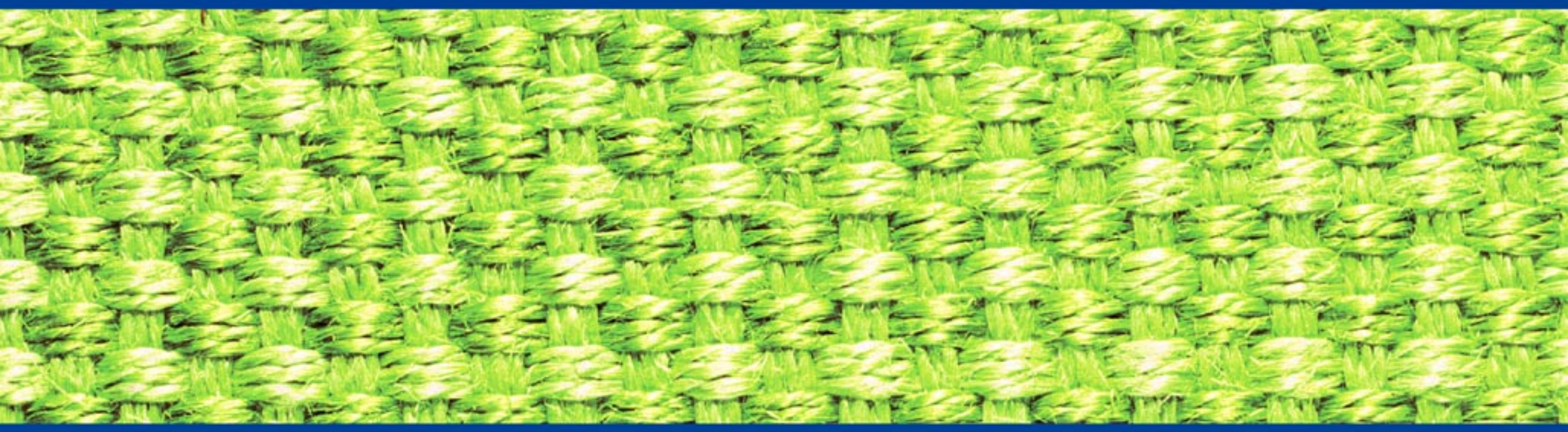
The SAGE Handbook of Social Science Methodology



Edited by
William Outhwaite
Stephen P. Turner

SAGE

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Contents

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Margo Anderson is Professor of History and Urban Studies at the University of Wisconsin, Milwaukee. She received her Ph.D. in History from Rutgers University in 1978. Her research and teaching interests have focused on the history of the social sciences and the development of official data systems, particularly censuses and surveys. She has published several books on the history of the American census, most notably *The American Census: A Social History* (Yale University Press, 1988); and with Stephen E. Fienberg, *Who Counts? The Politics of Census Taking in Contemporary America* (Russell Sage Foundation, revised edition, 2001). She teaches American labor, urban and women's history, and has taught quantitative history since the late 1970s. She taught Quantitative Historical Analysis in the ICPSR Summer Program from 1991 to 1995 and from 1996 to 2001, and she also served as a member (1998–2003) and chair (2000–2002) of the ICPSR Council. In 2006 she served as the president of the Social Science History Association. Her current research focuses on the use of population data in time of war and the ethical issues surrounding public data use.

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Norman K. Denzin Distinguished Professor of Communications at University of Illinois at Urbana Champaign; Research Professor of Communications, Cinema Studies, Sociology, Criticism and Interpretive Theory; received his Ph.D. from the University of Iowa in 1966. He joined the Sociology Department at Illinois in 1966. Professor Denzin's academic interests include interpretive theory, performance studies, qualitative research methodology, and the study of media, culture and society. He is the author, co-author or co-editor of over 50 books and 200 professional articles and chapters. He is the past president of The Midwest Sociological Society, and the Society for the Study of Symbolic Interaction. He is founding president of the International Association of Qualitative Inquiry (2005–), and director of the International Center of Qualitative Inquiry (2005–). He is past editor of *The Sociological Quarterly*, founding co-editor

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Ricca Edmondson is senior lecturer in political science and sociology at the National University of Ireland, Galway. Her training and subsequent work in Lancaster, Oxford and Berlin allowed her to combine philosophy, politics, sociology and ethnography, in the belief that argumentation, in the social sciences and elsewhere, cannot be understood except interdisciplinarily. Previous books have dealt with rhetoric, organizations and culture; she has also published on social capital, time, ageing, health and interculturality. She co-edits two book series on interdisciplinary approaches to the problems of modernity, and co-convenes the network on Ageing in the European Sociological Association. Recent research fellowships have come from the Irish Research Council for the Humanities and the Institute for Advanced Studies in the Humanities at the University of Edinburgh. She is now working on an international project on the idea of wisdom and its application to fields such as argumentation and ageing.

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John R. Hall, Professor of Sociology at the University of California, Davis, has also served as Director of the UC Davis Center for History, Society, and Culture, and Director of the University of California Edinburgh Study Centre. His scholarly research spans epistemology, social theory, economy and society, the sociology of religion, and the sociology of culture. His published books include an edited volume, *Reworking Class* (Cornell University Press, 1997), *Cultures of Inquiry: From Epistemology to Discourse in Sociohistorical Research* (Cambridge University Press, 1999), *Apocalypse Observed: Religious Movements and Violence in North America, Europe, and Japan*, co-authored by Philip D. Schuyler and Sylvaine Trinh (Routledge, 2000), *Sociology on Culture*, co-authored by Mary Jo Neitz and Marshall Battani (Routledge, 2003), and *Visual Worlds*, co-edited by Blake Stimson and Lisa Tamiris Becker (Routledge, 2005). His current research focuses on apocalyptic terrorism and modernity.

Leslie A. Hayduk is Professor of Sociology at the University of Alberta. He has published several articles and two books on structural equation modeling, and is a co-author of two books on the sociology of education. He has been an active participant on the SEMNET web discussion group for the past several years. His research interests span the physiological/biological foundations of sociology, social psychology, and structural equation modeling. His recent publications have addressed issues like an improved definition of R^2 (the blocked-error-R^2), the development of a new class of indicators (reactive indicators), and a demonstration that cancer-fighting T-cell activity (with or without interferon-γ) is better measured at low effector/target cell ratios. He introduced the saying: A picture is worth a thousand words, but is only worth a thousandth of an equation, Picture = 1000Word = .001Equation.

Susan Hekman is a Professor of Political Science and the Director of Graduate Humanities at the University of Texas at Arlington. Her most recent books are *The Future of Differences: Truth and Method in Feminist Theory* and *Private Selves, Public Identities: Toward a Theory of Identity Politics*.

David Henderson is Professor of Philosophy at the University of Memphis. He received his Ph.D. from Washington University in St Louis. He has published numerous articles in the philosophy of the social sciences, many focusing on questions concerning the role of rationality in the social and psychological sciences. Some of this work coalesced in *Interpretation and Explanation in the Human Sciences* (1993). He has produced related work on conceptual schemes, and on the respective roles in interpretive understanding of empirical results and of capacities for simulating others. Recent publications have also addressed central issues in epistemology. These include an account of objectively justified belief (one that makes principled room for both coherentist and foundationalist themes), discussions of the implications of recent work in cognitive science for contemporary epistemology, and a revisionist account of *a priori* knowledge. Much of his work in epistemology has been undertaken jointly with Terrence Horgan of the University of Arizona.

David C. Howell is Emeritus Professor at the University of Vermont. After gaining his Ph.D. from Tulane University in 1967, he was on the faculty of the Department of Psychology at the University of Vermont. He retired as chair of the department in 2002. He also spent two separate years as Visiting Professor at the Universities of Durham and Bristol in the United Kingdom. Professor Howell is the author of several books and many journal papers, and he continues to write and serve on editorial boards even after retiring. His latest project was the *Encyclopedia of Statistics in Behavioral Science*, of which he and Brian Everitt were editors-in-chief. Professor Howell now lives in Colorado, where he has all of the outdoor recreational opportunities anyone could want, as well as the time to remain professionally active.

Douglas Kellner is George F. Kneller Chair in the Philosophy of Education at UCLA and is the author of many books on social theory, politics, history and culture, including works in cultural studies such as *Media Culture* and *Media Spectacle*; a trilogy of books on postmodern theory with Steve Best; a trilogy of books on the Bush administration, including *Grand Theft 2000*, *From 9/11 to Terror War*, and his latest text *Media Spectacle and the Crisis of Democracy*. His website is at http://www.gseis.ucla.edu/faculty/kellner/kellner.html.

