Getting Sociology Right

A HALF-CENTURY OF REFLECTIONS

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Introduction

(2013)

Over a period of nearly four decades covering most of my professional career (1967–2005) I wrote a dozen-plus essays on the nature, status, methodology, problems, current situation, and future of the academic discipline of sociology. The topics of these essays were very different from one another and they were published in the greatest mix of accessible and inaccessible places. Yet they all had one characteristic in common: I initiated none of them. Every essay was written at the behest of an organizing committee of a scholarly meeting, an editor of a journal, or the reigning president of the American Sociological Association for presentation in a symposium. In every case a topic was assigned, but I was given freedom to develop that topic as I chose.

These circumstances do not bode well if one is searching for a systematic pattern in those essays. After all, they were externally initiated at different times, for different occasions, by different people with different agendas on their mind, and in different environments. The essays stimulated varying levels of interest and response at the time of delivery and publication, but for a long time they have resided as discrete and barely visible items in my formal bibliography, which almost nobody sees, much less reads.

In the spring of 2012, during the course of interviews for my oral history (Bancroft Library, University of California, Berkeley), it occurred to me that there is a heretofore-unrecognized continuity in the essays. It also occurred to me that it might be of some interest to draw them together in one place and venture some reflections. I assembled and read them and confirmed that they reveal a certain pattern in their development. The reason for this, moreover, is that while each essay was on an assigned topic, I wrote what was on my mind at the moment, though I did not do this as a matter of assertion or defiance of my assignment. What was on my mind, moreover, reflected where I was in my sociological career and the influence of my intellectual, academic, and political experiences in the period leading up to each episode of writing.

The overall pattern is this: the emphases of the earliest essays (late 1960s) were somewhat formal, treating sociology and the other social sciences as autonomous enterprises, driven by the ideals, norms, and methods of science. In retrospect, these essays reflected the idealism of my training in the 1950s and my early career enthusiasm. As the later essays unfolded, I extended this limited view and explored one "extrascientific" influence on the discipline after another, yielding a much more complicated, kaleidoscopic version of the nature of the sociological enterprise. I explain the logic of this development in the remainder of this introduction. Interestingly, cumulatively complicating my early views of the sociological program did not weaken my commitment to my own discipline, as one might expect as one's idealism diminishes and one's realism grows. I remain a fully committed and enthusiastic sociologist and social scientist, convinced of the fundamental value of the scientific mission of the larger social science enterprise.

Chapter 1, "The Optimum Scope of Sociology," was presented in 1967 to a gathering of mainly senior sociologists, virtually all of whom had been or were destined to be presidents of the ASA. It was organized by the American Academy of Social and Political Science in Philadelphia, and the proceedings were printed in the Academy's journal. The format was formal presentations by three sociologists regarded as young Turks at the time—James S. Coleman, Peter Blau, and myself—each commented on by two wise seniors and then thrown open for discussion.

My assignment was a "normative" one, presupposing sociology to be a kind of purposeful, objective endeavor and daring me to define its nature

and lay out its intellectual contours. This was, of course, a mission impossible for me, because every aspect of my assigned topic had a contested history, and the late 1960s were a period of especially vigorous contestation. In writing about this conference in 1999 (see pages 245–6), I remarked that it was "rash" on my part to respond to this assignment as defined; today I might strengthen that and say "foolhardy." Nevertheless I made an effort to lay out, more or less systematically, (a) the five major frameworks that inform genuine sociological inquiry—the demographic-ecological, the psychological, the group, the social structural, and the cultural; (b) the subdivisions of the field and what has produced them; and (c) the proper nature of sociological explanation. I still regard this effort as informative and relevant within its own scope, but it rested almost exclusively on an autonomous, self-contained, and limited vision of the nature of sociological inquiry, and it ignored almost altogether the "nuts and bolts" of what one does as a sociological investigator and the multiple influences on sociological inquiry from outside the discipline, narrowly conceived. I think I was simultaneously reflecting in part the hopeful view of sociology I had inherited from my training in the 1950s, the formalism of the course on sociological theory that I had been teaching at Berkeley for seven years (see my book *Sociological Theory—A Contemporary View,* published originally in 1971 and again in 2011 by Quid Pro Quo Press), and, more generally, the optimism and muscularity of Berkeley sociology in its golden years of growth, 1952 to 1964.

