THEORY AND PROGRESS IN SOCIAL SCIENCE

This work attacks questions that have long troubled social science and social scientists – questions of the cumulative nature of social inquiry. Does the knowledge generated by the study of social, political, and economic life grow more comprehensive over time? Do today's social scientists in any meaningful sense know more than their intellectual predecessors did about such perennial concerns as the origins of war and peace, or the causes of economic growth, or the forces shaping social stratification, or the origins of civil upheaval? These questions go to the heart of social scientists' soul-searching as to whether they are indeed engaged in "science" and, if so, what kind of science is involved.

The author pursues these questions through in-depth examination of various theoretical programs currently influential in social science, including feminist social science, rational-choice theory, network analysis, and others.

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For Alix and Adam

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Preface

This book concludes a two-part work that began with *Theories of Civil Violence* (1988). The two works, written over a period of more than ten years, seek judgments on the progressive character of social science. Can our overall understanding of social life reasonably be said to increase over time? Do the analytical successes of earlier thinkers form necessary stepping-stones for the work of those who follow? Do present-day social scientists in any meaningful sense know *more* than our predecessors knew decades or centuries ago?

My concern with such questions has grown out of deeper perplexities about the conduct of social inquiry and its role in the larger social context. On the one hand, working social scientists normally defend their chosen approaches as superior to the alternatives – that is, as providing more accurate, more profound, more veridical insight into the subject matter. Yet even casual acquaintance with our literatures reveals the predictable obsolescence of such perceptions. The appeals of any particular way of studying the social world over others are enormously context-sensitive. In retrospect, prevailing theoretical mind-sets often seem to tell us more about the tensions or obsessions affecting particular ages or intellectual constituencies than about the social world. To the extent that our understandings have this context-bound character, it would appear that every theoretical school begins the work of social analysis anew. And insofar as this is true, it is hard to argue that the sum total of knowledge grows over time.

The question is: Should we expect anything else of social inquiry? Should we simply expect our theories to capture and focus the perceptions and concerns of a particular age or intellectual constituency? Or should our analyses aim at insights that withstand shifts of social context and, hence, of theoretical fashion? Any answers to such questions, I hold, have the most far-reaching implications for the way we go about our work.

These questions challenge all the disciplines involved in the study of

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social institutions and behavior – including sociology, political science, anthropology, economics, elements of legal and policy studies, and various other endeavors. In this book, I focus most often on examples and works from my own discipline, sociology. But the arguments put forward here apply to all domains of social science. If discussion were to trace the vicissitudes of such theoretical programs as game theory (most avidly pursued by economists and political scientists), or identity theory (more the province of psychologists), or theories of cultural change (as pursued by anthropologists), I am convinced that similar patterns and conundrums would be evident. Indeed, the four theoretical programs discussed in detail in Part II of this book – rational-choice theory, "general theory" in the tradition of Talcott Parsons, network analysis, and feminist social science – sprawl across disciplinary boundaries. All have had practitioners and detractors from most social science disciplines.

And all our disciplines are evidently subject to the phenomenon of central interest here, the transience of theoretical programs. Concomitant with such transience is endemic uncertainty as to whether achievements registered in any one line of theoretical inquiry will matter to theoretical "outsiders" – or, indeed, will continue to move anyone at all, once the immediate flush of theoretical sex appeal has subsided.

An apparent exception here is economics, often vaunted as more "successful" than its sister social sciences. The usual accounts for this supposedly more authoritative status point to the high levels of quantification and the allegedly more settled state of its theoretical structures. Yet these distinctions are misleading. The familiar, highly quantified modes of analysis are indeed characteristic of the neoclassical school. But this theoretical program predominates mainly in the English-speaking world, where its supremacy dates back no further than the 1940s (Yonay forthcoming). The historically recent advent of this mind-set suggests that it may ultimately prove no more permanent than other theoretical ascendancies considered in this book.

In *Theories of Civil Violence,* I sought to trace the course of theoretical change with regard to a single substantive issue – the history of attempts to understand the origins of such phenomena as riot, rebellion, and other forms of militant collective action. This book pursues the same underlying questions in a yet broader context. Rather than comparing theoretical accounts of a single subject matter, this work weighs the progressive claims of broad theoretical visions of social life – "theories" in the sense of comprehensive programs of social inquiry.

