. While stressing the need to recognise differences and particularisms, it does not grant the same significance to common interest in concrete practices, or even to universal rights without which there can be no full respect for difference. Like the cultural relativists before them, postmodernists grant no integration based on communication and trust, and ironically encourage the very cultural seperatism that they see as the inevitable by-product of universal standards. As Donna Haraway says in her argument against feminist standpoint theory, "it

incorporates the very presumption that women are all in crucial regards the same, and that was one of the most damaging products of the essentialist reasoning predominant since the Enlightenment" (Calhoun, 1995: 180). As argued previously, we must be careful to avoid the presumption that standpoints, cultures or discourses are inherently unitary, and be open to the internal critiques and competing interpretations within a given standpoint or discourse. This presumption leads to the ironic position in which postmodernists find themselves. As pointed out by Calhoun,

> "postmodernists are often the mirror image of the Enlightenment universalists they challenge, making of difference - especially Derrida's différance - an absolute as rigid as unitary identity or universalism is to their enemies. And if positive, unitary identity is a form of violence against difference, so absolutized difference is a form of violence against intersubjectivity, or more specifically, the human will to bridge the gap between people, traditions, cultures" (Ibid. 91).

As argued in the section on the work of Scott Gordon, like the positivists themselves, their critics have gone too far, claiming that if scientific theories cannot be certain they cannot be objective, and that objectivity must therefore be abandoned, even as an ideal. We are, however, not called upon to believe that a scientific theory is empirically true, only that it is empirically adequate. The choice between theories is dependent on their comparative usefulness as instruments of investigation, and hence scientific explanation is an exercise in pragmatics. As pointed out by Mouton, a commitment to a moral vision of social science does not mean that we have to abandon claims to scientific validity. That value-judgements may, and typically do, enter into all domains of scientific inquiry, more especially in the study of social phenomena, does not mean that the notion of objectivity must be abandoned. Objectivity must be regarded as a methodological ideal, and not as a characterising property of scientific knowledge. The important issue is then a pragmatic one, raising the question of

how the degree of objectivity in scientific work can be improved. This, it has been argued, can only be pursued intersubjectively as a self-correcting mechanism within science as a collective endeavour, when scientists are free to criticise each other, and even to make epistemic gains in the process of openness and competition. Objectivity then, can only be hampered by forms of social organisation which constrain free competition and scientific research. As Horkheimer put it: "That all our thoughts, true and false, depend on conditions that can change .... in no way affects the validity of science. It is not clear to me why the fact of Seinsgebundenheit (i.e., of being historically conditioned) should affect the truth of a judgement - why shouldn't insight be just as seinsgebunden as error?" (Hoy & McCarthy, 1994: 10). To grant that there are no final truths, is not to abandon the distinction between truth and error. We make this distinction according to the available means of knowledge, and test our claims through experience and practice in the present.

Bibliography

Aron, R. (1967), *Main Currents in Sociological Thought*, Middlesex, Penguin Books.

Barnes, B. (1974), *Scientific Knowledge and Sociological Theory*, London: Rouledge & Kegan Paul Ltd.

Barnes, B. (1977), *Interests and the growth of knowledge*, London: Routledge & Kegan Paul Ltd.

Barnes, B. & Edge, D. (eds.) (1982), *Science in context*, Milton Keynes: The Open University Press.

Bendix, R. & Roth, G. (1971), *Scholarship and Partisanship: Essays on Max Weber*, Berkely: University of California Press.

Berger, P.L. & Kellner, H. (1981), *Sociology Reinterpreted*, Middlesex: Penguin Books.

Best, S. & Kellner, D. (1991), *Postmodern Theory*, London: MacMillan Education Ltd.

Bohman, J. (1991), *New Philosophy of Social Science*, Cambridge: Polity Press.

Brown, H. (1988), *Rationality*, New York: Routledge.

Brown, J.R. (ed.) (1984), *Scientific Rationality: The Sociological Turn*, Boston: D. Reidel Publishing Company.

Brown, S.C. (ed.) (1984), *Objectivity and Cultural Divergence*, Cambridge: Cambridge University Press.

Burger, T. (1976), *Max Weber's Theory of Concept Formation*, Durham: Duke University Press.

Calhoun, C. (1995), *Critical Social Theory*, Oxford: Basil Blackwell Ltd.