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Hans-Herbert Kögler is Chair Professor of Philosophy at the University of North Florida, Jacksonville. He received his Dr. Phil. at the Goethe University of Frankfurt (advisor J. Habermas) after graduate studies at Northwestern, the New School, and the University of California at Berkeley. His research centers on the methodological grounds of understanding and criticism in the human and social sciences, a hermeneutic theory of cultural self-identity and the normative implications of interpretation. Major publications include *The Power of*

Dialogue: Critical Hermeneutics after Gadamer and Foucault, (1999); *Michel Foucault* (2nd edition, 2004) and the co-edited volume *Empathy and Agency: The Problem of Understanding in the Human Sciences* (2000). In 1997, the journal *Social Epistemology* dedicated a special issue to Kögler's article 'Alienation as Epistemological Source: Reflexivity and Social Background after Mannheim and Bourdieu'. His further work includes many journal articles and book chapters in English and German as well as translations into Czech, Russian, and French. He has been invited to serve as guest professor at the University of Klagenfurt, Austria, and the Czech Academy of Social Sciences, Prague. Since 2005 Kögler has coordinated the newly inaugurated graduate program M.A. in Practical Philosophy & Applied Ethics at UNF.

John Law is a Professor at Lancaster University in Sociology and the Centre for Science Studies. With a background in both sociology and Science, Technology and Society (STS), he is interested in disorder, multiple orderings and materialities. He works primarily on nature and culture, agriculture, spatiality, and catastrophes, and he is currently exploring the 2001 UK foot and mouth epizootic. His most recent book, *After Method* (Routledge, 2004), is on methodologies for knowing disorderly phenomena, and it brings together humanities and social science insights to propose a much more generous and inclusive understanding of research method that is able to deal with 'mess'. His website is at http://www.lancs.ac.uk/fss/sociology/staff/law/law.htm.

Tyson Lewis received his Ph.D. in educational philosophy from UCLA in 2006 and is currently an assistant professor of education at Montclair State University. His work in the fields of educational theory and cultural studies has appeared in such journals as *Educational Theory*, *Educational Philosophy and Theory*, *The Philosophy of Education Society's Yearbook*, *Utopian Studies* (with Richard Kahn), and *Cultural Critique* (with Daniel Cho). Currently his research focuses on the relation between critical theory, critical pedagogy and the emerging field of biopolitics.

Michael Lynch is a Professor and Director of Graduate Studies in the Department of Science & Technology Studies, Cornell University. As a student and collaborator of Harold Garfinkel, he took an ethnomethodological approach to natural science practices during doctoral and post-doctoral studies at UC, Irvine and UCLA. His book, *Art and Artifact in Laboratory Science* (1985) was part of the first wave of ethnographic studies of laboratories in social studies of science. His book, *Scientific Practice and Ordinary Action* (1993), critically reviewed research in ethnomethodology and social studies of science, and recommended non-foundational empirical investigations of the topics of epistemology: practices of measurement, observation and representation in specific settings of ordinary and scientific inquiry. The book won the 1995 Robert K. Merton Professional Award from the American Sociological Association. His current research examines the interplay between law and science in criminal cases involving DNA evidence. He is currently editor of *Social Studies of Science*, and was recently elected president of the Society for Social Studies of Science (4S).

Peter T. Manicas is currently Director of Interdisciplinary Studies at the University of Hawai'i at Mānoa. He has published widely in the philosophy of social science, and social and political philosophy. In addition to many articles published in range of academic disciplines, his books include: *A History and Philosophy of the Social Sciences* (1987), *War and Democracy* (1989), *Social Process in Hawai'i: A Reader* (2004), *Globalization and Higher Education* (with Jaishree K. Odin, 2004) and, most recently, *A Realist Philosophy of Social Science* (2006).

Jon P. Mitchell is Reader in Anthropology at the University of Sussex. He has written on the anthropology of politics and identity, religion and ritual, performance, memory and modernity, primarily in the Mediterranean context of Malta. His books include *Ambivalent Europeans:*

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Nancy A. Naples is Professor of Sociology and Women's Studies at the University of Connecticut where she teaches courses on qualitative methodology; contemporary social theory; feminist theory; feminist methodology; sexual citizenship; gender, politics and the state; and women's activism and globalization, She is author of *Feminism and Method: Ethnography, Discourse Analysis, and Feminist Research* (Routledge, 2003) and *Grassroots Warriors: Activist Mothering, Community Work, and the War on Poverty* (Routledge, 1998). She is also editor of *Community Activism and Feminist Politics: Organizing Across Race, Class, and Gender* (Routledge, 1998) and co-editor of *Women's Activism and Globalization: Linking Local Struggles with Transnational Politics* (with Manisha Desai) and *Teaching Feminist Activism* (with Karen Bojar), both published by Routledge in 2002. Her next book, *Restructuring the Heartland: Racialization and the Social Regulation of Citizenship*, reports on a long-term ethnographic study of economic and social restructuring in two small towns in Iowa. She is currently working on a comparative intersectional analysis of sexual citizenship and immigration policies.

Maureen A. O'Malley is currently a Research Fellow at Egenis, University of Exeter, where she examines philosophical and sociological issues in microbiology and systems biology. This work builds on her years in Ford Doolittle's lab at Dalhousie University in Halifax, Nova Scotia, where she studied evolutionary microbiology and was part of the university's interdisciplinary Evolutionary Study Group (http://evolutionstudygroup.biology.dal.ca/contents.html). The chapter she wrote for this anthology was greatly informed by involvement in that group's discussions and also draws on her earlier Ph.D. work, which compared a variety of evolutionary approaches in the social sciences and humanities with evolutionary biology.

William Outhwaite studied at the Universities of Oxford and Sussex, where he taught for over thirty years, and is now Professor of Sociology at Newcastle University. He is the author of *Understanding Social life: The Method Called Verstehen* (Allen & Unwin, 1975, second edition Jean Stroud, 1986); *Concept Formation in Social Science* (Routledge, 1983); *New Philosophies of Social Science: Realism, Hermeneutics and Critical Theory* (Macmillan, 1987); *Habermas. A Critical Introduction* (Polity Press, 1994), *The Future of Society* (Blackwell, 2006), and (with Larry Ray) *Social Theory and Postcommunism* (Blackwell, 2005). He edited *The Habermas Reader* (Polity Press, 1996); (with Tom Bottomore) *The Blackwell Dictionary of Twentieth-Century Social Thought* (Blackwell, 1993); *The Blackwell Dictionary of Modern Social Thought* (Blackwell, 1993); (with Luke Martell) *The Sociology of Politics* (Edward Elgar, 1998), and (with Margaret Archer) *Defending Objectivity* (Routledge, 2004). He is currently working on a book on Europe in society.

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Hannah Pazderka-Robinson received her Ph.D. in Neuroscience from the University of Alberta. In 2004, she was awarded a one-month fellowship to study the human frontal lobes from the International Neuropsychological Society. She is interested in a number of different methodologies, including structural equation modeling, neuropsychological assessment, and electrophysiology. Her research interests are primarily in the area of mental health, particularly addictions and impulse control disorders. She is currently employed with the Alberta Mental Health Board as their Science and Academic Lead, and is affiliated with the University of Alberta as a sessional lecturer in psychology and a clinical lecturer in psychiatry. Her work has appeared in a number of journals including *Structural Equation Modeling*, the *International Journal of Psychophysiology*, *Psychopharmacology*, *BMC Medical Research Methodology*, and the *Canadian Journal of Public Health*.

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Stephen P. Turner is Graduate Research Professor in Philosophy at the University of South Florida, where he is also appointed in the Department of Management. His writings on methodology have ranged from issues in explanation, in such books as *Sociological Explanation as Translation* (1980) to issues of theory construction and issues with statistical approaches to causality, including *Causality in Crisis? Statistical Methods and Causal Knowledge in the Social Sciences* (1997, co-edited with Vaughn McKim). He has dealt with methodological issues in such fields as organization studies and international relations. He has also written extensively on the history of methodology, especially of statistics and probabilistic thinking, including writings on Comte, Mill, Quetelet and Durkheim, and on the origins of quantitative sociology in the United States. He was co-editor of the *Blackwell Guide to the Philosophy of Social Science* (2003, with Paul Roth) and *Philosophy of Anthropology and Sociology* (2007, with Mark Risjord) in the *Handbook of Philosophy of Science* series.

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General Introduction

William Outhwaite and Stephen P. Turner

This handbook is designed to meet the needs of disciplinary and non-disciplinary, problem-oriented social inquirers for a comprehensive overview of the critical issues in the methodology of the social sciences and its various and often extremely complex and controversial literatures. In the social sciences the term 'methodology' tends to indicate two increasingly differentiated areas of work—first, methodological issues arising from and related to theoretical perspectives, as in Marxist, functionalist or feminist methodology; and, second, issues of specific research techniques, concepts and methods. A glance at the contents of this book will show that we aim to cover both these fields. Our understanding of the needs of the reader, and thus of the content of the volume, however, requires some explanation.

