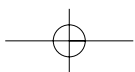
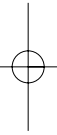
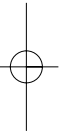
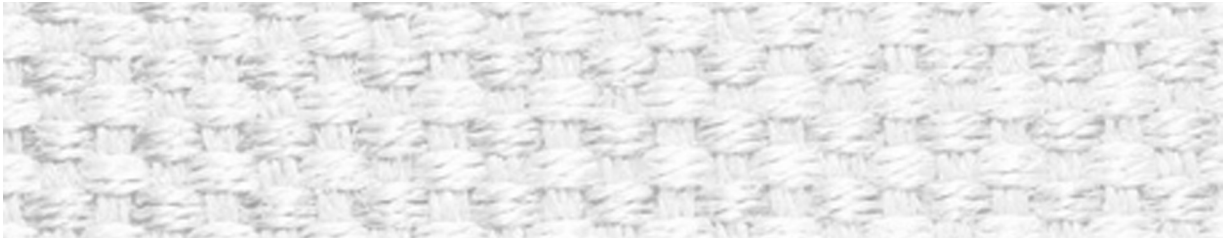
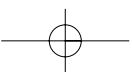
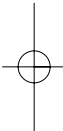
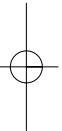


SECTION I

Overviews







Introduction

William Outhwaite

The second half of the twentieth century saw the institutionalization of the social sciences and the rise and fall of the view that the methodologies for the social sciences had to be modeled on those of the natural sciences. This view favored econometrics, behaviorism in psychology, behavioralism in political science, empirical survey research in sociology and, in an extended rather weaker form of the doctrine, functionalism and structuralism in anthropology, sociology and political science and in certain varieties of Marxism. By the mid-seventies, it was generally recognized, except in economics, that this was just one conception of social science and that more qualitatively oriented approaches also had something to offer, especially to feminist social science. The last decade of the century was marked by the continuing rise of rational choice theory and the revival of evolutionary theory at the 'hard' end of the spectrum, and by deconstruction, antifoundationalism and postmodern relativism at the 'softer' end.

Peter Manicas, author of the magisterial *A History and Philosophy of the Social Sciences* (1987), traces in his chapter the intellectual and institutional contours of 'western' social science since 1945. What he calls a scientific approach went along with disciplinary specialization and professionalization, most

strikingly in North America but to an extent also in the UK and Western Europe, for all the differences between these sites. (In the present century, when social science has become substantially globalized, it is important to remember how high national barriers used to be as late as the 1980s: not just across the Iron Curtain, but even within a small space like that of Western Europe.)

Manicas traces the 'rise and fall' of scientism but, as he notes at the end of his essay, the future remains open, with some social scientists, especially in economics and psychology but not only there, looking to a revival of scientific programs, others questioning the very idea of social science, and a third group, including such figures as Pierre Bourdieu and Anthony Giddens, pursuing the idea of social science in non-scientific ways which recall in some respects the social theory of the late nineteenth and early twentieth century. One of the most influential attempts to reinstate the scientificity of the social sciences while recognizing the force of hermeneutic and historically based critiques of positivism has been, especially in the UK, a realist approach derived from the work of Mary Hesse and Rom Harré on models in natural science and extended to the social sciences by Harré himself and by Roy

Bhaskar. On this view, theories are seen as offering fallible models of the real relations between structures and mechanisms in the natural and/or social worlds. A supporter of this approach, Manicas closes with the suggestion that this may offer a way forward for social science.

The disciplinary specialization of the twentieth century was also accompanied by the growth of what came to be called interdisciplinary social science. This is the subject of the second chapter in this section, by Julie Klein, author of *Interdisciplinarity: History,*

Theory, and Practice (1990) and *Crossing Boundaries: Knowledge, Disciplinarity, and Interdisciplinarity* (1996). Klein traces the theory and practice of interdisciplinarity across the century. Like Manicas, she sees an ambiguous situation at the beginning of the twenty-first century: ‘...talk of interdisciplinarity becoming more the “norm” begs the question of how well prepared researchers are for this kind of work.’ Together, these two chapters set the scene for the rest of the volume and demonstrate the need for it.



The Social Sciences Since World War II: The Rise and Fall of Scientism

Peter Manicas

INTRODUCTION

It is well to keep in mind that the disciplines of the social sciences are not 'natural kinds' and that, accordingly, they have a history, intellectual and institutional. While this is not the place to review this aspect, we should note that the disciplinary divisions and the view of science generally taken for granted among most social scientists are both fairly recent, dating only from the immediate post-World War I period. As I have noted elsewhere (Manicas, 1987), were we as social scientists to transport ourselves to Oxford, the Sorbonne, Harvard or Berlin in, say, 1890, we would find practices unfamiliar. There were no 'departments' of sociology or psychology; the research practices would be for us a hodgepodge of philosophy, social theory, history and hard science methods. But if we were to make a similar visit to any prominent American university in 1925, we would find very little which is not familiar.

'American university' is critical in the foregoing statement. As Peter Wagner has

argued, the 'modernization' of the social sciences, including the tendencies toward 'scientization' and 'professionalization', was globally an uneven development. While 'it occurred almost across the board in the United States,' the trajectory was different in Europe, and, indeed, different in England, France, Italy, Germany and Scandinavia (Wagner et al., 1991: 350). No doubt these differences resulted from larger differences in the intellectual legacies of these states, in the particular nature and configurations of the state and civil society, and, more specifically, in differences in the policies and institutions available to meet problems of industrializing mass society. These differences will be pertinent, as I shall try to suggest, in the developments following World War II.

'Professionalization' could be achieved with disciplinary specialization, but the authority to be derived from this required 'scientization' is that social scientists be *scientists*. But one cannot simply assume that this idea is perfectly clear or that prevailing views are not

contestable—and may be mistaken. It is of considerable importance to notice that during the period of the institutionalization of the social sciences, beginning at the turn of the last century, there was a dominating conception of what a science was. This view was profoundly propelled in the 1930s by Vienna ‘logical positivism’ and became by the 1950s the dominating conception among both philosophers and social scientists.¹

In this view, the sciences were not metaphysical: they did not import into their explanations assumptions which could not be tested in experience. In this sense, then, the sciences were ‘empirical’. This meant that the referents of terms in use had to be experimentally available. ‘Hypotheses’, understood as potential explanations, linked ‘variables’ that required evidence which had to be ‘theory neutral’. A theory was simply a premise, a set of hypotheses for which there were deducible empirical consequences. Finally, if metaphysical assumptions were not to be allowed, then explanation had to be in the form of ‘laws’, which, following Hume’s expunging of metaphysics from causality, were ‘regularities’ between associated ‘variables’—‘whenever this, then that’. Explanation, accordingly, proceeded by subsumption under law.

Here is an example from *Research Methods in the Social Sciences* by Frankfort-Nachmias and Nachmias, a textbook in wide use today. They write:

Often the empirical attributes or events that are represented by concepts cannot be observed directly . . . In such cases, the empirical existence of a concept [*sic*] has to be inferred. Inferences of this kind are made with operational definitions (Frankfort-Nachmias and Nachmias, 1992: 31).

The structure of operational definitions is straightforward:

If a given stimulus (S) is applied to an object, consistently producing a certain reaction (R), the object has the property (P) (*ibid.*, 1992: 32).

Similarly:

Ever since David Hume (1711–1776) . . . an application of the term explanation has been considered

a matter of relating the phenomenon to be explained with other phenomena by means of general laws (*ibid.*, 1992: 10).

Modeled on the assumption that there were no critical differences between the natural and social sciences, the approach eschewed subjectivity, theorized society as an objective functioning system, and employed objective methods to identify objective ‘social facts’. This view favored econometrics; behaviorism in psychology; behavioralism in political science; and empirical survey research and quantitative methods and functionalism and structuralism in anthropology, sociology, political science and, perhaps paradoxically, in textbook versions of Marxism. Social science would be science—with a vengeance.

But just as this view of science began to be taken for granted in social science departments in the US, it was coming under attack from philosophers, including its most important expositors. W.V. Quine’s remarkable ‘Two Dogmas of Empiricism’ (1950) and C.G. Hempel’s criticism of his own previous work on explanation (e.g., ‘The Theoretician’s Dilemma’, 1958) led the criticism from within. New directions were taken by Stephen Toulmin’s *Foresight and Understanding* (1961), Thomas S. Kuhn’s incredibly influential *The Structure of Scientific Revolution* (1962), Rom Harré’s generally ignored *Principles of Scientific Thinking* (1970), and Mary Hesse’s *Models and Analogies in Science* (1970). By the mid-70s, *not one of the defining planks of positivism remained*.² Most critical was the idea that a theory of science could be epistemologically ‘foundationalist’ and metaphysically neutral. Thus, neither sense data nor appeals to putative theory-neutral ‘basic sentences’ could warrant truth-claims, for indeed there could be no ‘God’s eye view of the world’. Deductivism was replaced by an ontological realism which made sense of the role of theory in explanation. While there had been decisive criticisms of the covering law model of explanation since at least the 1950s, once Humean causality was replaced by a robust notion of causes as productive powers, the

covering law model also finally had to be rejected.³

It is probably true that a good deal of mainstream thinking in the social sciences is still uncritically beholden to these views. Social scientists, like all others, are not comfortable with fundamental challenges to their ways of doing things. On the other hand, there were always challenges to this dominating view, beginning in a clear way with the work of Max Weber and extending in the recent past to a wide range of alternatives usually termed 'hermeneutic' or 'interpretative' sociologies. These critics sometimes argued that positivism and logical empiricism, or simply empiricism, may well be appropriate for the natural sciences, but that this is a fatally mistaken 'scientistic' approach to the social sciences. Or, more radically, these critics abandoned altogether the idea of a social or human *science*. Critically, neither party challenged the idea of science that was being assumed. But the undermining of the dominating theory of science has opened the way for a deep reconsideration of the nature and methods of social science, including resolution of the older dispute between 'naturalistic' and hermeneutical views of social scientific inquiry.⁴