Collins, R. (1986), *Max Weber: A Skeleton Key*, London: Sage Publications.

Coser, L.A. (1977), *Masters of Sociological Thought*, New York: Harcourt Brace Jovanovich, Inc.

Crawford, T.H. (1993), *An Interview with Bruno Latour*, FILE: ///A/LATOUR: HTM.

Dreyfus, H.L. & Rabinow, P. (1982), *Michel Foucault: Beyond Structuralism and Hermeneutics*, Sussex: The Harvester Press Limited.

Fay, B. (1987). *Critical Social Science*, Cambridge, Polity Press.

Gellner, E. (1975), *The Legitimation of Belief*, Cambridge: Cambridge University Press.

Gellner, E. (1985), *Relativism and the Social Sciences*, Cambridge: Cambridge University Press.

Gerth, H.H.& Wright Mills,C.(eds.) (1948), *From Max Weber: Essays in Sociology*, London, Routledge.

Giddens, A. (Ed.) (1974), *Positivism and Sociology,* London: Heinemann Educational Books Ltd.

Gordon, S. (1991), *The History and Philosophy of Social Science*, New York: Routledge.

Harding, S. (ed.) (1987), *Feminism and Methodology: Social Science Issues*, Milton Keynes: Open University Press.

Hearn, J. (1987), *The Gender of Oppression: Men, Masculinity and the Critique of Marxism*, Brighton: Wheatsheaf.

Hooks, B. (1982), *Black Women and Feminism*, London: Pluto Press.

Johnson, T., Dandeker, C & Ashworth, C. (1984), *The Structure of Social Theory*, London: MacMillan Education Ltd.

Lackmann, L.M. (1970), *The Legacy of Max Weber*, London: Heinemann.

Latour, B. (1987), *Science in Action*, Cambridge, Mass.: Harvard University Press.

Latour, B. & Woolgar, S. (1979), *Laboratory Life: The Social Construction of Scientific facts*, London: Sage Publications.

Lee, D. & Newby, H. (1983), *The Problem of Sociology,* London, UNWIN HYMAN.

Lewis, J. (1975), *Max Weber and Value-free Sociology*, Southhampton: The Camelot Press Ltd.

Lyon, E.S. & Busfield, J. (1996). *Methodological Imaginations*, London: MacMillan Press Ltd.

Mouton, J. & Joubert, D. (Eds.) (1990), *Knowledge and Method*, Pretoria: Human Sciences Research Council.

Nicholson, L.J. (ed.) (1990), *Feminism/Postmodernism*, New York: Routledge.

O'Neill, J. (1987), *The Disciplinary Society: from Weber to Foucault*, British Journal of Sociology, vol. 37, pp 42-60.

Pfohl, S. (1994), *Images of Deviance and Social Control*, McGraw-Hill.

Rabinow, P. (ed.) (1984), *The Foucault Reader*, London: Penquin Books.

Root, M. (1993), *Philosophy of Social Science*, Oxford: Blackwell Publishers.

Rosenau, P.M. (1992), *Post-modernism and the Social Sciences*, Princeton: Princeton University Press.

Sarup, M. (1988), *Post-structuralism and Postmodernism*, Hertfordshire: Harvester Wheatsheaf.

Scott, J. (1994), *Power: Critical Concepts*, London: Routledge.

Seidman, S. (1994), *Contested Knowledge: Social Theory in the Postmodern Era*, Oxford: Blackwell Publishers.

Seidman, S. & Wagner, D.G. (1992), *Postmodernism and Social Theory*, Oxford: Blackwell Publishers.

Skinner, Q. (1985), *The return of Grand Theory in the Human Sciences*, Cambridge: Cambridge University Press.

Swartz, H. & Jacobs, J. (1979), *Qualitative Sociology: A Method to the Madness*, Free Press.

Tarnas, R. (1991), *The Passion of the Western Mind,* Ballantine Books.

Turner, B.S. (ed.) (1996), *The Blackwell Companion to Social Theory*, Oxford: Blackwell Publishers Ltd.

Weber, M. (1949), *The Methodology of the Social Sciences*, New York: Free Press.

Wrong, D.H., *Some problems in defining social power*, American Journal of Sociology, vol. 73, pp. 673-681.

Wrong, D.H. (Ed.) (1970), *Max Weber*, New Jersey: Prentice-Hall, Inc.