Two years earlier I had undertaken another "mission impossible" for Paul Lazarsfeld, who was president of the American Sociological Association and had chosen as his presidential theme the uses (applications) of sociology. After the meetings Lazarsfeld wished to publish a sizable number of delivered papers in a volume to be entitled *The Uses of Sociology.* He enlisted the services of William H. Sewell and Harold L. Wilensky as coeditors. It occurred to them that, without stretching the idea of "uses" too far from the practical, sociology might be "useful" to the other social sciences. A relevant essay would presumably refer to the intellectual and other benefits sociology brings to its sister disciplines. They asked me to write this essay (chapter 2). To accept an assignment of such scope and complexity also now occurs to me as foolhardy, but I took it on. To execute this task, I had to articulate a number of "dimensions" on which the various social sciences could be compared with one another. For this

purpose I chose four dimensions—favored dependent variables, favored independent variables, models of explanation, and research methods primarily employed—and gave concise expositions of sociology, economics, political science, anthropology, history, and psychology according to these dimensions. I extended the analysis and attempted to pinpoint specific kinds of "services" or articulations that sociology and these other social sciences could provide one another, given their contrasting emphases.

I still regard this enterprise as useful and relevant, but limited in several ways. First, it represented the various social sciences as they were then and necessarily ignored what has happened in all of them in the ensuing decades. Second, and related, I overrepresented the functionalist view (my own training) within sociology, which was just coming under fundamental attack. Third, it also was an "idealized" essay in many respects, in that I treated the social sciences as largely autonomous intellectual enterprises. The chapter manifests my love for analytic neatness, which still exists but has been tempered over time. Fourth, like the "optimum scope" essay, it represented the various social sciences as being "driven" mainly by scientific aims and methods, and it downplayed the insularities, jurisdictionalism, sectarianism, conflicts within and between them, and outside pressures—all of which surely influence their usefulness or lack of usefulness for one another. Finally, I had the idea that all the social sciences were striving for and moving toward scientific "maturity" and because of that would display certain maturing tendencies; that was also an idealized and limited view, even though I noted lack of consensus and conflict from time to time. Both essays expressed a certain optimism and serenity that has not been justified by the succeeding half century of the history of the social sciences. To record these observations now is not an act of renunciation on my part but a recognition of the limits of those early enterprises.

As it turned out, I had an opportunity to extend and qualify these formal expositions almost immediately. In 1968 the editors of a special issue of the journal *Sociological Inquiry,* entitled "The Craft of Sociology," asked me for a contribution. The issue was designed to give recognized authors an opportunity to reflect on and provide a biographical account of their own sociological work. I chose to focus on the evolution of my thinking and strategic intellectual choices in my first two single-authored books, *Social Change in the Industrial Revolution* and *Theory of Collective*

Behavior. Responding to the invitation to take a biographical approach, I discovered how important "accidental" influences were on my thinking and choices, and how "messy" the research process is in terms of its unfolding of false starts and dead ends, as well as productive lines of research and "methodologically correct" presentation of arguments. I did not reflect very much at the time on the relations between this exposition and my earlier theoretical-conceptual-methodological thinking on sociology and the other social sciences, but in retrospect this essay, reproduced as chapter 3, simultaneously marked an expansion of those earlier perspectives and an extension of my understanding of social science scholarship.

Ten years later I accepted an assignment from Hubert M. Blalock to present a paper in one of his presidential sessions at the ASA annual meetings. His general theme was the relation between theory and research in sociology. In preparing this paper I did not think of it as having any special relation to my earlier biographical essay. But it did; it was a formal extension of thinking begun in that essay. I argued basically that there are three stories to be told in the understanding and assessment of sociological work:

1. Biographical, or what in the ongoing life of the investigator determines choice of problems, polemics, emphases, and styles of sociological work. This process is not systematic and involves many diversions, redirections of inquiry, and often arbitrariness in selecting what lines of research to pursue and not to pursue.
2. Formal, or how one constructs and refines an interpretative architecture to present the results of research. Examination of this process reveals the basic theoretical, methodological, and evaluative options chosen to explicate the research. It is how the biographical meanderings are converted into more systematic presentations, polemics, and conclusions.
3. Evaluational, or the ex post facto operation of bringing the formal standards of scientific inquiry—theoretical, empirical, and methodological norms—to bear in assessing the research results. Much of this activity, of course, emanates from reviewers and critics.

In this essay, reproduced as chapter 4, I argued that each of these stories has its own autonomous significance, reality, and integrity and that, above all, *the stories should not be assimilated to one another and mistakenly compressed into one story*. Social scientists, who generally regard themselves as

belonging to the rational classes, tend to subordinate unsystematic biographical stories to formal-scientific standards and to represent their work as idealized rational exercises. I challenged that kind of idealization in this essay, and in the process I sketched a more contingent and, I believe, more realistic account of what scientific investigation entails—and certainly a revision and extension of my earlier views. As usual, however, at the time I neither explained nor even recognized this as the intellectual shift from my earlier formulations.