POST-WORLD WAR II AMERICAN SOCIAL SCIENCE

The work of Talcott Parsons (1937, 1951, 1968) was central insofar as he offered a theory which could claim scientific status and could, even more importantly, easily accommodate the idea that quantitative social science provided the tools for applying a natural science model to the social sciences. Dismissing Marx, Parsons ingeniously absorbed and synthesized interpretations of Durkheim, Weber, Marshall and Pareto into his structural-functionalism. The result was not merely a sociology but a general theory of action, pertinent for all the human sciences. Here indeed, was a general theory reminiscent of Comte's early vision.⁵ Parsons's work captured social scientific

theorizing on the American side of the Atlantic (e.g., Almond and Verba, 1965; Rostow, 1960; Smelser, 1964). But as Hans Joas (1987: 82) has remarked: 'When American sociology set on its triumphal march around the world after the end of the Second World War, it had passed its own historical turning point only a short time before.' Joas's reference is to the pragmatist theory of John Dewey and George Herbert Mead, 'the pioneering methodological achievements of the Chicago School of sociology and the theoretical implications of their large-scale empirical investigations' (ibid., 1987: 82). And Parsons, as Joas notes, 'literally did not devote a single word' to this tradition.

Anti-Scientism in Pragmatic Social Theory

In the academy, two pragmatist strands, both marginal, remained. The first, always acknowledged in anthologies of social theory, is 'symbolic interactionism', named in 1937 by Herbert Blumer. It drew directly on Mead. The other was the work of C. Wright Mills, whose dissertation (written in 1942 and retitled for publication (1964) *Sociology and Pragmatism: The Higher Learning in America*), omitted discussion of Mead and focused on Dewey (Mills later noted that the omission was a big mistake). But it now seems clear that even where there were no explicit references, much of his work was profoundly indebted to both Mead and Dewey.

Mills's best-known book, highly pertinent for present purposes, is *The Sociological Imagination* (1959). In it, Mills offered a savage criticism of both 'Grand Theory' and 'abstracted empiricism'. The attack on Grand Theory was aimed squarely at Talcott Parsons. 'Abstracted empiricism' referred, of course, to the quantitative hard science approach then being powerfully propelled by Mills's Vienna-influenced colleague at Columbia University, Paul Lazarsfeld (1955).

For Mills, echoing a version of Weber which had been submerged by Parsons,⁶

Grand Theory was ahistorical and operated at such levels of abstraction that it could not get down to the real concrete. The 'findings' of abstracted empiricists were, by contrast, uninteresting except for the scientific-bureaucratic and ideological uses to which they were so easily put. Mills called for a different *kind* of social science. It would, in Deweyan fashion, serve concrete human concerns by cultivating the 'sociological imagination', a linking of biography and history. It would be emancipating because it would enable persons to connect their domestic and local situation to the historical and global causes which explained their immediate milieu. Deweyan concerns with eclipse of 'the public' (1927) are evident in Mills's *Power Elite* (1956), an excellent example of how Mills put 'science' to work.

While 'the sociological imagination' is a term that has found its way into all the textbooks, and while Mills's work was important in the 60s and 70s among New Left writers and activists, he had little influence on the direction of inquiry in the social sciences. But at the margins, there is a continuing tradition of writers who, like Mills, draw on their understanding of Weber and Marx, even while they are explicitly non-Marxist. In this tradition, Barrington Moore Jr, a close friend of Herbert Marcuse, is perhaps the most outstanding example. Aspects of this approach find expression today among a range of 'institutionalists' and others working in historical and economic sociology, both of which seem to be having a renaissance in American social science (Theda Skocpol, 1984; Smelser and Swedberg, 1994; Margaret Somers, 1998; Stinchcombe, 1983; Charles Tilly, 1982, 1984).

Blumer similarly made an assault on the presumed science of prevailing social science. Perhaps the most persistent theme in this attack was rejection of the covering law idea—that behavior can be explained by appeal to regularities between 'causative' factors and 'the behavior they are supposed to produce'. 'Thus, the typical sociological scheme ascribes behavior to factors such as status position, cultural prescriptions, norms,

values, sanctions, role demands, and social system requirements ... Similarly, in the typical psychological scheme such factors as motives, attitudes, hidden complexes, elements of psychological organization, and psychological processes are used to account for behavior...' (Blumer, 1969: 7). The fallacy was obvious to him. In both cases, 'the meanings of things for the human beings who are acting are either bypassed or swallowed up in the factors used to account for behavior'. Moreover, they fail to see that 'the use of meanings by a person in his action involves an interpretative process.' Closely following Mead, the actor, in 'communication with himself', 'selects, checks, suspends, regroupes, and transforms meanings in the light of the situation in which he is placed and the direction of his action' (ibid.: 5).

Blumer incorporated the powerful theory of meaning of Mead and Dewey: meanings are not 'psychical accretions' but are instead 'creations that are formed in and through the defining activities of people as they interact' (ibid.: 5). Indeed, for Blumer, 'social interaction is a process that forms human conduct instead of being merely a means or setting for the expression or release of human conduct'.⁷ The rejection of empiricist assumptions regarding explanation demanded a rejection of

the mythical belief that to be scientific it is necessary to shape one's study to fit a pre-established protocol of empirical inquiry, such as adopting the working procedure of advanced physical science, or devising in advance a fixed logical or mathematical model, or forcing the study into the mould of laboratory experimentation, or imposing a statistical or mathematical framework on the study, or organizing it in terms of preset variables, restricting it to a particular standardized procedure such as survey research (ibid.: 48).

Following the Chicago tradition of W.I. Thomas and Robert Park, this reconceptualization of sociology entailed what Blumer termed 'a naturalistic approach' to inquiry, a deep immersion into the life-worlds of transacting actors. As part of this, agency was restored to inquiry. Indeed, this approach took the 'revolutionary' posture of what Rom

Harré and Paul Secord (1973: 6) later referred to as 'the anthropomorphic model of man': for scientific purposes, it would treat people as if they were human beings.

But, writing in 1964, Anselm Strauss noticed that the dominating structural-functional theories found a way to de-radicalize Mead by incorporating some of his seminal ideas into their programs. Thus, 'the generalized other became just another way of talking about reference group affiliation and Mead's notion of role tended to be reinterpreted to fit with the structural concept of status and its associated role-playing' (Kurtz, 1984: 40, quoting Strauss). Meanwhile debate as to whether Mead was behaviorist or phenomenological led to an 'Iowa school' and an 'Illinois school', splitting from the 'Chicago School'. Lingered in the background was the important question of whether Symbolic Interactionism was essentially a *social psychology* which had to be supplemented with a macro orientation or whether, as the founders had suggested, it was an entirely different way to carry on sociology.

There remain in the academy card-carrying symbolic interactionists of various stripes, and many others who do qualitative work but may not explicitly acknowledge the genesis of their approach (see Denzin and Lincoln, 1994). Indeed, many of these seem to have adopted a methodological eclecticism or pluralism which owes in part, perhaps, to Clifford Geertz's (1973, 1983) idea of 'thick description'. Current inquirers would seem to have absorbed a range of interpretive modes, including symbolic interactionism, ethnomethodology, hermeneutics, structuralism and post-structuralism. And many would seem to be comfortable with the idea that their concerns are descriptive and that, in what is seen to be a useful division of labor, macro concerns may be left to others.

Alfred Schütz and Phenomenology

At approximately the same time that Parsons was becoming dominant, the ideas of Alfred Schütz were becoming known in the United States. Schütz had been a member of a

remarkable seminar which regularly met in Vienna in the 1920s, 'the *Mises-Kreis*'. In addition to Ludwig von Mises, it included among its distinguished regulars Friedrich von Hayek, Fritz Machlup, Felix Kaufmann (a member also of Moritz Schlick's more famous Vienna seminar), Oskar Morgenstern and Eric Voegelin. Mises reported that 'in these meetings we informally discussed all the important problems of economics, social philosophy, sociology, logic, and the epistemology of the sciences of human action' (Augier, 1999: 154). Weber and the earlier debates of the *Methodenstreit* were central. Critical here was the question, introduced by Weber, of 'subjective understanding'. But, and this cannot be overlooked, the group accepted Weber's view that *Verstehen* was but the first step in the effort to provide causal explanations in the human sciences. More generally, for the *Mises-Kreis*, there was the question of whether there was necessarily a distinct science of human action that would incorporate economics, sociology and politics. Mises had originally titled this 'sociology,' but by then the discipline was sufficiently well entrenched, and he therefore renamed his project 'praxeology'. A convincing case has been made that it was in this context, rather than the context of Husserlian phenomenology, that Schütz initially formed his ideas (Augier, 1999; Prendergast, 1986).

The Nazis would force the *Mises-Kreis* (as with the *Wiener Kreis*) to immigrate to the US (or to Britain). Schütz moved to New York in 1939. Von Hayek, then in London, suggested to Schütz that he review for *Economica* a new book by Talcott Parsons, *The Structure of Social Action* (1937). This initiated a correspondence between Parsons and Schütz which led to Schütz's decision not to publish his review. As the editor of this material notes: 'The reader will find himself engaged in an intense, sometimes stormy, and, at places, embittered exchange of notes and letters, which leads into a rather poignant debate on the differences between phenomenological and structural-functional analyses' (Grathoff, 1978: xvii). These texts allow us to get clearer on

some of the central issues in social science, and especially the genesis of the important work of Goffman and Garfinkel. But unfortunately, some still hotly contested issues remain.