The world cannot be said to suffer a shortage of works on either of the two kinds of methodology mentioned above. Books explaining techniques and even handbooks on various methods or kinds of methods are common. Nevertheless, there is a daunting problem for the student or practitioner, as well as for the senior scholar. The problems and disputes over methods are usually not readily accessible. A person trained in a psychology department program in behavioral science methods will, for example, be told that there are 'assumptions' in the kinds of experimental designs that are taught in these programs. But the same person may never be aware of the large and important technical literature on 'selection bias', a specific problem with the assumptions that routinely undermines the applications of these methods—for example, to such standard problems as evaluating the effectiveness of a social service program. Similarly, the readers of published research reports on such topics as the effectiveness of particular social interventions, even if they are reasonably sophisticated, will find it difficult to know what questions an appropriately skeptical reader should ask about the research design.

This volume is an attempt to make these kinds of issues accessible. One way of doing this is by providing technical chapters on a

range of interrelated problems that plague causal inference. The approach is not to provide 'solutions', though solutions to many of the problems are discussed. The approach is to explain the kinds of problems that routinely arise in these settings, and the tradeoffs that researchers are routinely compelled to make in order to come up with the results that are presented as fact. At this level of methodological detail, matters are seldom as simple as textbooks make them appear. Things that we think we know—for example, that minorities are greatly under-reported by the US Census, turn out to depend on reasoning that is more problematic than the original enumeration. Knowing why is crucial to reading in a sophisticated way.

A second daunting problem is the sheer variety of methodological approaches, especially qualitative approaches. These present some different problems of explication. What is 'cultural studies'? What are the distinctive background ideas and theories that motivate it? Why do its practitioners not just do surveys? What is 'grounded theory'? Answering these questions often requires a bit of historical background, and typically requires an introduction to the motivating theoretical ideas.

In each case, however, we have tried to ensure that contributors keep an eye on broader perspectives as well as on the specific topic with which they are dealing. Thus, as Adele Clarke (Chapter 23) notes in her chapter in this book, the relatively delimited approach of 'grounded theory' raises central questions about the overall orientation of the social sciences. In Denzin and Ryan's discussion (see Chapter 32) of the focused interview, they explain the way in which this familiar method has become a means of recognizing and accounting for the 'postmodernist' recognition of 'different voices'. In discussing the idea of feminist methodology, we have been concerned both to have the theoretical background of such ideas as standpoint theory explained (see Chapter 29), and also, in a second chapter, to discuss the kinds of problems that arise in actual attempts at collaborative action research in the face of different voices (see Chapter 30).

One of the authors we recruited for the volume, after having its purpose explained, replied: 'I see what you are doing: you are surveying the new geography of knowledge.' This volume *is* an attempt to cover a much wider range of approaches and problems than methodology books have traditionally included. One innovative feature of the volume is the extensive discussion of the new situation in which the knowledge of the subjects of the research is incorporated into the research and in which scholars are engaged researchers collaborating with their subjects. We have tried to cover the main problems on which a developed literature exists. But the sheer variety of topics that the omnivorous reader is likely to encounter extends beyond this volume, and will continue to expand. Methodological controversy has gone far beyond the simple conflicts over 'positivism' of the sixties. This volume is an introduction to that transformation.

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 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, anti-foundationalism 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 scientistic 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 scientistic 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-scientistic ways which recall in some respects the social theory of the late nineteenth and early twentieth centuries. 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, Disciplinarities, and Interdisciplinarities* (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.

1

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) (Frankfort-Nachmias and Nachmias, 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 (Frankfort-Nachmias and Nachmias, 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', 1950) 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 Revolutions* (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' (Joas, 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' (SI), 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 (1966) *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 scientistic–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 milieux. 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, regroups, 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'. Lingering 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 poststructuralism. 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 pregiven 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 (1999: 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 sociocultural 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 neoclassical theory. The concepts of economics were not, in Mises's 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 is 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 phenomenonology, 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 metatheories 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 scientistic and non-scientistic forms, was a challenge to mainstream scientism. The scientistic 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 and Vernoff, 1982: 53). A historically oriented political economy became important in the 1960s in direct response to the anti-war, civil rights and feminist 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 'postmodernist' 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 'scientistic' 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 Heideggarian 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, 1985: 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 fitted into the revolutionary class analysis of standard Marxism. Foucault and others, including Derrida, Deleuze, Guattari, Castoriadis, Lefort, Lyotard and Baudrillard 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 Canguilhem 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 (Rainbow, 1985). 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, phenomenonology, structuralism and poststructuralism, 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 'metatheory' 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 antiscientism 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 ethnomethodologists 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 metatheory, 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, 1981; 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 (1976) 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, 1975/78).[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 metatheories 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 (1978) 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 'interpretativist 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 metatheory 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, theoretized 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 knowledgeability 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 poststructuralism 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 *scientistic* 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, 1964: 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 (1975). See also Bunge (1979) and Bohm (1984). 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 (1987); 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 'Situated 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 Mulkay (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, Rudolfo 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 extra-linguistic materials. See his *Anthropologie Structurale* (1958) and *La Pensée Sauvage* (1962). For a very useful discussion of Lévi-Strauss, see James 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 to 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, et al., 2002: 204).

21 This is evidently different from both the 'interpretativist 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 (1967) 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's 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' (1978) 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 1976 (Introduction, and pp. 144–154). Since indeed, the Edinburgh group is doing sociology of knowledge and since it is a 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.

2

Interdisciplinary Approaches in Social Science Research[1]

Julie Thompson Klein

Claims for the origin of interdisciplinarity span the centuries. Its formal emergence, though, is linked with the institutionalization of disciplines as a system of *Wissenschaft* marked by both differentiation and cooperation (Vosskamp, 1986: 20–1). Interdisciplinarity assumes the existence as well as the relative resilience of disciplines as models of thought and institutional practices (Moran, 2002: 17). A clear set of categories for denoting domains of social inquiry crystallized during the period extending from 1850 to 1914 (Wallerstein, 1995: 840). Even at that early point, the interplay between movements for specialization and for integration was apparent. August Comte, for one, envisioned a unified social science from the outset (Miller, 1982: 1). Over the course of the 20th century, competing intellectual syntheses emerged, and the number and variety of interdisciplinary activities increased. As a result, interdisciplinarity is a now familiar part of the intellectual landscape in Europe and North America. Familiar as it is, however, individuals and teams are often uncertain about its definition and nature. This chapter answers their most common questions. It sorts out the plurality and historical patterns of activities in social sciences, core terminology and differing practices, and key methodological issues.

PLURALITY AND HISTORICAL PATTERNS

The history of social sciences in Europe and North America differs. Influences and rates of development vary, as do national interests. However, Neil Smelser emphasizes that the ultimate outcome has been similar—the forming of separate academic departments or faculties in universities. The 'mainstream' disciplines are anthropology, economics, political science, psychology and sociology. Yet, Smelser cautions, describing social sciences solely with reference to the 'big five' disciplines distorts reality in two ways. First, under those headings, various subareas of investigation rely on variables and explanations outside the commonly understood scope of social sciences. Geopolitics, socio-biology, behavioral genetics and behavioral neuroscience all appeal to non-social and non-psychological explanatory variables and explanations. Second, another range of disciplines could be labeled behavioral

and social–scientific, although not entirely so. Demography might be considered a separate social science, or part of sociology, economics and anthropology. Archaeology might be classed as part of anthropology or as an independent social science. Geography, history, psychiatry, law and linguistics present similar complications for taxonomy. So do relations with the 'intersecting fields' of genetics, behavior, and society; behavioral and cognitive neurosciences; psychiatry; health; gender studies; religious studies; expressive forms; environmental/ecological sciences and technology studies; area and international studies; and urban studies and planning public policy. Assignment to one category of inquiry or another would vary according to the criteria used (Smelser 2004: 44, 48, 60–1).

Intersecting fields, in particular, have attracted a great deal of attention in discussions of interdisciplinarity because they are linked closely with innovation and novel approaches. Observing the increased number of hybrid formations, Dogan and Pahre (1990) proposed a theory of hybridization. The first stage of this process is specialization, and the second stage is continuous reintegration of fragments of specialities across disciplines. There are two types of hybrids. The first kind becomes institutionalized as a sub-field of a discipline or as a permanent cross-disciplinary committee or program. The second kind remains informal. Hybrids often form in the gaps between sub-fields. Child development, for example, incorporates developmental psychology, developmental physiology, language acquisition, and socialization. Hybrids may also beget other hybrids. Genetic epistemology is a hybridization of epistemology and general psychology that has fostered new affiliations. Psychologists interested in child development, for instance, are less likely to study clinical psychology than to use developmental psychology or the linguistic literature on language acquisition. Likewise, a sociologist interested in urbanization will have more in common with a geographer doing research on the distribution of cities than a sociologist studying social stratification.