Enthusiasts of such programs typically see them as spearheading intellectual progress, as constituting decisive "steps ahead" in our overall understanding. My question is simply: What should we make of such claims? Does the characteristic promise of new theoretical movements to

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"move the field forward" withstand close examination? Does the historical record indeed reveal patterns of theoretical change that could be interpreted as progressive?

As I look back on the completed manuscript of this volume, I am struck by what a conspicuously *un-trendy* work it is. As in *Theories of Civil Violence,* many of the ideas targeted for attention here are now utterly out of fashion, if not anachronistic. I have tried to take them no less seriously for that. By the same token, where discussion focuses on works of high intellectual sex appeal at the moment of my writing, I have resisted the temptation to climb on the bandwagon.

There are good reasons for such an approach. I have grown convinced that social scientists' obsession with remaining ahead of the curve of theoretical fashion – by pursuing the latest theoretical "revolutions," "breakthroughs," "reorientations," "new syntheses," and the like – comes at a cost far greater than we usually acknowledge. Perhaps the best that can be said for this obsessive revamping and redefinition of basic directions and concepts is that it generates steady outlets for the talents and energies of those engaged in it. The drawback is that theoretical innovations are developed more for show than for use. Who would deny that the marketplace of theoretical ideas often recapitulates the frenzy of the stock exchange – with no distinction being made between the enduring value of ideas and their current selling price?

Thus I conclude that theory in social science is too important to be left exclusively to professional theorists. If the "selling price" of theoretical ideas indeed fluctuates too rapidly for comfort, perhaps we need to consider more enduring standards of value. The best way to do this, I maintain, is to weigh the potential of different sorts of insight to outlast the special social and intellectual contexts in which they arise.

As in *Theories of Civil Violence,* I have struggled to pursue these goals with the greatest possible clarity of language. I am convinced that the obscure and self-referential writing that plagues so much social science itself supports the forces countervailing against meaningful cumulation of knowledge. Most troublesome among these is the tendency to focus more and more narrowly on issues of interest only to restricted and self-absorbed intellectual constituencies.

By making one's ideas unintelligible to the skeptical, one does at least gain a certain protection against criticism. But the price, from any broader assessment, is prohibitive – the creation of a world where developments within any one theoretical niche are matters of indifference to everyone else. Thus my concern to assess all the ideas considered here from the standpoint of a theoretical "outsider" – a concerned thinker who begins with no prior stake in those ideas. And thus my effort to rely on language accessible to any thoughtful reader. If sunlight is indeed the best disinfectant in public affairs, as Brandeis had it, let us hope that something similar holds for the relationship between straightforward writing and scholarly communication.

In the years since beginning this book, I have enjoyed support and advice from many more sources than I can adequately acknowledge. My home institution, the State University of New York, Stony Brook, has provided sabbatical leave and other important local support. The Institute for Advanced Study at Princeton made a major contribution by appointing me as a Mellon Fellow for the 1992–1993 academic year. My particular thanks go to Professors Geertz, Scott, and Walzer of the Institute for making possible this indispensable period of intensive research and reflection. The Russell Sage Foundation has provided crucial support for the final editing and organization of the mansucript.

Other debts are less institutional. Above all, Stephen Cole, my colleague at Stony Brook and my contemporary at the Institute for Advanced Study, read and criticized much of this manuscript in various stages of its preparation. In addition, I have received important critical support and advice from Robert Alford, Said Arjomand, R. Douglas Arnold, Stephen Brush, Lewis Coser, Cynthia Epstein, Scott Feld, Steven Finkel, Debra Gimlin, Mark Granovetter, Melissa Grogan, Leslie Irvine (coauthor of Chapter 6), Nilufer Isvan, Michael Kimmel, Larry Laudan, Rachel Laudan, Doug McAdam, James Rauch, Ian Roxborough, Barrie Thorne, Charles Tilly, Axel Van Den Berg, and Dennis Wrong. Alford, Cole, Roxborough, Van Den Berg, and Wrong have labored over the entire manuscript, going over some parts more than once; if there is an editorial equivalent of the Purple Heart, they surely deserve it. Needless to say, none of these thoughtful people could possibly endorse all the positions taken here, and some would take exception to nearly all of them. But all have helped me to make these observations and arguments as strong and as clear as they can be.

Aniane, Herault, France
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