Since both Schütz and Parsons had taken Weber as a point of departure, one might have supposed that they could easily achieve a meeting of minds. Such was not the case, and, as now seems clear, they disagreed fundamentally on the nature and status of subjective meaning as regards the theory of action. Schütz insisted that inquiry needed to investigate the meaning *actually meant* by actors and thus had to address the taken-for-granted problem of intersubjectivity—a problem missed also by the Austrian economists. Parsons, by contrast, ‘made subjective meaning a theoretical concept and, consequently, was largely substituting socially pre-given norms and values for individual motivations’ (Wagner, 1983: 77). It was just this move, of course, which allowed for Parsons’s ‘macro’ solution to ‘the voluntaristic theory of action’ and which, from the point of view of Schütz (and Blumer!) as well as Goffman and Garfinkel) led them to the conclusion that agents had effectively disappeared. Thus Schütz writes:

Professor Parsons has the right insight that a theory of action would be meaningless without the application of the subjective point of view. But he does not follow this principle to its roots. He replaces subjective events in the mind of the actor by a scheme of interpretation of such events, accessible only to the observer, thus confusing objective schemes for interpreting subjective phenomena with these subjective phenomena themselves (Grathoff, 1978: 36).

But,

the answering of our question, ‘What does the social world mean for me, the observer?’ has as a prerequisite the answering of the quite different questions, ‘What does this social world mean for observed actors within this world, and what did he mean by his acting within it?’ With these questions, we no longer naively accept the world and its current idealizations and formalizations as ready-made and meaningful beyond all doubt, but undertake to study the process of idealizing and formalizing as such, the genesis of the meaning which social phenomena have for us as well as for

the actors, the mechanism of the activity by which human beings understand one another and themselves (Wagner, 1983: 48).

It was but a short step from this to the projects of Goffman and Garfinkel and more generally to the key ‘social constructionist’ idea, shared with Symbolic Interactionism, that social phenomena are the outcome of practical activities by skilled actors engaged in a taken-for-granted world and that any valid inquiry in social science must begin with an effort to grasp the meaning of an action actually held by them.⁸

But Schütz seems to have assumed—or assumed away—the problem of intersubjectivity. Augier (1996: 159) argues that ‘Schütz wanted the concept of intersubjectivity to be unquestionable’ and did not, for this reason, want to enter into question about ‘the transcendental constitution of the ‘natural attitude’. But as Schütz later admitted, ‘it is “a scandal of philosophy” that so far the problem of our knowledge of other minds and, in connection therewith, of the intersubjectivity of our experience of the natural as well as the socio-cultural world, has not found a satisfactory solution ...’ (Schütz, 1954: 265). Here one might insist that Mead’s social behaviorism and Dewey’s account of experience is the far better response just because it disavows at the outset a Cartesian ego (Manicas, 1992).

The foregoing discussion also responds to the question of the relation of Schütz and Parsons to a ‘positivist’ theory of science. Schütz, like Weber, was very often explicitly anti-positivist, but it is critical to see why. In the well-known essay of 1954, ‘Concept and Theory Formation in the Social Sciences’, Schütz directly engaged Ernest Nagel and C.G. Hempel, leading empiricist participants in an APA symposium of the same title. First, there was no argument that for both the natural and social sciences, ‘the principles of controlled inference and verification by fellow-scientists and the theoretical ideals of unity, simplicity, universality, and precision prevail.’ This seems fundamental and sufficiently neutral between possible alternative conceptions of science. But the second point at issue is a different matter.

Schütz agreed that “theory” means in all empirical sciences the explicit formulation of determinant relations between a set of variables in terms of which a fairly extensive class of empirical regularities can be explained’ (Schütz, 1954: 260). This ‘deductivist’ idea was, of course, a pillar of the positivist theory of science, fully shared by Parsons and by many contemporary writers like Jonathan H. Turner (1987). Parsons, as noted, ‘hoped that the theory of action would ... eventually “be stated as system of simultaneous equations”—a system whose several variables were duly allocated to the different social sciences’ (Camic, 1987: 431, quoting Parsons).

Schütz’s encounter with Weber was mediated by Mises, who had offered a powerful critique of Weber’s ideal-type reading of neo-classical theory. The concepts of economics were not, in Mises’ view, ‘one-sided intensification of one or several aspects’ of a concrete, but were, as Schütz put the matter, ‘derived by abstraction from aspects of each of the individual phenomena taken into consideration’ (Augier, 1999: 158). But Schütz’s sympathy with Mises’s conception of economic theory worked against his more fundamental Weberianism. Thus, it is easy to show that Schütz should not have been so polite and should have rejected the conception of theory as a deductive system whose entailments were ‘laws’ or events to be explained by subsumption under laws. For Schütz, once having established the subjective meaning shared by the actors, theory involved the construction of models of ‘typical’ behavior by ‘personal types’. These are constructed ‘homunculi’ or ‘puppets’ to which we ascribe in-order-to and because motives. Implicit here is the idea that reasons are causes. And in contrast to the positivist dream, nothing would be deduced. Rather, theory would yield *understanding* by giving us ‘the mechanism of the activity by which human beings understand one another and themselves’.

Neo-classical economic theory had provided a model which provided an account of the mechanisms which produced prices, and these were, as Schütz agreed, derived from

postulates regarding the motivation and beliefs of individuals. But as was acknowledged, these postulations could not be said to be true of *actual* economic behavior—a problem for economics and, more generally, as we shall see, till today for what is called ‘rational choice theory’.

What then was the objection to Nagel’s and Hempel’s naturalism? For Schütz, both had misunderstood Weber’s ‘postulate of subjective understanding. *Verstehen* has nothing to do with introspection, but ‘is the result of processes of learning and acculturation ...’ It is not a private affair and it can be controlled through the use of evidence. Finally, and paradoxically, given the emphasis on prediction in the empiricist theory of science, predictions based on *Verstehen* are continuously and with high success made in common-sense thinking (Schütz, 1954: 264). The consequence was a redefinition of the tasks of an empirical human science. As already noted in his criticism of Parsons,

all forms of naturalism and logical empiricism simply take for granted ... social reality ... Intersubjectivity, interaction, intercommunication, and language are simply presupposed as the unclarified foundation of these theories. They assume, as it were, that the social scientist has already solved his fundamental problem, before scientific inquiry starts (Schütz, 1954: 261).

Erving Goffman

As is well known, both Parsons and Schütz were important with regard to the work of Goffman and Garfinkel, both of whom also acknowledged debts to William James and Ludwig Wittgenstein. But getting clear on where they stand with respect to these writers—or to SI theory—remains contested. A big part of the problem regards their respective understanding of phenomenology and whether, unlike SI theory, what they offered was a challenge to the way of doing sociology or, rather, a supplement to this. In what must be taken as a provocative disclaimer, Goffman remarked that in *Frame Analysis* (1974), his most self-consciously theoretical book, he was making

no claim whatsoever to be talking about the core matters of sociology—social organization and social structure ... I am not addressing the structure of social life but the structure of experience individuals have at any moment of their social lives. I personally hold society to be first in every way and any individual's current involvement to be second; this report deals only with matters that are second ... The analysis developed does not catch at the differences between the advantaged and the disadvantaged classes and can be said to direct attention away from such matters. I think that this true. I can only suggest that he who would combat false consciousness and awaken people to their true interests has much to do, because the sleep is very deep. And I do not intend here to provide a lullaby but merely to sneak in and watch the way people snore (Goffman, 1974: 13–14).

Indeed, with its emphasis on ‘the structure of experience’, the posture taken in *Frame Analysis* seems more phenomenological than anything offered by Schütz. On the other hand, much of the substantive work of Goffman looks very much like a Schützian construction of a model of typical types—and indeed, as the foregoing hints, with critical, even emancipatory, implications.

Consider here *Asylums* (1961). Goffman establishes two typical sorts of actors standing in a well-defined social relation: ‘the managers’ and ‘the managed’ (‘professionals vs. clients’, ‘staff vs. inmates’). They jointly participate in the construction of their identities and roles. Thus, the managed get constructed as something less than full persons, while managers are constructed as competent to ‘treat’ the managed. Each of the two parties has goals (which ‘provide a key to meaning’) and each has a system of beliefs (for the managers, an ‘interpretative scheme’ which includes ‘a theory of human nature’). Typically, the ‘managed’ undergo ‘mortification’, the construction of a different self. The managed also have resources. Resistance by them takes on a number of forms, including contesting the meaning of rules, ‘fraternization’, and ‘playing it cool’ (Goffman 1961: 61–65). ‘Institutional ceremonies’—including, for example, a newsletter produced by inmates, an annual party and an open house—are regular events in the life of the institution. These are intended to produce a

joint commitment to the official goals, even if, to be sure, everyone ‘on the inside’ knows better. Goffman very convincingly shows how the beliefs of actors, true *and* false, function in sustaining an institution in which there is a manifest disjunction between the official goals of the institution and the actual outcomes, and how even the inmates, contrary to their intentions, contribute to the outcomes.

In this marvelous account, one can easily discern key elements of both SI and Schützian perspectives. Moreover, it is distinctly antagonistic—and not complementary—to a Parsonian account.