Dogan and Pahre (1990) reject the term 'interdisciplinary research' as a catch-all notion that lacks the specialist focus of intersecting sub-fields. They also tend to universalize the notions of core and frontier, when in fact the balance of conventional and innovative practices varies by discipline. Nonetheless, they document the growing presence of hybrid forms. They also acknowledge the varied trajectories and conflicting interests in the same domain. Social psychology, for example, is often touted as a major interdiscipline, but it exhibits divisions along sociological and psychological interests. In contrast to hybrid formations, other activities may be less visible on knowledge taxonomies. One of the most common is the borrowing of tools, methods, concepts, and theories, including such notable examples as the importation of rational-choice models from economics into political science and sociology. Depending on one's point of view, Smelser (2004: 53–4) advises, the business of importing and exporting may be viewed as 'borrowing' or as 'imperialism'. Borrowing is generally regarded as a lower-level utilitarian type of interdisciplinarity. It is an important indicator of change, though. Patterns of borrowing signal new networks of affiliation and some borrowings become so incorporated into daily practices that they are no longer regarded as 'foreign'.

Synoptic work is another activity that has fostered claims of 'inherently interdisciplinary' identity. Geography's broad scope is evident in a multitude of conceptual and analytical approaches, from earth sciences to humanities. Synoptic scope, though, is more a matter of multidisciplinary expanse than deliberate interdisciplinary integration. Synthetic work of a different kind occurs in efforts to combine basic research findings from a large number of sub-fields, to integrate results from cognate disciplines, and to merge existing and new knowledge about a particular place or a region into a cohesive portrayal of an area. At the same time, another kind of interdisciplinary activity occurs in applied research on societal problems (Association of American Geographers, 1995: 39).

Even economics, which patrols its boundaries more closely than other social sciences, has multiple affiliations. In a study of disciplinarity based on a literature review and interviews, Tony Becher (1989: 36) found economics portrayed as having 'one common frontier with mathematics and another with political science; some trade relations with history and sociology; and a lesser measure of shared ground with psychology, philosophy and law'.

Sociology too exhibits a plurality of activities. Craig Calhoun (1992) reports that it is in principle a synthetic discipline that aspires to be the most synthetically encompassing of all social sciences. Yet, beyond holistic and generalist claims, it is also an interstitial discipline that fills in gaps among other social sciences and works along their borders. Sociology, Hunter and Brewer (2003: 577) add, has drawn more eclectically from the methodologies of other disciplines, borrowing fieldwork from anthropology, experiments from psychology, voting and public opinion polls from political science, and archival research from history. Calhoun's study of citation patterns in major sociological journals between the late 1940s and late 1980s demonstrates why the principle of clear and principled disciplinary divisions of labor does not hold. Citations to economics and interdisciplinary fields of organizational, administrative, management, and labor studies became more prominent over the years. So did references to interdisciplinary social and behavioral science publications and the field of political economy, especially with the rise of Marxist journals and development studies. Population became more prominent too, and also political science in the later 1960s. Citations to 'non-disciplinary' statistics and measurement journals grew gradually, and public opinion and public affairs journals were frequently cited in the middle of the period (Calhoun 1992: 137–8, 140–5, 148, 170).

Anthropology too has multiple affiliations. Its relationship with sociology is particularly long-standing. The early Chicago School of social science stressed ethnographical methods, and anthropology was the discipline implicated most closely in the efforts of Talcott Parsons and others to develop a general theory of society. Anthropology also shared a broadly functionalist orientation with sociology for many years and a broadly evolutionary orientation before that (Calhoun, 1992: 148). Interdisciplinary activities pluralized as the discipline expanded beyond the 'sacred bundle' of four fields that Franz Boas defined, spanning biological history, linguistics, ethnology, and prehistoric archaeology. Since 1983, George Stocking Jr recounts, many 'adjectival anthropologies' have emerged, and the number of subsidiary societies, associations and councils have increased. Anthropology's boundaries have always been problematic, but even more so in the period of 'crisis' and 'reinvention'. Anthropologists were more open to poststructuralist and postmodernist thought than other social scientists, destabilizing and relativizing a broad range of intellectual categories at the same time that a general blurring of genres and disciplinary boundaries was underway (Stocking, 1995: 933–5, 954–5; Calhoun, 1992: 153).

Historical Patterns

As the introductory overview reveals, there are historical patterns to interdisciplinary activities. Roberta Frank speculated that the very idea might have been born in the 1920s at the corner of 42nd and Madison Avenue in New York City, where the Social Science Research Council (SSRC) was located. The term was shorthand for research that crossed more than one of the Council's seven societies. The SSRC was the first council of its kind in the world and became a model for councils in other countries. Even at this early point in the formation of modern disciplines, the SSRC aimed to accelerate the tendency toward breaking down boundaries by cross-fertilizing ideas and joining methods and techniques. It brought together representatives of anthropology, sociology, political science, economics, psychology, statistics and history, with the aim of producing purposive

and empirical social problem-oriented applied research, including targeted programs in such problem fields as social security and public administration (Frank, 1988: 91; Fisher, 1993: 4, 6, 9, 205–6, 220–3, 229).

In characterizing the early history of interdisciplinary approaches in social sciences, Landau et al. (1962: 8, 12–17) distinguish two phases. The first phase, dating from the close of World War I to the 1930s, was embodied in the founding of the SSRC, the University of Chicago School of Social Science, and publications such as Ogburn and Goldenweiser's *The Social Sciences* (1927). The interactionist framework at Chicago fostered integration, and members of the Chicago school were active in the efforts of Otto Neurath and others to construct a unified philosophy of natural and social sciences. The impact of these efforts was widely felt, and the scope and data of disciplines altered as a result. On occasion disciplinary 'spillage' even led to the embryonic formation of hybrid disciplines such as social psychology, political sociology, physiological psychology, and social anthropology. Yet, traditional categories of knowledge, structures of fields, and the organization of academic work remained intact. There was also a distinct pattern of interactions. Social scientists tended to emulate natural sciences, heightening concern for objectivity, precision and quantification. In the interests of scientific analysis, techniques and instruments were borrowed to support testing and measurement. Hence, the first phase was empirical in nature and instrumental in character.

The second phase, dating from the close of World War II, was stimulated by developments in logic and in the philosophy and sociology of science. It was embodied in 'integrated' social science courses, a growing tendency for interdisciplinary programs to become 'integrated' departments, and the concept of behavioral science. The traditional categories that anchored the disciplines for over a half-century were questioned and lines between fields began to blur, paving the way toward a new theoretical coherence and alternative divisions of labor and distribution of resources. The emergence of area studies in the late 1930s was a particularly prominent development. The concept of area differed from earlier and more limited forms of 'interdisciplinary' borrowing. It was a new 'integrative' conceptual category with greater analytic power, stimulating a degree of theoretical convergence that was also potential in the concepts of role, reference groups, mobility, status, self, decision-making, action, information, communication, and applications of game theory.

Landau et al. (1962) liken the early 'interdisciplinary' approach to the older Baconian belief that broad basic generalizations will almost automatically drop out of the mass accumulation of discrete facts. In contrast, the behavioral science movement did not aim to borrow, reify and tack methods and concepts onto traditional categories. It sought an alternative method of organizing social inquiry based on theories of behavior that fixed the field of focus in a different way. When a political scientist, for example, adopts decision-making explicitly as a frame of reference, the nature of the field of focus changes and the work is not just 'politics'. The alternative construct attempts to order behavioral events in a theoretical context that is also sociological or psychological, or both. Differentiation of fields of focus also becomes a matter of theoretical relevance and conceptual clarity, not simply a function of 'convention and treaty'.

The culture–personality movement was another example that focused on links between macro and micro levels. In addition, a spirit of reform in the post-World War II period encouraged integrative thinking in government and private agencies about societal problems such as war, labor relations, population shifts, housing shortages, crime and welfare (Landau et al., 1962: 12). Applied social science was further stimulated by technological advances during the war. New engineering and technological methods evolved from operational research, feedback systems and computer manipulation. New conceptual tools of communication

theory, game theory and decision theory also promoted common ground, fostering a new 'cross-disciplinary intelligence' with conceptual power (Mahan, 1970: 104). The import and export of tools was evident in the rise of behavioral political science during the 1950s and 1960s. Researchers relied heavily on methodological tools (such as survey research), theoretical formulations (such as modernization) and general theoretical orientations (such as structural–functionalism) that were all established to some degree in sociology at the time (Smelser, 2004: 53).

In the latter half of the 20th century, new developments expanded the scope of interdisciplinary research. One set of developments looked to the sciences. From the mid-1800s onwards, the sciences have been influential in the formulation of social research methods associated with positivism, empiricism, and quantitative or numerical techniques. These approaches reflect a set of assumptions about the social world: that it can be observed and measured directly, that meaning is fixed and universal, and that the study of human behavior can produce general statements or laws. Survey research, for instance, aims to control the elements that are being examined through construction of a closed system of sampling, and experimental research is controlled by the design of the experiment. The focus has tended to be on individual units of a system, and research is driven by processes of hypothesis formation and concept operationalism (Yates, 2004: 5, 12–14).