Ten years later I expanded my account in yet another direction, again without conscious recognition that I was doing so. One of the unspoken assumptions of my earlier expositions was that sociologists and other social scientists choose their own problems, and that by implication this choice is informed by a dialogue with past researchers, unsolved problems in the sociological paradigm(s) within which one is working, and the free pursuit of new problems. The occasion was yet another invitation by a president of the ASA—Herbert Gans in this case—to contribute a general essay on understanding sociology in America. I do not remember how or why I thought of the specific topic, but I chose to write on external influences on sociology, thus acknowledging the field's embeddedness in its larger society. This essay appears as chapter 5. I chose cultural influences (decisive in shaping different "national sociologies"), influences emanating from large and decisive social changes in society, and political influences, including those affecting research funding. To focus on these marked a further departure from the posture of relative innocence that had informed my earlier essays.

I am also convinced that the recognition and choice of this emphasis reflected fundamental changes in my professional career. My early years were dominated by determined efforts to establish myself as a research scholar in my own field, and my main audiences were my sociological colleagues at Berkeley and the larger professional world represented by the ASA and, less visibly, the International Sociological Association. As time went on I became more and more involved in the "real world," initially in the politics of my own university and later in the social science "establishment." The stories of many of these engagements appear in this book, especially in chapter 12, so I only tick them off here. The 1980s were especially significant. I had become involved in the rough-and-tumble of University of California politics in my services as chair of the Berkeley and

UC systemwide academic senates; I was immersed in the politics of the International Sociological Association in co-initiating the research committee on Economy and Society and as a member of the ISA's executive committee; and I was mobilized in the organized efforts of the National Research Council of the National Academy of Sciences to resist the efforts of the Reagan administration to reduce support for the social sciences in the early 1980s. All these involvements exposed me—firsthand, involuntarily, and sometimes brutally—to the "external influences" affecting my field and the larger social science scene. Chapter 5 is an effort to identify and reflect on the significance of the external forces involved.

About the same time I was asked by the editors by the Canadian journal *Cahiers de Recherche Sociologique* to contribute to a special issue on the future of sociology. My assignment was relatively open-ended. I had referred in past writings to the incessant fissiparous (specializing and fragmenting) tendencies in sociology and the other social sciences and other disciplines, but by now I saw that process as providential and as continuing to change the basic nature of the field. In that essay, which appears as chapter 6, I tried to identify the forces that produced these splintering effects, the conflicts that accompanied them, and some theoretical and organizational efforts to strive toward reintegrating the discipline. The last motif was something of a sentimental reminiscence on my part—looking toward the systematic efforts to synthesize and unify the field that were so much a part of my sociological apprenticeship. The strongest word I could muster for these integrative efforts in the 1990 essay was *accommodation.* I cannot identify all the trends, situations, and events in my life that combined to produce that essay, but I would have to imagine that my experience of a quarter century of deep division and conflict in my own department at Berkeley (beginning with the Free Speech Movement in 1964) was a major sensitizing influence.

A couple of years later an old friend and colleague, Kai Erikson, asked me to contribute to a symposium at Yale, which he called "The Art and Science of Sociology." Again, my assignment was open-ended. But as it turned out, the essay dealt with the same issues of division and conflict in our field as did the *Cahiers* essay. This time, however, I took a new tack. I argue in that essay (chapter 7) that three fundamental visions or impulses have informed the field of sociology from its beginnings and that the

tensions among these visions have never been resolved and can be evoked to explain fundamental and enduring conflicts in the discipline. These visions are (a) the scientific, judging the intellectual work of sociology's practitioners according to criteria evolved in the natural sciences and adapted to the social sciences, and valuing above all the creation of knowledge that measures up favorably to the theoretical and methodological standards of science; (b) the humanistic, giving priority to the amelioration of social problems and the improvement of society through reform and/or revolutionary change; (c) the artistic, to which I assigned two meanings—first the aesthetics or elegance of intellectual formulation, referring to originality and beauty of insights and theories, and second, the "art" of applying sociological knowledge to practical decision making, social problems, and social policy. Taken together, chapters 6 and 7 take on the unity-diversity and consensus-conflict issues once again, but this time in more textured ways than I had previously thought about.

Also by 1990 I had become deeply immersed in international sociology, largely through my involvements in the International Sociological Association and my leadership in organizing three conferences on sociological theory as joint enterprises of the American and German Sociological Associations. So, when I received an invitation to write a piece on internationalization of knowledge for *Current Sociology,* I felt ready to do so. The starting point was to ask in what sense sociology (or any other social science) could be regarded as producing knowledge that was valid comparatively at least and universally at best. I also wanted to ask what facilitated the international applicability, diffusion, and uses of social science knowledge. The result (chapter 8) is a series of analytic and empirical reflections on these questions. To remind the reader of the larger significance of this adventure, it marked a further branching out, even a departure, from my initial orientation to sociology as a field that manifested formalism, positivism, and scientific standards and marked an extension of my inclinations to portray it as much as an evolving "art of the possible" in an uncertain environment.