Harold Garfinkel

Similarly Garfinkel, after distinguishing ‘Formal Analytic (FA) technology’ (mainstream sociology) and Ethnomethodology (EM), insisted that ‘FA’s achievements are well known and pointless to dispute.’ ‘Ethnomethodology (EM) is proposing and working out ‘What More’ there is to the unquestionable corpus status of formal analytic investigations’, namely,

to find, collect, specify, and make instructably observable the local endogenous production and natural accountability of immortal familiar society’s most ordinary organizational things in the world, *and to provide for them both and simultaneously as objects and procedurally, as alternative methodologies.* (Garfinkel, 1996: 6)

FA and EM are, he insists, ‘incommensurably different. Nevertheless, they are inextricably related’ (ibid., 1996: 10). What is their relation? Garfinkel offers, enigmatically, that ‘it is a social fact in its own right’ that ‘they are asymmetrically alternate’ (ibid., 1996: 10). Maynard and Clayman (1991: 387) argue that ethnomethodology is ‘neither a critique, reaction, or rebellion against other forms of social theory, but rather a positive respecification of how investigators might approach sociology’s most awesome phenomenon—the objective, immortal reality of social facts.’

But it would surely seem that ethnomethodology, like SI theory, is a distinctly

different way *to do* sociology. It is even less clear, but probably true, that neither Schütz nor Garfinkel were much indebted to Husserl's introspective and cognitive version of phenomenology, even while they adopt a Husserlian *Epoché* as regards 'reality'. Methodologically at least, they would seem to share with Blumer a distinctly sociological perspective which demands a commitment to naturalistic observation and participation.

Ethnomethodology has spawned a wide variety of empirical work, including efforts to discern generalizable properties of practical common-sense reasoning, and more particular instantiations of these procedures in a wide variety of contexts, including the criminal justice and health systems and, importantly, in the sociology of scientific knowledge.⁹ Conversation analysis is an offshoot which was eagerly adopted but seems not to have been sustained. But indeed ethnomethodology, like the work of Goffman, has been integrated into the impressive meta-theories of Giddens and Bourdieu. Before turning to them, one last distinctly American development, rational choice theory, needs some attention.

Rational Choice Theory

There was one response to structural functionalism which aimed 'to bring people back in', but fully endorsed a positivist theory of science. Rational Choice Theory (RCT) has, to be sure, a long lineage in social theory, going back at least to Hobbes, who clearly articulated several of its main premises, for example, that we must look at individuals acting 'rationally' if we are to understand society, and that 'rationality' can be unpacked in terms of maximizing 'utility'. These ideas were systematically extended in the development of political economy, but especially in neo-classical economic theory. But until the 1950s, sociologists and political scientists remained unaffected by this fundamental orientation.

Homans was explicit in his view that action can be explained by appeal to fairly straightforward principles of behavioral

psychology and that this proceeded by appeal to the covering law model of explanation. Homans insisted, with some credibility, that 'many social scientists who in fact use behaviorism do not realize that they are doing so. They call it utilitarianism or rational-choice theory,' and indeed,

one advantage that would accrue to all of us if we accepted and acted upon the covering law view of theory is that different schools would have to ask themselves what covering laws they would in fact use if they formalized their theories ... I think that all the schools would find that they would use principles of behavioral psychology, either in what I have called the stripped down form or in one that embodies more fully the still-developing experimental findings (Homans, 1987: 79).

One might here be reminded of C.S. Peirce's observation that 'the yoking together of the scientific ox and speculative ass' remains a problem for too much of social science.

But Homans inspired what became 'exchange theory', perhaps initiated by Peter Blau (1964). Working explicitly within an economic framework, Blau argued that the 'costs' and 'rewards' of social exchange—e.g., a marriage—answered to the same principles as market exchanges for goods, even if, to be sure, assessing the 'values' was more difficult. Unlike Homans, Blau acknowledged that some outcomes of interactions by rational individuals were emergent, for instance that while RCT can explain, presumably, the behavior of bureaucrats, bureaucracies have features which are not reducible to the exchanges of the parties. James Coleman (1990) has made the latest effort to systematically build social theory on generously conceived RCT premises. While not without its critics (Green and Shapiro, 1996), RCT is vigorous in American political science. Indeed, RCT also defines what is called 'Analytic Marxism' (Roberts, 1996)!

No doubt much of the motivation for the development of RCT as a general theory of action came from dissatisfaction with Parsons's theory, with the notion that modern micro-economics was an eminently successful science and with the idea, encouraged by positivist conceptions of theory, that theory

construction could now proceed in sociology and political science with the use of sophisticated mathematics and the new powerful computers.

But of course, one can reject the assumption that neo-classical theory—including its most sophisticated mathematical and econometric forms—is a successful social science.¹⁰ It is pertinent to notice that Mises and Hayek were already critics of general equilibrium theory exactly on the grounds that critical assumptions of the theory could not be met. They shared this line of criticism with Veblen in the US and the later institutionalists and economic sociologists who followed in this tradition. One might defend the mainstream view by taking the explicitly positivist posture well put by Milton Friedman (1953) that the assumptions of a theory need not be true—e.g., assumptions regarding rationality—if indeed, the theory provides ‘good predictions’. But even if it could pass this test—which it does not—it is hard to see how one can *explain* an outcome on the basis of assumptions known to be false?

Marxism and the American Academy

Marxism, in both scientific and non-scientific forms, was a challenge to mainstream scientism. The scientific form (Second International variety) never made much headway in American academic social science, but, as Gintis has noted, ‘Marxian economics has dwelt as an undercurrent in American academic thought for at least a century’ (Ollman, 1992: 53). A historically oriented political economy became important in the 1960s in direct response to the anti-war, civil rights and feminists movements, which challenged the consensus of the dominating paradigms: neo-classical economics and Parsonian theory in sociology. As Bruce Cumings has more recently noted,

Because of the ferment of the 1960s, there emerged in the 1970s a social science which met a high standard of quality and relevance. In political science, sociology, and even to some extent economics, political economy became a rubric under

which scholars produced a large body of work on the multinational corporation, the global monetary system, the world pool of labor, peripheral dependency, and American hegemony itself. (Cumings, 1998: 180)

But, writing in 1998, he also says that ‘it was amazing to witness the alacrity with which social scientists abandoned this political economy program’ (1998: 181). Times had indeed changed. Similar considerations apply to the work of Herbert Marcuse, a long-standing member of the exiled Institute for Social Research. Marcuse remained in the US after the institute returned to Frankfurt and was important to the development of radical social analysis, especially in the so-called ‘New Left’ in the 1960s. But while the critical theory of Horkheimer, Adorno and Benjamin remains pertinent to contemporary concerns, it never did take hold in the US, and has, along with the work of Marcuse, remained marginal in the US academy. But, as noted with reference to Barrington Moore, features of Marx’s orientation filtered into a wide range of non-Marxist work.

One important possible exception is the work of Immanuel Wallerstein (1974). ‘World Systems Theory’ certainly entered the thinking and vocabulary of many social scientists, Marxist and non-Marxist alike. Wallerstein drew on Braudel, whose ‘structuralism’ was a part of the French structuralist movement. We say ‘possible exception’ here since, as Brenner (1977) has argued, Wallerstein is better described as ‘a neo-Smithian Marxist’ rather than as Marxist *tout court*. Put briefly, as with Braudel (Tilly, 1984), Wallerstein’s concern is ‘conditions of exchange’ rather than the classical Marxist ‘mode of production’.

MARXISM AND THE EUROPEAN RESPONSE TO SCIENTISM IN SOCIAL SCIENCE

It is misleading, of course, to write as if there were not continuous influences between Europeans and Americans over the contested terrain of the social sciences. Not only were

many American and European social scientists reading texts being produced by colleagues across the Atlantic but, as already noted, Schütz and the *Mises-Kreis* were but part of a large exodus of intellectuals from Germany and Austria following the accession to power of Hitler. Others included members of the critically influential Vienna Circle, and at about the same time both the entire *Institut für Sozialforschung*—the so-called Frankfurt School—and the intellectually heterogeneous group which found a home at Alvin Johnson's New School for Social Research. These included Hannah Arendt, Leo Strauss, Aron Gurwitsch, Claude Lévi-Strauss, Roman Jakobson and Adolph Lowe. Many others scattered in the American academy should be mentioned, including Eric Voegelin, Norbert Elias and Franz von Neumann. Vienna positivism was not in the least alien to the American scene and quickly took hold. Schütz's influence has also been noted. But while the members of the Frankfurt School, along with almost all the others, were in the 1940s and 50s already arguing for styles of social science which were explicitly historical and anti-positivist, these European writers, both Marxist and non-Marxist, have had but marginal influence as regards the US academy.

The European scene was different. Mainstream American social science came late to European social science. In part, this was a consequence of the continuing tradition established by Weber of a historical sociology (or of sociology as a propaedeutic to historical inquiry) and, more critically, the continuing pertinence of Marx. This last stems, in part at least, from the presence in Europe of viable working-class and Marxist political parties—a feature entirely absent from the American experience. In what follows I concentrate on developments within Marxism and its role in redefining the nature and character of social science. It is not an overstatement to say that nearly all the interesting recent alternatives to US mainstream social science were European and also drew on Marx.¹¹ This includes the development of Critical Theory, the innovations owed to the

Italian Antonio Gramsci, the existential Marxism of Jean-Paul Sartre and Maurice Merleau-Ponty, the hermeneutics of Gadamer and Ricoeur, and both structuralism and post-structuralism.

Germany

The Institute for Social Research had been created in 1923 to promote Marxist studies. The first generation, Horkheimer, Adorno and Marcuse, prominently and with differences, reconsidered the debt of Hegel, incorporated Freud and the lately published writings of the young Marx, rejected the eschatology of the Second International reading of historical materialism and turned their attention to cultural concerns that were missing in the older Marxist tradition. After the Institute returned to Frankfurt in 1950, as Jay (1973: 292) notes, 'instead of developing in relative isolation' it would become 'one of the major currents of German sociological and philosophical thought'. Some critics, of course, have argued that the turn taken early on and reinforced by its exile in the US made it less and less convincingly Marxist (Anderson, 1976). But of course that depends to a considerable extent on what is to count as 'Marxist'.