With the growing sophistication of scientific tools and approaches, new biological explanations of human behavior became possible and new hybrids developed with affiliations to cognitive science and neurosciences. New quantitative methods and advanced computing power facilitate the sharing of large quantities of data across disciplinary boundaries. Technologies of brain imaging and magnetic resonance imaging also facilitate mapping brain functions with increasing precision, and a new 'postdisciplinary' community of interests is emerging in projects such as Thomas Spence Smith's investigation of the neurosociological foundations of human interactions during the earliest years of life (2004: 200–1).

The other set of developments looked toward humanities, informed by postpositivist, poststructural, constructivist, interpretive, and critical paradigms (Tashakkori and Teddlie, 2003b: 23). Some new approaches such as feminism, neo-Marxism, and the expanding field of cultural studies were self-described 'interdisciplinary' interventions into traditional practices. In 1980, anthropologist Clifford Geertz also identified a broader shift that was occurring within intellectual life in general and social sciences in particular. The model of physical sciences and a laws-and-instances ideal of explanation was being supplanted by a case-and-interpretation model and symbolic form analogies. Social scientists were increasingly representing society as a game, a drama or a text, rather than as a machine or a quasi-organism. They were borrowing methods of speech-act analysis, discourse models and cognitive aesthetics, crossing the traditional boundary of explanation and interpretation. Former keywords of 'cause', 'variable', 'force' or 'function' were being replaced by a new vocabulary of 'rules', 'representation', 'attitude', and 'intention'. On the other side of the fence—as social scientists were talking of 'actors', 'scenes', 'plots', 'performance' and 'personae'—humanists were talking of 'motives', 'authority', 'persuasion', 'exchange' and 'hierarchy'. Geertz rejected 'interdisciplinary brotherhood' or 'highbrow eclecticism'. Yet, he acknowledged, the principles of mapping knowledge were changing—conventional rubrics remained, but they were often jerry-built to accommodate a situation that was increasingly 'fluid, plural, uncentered, and ineradicably untidy'.

Increasing frustration with methodological purism and naïve empiricism, coupled with critical debates on methodology, also encouraged a 'third methodological movement'. The mixed-methods movement is young, and not all combinations or techniques of 'triangulation' are interdisciplinary. Yet, mixed methods are generating more complex borrowings

across disciplinary lines (Rallis and Rossman, 2003: 491; Tashakkori and Teddlie, 2003c: ix–xii; Tashakkori and Teddlie, 2003b: 24). Mixed methods draw from both quantitative and qualitative traditions, combining them in unique ways to solve practical research problems and to answer research questions. Quantitative and numerical methods are more strongly associated with areas of sociology, psychology and politics. Qualitative and textual methods are more strongly associated with sociology, social anthropology, social psychology and cultural studies. Yet, alignment of quantitative work with the nomothetic goal of constructing general laws and alignment of qualitative work with the ideographic goal of detailed description of particular circumstances is not an absolute split. Researchers from all these disciplines, Simeon Yates (2004: 133, 135) reports, use one or more of these methods.

TERMINOLOGY AND TYPOLOGY

In early books that presented the entire field of social science as a unit, the word 'interdisciplinary' was not prominent. The term 'cooperative', Frank reports, was more customary. Books published between 1925 and 1930 also stressed 'interrelation', 'mutual interdependence', 'interpenetration', 'interactions' of disciplines, and the need to explore 'twilight zones' and 'border areas' in order to fill 'unoccupied spaces' and to encourage 'active cultivations of borderlands between the several disciplines'. The word 'interdisciplinary' also did not appear in the 15–volume *Encyclopaedia of the Social Sciences* (1930–1935). By the early 1930s, though, 'inter-discipline' and 'interdisciplinary' were appearing more widely. The first citation for the term in *Webster's Ninth New Collegiate Dictionary* and *A Supplement to the Oxford English Dictionary* came from a December 1937 issue of the *Journal of Educational Sociology*, in a subsequent notice about SSRC postdoctoral fellowships. By mid-century, Frank (1988: 92–6) surmised, it was common coin in social sciences and grew during the late 1960s and 1970s into a 'kind of weather'.

The new 'weather' was stirred by worldwide demands for reform of universities. Heightened interest led to the first international conference on problems of interdisciplinary research and teaching in member countries of the Organization for Economic Cooperation and Development (OECD). Held in 1970 in France, the meeting produced the most widely influential set of terminology (OECD, 1972). The OECD typology presented four descriptors for teaching and research beyond disciplinary approaches. Over the next three decades, 'multidisciplinary' and 'interdisciplinary' became widely known; 'pluridisciplinary', while used, is cited less widely. 'Transdisciplinarity' had a restricted application at first, though by the start of the 21st century it had attained a new currency. The three words that gained the widest usage provide a framework for thinking about different types of practice. The differences represent points on a continuum of integration rather than absolute states. A program, a project or a field may move across points of the continuum over time and in sub-units. The distinction between the first two terms—*multidisciplinarity* and *interdisciplinarity*—is a matter of fairly wide consensus, based on the following characteristics.

Multidisciplinary approaches juxtapose separate disciplinary perspectives, adding breadth of knowledge, information and methods. Individuals and groups work independently or sequentially in an encyclopedic alignment or ad hoc mix. They retain their separate perspectives, and disciplinary elements remain intact. The OECD definition of 'interdisciplinary' was quite broad, ranging from simple communication of ideas to mutual integration of organizing concepts, methodology, procedures, epistemology, terminology, data, and organization of research and education (OECD, 1972: 25). Interdisciplinarity, though, is conventionally defined as a more conscious and explicitly focused integration that creates a holistic view or common understanding of a complex issue,

question or problem. Seminar participants also introduced finer distinctions and, as the idea of interdisciplinarity proliferated, other terms emerged that signify differing practices and claims of what constitutes 'genuine' or 'true' interdisciplinarity. Some distinctions refer to scope. 'Narrow interdisciplinarity' occurs between disciplines with compatible methods, paradigms and epistemologies, such as history and literature. It has a different dynamic than 'broad' or 'wide' interdisciplinarity between disciplines with little or no compatibility, such as sciences and humanities (Van Dusseldorp and Wigboldus, 1994; Kelly, 1996). The Nuffield Foundation also proposed a macro distinction premised on two basic metaphors: bridge-building and restructuring. Bridge-building occurs between complete and firm disciplines. Restructuring detaches parts of several disciplines to form a new coherent whole, often with an implicit criticism of the state of those disciplines (The Nuffield, 1975: 43–4).

Ten years after the pioneer OECD seminar, Raymond Miller presented a more detailed typology for social sciences based on seven categories of 'cross-disciplinary' efforts: topical focus, professional preparation, life experience perspective, shared components, cross-cutting organizing principles, hybrids, and grand synthesis. *Topics* are associated with problem areas. 'Crime', for instance, is a social concern that appears in multiple social science disciplines and in criminal justice and criminology programs. Likewise, the topics of 'area', 'labor', 'urban' and 'environment' led to new academic programs. So did 'gerontology'. *Professional Preparation* led to new fields with a vocational focus, such as social work and nursing and, Smelser (2004: 61) adds, fields of application to problem areas such as organization and management studies, media studies and commercial applications, and planning public policy. The category of *Life Experience* became prominent in the late 1960s and 1970s with the development of ethnic studies and women's studies. And, the category at the heart of Dogan and Pahre's theory, *Hybrids* formed 'interstitial crossdisciplines' such as social psychology, economic anthropology, political sociology, biogeography, culture and personality, and economic history (Miller, 1982: 11–15, 19).

Miller's category of *Shared Components* is particularly relevant to a volume on methodology. It has a 'longer and quieter history' than the other classifications. *Components* refers both to shared methods, such as techniques of statistical inference, and to conceptual vehicles, such as the mathematics of probability, or game theory, and information. Smelser adds the examples of computer sciences; methodological issues associated with design, execution, and assessment of empirical research; and logic of inquiry and research design including non-statistical examples such as comparative analysis, experimental methods and ethnography (2004: 60). Some methodologies, Tony Becher observed, even form the basis of recognized specialties, such as statistics, oral history, and econometrics (1989: 49). Miller's fifth category of *Cross-Cutting Organizing Principles* is similar. A focal concept or a fundamental social process such as 'role' or 'exchange' may cut across disciplines. Comparably, Ursula Hübenthal labeled the adoption of a model from another science *Concept Interdisciplinarity*, citing the examples of system theory, cybernetics, information theory, synergetics, game theory, semiotics and structuralism (1994: 66).