Chapter 9, on the application of sociology to social problems, parallels chapter 8 in an important sense. Like that chapter, its starting point is the knowledge created by social scientists. In this chapter, however, I reach out in a very different direction and address the "artistic" issue of applying

that knowledge. Two sources of sensitization to that issue should be made explicit. The first was my decade-long leadership in the National Research Council in its efforts to demonstrate the applicability of the social sciences in the past, present, and future (see pages 283–7). The second, quite accidental, was my membership, beginning in 1988, on the President's Advisory Committee (University of California) on the national laboratories at Los Alamos, Livermore, and Berkeley, which involved me deeply in applications of natural science knowledge, initially in weapons research and national defense and later in a range of other areas as the laboratories began to diversify their activities at the end of the Cold War. It was, in fact, my acquaintance with numbers of physicists and other natural scientists that resulted in an invitation for me to deliver a paper at a conference at Sigma Xi, the scientific honorary society, that celebrated the fiftieth anniversary report of Vannevar Bush report on science and the nation. Again the topic was left more or less open, but I decided to reflect as systematically as I could on the issues involved in the application of social science knowledge to social problems. The result is seen in chapter 9. In that essay I make some prognostications about what kinds of social problems seem likely to appear on the horizon. But more fundamentally, I undertake to redefine the nature of a "social problem," refusing to define it as a "thing" in society in keeping with positivistic view of social problems, and regarding it as the result of a complicated social and political "process." Having thus redefined its nature, I could then move to a discussion of the ways that social science research could provide useful insights and guidance. I regard that reformulation as important, somewhat novel, and reflecting in part an influence from the perspective of social constructivism, but I tried to turn the discussion in a positive direction, as many social constructivists do not do.

The final essay of Part II—chapter 10, entitled "The Questionable Logic of 'Mistakes' in the Dynamics of Knowledge Growth in the Social Science"—is out of temporal place in that it is the most recently published (2005) of all essays in this volume. The chapter deals with the distinctive features of the growth, transfiguration, and "death" of social science knowledge, and, as such, is an effort to systematize the often-bewildering patterns of change in that knowledge. I wrote it in response to a request to write an article on "big mistakes" in the social sciences—a concept meant

to correspond to major errors in the history of the natural sciences. I initially declined to write such an article for a special issue of *Social Research* because I thought that the logic of "mistakes" in that sense could not apply to the social sciences. The editors of that issue did not accept my initial decision and argued that I should, instead, write an article explaining why that logic did not apply. In the end I accepted their reasoning and produced an essay on some principles of acceptance, rejection, and alteration of knowledge in the social sciences.

The three essays in Part III contain the most recent reflections and depart in many ways from most of the others. The first resulted from an invitation from the editors of the *Annual Review of Sociology*. I had played an important role in the creation of that publication in the late 1960s and served as associate editor and member of its editorial board in its early years (see page 237). When the publication decided to celebrate its twenty-fifth anniversary in the 1990s, they asked me to review its history of publishing progress-report essays on different sociological topics and to offer some additional reflections on the changing character of sociology and of the journal's way of recording these developments. My efforts appear in chapter 11 and offer some ideas on the contingencies and difficulties of "reflecting" the developments of a field as complex and changing as sociology.

Chapter 12 is a kind of capstone essay for all the rest, though I did not write it as such. I wrote it as a response to an invitation from the editor of the *American Sociologist* to produce a general article of my choosing. I chose to write an autobiographical-intellectual essay focusing on the tension between being a "disciplinary" sociologist and an "interdisciplinary" social scientist, a main theme in my career. The first part of the essay goes well beyond that topic itself and attempts to reveal the circumstances of my childhood and youth that might have given rise to this particular style in the social science world. In the main part of the essay, I chronicle, in an autobiographical way, the alternation of disciplinary and interdisciplinary emphases in my own research and other professional activities, a constant and recurring theme. This essay also makes much of the complex mix of personal motivation, opportunities taken and foregone, and "chance" in the unfolding of one's career.

I wrote chapter 13, the "Afterword," in 2013, to add some final reflections and afterthoughts on the essays. It has three parts. First I review my intel-

lectual projects and publications in the first decade of the twenty-first century, commenting on their intellectual substance and their "interdisciplinarity." Second, I reconsider the sources that influence the choice of sociological problems and shape the contours of the field. Finally, I reflect on some past and ongoing trends in society and the discipline, concluding with some observations on trends and troubles in the country's system of higher education and their implications for the future of the social sciences.

In the end these essays, taken together, can be regarded as having both an unsystematic and a systematic quality. They are unsystematic in that they were produced as a result of changing career involvements and the accidental circumstances of being invited by different people with different agendas. But they are systematic in the sense that they reflect a gradual process of learning, of fanning out my intellectual interests and producing, in sequence, a different, more contingent view of the circumstances that have conditioned the existence and evolution of the academic subject of my life's calling.