While these issues cannot be pursued here, we can also note that the current dominant second-generation figure, Jürgen Habermas, has not adopted the pessimism which was the result of the first generation's analysis of the highly repressive forces of 'rationalization', a key legacy of the work of Weber. To avoid pessimism and to combat more recent attacks on 'reason' from 'post-modernist' quarters, Habermas has returned to a version of Kant which offers a novel way to defend an 'Enlightenment' concept of reason in the face of repressive 'rationalization' (Outhwaite, 1994). Habermas, whose Weber tends in the direction of Parsons, has made serious efforts to incorporate American traditions into his version of critical theory, and, perhaps as part consequence, his work appears in US mainstream contexts. Related currents in Germany include a revitalized Parsons in the

work of Richard Münch (1987) and the systems theory approach of Niklas Luhmann (1997).

As noted, the tradition of Weber was also a key part of the German scene, including but not restricted to the radicalization of the idea of *Verstehen* with the work of Hans-Georg Gadamer. If it be granted that there is a necessary hermeneutic moment in any social science and that interpretation requires ‘a fusing of horizons’, was Habermas correct in claiming (versus Gadamer) that ‘hermeneutic consciousness remains incomplete as long as it does not include a reflection upon the limits of hermeneutic understanding?’ (quoted by Outhwaite, 1985: 190). This is also a theme confronted by Bourdieu, Giddens and Bhaskar.

France

There are similarities in the French tradition, where Marxism was also a vital intellectual force. The pre-World War II work of Henri Lefebvre (who offered, in 1937, the first French translations of the writings of the young Marx), and the work of Lucien Goldmann, a student of Lukács, established the presence of Marxism in France. The War assured its prominence. Mark Poster (Poster, 1975: 4) writes:

The only moral force left in France, on the eve of Liberation, came from the resistance movement, which had been dominated by politically progressive groups ... With a combined socialist and Communist vote reaching a majority, intellectuals harbored the dream of an imminent and radical social transformation.’

Alexandre Kojève and Jean Hyppolite had brought Hegel to the attention of French intellectuals.¹² Simone de Beauvoir summarized his pertinence to them: ‘We had discovered the reality and weight of history; now we were wondering about its meaning’ (quoted by Poster, 1975: 20). Coupled with the powerful new ideas on alienation, and the attending incorporation of phenomenology, the study of Hegel’s *Phenomenology* became ‘an intellectual source for the renewal of

Marxism, for Sartre’s existentialism, and perhaps even for the structuralism of the 1960s’ (Poster, 1975: 5). Indeed, as now seems clear, ‘structuralism’ was a specific response to this ‘renewed Marx’, especially as promoted by the ‘existential Marxists’ Sartre and Merleau-Ponty.

Existential Marxism drew on Hegel and phenomenology—making it dubiously ‘scientific’, at least as that was conceived by many. And the Second Internationalist idea of a ‘scientific’ Marxism was hardly dead. Indeed, perhaps, as Althusser (1969) was to insist, there were two Marxes, one ‘romantic’ and ‘metaphysical’, and the other ‘scientific’. Worth noting, the official position of the French Communist Party favored a more ‘scientific’ Old Marx, ‘shorn of the idea of alienation’, and, indeed, of any ‘humanist’, non-scientific, ‘philosophical’ strands.¹³

Althusser’s structuralism (developed between 1960 and 1965) was a response to this question. But there were a range of other theories—all French—which have been termed ‘structuralist’. All these, despite differences, start with Marx and share in rejecting both phenomenology and the turn to a ‘humanist’ Marx. And all of them represent an anti-empiricist, alternative conception of social science. These include the linguistic structuralism of Roland Barthes, the work of Lacan, ‘a psychoanalyst combining Freudian orthodoxy with Heideggerian overtones’, Lévi-Strauss’s Durkheimian structuralist anthropology, and the *Annales* historians whose work offered that ‘the individual agent and the individual occurrence cease to be central elements in social explanation’ (Clark, 1985: 180).¹⁴ But Kurzweil rightly notes that ‘traces, or influences of existentialism and/or Marxism continue to be found in the work of such diverse figures as Barthes, Foucault, Lacan, Lévi-Strauss, and others,’ and, to add to the confusion, these figures very often share attitudes toward economic injustice with Marxists (Kurzweil, 1980: 3).

As regards Althusser’s structuralism, Perry Anderson writes: ‘For the first time, a major theoretical *system* was articulated

within the organizational framework of French Communism, whose power and originality were conceded even by its most determined opponents' (1976: 38).¹⁵ As is well-known, Althusser argued that there was 'an epistemological break' in the Marxian corpus.¹⁶ His very influential structuralist alternative drew on all the structuralists, but especially on Saussure and Lacan. A key theme pertinent for present purposes is summarized by Poster (1984: 34): 'In Lacan's complex and often opaque formulations, the subject is constituted in the unconscious through a process mediated by language, which fixes the subject in decentered misrecognition of itself.' This idea could be enriched by the structuralist linguistics of Saussure. Reading Marx 'symptomatically', the 'objective text' could then be 'decoded'.

Althusser offered a host of new ideas that became familiar—if often unclear—coin. Thus, a society was an ensemble of *practices*: economic, political, ideological and theoretical, comprising a 'social formation' (Althusser 1969: 166f.). A practice, for Althusser, was 'any process of *transformation* of a determinant given raw material into a determinant product' (ibid.). Practices then include different kinds of 'parts'. For example, economic practice includes raw materials, tools and workers, all united in the production process. Theoretical practice includes (as raw materials) 'ideology', the pre-given concepts which are the ideas of the 'lived' common-sense world, and theory. With theory, then, these are transformed into scientific knowledge (ibid.: 182f.).

An enduring problem of historical materialism was the relation of the 'base' to the 'superstructure'. This was 'solved' with the idea of 'structure in dominance'. The elements of the 'totality' are asymmetrically related. But the base 'determines' which of the asymmetrical elements are dominant at any given time (1969: 213). This allowed Althusser to refocus the problem of revolution and more generally of historical change. Finally, with practices as the unit of analysis, Althusser was able explicitly to expunge agents from his explanatory framework, and

thus any reference to humanism or phenomenology. Thus,

The structure of the relations of production determines the places and functions occupied and adopted by agents of production, who are never anything more than the occupants of these places, insofar as they are supports (*Träger*) of these functions. The true 'subjects' (in the sense of constitutive subjects of the process) are therefore not these occupants or functionaries, [who] are not, despite all appearances, the 'obviousness' of the 'given' of naïve anthropology, 'concrete' individuals', 'real men'—*but the definition and distribution of these places and functions* (quoted from James, 1984: 151).

History, accordingly, is 'a process without a subject'.

These ideas are powerfully in the background of arguments in British Marxism, and thence to the work of Roy Bhaskar and Anthony Giddens. But before leaving the French academy, we need to notice the responses of Foucault and then of Bourdieu. In the 1960s Foucault was in agreement with structuralist writers in rejecting Marxist humanism and phenomenology. He agreed also on the decisive role of language in constituting social reality. But he never quite succumbed to an agentless fatalism. The days of May 1968 are critical. As Poster argues, 'The events of May 1968 signified that an oppositional stance toward existing society was possible beyond the confines of contemporary Marxist orientations' (1984: 7). What came to be called 'the New Social Movements': the women's movement, gay rights, ecology, anti-nuclear, prison reform, patient's rights, etc., could not be fit into the revolutionary class analysis of standard Marxism. Foucault and others, including Derrida, Deleuze, Guattari, Castoriadis, Lefort, Lyotard and Braudrillard struggled for answers for what they took to be an entirely new social and political condition.

Some of these writers—Derrida, for example—seemed to have despaired not merely of offering an emancipatory social science but of the possibility of knowledge and truth at all. Having already rejected humanism, Derrida abandoned completely the idea that reality could be 'represented'.

He opted for 'deconstruction'. As summarized by Hoy (1985: 4): 'Deconstruction shows the failure of a work's attempt at representation and by implication, the possibility of failure of any such work, or by any text whatsoever.' As Hoy sees it, 'grammatology' was a very radical hermeneutics: Instead of arguing that there was a problem to be solved in interpreting a text, 'Derrida would make us unable to read it.'

Dissidents in anthropology, especially sensitive to issues of neo-colonialism, sexism and racism, found the Derridian challenge liberating.¹⁷ It was not difficult to show that the standard ethnographies offered representations which were in the interests of the colonizers and of elite males. But political critique would seem to require that there had to be *some* veridical representation.

Sometimes unnoticed, Foucault, despite sharing some key assumptions with Derrida, was one of his sharpest critics—exactly because the only politics which it seemed to allow was dubious. Instead of offering deconstruction, Foucault, drawing on Bachelard via Canguihem and Althusser, offered first 'archaeology', a way to inquire into the groundwork of bodies of knowledge; and in the post 1968 writings, 'genealogy'—'a form of history which can account for the constitution of knowledges, discourses, domains of objects, etc., without having to make reference to a subject which is either transcendental in relation to the field of events or runs in its empty sameness throughout the course of history' (Foucault, in Calhoun et al, 2002: 204).¹⁸

Foucault, then, like the structuralists, is properly seen as providing a critique of the conventional wisdom as regards the sciences, but especially those sciences whose focus is 'life, labor and language'. While 'archaeology' and 'genealogy' parallel efforts in the sociology of knowledge,¹⁹ his aim would also seem to be critical—without assuming that there is some system of thought which could be known to actually 'represent' 'reality' and, as part of this, without assuming any sort of 'autonomous' self.²⁰ We might say that this is social construction with a

vengeance; and indeed, it raises a host of questions and possible responses.