Wilhelm Vosskamp (1986: 25) grouped method-interdisciplinarity and concept-interdisciplinarity together as aspects of methodology. So did Jack Mahan when he linked common-ground methods, concepts, languages, logics, techniques, and strategies that facilitate communication across disciplines. In addition to statistical design, experimental design, mathematical models, and computer models, Mahan (1970: 114–15) cited procedures of data acquisition, surveying, interviewing, sampling, polling, case studies, and cross-cultural analysis.

The Academy of Finland Integrative Research (AFIR) team shed light on the core

action in *Methodological Interdisciplinarity*. In a study of research proposals submitted to the country's key national funding agency for basic research, the team found the typical activity was combining methods from different disciplines or fields in order to test a hypothesis, to answer a research question or to develop a theory. The typical motivation is to improve the quality of results, not to generate a new theoretical construct (Bruun et al., 2005: 51). Hence, Methodological Interdisciplinarity is also regarded as a lower form of interaction. Heinz Heckhausen (1972: 86) labeled the borrowing of analytical tools such as mathematical models and computer simulation 'pseudo' and 'auxiliary' interdisciplinarity. The distinction depends on whether need for the method is transitory or enduring.

Another difference of type emerged as well. Empirical and methodological forms are strongly apparent in 'strategic', 'pragmatic', 'opportunistic', and 'instrumental' research that focuses on technologies of information and application for economic and technological problem-solving. In the 1980s and 1990s, interdisciplinarity gained heightened international visibility in science-based areas of economic competition such as computers, manufacturing, biotechnology and biomedicine. Strategic forms integrate disciplinary, professional and/or interdisciplinary approaches without regard for questions of epistemology or institutional structure. In contrast, 'critical' and 'reflexive' approaches interrogate the existing structure of knowledge and education with the aim of transforming them, raising questions of value and purpose that are silent in strategic forms.

New fields in Miller's Life Experience category were often imbued with a critical imperative, and many poststructuralist and postmodern practices constructed interdisciplinarity as an inherently political project. In a collection of research stories from the Canadian academy, Salter and Hearn (1996) called interdisciplinarity the necessary 'churn in the system', aligning the concept with a dynamic striving for change. Interdisciplinary work is most successful, Steve Fuller contends, when borders are constantly engaged and when boundaries are being moved around as the result of constructive border engagements, not when there are rigid boundaries or no boundaries at all. Disciplines begin to see each other involved in a common enterprise and their boundaries are renegotiated (Fuller, 1993: 185). The distinction between *Instrumental* and *Critical* forms is not absolute. Research on problems of the environment and health often combine critique and problem-solving. Yet, critique is a major fault line in the debate over what constitutes 'genuine' interdisciplinarity.

Theoretical Interdisciplinarity and Transdisciplinarity

Theoretical Interdisciplinarity lies at a further point along the continuum of integration. The AFIR team found that the primary focus is developing, applying or combining conceptual tools with the aim of building a comprehensive general view, a theoretical synthesis or an integrative framework. To illustrate the difference, researchers in one project sought to develop a theoretical model of mechanisms that mediate mental stress experiences into physiological reactions and eventually the somatic illness of coronary heart disease. Previous studies emphasized correlation of single stress factors or separate personal features with the disease. In contrast, the project aimed to develop an interdisciplinary theory based on integration of psychological and medical elements and testing the conceptual tool of inherited 'temperament' (Bruun et al., 2005: 52). The outcomes of theoretical interdisciplinarity may also include conceptual frameworks for analysis of particular problems, integration of propositions across disciplines, and frameworks based on continuities between models, analogies or metatheory.

Macro social theory is a form of Theoretical Interdisciplinarity that has long been pursued in social science, including the work of Emile Durkheim, Georg Simmel, Max Weber, Robert Park and Talcott Parsons. Camic's and Joas's review of recent efforts

documents the continuing quest in North America and Europe. British theorist Anthony Giddens has sought a new synthesis geared toward the contemporary world that is based on conceptual innovation and combines the strengths of multiple perspectives and approaches that would yield a new structuration theory. In the US, Randall Collins called for a comprehensive theory of every area of society that would arise from comparing, synthesizing and cumulating findings of specialized areas. He aims to link micro-level social interactional processes to macro-level social structures. Jeffrey Alexander has worked toward a convergence of all major classical and contemporary sociological theories, promoting multi-dimensional synthesis of normative and instrumental conceptions of action, material and ideal conceptions of order: a micro–macro synthesis that integrates actions and structure as well as subjectivity and objectivity, a new synthetic social theory along the model of culture-as-language, and an emerging neo-functionalism that might re-link theorizing about action and order, conflict and stability, and structure and culture. In France, Alain Touraine has urged reunification based on a general representation of society, a general vision of change, and an analysis of how actors are shaped and how humans can create a new society. In Germany, Jürgen Habermas has worked to preserve ties between system and lifeworld in an encompassing theory of communicative action. Niklas Luhmann has also drawn on biology and cybernetics to create a synthetic framework for analysis of autopoetic, or self-referential, social systems that might inform a comprehensive theory of everything that is 'social' (Camic and Joas, 2004: 1, 3–4).

Synthetic theoretical activity, Smelser (2004: 54) and the Nuffield Foundation observed, overlaps with the OECD notion of transdisciplinarity. The Nuffield Foundation noted a third possibility beyond bridge-building and reconstruction that occurs when a new overarching concept of theory subsumes theories and concepts of several existing disciplines, comparable to Miller's notion of *Grand Synthesis* (The Nuffield, 1975: 47). In the OECD typology, transdisciplinarity was defined as a 'common system of axioms for a set of disciplines', such as anthropology construed as the science of humans and their accomplishments. Transdisciplinary approaches transcend the narrow scope of disciplinary worldviews through a comprehensive and overarching synthesis (OECD, 1972: 25–6). Characteristic of the time, the organizing languages of general systems theory, structuralism, and cybernetics were prominent among OECD seminar participants. Today, the word has a new heightened currency evident in three trendlines of meaning.

One trendline is the contemporary version of the ancient quest for systematic integration of knowledge, not in the name of a single totalizing theory but new paradigms that recognize complexity and difference. This effort is being advanced by several groups, including the International University Reforms Observatory (ORUS) network of European and Latin American academics (http://www.orus-int.org/) and the Centre International de Recherches et Études Transdisciplinaire (http://perso.club-internet.fr/nicol/ciret). The second trendline is an extension of the OECD connotation of new synthetic frameworks. General systems, structuralism, Marxism, policy sciences, feminism, ecology and sociobiology became leading examples. The notion of 'transdisciplinary science' has also emerged in broad areas such as cancer research. This usage labels 'transcendent interdisciplinary research' that creates theoretical frameworks for defining and analyzing social, economic, political, environmental, and institutional factors in human health and well-being (Rosenfield, 1992).

The third trendline is the heightened imperative of problem-solving. This mandate is not new in social sciences. The pressing weight of social problems, though, prompted the OECD to declare in 1982 that interdisciplinarity *exogenous* to the university now takes priority over *endogenous university interdisciplinarity.* The exogenous originates in the continuous momentum generated by 'real' problems of the community and the demand that universities perform their pragmatic social mission (OECD, 1972: 130).

The momentum continued to grow. Eight years later, Robert Costanza proposed making transdisciplinary problem-solving the primary function of academics, requiring the creation of colleges, departments or programs of integrated transdisciplinary studies and fields (1990: 100–1). Gibbons et al. (1994) took a step further, proposing that a new mode of knowledge production has emerged that is fostering synthetic reconfiguration and recontextualization of knowledge. The older Mode 1 was hierarchical and homogeneous, with emphasis on disciplinary boundary work and certification. The new Mode 2 is characterized by complexity, hybridity, non-linearity, reflexivity, heterogeneity and transdisciplinarity. New configurations of research work are being generated continuously; the number of places where research is performed has increased; and a new social distribution of knowledge is occurring as a wider range of organizations and stakeholders brings heterogeneous skills and expertise to problem-solving.

Gibbons et al. (1994) initially highlighted contexts of application and use, such as aircraft design, pharmaceutics, electronics, and other industrial and private-sector research. In 2001, Nowotny et al. extended the Mode 2 theory to argue that contextualization of problems requires moving from the strict realm of application to the *agora* of public debate. When lay perspective and alternative knowledges are recognized, a shift occurs from solely 'reliable' scientific knowledge to inclusion of 'socially robust knowledge'. The emergence of a new discourse of transdisciplinarity was evident in the late 1980s and early 1990s in contexts of environmental research. At the 2000 International Transdisciplinarity Conference in Switzerland, results were reported in all fields of human interaction with natural systems and technical innovations as well as the development context (Klein et al., 2001). Problem domains vary. Some collaborations involve consumers in the process of innovative technology and product development. Other projects focus on controversial social issues involving members of communities that are affected by planning and decision-making. The common link, though, is the externality of a complex problem and the participation of a wider range of stakeholders.