The power/knowledge couplet is a central and influential feature of Foucault's effort to rethink history and the constitution of subjects. For Foucault, power is an inherent feature of all social relations and functions where there are alternative possibilities of action to constrain or direct action. Moreover, power is an inherent component of the production of truth (knowledge). But, in contrast to liberal and Marxist thought, Foucault's anti-realism makes this insight epistemologically relevant. That is, a liberal or Marxist might assent that claims made by various 'disciplines' are secured as authoritative through the use of structured power, but still argue that some or all of these claims are false. Foucault would insist that this is not a helpful response. At the same time, he has provided important historical trajectories of the constitution of modern medicine, psychiatry, punishment, sexuality, and the attending construction of active subjects—active because they are participants in this construction. He has argued that these are forms of 'disciplinary technology' and are, as such, forms of domination. Indeed, for Foucault, aligned with Weber and Critical Theory, while disciplinary technologies were a precondition for capitalism, we are, for him, fast approaching a 'disciplinary society.'

That Foucault stands in opposition to this is plain. But his critics have often noted that he would seem to lack epistemological ground for this posture (Philip, 1985: 79). While his pronouncements are often unclear, ambiguous and perhaps equivocal, he seems to offer a version of anarchism—a generalized resistance to power in all its forms (Schürmann, 1985: 546; Rabinow, 1984: 22).

In the US, Foucault's influence is considerable in Women's Studies. It commands some attention in political science and anthropology, but only recently does it seem to have made some inroads into sociology departments. Even so, his work is usually thought of as a strand of what is unhelpfully called 'postmodern' theory. Here, the emphasis seems more structuralist than

Foucauldian. As the editors of a recent American collection have noted, it is not clear 'whether Foucault should be considered a philosopher or a historian' (Calhoun et al., 2002: 188). But in the case of France, we need to compare his work to the work of Pierre Bourdieu, the successor to Foucault's chair in the *Collège de France*.

Like Foucault, Bourdieu absorbed the vigorous French debate between existential Marxism, phenomenology, structuralism and post-structuralism, and, like Foucault, he made the effort to transcend the whole string of polarities and dichotomies which had characterized that debate. These included the antinomy between 'subjective' and 'objective' modes of knowledge, the separation of the cultural and symbolic from the material, the divorce of theory and practice, and, more familiar to American sociology, the 'micro-macro' gap and the dualism of agency and structure.

Typically European, his effort to re-vision social science begins, logically, with epistemology and ontology. Indeed, as with Foucault, Bhaskar and Giddens, it is probably best to call the work of these writers 'meta-theory' insofar as they are philosophical theories about the nature and domain of a human science, and how this is to be studied. And the most direct way into his effort is to suggest a comparison to the work of Foucault.

Bourdieu agrees with Foucault (and Derrida) that the idea of scientific 'objectivity' must be deconstructed, that power always plays a role in sustaining scientific belief. He agrees also that 'reason' needs to be historicized and that there can be no appeal to a transcendental subject. But employing a version of Foucault's appropriation of Althusser, he 'partakes wholeheartedly of the Enlightenment project of reason' (Bourdieu and Wacquant, 1992: 47n.). Wacquant (Bourdieu and Wacquant, 1992: 47) quotes him:

Against this antisocialism which is the fashion of the day and which brings grist to the mill of new ideologists, I defend science and even theory when it has the effect of providing a better understanding of the social world. One does not have to

choose between obscurantism and scientism. 'Of two ills,' Karl Kraus said, 'I refuse to choose the lesser.'

He hopes to manage this with two moves, with his concept of 'epistemic reflexivity' and with the Althusserian idea of 'scientific practice'.

Wacquant summarizes 'epistemic reflexivity' as 'the inclusion of a theory of intellectual practice as an integral component and necessary condition of a critical theory of society' (Bourdieu and Wacquant, 1992: 6). It differs from the usual notions of reflexivity in three ways: 'first, its primary target is not the individual analyst but the social and *intellectual unconsciousness* embedded in analytic tools and operations; second, it must be a *collective enterprise* rather than the burden of a lone academic; and third, it seeks not to assault but to *buttress the epistemological security of sociology* (1992: 6).²¹

As with Althusser, 'practices' are the key unit of analysis (Turner, 1994), and, as for him again, a form of realism is sustained by the theoretical practice of a proper social science. The task of sociology, he writes, is to 'uncover the most profoundly buried structures of the various social worlds [fields] which constitute the social universe, as well as the "mechanisms" which tend to ensure their reproduction or their transformation' (Bourdieu and Wacquant, 1992: 7). As the product of properly reflexive theoretical work, these are 'objectivities', but there is a 'constructivist' 'moment', identified but misconceived by 'subjectivist' approaches.²² Thus, 'if it is good to recall, against certain mechanistic visions of action, that social agents construct social reality, individually and also collectively, we must be careful not to forget, as the interactionists and ethnomethodologist often so do, that they have not constructed the categories they put to work in this construction' (Bourdieu and Wacquant, 1992: 10).

Two central concepts in this proffered solution are 'habitus' and 'field'. 'Habitus' are 'systems of durable, transposable dispositions, structured structures predisposed to

function as structuring structures, that is, as principles which generate and organize practices and representations that can be objectively adapted to their outcomes without presupposing a conscious aiming at ends or an express mastery of the operations necessary in order to obtain them' (in Calhoun et al, 2002: 277). They are 'embodied history', traits of character, attitudes and capacities acquired by individuals who have 'internalized' structure. They get played out in a 'field', which 'may be defined as a network, or a configuration, of objective relations between positions.' Paralleling Foucault's 'discourse/practices', one can speak of the field of the academy, or the economic, artistic, religious, or political field. And in an Althusserian mode, these are 'relatively autonomous', 'spaces of objective relations that are the site of a logic and a necessity that are *specific* and *irreducible* to those that regulate other fields' (Bourdieu and Wacquant, 1992: 97). Thus, what is called 'society' is not an integrated 'system' and thus it cannot be reduced to an overall logic, e.g., capitalism. Actions are neither autonomous nor mechanical products, but are the outcome of specific tendencies (constitutive of habitus) of agents located in a field which defines the possibilities of action. Finally, one can speak of 'fields of power.' Given this meta-theory, specific theories pertinent to a specific time and place are then called for—e.g., as regarding a specific field of power.

The field of power is a *field of forces* defined by the structure of the existing balance of forces between forms of power, or between different species of capital. It is also simultaneously a *field of struggles for power among the holders of different forms of power* ... The struggle for the imposition of the dominant principle of domination leads, at every moment ... to a *division of the work of domination*. It is also a struggle over the legitimate principle of legitimation ... (Bourdieu, 1996: 376 emphasis in the original).

Wacquant is probably correct in judging that Bourdieu's overall re-visioning of the social sciences has not much penetrated US academic social science. This includes not only Bourdieu's effort to reformulate an

epistemology and ontology for the social sciences but also his attack on the very idea of disciplines in the human sciences.²³ Instead, pieces of his project have been appropriated—e.g., the idea of symbolic capital, his analysis of the field of cultural production, and his critical ethnographies.

Great Britain

One might hold that the divorce of social science from history was the most critical step in the empiricist effort to assure the 'scientific' character of social science. In part, at least because the tradition of Weber and Marx remained viable in Europe, there was never there a complete divorce of history and social science. But this was perhaps most pronounced in British social science, which, as in France and Germany, came late to a 'disciplinary' division of labor (Soffer, 1978; Tribe, 1999; Vout, 1991). But the particular character of British social science is especially shaped by the early genesis of capitalist society in England from the 17th century and by British imperialism. As regards the latter, in a story too complicated to even sketch here, we can think of the critical role of British anthropology (Gellner, in Evans-Pritchard, 1981). As regards the former, there is both the tradition of British political economy from Adam Smith to Alfred Marshall to John Maynard Keynes, and the important tradition of British Marxism, especially beginning with Christopher Hill's *The English Revolution* (1940). As in France, Marxism was comfortably part of the intellectual atmosphere of Britain, but among Marxists, especially in the generation of the post-World War II period to the 70s, historians dominated. As Tribe writes: 'The history of theoretical Marxism in Britain assumes the form of writings on history' (1981: 1). The most important writers here include Maurice Dobb, Rodney Hilton, E.J. Hobsbawm, G.E.M. de Ste. Croix, (the expatriated) M.I. Finley, E.P. Thompson, Raymond Williams and Perry Anderson. This history is not absent of either controversy or of consequences regarding thinking in Britain in the social sciences.

For our purposes the critical problem is the proper understanding of 'historical materialism' (a term never used by Marx).²⁴ Although it is clear enough that the key authors, beginning with Hill and certainly including E.P. Thompson, had long since departed from Second International orthodoxy, they proceeded in their historical work without much explicit theory. Structuralism and the French debates had filtered across the Channel in the 60s. At the same time, developments in the philosophy of science in both the US and France entered the argument. Out of this welter came the efforts of Roy Bhaskar (1978) and Anthony Giddens (1978) to resolve the 'agency/structure' bifurcation; Cultural Studies, initiated by Stuart Hall (1980), and the attending question of a 'structure/culture' bifurcation; the development in Edinburgh by David Bloor (1976) and Barry Barnes (1977) of the so-called 'Strong Programme in the Sociology of Science'; and, finally, the emergence of a 'realist' theory of science appropriate to the human sciences (Bhaskar, 1976/78).²⁵ Critical here was Bhaskar's effort to show that the long standing conflict between 'naturalist' and 'hermeneutic' views of social science depended on a spurious empiricist theory of science; and that once one adopted a realist theory of science, the insights of both naturalistic and hermeneutic approaches would find their place.

Bhaskar and Giddens seemed to have arrived at their social scientific meta-theories at about the same time, and while they share much, there are differences. Bhaskar identified himself as a Marxist. At Oxford, he was powerfully influenced by the groundbreaking work in the philosophy of science of Rom Harré (1970). Bhaskar (1976) pressed these themes and added a novel philosophical argument in defense of his version of realism—'transcendental realism.' This includes a critique of the usually unnoticed ontology presumed by an empiricist theory of science, and a penetrating analysis of the nature and role of experiment in the natural sciences, an analysis with serious implications for the social sciences. He turned his attention to the social sciences in

his 1978 essay and then in a book, *The Possibility of Naturalism* (1979). Giddens has explicitly denied an identity as a Marxist, even though he has defended Marx's *Capital*, and has remarked that his project 'might accurately be described as an extended reflection upon a celebrated and oft-quoted phrase to be found in Marx ... that "Men [let us immediately say human beings] make history, but not in circumstances of their own choosing"' (Giddens, 1984: xxi).

In what follows, I concentrate on what is broadly shared by Bhaskar, Giddens, Bloor and Barnes.²⁶ In contrast to the 'interpretivist skepticism' which characterizes 'post modern' epistemology, the point of departure for Bhaskar, Giddens and the Edinburgh group is a realism which posits *a knowable and causally efficacious independently existing nature*. But for all four, versus positivist epistemology, given the impossibility of standing outside of a historically constituted conceptual scheme, 'objectivity' is not absolute and requires a hermeneutic moment.²⁷

Second, for all four, society is a social construction, the outcome of 'a skilled performance, sustained and 'made to happen' by human beings' (Giddens, 1976: 15). But, following Marx, actors work with 'materials at hand'—historically sedimented structured practices. For Giddens, 'structures' are constituted by indexically interpreted 'rules' which legitimate, define and sustain social relations. These relations in turn constitute 'resources' for actors. Resources are means of power, and as Mills, Foucault and Bourdieu insist, power is the central concept of social theory. But for Giddens, structure, as incarnate in activity, has but 'virtual existence'. Accordingly, for Giddens, agency/structure dualism is replaced by a 'duality' in which there are no agents without structured practices and no structured practices without agents. The central concept of his meta-theory is 'structuration'—'the attempt to determine the conditions which govern the continuity and dissolution of structures or types of structure' (1976: 120). For Giddens, then, as for all four of these writers, since these conditions are historically various and

contingent, social science is inevitably historical and concrete, and there can be no general theory of social change. Finally, Giddens is committed to the idea that apart from natural causes, *only* agents are causes.

Bhaskar refers to his theory as 'the transformational model of social activity' (TMSA). Since 'structures' pre-exist for any individual (but not for all), human activity does not create structure: agents reproduce and transform it (1979: 42). Parallel to Giddens, he writes of a duality of practice. Bhaskar provides a convincing dispositional analysis of reasons as causes, an elaborated theory of ideology, a critique of the hermeneutical circle, and an account of the critical consequences for confirmation of the absence of experiment in the social sciences. But he is less clear regarding the ontology of 'structure'. The question is not the non-observability of social structures (since on realist grounds, theorized structures of the natural world need not be observable), but rather whether, as in natural science, they have a causal role and if so, in what sense? Thus, he offers that we can assume that 'there are structures producing social phenomena analogous to the causal mechanisms of nature' (1986: 8). As with Bourdieu's notion of 'fields', if social structures are 'like' magnetic fields, then, of course, they play a causal role, but they would then seem also to exist independently of action.²⁸

Third, for all four, acknowledging the power of the tradition of interpretative sociology, there is a critical hermeneutic moment for all social science. But in contrast to Schütz, for example, actors' understandings of themselves and their social world are corrigible. Thus, getting a grasp of the actors' understanding is but a first step (albeit an essential one) for social science. For Bhaskar, staying within the tradition of Marx, there is always the possibility of ideology; for Giddens, 'the knowledgeable ability of human actors is always bounded on the one hand by the unconscious and on the other by unacknowledged conditions/unintended consequences of action' (1984: 282). Hence, as with C. Wright Mills and Bourdieu, but in

contrast to the usual readings of Goffman and Garfinkel, social science is potentially emancipating.

CONCLUSION

Beginning in the 1950s, we have seen both a vigorous critique of the empiricist philosophy of science and a clear and defensible alternative in some form of realism. Attending this was an explosion of efforts to redefine social science in non-positivist terms: from pragmatism to hermeneutics to structuralism to post-structuralism to the synthetic efforts of Bourdieu, Bhaskar and Giddens. But it is not clear that positivism and its correlative scientism have been expunged, except perhaps among philosophers. On the other hand, dissidents in the academy seem more attracted to the view that the very idea of a human science is a mistake. But while Foucault, Bourdieu, Bhaskar and Giddens would agree that a *scientific* social science is part of the problem, unlike many fashionable dissidents they would insist also that a proper human science is also a critical part of the solution.

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NOTES

1 The foundations were laid in the US just after World War I when German-inspired historical social science was expunged and replaced by quantitative and behaviorally oriented programs. Symptomatic is Herbert Hoover's 1929 gathering of a distinguished group of social scientists 'to examine the feasibility of a national survey of social trends'. Funded by the Rockefeller Foundation with the full support of the Social Science Research Council and the Encyclopedia of the Social Sciences, four years of work by hundreds of inquirers resulted in 'The Ogburn Report', 1600 pages of quantitative research. Pitirim Sorokin, who had no objection to the appropriate use of statistics, was not impressed. He noted: 'In the future some thoughtful investigator will probably write a very illuminating study about these 'quantitative obsessions' ... tell how such a belief became a vogue, how social investigators tried to 'measure' everything; how thousands of papers and research bulletins were filled with tables, figures, and coefficients; and how thousands of persons never intended for scientific investigation found in measurement and computation a substitute for real thought' (cited from Smelser, 1986: 27; see also Manicas, 1990).

2 Although the work of Harré is omitted therein, a useful one-volume review of this history is Suppe (1977).

3 The ground-breaking work on causality is Harré and Madden (1974). See also Bunge (1979) and Bohm (1894). One might argue that the covering law model is a defining attribute of 'empiricist' (positivist, neo-positivist) understandings of science. For a sample of some of the critical philosophical literature see Scriven (1959, 1962); Harré (1970, 1986); Dretske (1977);

Bhaskar (1975); Salmon (1978, 1984); Achinstein (1981); Aronson (1984); Woodward (1984); Lewis (1986); Kim (1987), Manicas (2006).

4 Most of the writers in a very influential 1963 text edited by Maurice Natanson assumed a positivist theory of science which was then polarized against a phenomenological alternative (see Natanson 1963). An essay by Thelma Lavine offered that the problem was to 'naturalize' *Verstehen*, an idea roundly rejected by both Ernest Nagel and Natanson. Natanson offered: 'To reinvoke naturalistic criteria as correctives for a reconstructed naturalistic method is to take a step forward and follow with a step back.' For Natanson, since *Verstehen* was 'foundational', the 'way out' was 'the transcension of naturalism in favor of a phenomenological standpoint'. Indeed, after saying that W.I. Thomas, Cooley and Mead were 'all representatives of the phenomenological standpoint', Natanson offered that this "'transcension" could be achieved by adopting the phenomenological stance of Edmund Husserl.' But it is not clear that the Americans should be so identified, despite some similarities. Nor indeed, is it even clear what program Alfred Schütz was pursuing.

5 Already in 1937 Parsons had insisted that 'not only do theoretical propositions stand in logical interrelations to each other so that they may be said to constitute "systems" but it is in the nature of the case that theoretical systems should attempt to become "logically closed". That is, a system starts with a group of interrelated propositions which involve reference to empirical observations within the logical framework of the propositions in question.' And indeed, 'the simplest way to see the meaning of the concept of a closed system in this sense is to consider the example of a system of simultaneous equations. Such a system is determinate, i.e., closed, when there are as many independent equations as there are independent variables' (1937: 9-10) This was Pareto's dream, too often unacknowledged in the theoretical work of Parsons.

6 The relation of Mills to Hans Gerth, and their relation to Parsons's Weber makes for a good story in the sociology of the academy. See Oakes and Vidich (1999).

7 Explicitly drawing on Mead, in his 1940 'Situating Actions and Vocabularies of Motive', Mills had said much the same: 'As over against the inferential conception of motives as subjective "springs" of action, motives may be considered as typical vocabularies having ascertainable functions in delimited social situations ... Rather than fixed elements "in" an individual, motives are the terms with which interpretation of conduct by *social actors* proceeds. This imputation and avowal of motives are social phenomena to be explained' (Mills, 1963: 439). That is, for Mills, as for Blumer, the task is distinctly sociological and suggestive of the later work of Goffman and Garfinkel.

8 For Schütz: 'Summing up, we come to the conclusion that social things are understandable only if

they can be reduced to human activities; and human activities can be made understandable only by showing in-order-to or because motives. This fact has its deeper reason in that I am able to understand other people's acts while living naively in the social world only if I can imagine that I myself would perform analogous acts if I were in the same situation as the Other, directed by the same motives or oriented by the same in-order-to motives—all these terms understood in the restricted sense of "typical" sameness...' (Grathoff, 1978: 53).

9 See especially, Lynch et al. (1983); Lynch (1985); and Livingston (1986). Ethnomethodology and strands from France have influenced Mulkey (1985), Woolgar (1988), and Ashmore (1989) in a more radical 'reflexive' (anti-realist) program.