Labels can be deceptive, so it is always important to ask exactly what is being described. It is a mistake, Richard Lambert (1991) suggests, to think of area studies as predominantly an 'interdisciplinary' enterprise. He describes the field as a 'highly variegated, fragmented phenomenon, not a relatively homogeneous intellectual tradition' (Lambert 1991: 176). Much of what would be described as 'genuinely interdisciplinary' work occurred at the juncture of the four disciplines that provided the bulk of area specialists: history, literature and language, anthropology, and political science. In that hybrid intellectual space, a kind of historically informed political anthropology developed using material in local languages. History operated as a swing discipline. Blending of disciplinary perspectives occurred most often at professional meetings and in research by individual specialists. In the first instance, broadly defined themes have been the dominant pattern in scholarly papers, creating a collective 'multidisciplinary' perspective. The topic of any particular gathering 'drives the disciplinary mix.' In the second case, topics regarded as substantively important to understanding a particular country frequently 'do not respect disciplinary boundaries.' Area studies and other interdisciplinary fields are also 'transdisciplinary' due to the broad scope of 'nonenclaved endeavors' and breadth of disciplines. They are 'subdisciplinary' in the sense that research by individuals, especially in social sciences, has tended to concentrate on particular sub-domains (Lambert, 1991: 175, 189–92).

METHODOLOGICAL ISSUES IN INTERDISCIPLINARY PRACTICES

Hübenthal (1994: 57, 59, 61) contends that the task of interdisciplinary research is not to be solved with a global interdisciplinary theory. Instead, it must be pursued within

individual sciences in daily usage and in elements rather than wholes. Hübenthal's admonition shifts attention to the how-to of practices. There is no universal interdisciplinary methodology. Methodology is influenced by the purpose and goals of a particular project or program, the problems and questions that are addressed, the actors who are involved, their allegiances to particular research traditions and methodological preferences, the institutional setting, the balance of depth and breadth, and the type of interdisciplinarity that is being practiced. Even so, common issues arise. Two issues loom large: integrative process and collaboration.

Integrative Process

Integration is widely regarded as the crux of interdisciplinarity. In fields of critical interdisciplinarity, this premise is disputed. Suspicion of holism, synthesis, and integration runs high in fields and postmodern practices that critique meta-narratives which ignore differences, conflicts, or contradictions (Lattuca, 2001: 246). Nonetheless, all interdisciplinary activities integrate to some degree different disciplinary insights. The most frequently reported shortfall is the tendency to stay at the level of multidisciplinarity. Disciplinary defaulting is also common. Even as they appear across disciplines, Miller advises, cross-cutting principles are often embedded within a particular discipline's thought model. 'Role,' for example, is a prominent cross-cutting conceptual category. Yet, it is alternatively framed as the consumer role in the market model, an individual's role-playing in social structure seen through the lens of sociology's structural–functional model, a person's role in history, and a role model in one conceptual model used in sociology (Miller, 1982: 17–18).

Popular metaphors are misleading. Disciplinarity is usually signified as depth along a vertical axis, and interdisciplinarity as breadth along a horizontal axis. Breadth connotes multiple variables and perspectives. Depth connotes competence in pertinent disciplinary, professional, and interdisciplinary approaches. The depth/breadth dichotomy, however, fails to acknowledge the role of integrative actions that move across the vertical and horizontal planes. Synthesis is not reserved for a final step. The possibilities are tested throughout, moving in zigzags and fits and starts as new knowledge becomes available, new insights are generated, disciplinary relationships are redefined, and integrative constructs are built (Klein, 1996: 212, 222–3). In an ideal model of decision-making by individuals that is informed by complexity theory, William H. Newell (2007) argues that interdisciplinary study entails a two-step process.

Part A draws critically on disciplinary perspectives in an iterative process. Tentative syntheses are reformed as the insights of additional disciplines are incorporated. Decisions in Part A are predominantly disciplinary: focusing on what concepts, theories, and methods to use; what information to collect; what research strategies are feasible given the constraints; and how much breadth and depth of knowledge in each discipline are required given the problem at hand, the goals and the collaborators. Yet, Part A also involves distinctively interdisciplinary decisions: going back and forth between disciplinary part and complex whole, comparative evaluation of the various disciplines' strengths and weaknesses, and the narrowing and skewing that results from their respective redefinitions of the problem.

In Part B, the insights of different disciplines are integrated into a more comprehensive understanding that replaces either/or thinking with both/and thinking. The fundamental decisions address conflicts that exist among disciplinary insights. Latent commonalties need to be identified, either directly by modifying the concepts through which they are expressed or indirectly by modifying the assumptions on which they are based. Once common ground has been constructed, modified insights can be integrated into a more comprehensive understanding. The goal is not to remove the tension between insights, but to reduce their conflict. To illustrate, Newell (2007) examined how Kenneth Boulding, Robert Frank and Amitai

Etzioni created common ground in works that brought together economics and sociology. The technique of *redefinition* reveals commonalties in concepts or assumptions obscured by discipline-specific terminology. *Extension* addresses differences or oppositions by extending the meaning of an idea beyond a single domain. *Organization* identifies a latent commonalty in the meaning of different disciplinary concepts or assumptions, redefines them accordingly, and then organizes, arranges or arrays redefined insights or assumptions to bring out their relationship. *Transformation* is used where concepts or assumptions are not merely different (e.g., love, fear, selfishness) but opposite (e.g., rational, irrational).

Joel L. Fleishman (1991: 235–8) provides a description of the process from a problem-oriented field. Policy analysis is a framework for integrating knowledge about many problems that lend themselves to purposive individual or social action. It starts where economics and political science leave off, building on disciplinary descriptions and inferences to formulate alternative solutions and projecting likely consequences. In the process, policy analysis incorporates only a fraction of the contents of participating disciplines, choosing portions that appear relevant to solving a specific problem and adding useful elements from statistics, operations research, history and ethics. Policy analysts are not bound by the substantive knowledge and perspective of the problem areas to which they are applying their skills. They construct an integrative lens and analytic framework that fits around the problem.

Micro-level interviews of over sixty researchers in five exemplary organizations yielded further insights into the dynamics of integration in five exemplary organizations. The centers and researchers' projects varied in goal, scope and type. Some were geared toward producing explanatory theories and descriptive accounts. Others were geared toward practical solution of medical and social problems. On the basis of the interviews, Veronica Boix-Mansilla and Howard Gardner (2003) identified three core epistemic considerations in evaluating the content/substance of interdisciplinary work: consistency with disciplinary antecedents, balance and effectiveness. The second epistemic criterion of balance highlights the integrative leverage afforded by weaving together perspectives into a generative and coherent whole. Achieving 'reflective balance' does not imply equal representation of participating disciplines. Options must be weighed in a 'balancing act' that maintains generative tensions and reaches legitimate compromises in selecting and combining disciplinary insights and standards.

Mieke Bal's (2002) study of the methodological role of concepts in interdisciplinary study of culture contributes added insights. The metaphor of 'borrowing' suggests that concepts and methods occupy a designated place in the knowledge system. Yet, concepts exhibit both specificity and intersubjectivity. Concepts such as 'image', 'tradition', and 'performance' do not mean the same thing for everyone. However, they foster common discussion as they travel between disciplines, between individuals, between periods, and between academic communities. In the process of travel, their meanings and uses change. Concepts have an analytical and theoretical force with the potential to go beyond multidisciplinary diffusion. They stimulate productive propagation and prompt a new articulation with an emphasis on and ordering of phenomena within the cultural field that does not impose transdisciplinary universalism. The basis of interdisciplinary work, Bal maintains, is selecting one path while bracketing others. Interdisciplinary analysis has a specificity that is not lost in superficial generalisms. 'Surfing' and 'zapping', Bal cautions, only produce 'muddled multidisciplinarity'.

As these descriptions and illustrations suggest, interdisciplinary research has a highly generative nature. A priori unifying principles, theories, frameworks, and sets of questions provide coherence. Proven techniques for mediating different perspectives also help, such as Delphi method, scenario-building,

general systems theory, brainstorming, and computer analysis of multiple perspectives. However, context-related adaptations, deletions and additions may be expected. Reconsideration, reformulation and restating are vital activities for constructing higher-order comprehensive meanings. Creating an integrated product, solution, or perspective, Steve Fuller suggests, requires moving from lower-level translation of disciplinary perspectives by bootstrapping up to higher levels of conceptual synthesis. Linguistic models are not imported intact from metamathematics, set theory, symbolic logic, or any paradigm. They evolve in the creation of a trade language that may develop into a pidgin, an interim tongue or a creole, a new first language among a hybrid community of knowers (Fuller, 1993: 42). Bilingualism is a popular metaphor of interdisciplinary work. However, mastery of two complete languages rarely occurs. Interdisciplinary language typically evolves through development of interlanguage (Klein, 1996: 220).

Collaboration

Many consider interdisciplinarity to be synonymous with teamwork. It is not. Heightened interest in teamwork to solve complex intellectual and social problems, though, has reinforced the connection. Every collaboration creates a unique dynamic and organizational structure. Teams differ by duration, size and physical proximity of members; their age, gender, and racial and cultural composition; and participating disciplines, professions and functions. All teams, however, need a results-driven structure, clear roles, strong leadership, an effective communication system, methods of monitoring performance and giving feedback, and a means of recording and making fact-based judgments (Davis, 1995: 92). Social and cognitive factors are tightly interwoven (O'Donnell and Derry, 2005: 60). Interdisciplinary teams are status systems that reflect external hierarchies of power. A prestigious person or discipline may dominate, inhibiting others from speaking, impeding role negotiation, delaying communal work, and creating social and cognitive dependence. In her pioneering study of working relationships among psychologists, psychiatrists, and sociologists in mental-health projects, Margaret Barron Luszki (1958) also found that disciplines imported to help with a project often tended to be in subordinate power positions.

Simon and Goode's (1989: 220–1) account of a policy research project illustrates four models of collaboration that occur. The project focused on the efforts of laid-off employees and union leaders to save jobs in the supermarket industry. The dominance of an economic perspective and quantitative model restricted the anthropologists' role to supplying background context from interviews in a contracting mode rather than a full partnership:

1. *Background or context information*, an additive step that can be supplied separately from contributions of other researchers and may only appear as an appendix or separate case study;
2. *Elaboration or explanation of findings* from quantitative components; still limited to an additive role that typically produces a concluding chapter valued as descriptive detail, not findings;
3. *Definition of important variables or categories* for quantitative study, a step that sometimes occurs at the outset or prior to finalization of research design, structured instruments, or analytic approaches;
4. *Creative combination of ethnographic and multivariate approaches in research, analysis, and interpretation*, a rare occurrence in which fundamental questions are refined using participants' approaches on a mutually illuminating basis.

Shortfalls of integration in teamwork also occur for other reasons. Progress may be deterred by lack of incentives and an inadequate reward system, constraints of time and access to equipment, rigid budgetary and administrative categories, and restrictive legal mandates and policies. Social and psychological impediments block progress as well, including resistance to innovation and risk, mistrust, insecurity and marginality. Lack of integrative skills, systems thinking and familiarity with interdisciplinarity are added factors, along with the 'boundaries of reticence' that disciplinary socialization

creates. Individuals must avoid the tendency to make a 'regressive return to categorization' (Caudill and Roberts, 1951: 14). Conflict must also be addressed surrounding both technical issues (definition of problem, research methodologies and scheduling) and interpersonal issues (leadership style and disciplinary ethnocentrism).

Krauss and Fussell emphasize the importance of mutual knowledge-building (1990). Joint definition of a project is required, along with the core research problem, questions and goals. Team members need to clarify differences in disciplinary language, methods, tools, concepts and professional worldviews. Role clarification and negotiation help members to assess what they need and expect from each other. Ongoing communication and interaction foster mutual learning and a sense of 'teamness' and interdependence. The organizational framework should also provide for progressive sharing and interactive cross-testing of empirical and theoretical work with coordinated inputs from the beginning. If individuals hold back during the early phase, the prospect of arriving at a shared or interfacing cognitive framework is foreshortened. Certain contextual factors, Daniel Stokols (2006) advises, influence the 'collaborative readiness' of team members and their prospects for success. They include the presence or absence of institutional supports for interdepartmental and crossdisciplinary collaboration; the breadth of disciplines, departments and institutions encompassed by a particular center; the degree to which team members have worked together on prior projects; the extent to which their offices and laboratories are spatially proximal or distant from each other; and the availability or absence of electronic linkages. The more contextual factors aligned at the outset, Stokols admonishes, the greater the prospects for achieving and sustaining effective collaboration across fields.

The challenge of collaboration is magnified when trans-sector stakeholders are involved. Ideally, cooperation of academic and non-academic partners should occur at all stages, from planning through implementation. All aspects of a program or project should be included as well: from organization and management to consensus-building among stakeholders and knowledge production. Continuous evaluation provides feedback loops that improve the research process and the conceptual framework. Ultimately, Jack Spaapen et al. (pers. comm., 2003) advise, 'Quality' is a relative concept that is determined by relations within the environment of a research group and the goals of its members. Research must 'attune a pluralism of interests and values' within a dynamic set of programs and contexts where the interests of a variegated group of stakeholders may conflict but new opportunities arise. An empirical mode of evaluation and simplistic algorithmic models fail to capture the complexity, contingency, and emergent discovery and novelty that characterizes much of interdisciplinary research.

Maurice DeWachter's (1982) model of an interdisciplinary approach to bioethics bridges the gap between ideal models and the realities of practice. In bioethical decision-making, a particular problem forms the basis of a global question for all team members. The ideal model of integration and collaboration starts with the assumption that individuals will suspend their disciplinary/professional worldviews from the beginning, in favor of a global question based on the problem to be solved. Realistically, though, participants are usually unwilling to abstain from approaching a topic in terms of their own worldviews. The best chance of succeeding, DeWachter counsels, lies in starting by translating a global question into the specific language of each participating discipline, then working back and forth in iterative fashion, constantly checking the relevance of each answer to the task at hand. That way, no single answer is privileged.

Vosskamp (1994) and Klein (1996) treat interdisciplinarity as communicative action. Vosskamp proposes that the agreement/disagreement structure necessary for all communication shapes the possibility of interdisciplinary dialogue. Consent/dissent (*Alteritaet*) requires accepting the unforeseeable and

productive role of misunderstanding from the outset. Després et al. (2004) also invoked Habermas's (1987) notion of 'communicative rationality' in a study of a collaborative urban planning project to redefine suburban neighborhoods built between 1950 and 1975 on the outskirts of Quebec City. Scientific and academic knowledge alone, they explain, cannot deal adequately with the complexity of subjects and problem domains such as revitalization of residential neighborhoods. Following Habermas, instrumental, ethical and aesthetic forms of knowledge are needed as well. Rational knowledge comes out of not only 'what we know' but 'how we communicate' it. Stakeholders enter into a process of negotiation, confronting the four kinds of knowledge in a series of encounters that allow representatives of each type to express their views and proposals. In the process, a fifth type of knowledge progressively emerges. It is a kind of hybrid product, the result of 'making sense together'. 'Intersubjectivity' requires an ongoing effort to achieve mutual understanding. Simply bringing people together and coordinating conversations is not enough, Després et al. (2004) stress. Mediation is required to collectively define what could and should be done. Each stakeholder expresses individual interests or views that are discussed and criticized by others. The role of the mediator is to extract this knowledge. As progressively shared meanings, diagnoses and objectives emerge, individual interests and views are seen in different perspectives.

There is no interdisciplinary Esperanto that may be universally applied. Studies of interdisciplinary communication in practice settings reveal that everyday language is usually combined with specialist terms. 'Interdisciplinary discussions', Gerhard Frey (1973) found, 'normally take place on a level very similar to that of the popular scientific presentation.' They become more precise as individuals acquire knowledge of other disciplines. At a higher level of conceptual synthesis, new and redeployed terminology form the basis of a working meta-language. The quality of outcomes cannot be separated from development of a shared language culture and its richness. 'Most misunderstandings', Frey found, 'are caused by the fact that the same words are used with different meaning.' Luszki (1958) reported that members of mental-health teams paid a price for congeniality. By not dealing with conflicts in disciplinary definitions of such core terms as 'aggression', for instance, they reduced the number of creative problem-solving conflicts that would have promoted high-level, shared concepts. Difference, tension and conflict are not barriers that must be eliminated. They are part of the character of knowledge negotiation.

POSTSCRIPT

Talk of a 'postdisciplinary' age is premature. Disciplines have not disappeared; yet, Johan Heilbron (2004: 38) observes, they now stand alongside other modes of organization at a time when the significance of the classical disciplines is decreasing, practical fields have a growing importance in the knowledge system, and the heteronomy of academic institutions is increasing. Few doubt that change is occurring. Talk of increasing interdisciplinarity, though, begs the question of how well prepared researchers are for this kind of work. Many still learn on the job. As disciplines continue to respond to new needs and interests, and as interdisciplinary communities and hybrid fields secure a greater role in knowledge production, it is imperative that researchers become more self-conscious about the dynamics of integration and collaboration and more aware of cognate disciplines and intersecting interdisciplinary fields. These are becoming 'basic' to the conduct of research and education. Modern systems of higher education, Burton Clark (1995: 154–5) exhorted, are confronted by a gap between older, simple expectations and complex realities that outrun those expectations. Definitions that depict one part or function of the university as its 'essence' or 'essential mission' only underscore the gap

between simplified views and new operational realities that are transforming the way we think about knowledge and education.

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