10 Writing in 1982, Nobel Prize-winner Wassily Leontief had this to say: 'Page after page of professional economic journals are filled with mathematical formulas leading the reader from sets of more or less plausible but entirely arbitrary assumptions to precisely stated but irrelevant theoretical conclusions ... Year after year economic theorists continue to produce scores of mathematical models and to explore in great detail their formal properties; and the econometricians fit algebraic functions of all possible shapes to essentially the same sets of data without being able to advance, in any perceptible way, a systematic understanding of the structures and the operations of a real economic system' (Lawson, 1997, quoting Leontief, 1982: 104).

11 Two important non-European neo-Marxist developments must be noted here. 'Dependency theory' originated in Latin America with the early work of Paul Prebisch, Celso Furtado, Rodolfo Stavenhagen, Theotonio Dos Santos and Fernando Cardoso and Enzo Faletto. Appropriating key insights from the American Paul Baran, several variations, represented prominently by André Gunder Frank, Immanuel Wallerstein and Samir Amin, emerged. The central idea was the rejection of Marx's optimistic scenario, shared by mainstream modernization theory, in which the extension of capitalism globally would produce development globally. It was clear enough that this was not happening. 'Dependent development' produced pockets of development at the expense of continuing underdevelopment. Critical in the debate over the explanation of this was the question of the very idea of capitalism, whether it was defined in terms of the mode of production (as in Marx), or whether in terms of market relations (as in Wallerstein).

The other very important non-European development came from a group of Indian writers called 'the subaltern group'. As Edward Said remarked, these writers, 'fiercely theoretical and intellectually insurrectionary', sought an alternative to the problem that 'hitherto Indian history had been written from a colonist and elitist point of view, whereas a large part

of Indian history had been made by the subaltern classes...' (Foreword to Guha and Chakravorty Spivak (eds.), 1988: v). Said notes that all these writers are critical students of Marx, and that they have drawn on a variety of sources, including structuralist and post-structuralism writers. See below.

12 Kojève's classes in Hegel included Raymond Aron, Maurice Merleau-Ponty, Albert Camus, Georges Bataille and Jacques Lacan. Hyppolite taught Hegel to Michel Foucault, Gilles Deleuze, Louis Althusser and Jacques Derrida.

13 In this context, 'humanism' includes the following elements: (a) the assumption of a human nature which defines a human essence, (b) a rejection of assumptions of existential freedom, and (c) a denial of the enlightenment vision of historical progress. See Hoy 1985.

14 A very much shared set of assumptions and distinctions in use by structuralist writers derives from the early work of de Saussure, whose fragmented *Cours de Linguistique Générale* appeared posthumously in 1916. But it is not clear why his work became so important to the generation which followed World War II. Lévi-Strauss, often called the first structuralist, appropriated a host of distinctions, if not the de Saussurian linguistic model, to extralinguistic materials. See his *Anthropologie Structurale* (1958) and *La Pensée Sauvage* (1962). For a very useful discussion of Lévi-Strauss, see David Boon, 'Claude Lévi-Strauss', in Skinner (1985). For discussion of the structuralist 'linguistic model', see Philip Pettit (1977). Barthes's *Mythologies* (1957) was also critical in this development. His view 'that language does not follow reality but signifies it' and that the analysis of structure offers 'not so much reality as intelligibility', is found also in the work of the *Annales* group. See Clark (1985).

15 His ideas quickly became *de rigueur* in France, and rapidly spread to Great Britain and to various parts of the Third World. They made little headway in the US, but then neither had existential Marxism. Régis Debray was his student, and it is said that Ché Guevara favored his views. He was, unlike most of the existential Marxists, a member of the French Communist Party. Indeed, when it did not join the students in the events of May 1968—a genuinely critical moment for French intellectuals—many were disillusioned regarding his posture as an independent intellectual interested only in promoting a truly 'scientific' Marx.

16 The term, *coupure épistémologique* was made popular by Althusser, who, however, put it his own use. The term was introduced by the philosopher of science Gaston Bachelard in *La Formation de l'Esprit Scientifique* (1938) to refer to the necessary but discontinuous ruptures in conceptualization and framework from common sense to the scientific. Bachelard is ill-studied in the US, but his work was critical to a whole generation of French Marxists, including, importantly, via Canguilhem, the work of Foucault.

Preceding Kuhn, Bachelard sought to replace Cartesian foundationalist epistemology and to redefine 'objectivity' in historicist terms, similar in some ways to the effort of Weber. For an excellent account of Bachelard, see Mary Tiles (1984).

17 See J. Clifford and G.E. Marcus (1986), R. Rosaldo (1989), P.T. Clough (1992).

18 Commentators have noted a number of critical shifts following the events of 1968. In addition to an obviously overt political concern, these include the shift to genealogy, which, unlike archaeology, was understood in terms of power, a shift from systems of exclusion—e.g., the insane or criminal—to concern with how humans turn themselves into subjects; and, finally, a shift from language to 'discourse/practice', a shift which, it seems, was lost on some his American epigones.

19 Hacking speaks of 'systems of thought' as Foucault's domain and notes that these are not transparent and are studied 'by surveying a vast terrain of discourse that includes tentative starts, wordy prolegomena, brief flysheets and occasional journalism. We should think about institutional ordinances and the plans of zoological gardens, astrolabes or penitentiaries; we must read referees' reports and examine the botanical display cases of the dilettante' (Hacking, 1979: 42).

20 This is best seen in his rejection of the concept of ideology: 'The notion of ideology appears to me to be difficult to make use of, for three reasons. The first is that, like it or not, it always stands in virtual opposition to something else which is supposed to count as truth. Now I believe that the problem does not consist in drawing a line between that in a discourse which falls under category of scientificity or truth, and that which comes under some other category, but in seeing how effects of truth are produced within discourses which in themselves are neither true nor false. The second drawback is that the concept of ideology refers, I think necessarily, to something of the order of a subject. Thirdly, ideology stands in a secondary position relative to something which functions as its infrastructure, as its material, economic determinant, etc.' (Calhoun, 2002: 204).

21 This is evidently different from both the 'interpretivist skepticism' which characterizes work influenced by Derrida, and it is also very different from what Wacquant terms 'textual reflexivity', a posture which appropriates a hermeneutic approach. Bourdieu comments: 'What [has] to be done [is] not magically to abolish [the distance between the observed and the observer] by a spurious primitivist participation but to objectivize this objectivizing distance and the social conditions which make it possible, such as the externality of the observer, the techniques of objectivation he uses, etc.' (Bourdieu and Wacquant, 1992: 42f.).

22 Comparison to Berger and Luckmann (1969) is apt here. There is a Hegelian tone to both, but it is not clear whether the form of 'dialectic' transcends

'subjective'/'objective' or whether it collapses into a Cartesian ontology in which 'subject' and 'object' are related causally, reminiscent of Engels' classic effort. Appeal to 'dialectics' is always troublesome. Compare also Giddens (1984) and Bhaskar (1978)..

23 Once we adopt his re-visioning, we can see 'how artificial the ordinary oppositions between theory and research, between quantitative and qualitative methods, between statistical recording and ethnographic observation, between the grasping of structures and the constructing of individuals can be. These alternatives have no function other than to provide a justification for the vacuous and resounding abstractions of theoreticism and for the falsely rigorous observations of positivism, or, as the divisions between economists, anthropologists, historians and sociologists, to legitimate the limits of competency: that is to say that they function in the manner of a *social censorship* ...' (Bourdieu and Wacquant, 1992: 28).

24 Of significance, the successor to Isaiah Berlin's chair at Oxford was the Marxist philosopher G.A. Cohen, who, significantly, made his mark with an effort to 'defend' historical materialism. His 'defense' (1968) amounted to both a functionalism and a technological determinism. This generated a host of critical responses, including Derek Sayer's (1987) excellent work. See also Giddens (1981).

25 Derek Sayer (1979) makes a persuasive case that Marx's implicit theory of science was a powerful form of realism.

26 In addition to the work done by the Edinburgh group, other strands in Sociology of Scientific Knowledge (SSK) must be mentioned. All reject the Merton-defined American mainstream sociology of knowledge. Harry Collins (1985) inspired a group at Bath; and a 'Paris' group, led by Bruno Latour (1979, 1987) with Bachelard and Canguilhem in the background, emerged. Another 'continental independent'

is K. Knorr Cetina (1981). An extremely useful collection is Pickering (1992).

27 Compare Bourdieu (1992). There are also differences between the three on how to resolve the problem of relativism in epistemology. Bhaskar gives the most developed argument for his transcendental realism. Giddens has not pursued the problem in any detail, but see his remarks in his 1966 (Introduction, and pp. 144–154). Since indeed, the Edinburgh group is doing sociology of knowledge and since it is key feature of it being a 'strong programme' that 'the same types of causes would explain true and false beliefs' (Bloor, 1976: 7), their work has generated a huge critical literature from philosophers who are profoundly offended by its 'relativism'. For a critical review of some of this, see Manicas and Rosenberg (1985, 1988).

28 In Aristotelian fashion, Bhaskar distinguishes efficient causes—agents—and material causes, as for Marx, the 'materials of action'. For a critical account, see Varela and Harré (1996). Paradoxically, Giddens has also been read as dissolving agency into structure. See Ashley (1989: 277).

For a defense of Bhaskar and criticism of Giddens see Porpora (1989) and Archer (1995). These writers insist that on Giddens's account (but not Bhaskar's), 'structure' is insufficiently 'objective.' Compare here Bourdieu. This has been the more typical response to Giddens. See also Michael Burawoy (1998), who, while valuing 'reflexivity' and ethnographic depth, holds that for Giddens 'in the end intuitive notions of structure evaporate and we are left with a voluntarist vision that emphasizes the control we exercise over our worlds.' See also Manicas (2006) and Sewell (1992), who argues that 'resources' must be theorized as having actual rather virtual existence. But this would seem to reinstate the bifurcations that Giddens was trying to transcend.