Irwin Deutscher

Making a Difference

The Practice of Sociology

MAKING A Difference

MAKING A Difference
THE PRACTICE OF SOCIOLOGY

Irwin Deutscher

Routledge
Taylor & Francis Group
LONDON AND NEW YORK

First published 1999 by Transaction Publishers

Published 2017 by Routledge
2 Park Square, Milton Park, Abingdon, Oxon OX14 4RN
711 Third Avenue, New York, NY 10017, USA

Routledge is an imprint of the Taylor & Francis Group, an informa business

Library of Congress Catalog Number: 98–8590

Library of Congress Cataloging-in-Publication Data

Deutscher, Irwin, 1923–
Making a difference : the practice of sociology / Irwin Deutscher.
p. cm.
Includes bibliographical references (p.) and index.
ISBN 1–56000–359–6 (alk. paper)
1. Sociology. 2. Sociology—Research. I. Title.
HM51.D48 1998
301—dc21 98–8590
CIP

ISBN 13: 978-1-56000-359-5 (hbk)

With love to Verda who was there through it all

Contents

Introduction: On The Nature of Essays, Editing, and Organization and a Bit of Biography i

Part I: Toward a Useful Sociology

1. For Beginners: The Social Causes of Social Problems—From Suicide to Delinquency 3
2. A Short and Selective History of Evaluation Research in the United States 19
3. What Do Social Indicators Indicate? 43
4. For the More Advanced: Social Theory, Social Programs, and Program Evaluation—A Metatheoretical Note 57

Part II: On Doing Applied Research: Comments and Cues

5. The Stereotype as a Research Tool 77
6. Summarizing Research: Meta-analyzing Meta-analysis 89
7. Physicians' Reactions to a Mailed Questionnaire: A Study in "Resistentialism" 105
8. Secondary Data, Anecdotes, and Case Studies: Valid Evidence from Bad-Mouthed Sources 113

Part III: The Raised Eyebrow: Assumptions in Evaluation Research

9. Toward Avoiding the Goal Trap in Evaluation Research 129
10. Success and Failure: Static Concepts in a Dynamic Society 151
11. Traditions and Rules as Obstructions to Useful Program Evaluation: Part I, Rule-Making 169
12. Traditions and Rules as Obstructions to Useful Program Evaluation: Part II, Rule-Breaking 197
13. Public Issues or Private Troubles: Is Evaluation Research Sociological? 209

14. How Applied Sociology Can Save Basic Sociology: A Note on Consistency, Objectivity, and the Relationship between Basic and Applied Research 219
15. Project Head Start and the Cognitive Police 225

Part IV: Vignettes: Troubles in the Everyday World

16. On Public Housing: The Gatekeeper in Public Housing 253
17. On Aging in America: Misers and Wastrels—Perceptions of the Depression and Yuppie Generations 269
18. On Middle-Class Husbands and Wives in the 1950s: The Quality of Postparental Life 289
19. On Disaster Relief: Fun and Profit in a Disaster 305
20. On Town Drunks: The White Petty Offender in the Small City 321
21. On Anticipating Martin Luther King and the Integration of Public Facilities: Cohesion in a Small Group—A Case Study 327
22. On Delinquency: Some Relevant Directions for Research in Juvenile Delinquency 339
23. On the Education of Nurses: Professional Education and Conflicting Value Systems 351
24. On Development in "Developing" Nations: Home-Grown Development—The Education of Tribal Peoples 363

Part V: Polemics on Practice

25. The Moral Order of Sociological Work 377
26. Sociological Practice: The Politics of Identities and Futures 391
27. The Most Useful Knowledge for Everyone is the Most Useless Education of All: Social Needs versus Market Demands (being a consideration of projections, predictions, prophesies, and other magical forecasts of the future of liberal education) 403

Index 419

Introduction

On the Nature of Essays, Editing, and Organization and a Bit of Biography

When Irving Louis Horowitz, then president of Transaction Publishers, suggested that I edit a set of my older articles into one volume, I responded (probably with an ungrateful snarl), "That would be an exercise in vanity." Horowitz shrugged and without disagreeing snarled back, "What else is there?" A few years later, in a more serious vein, my publisher reminded me that his is not a vanity press and that he was convinced that this volume has public worth beyond any private satisfaction its publication might yield. I like that notion.

As I worked on the manuscript I thought of the many newly emerging programs in applied sociology and sociological practice and of the scarcity of helpful teaching materials for faculty and students. If nothing else, this volume can help students grasp the concept of an applied sociology and why it is important—to them as well as to the larger society. It can help faculty organize their own thinking and teaching in that area. Whether or not a professor agrees with my positions, they are surely provocative enough to capture the interest of students. Furthermore, it may provide more traditionally oriented sociologists with a better understanding of the legitimacy, the urgency, and the long history of sociological practice.

Beyond that, I view this volume as possibly serving (1) a historical purpose and (2) a function of accessibility and convenience. The articles and papers included were published or delivered over a five-decade period and collecting them might be helpful and informative to some future historian of American social science. In their own way, they reflect the temper of the times. Second, these chapters were published or delivered in a wide range of often obscure journals and venues. Their collection, in this more convenient form, makes them more accessible to practitioners, teachers, and students—especially at a time when there is rapid growth in the practice of sociology and in university curricula designed to provide the skills for that practice. Finally, I would like to believe that in becoming familiar with this volume, administrators and policymakers can learn better how to make use of social science and social scientists.

Essays

I have come to think of most of the chapters included in this volume as "essays." In an otherwise favorable review of a book I wrote a few decades ago, Alejandro Portes (1974:460) criticizes the work for its "consistent failure to pursue insights or to make explicit that a specific problem can be logically understood as a facet of a more general one, discussed earlier ." The criticism was fair and accurate, yet I would not do otherwise even if I could re-write the book. I thought then that perhaps my character was flawed by a childlike attention span—an urge to get on with it without pursuing insights or making things any more explicit than I had. It was only while writing this introduction that I opened my Webster's Unabridged (Webster 1980:6224) to the word "essay" and found the definition: "a short literary composition dealing with a single subject, usually from a personal point of view and without attempting completeness." I realize now that what Portes objected to and I enjoyed, was the essay style. I will comment further in the discussion of "specialization" on my limited attention span.

I do not stand alone in this tradition. In fact I am in excellent company. Everett C. Hughes attaches a value to this definition which I do not pretend to. In reference to an article by Robert E. Park, Hughes (1949:58) says, "I call it an essay, for its depth, breadth, and richness of hypotheses, neither required nor expected in an ordinary scientific paper." In a biographical essay, Lewis Coser (1994:13) describes Hughes himself as "primarily a writer of essays" while explaining that for Hughes "it was just too confining and restrictive to explore just a few phenomena in detail."

Georg Simmel is the prototypical sociological essayist. The magnificent sociological writings of C. Wright Mills and Robert K. Merton are largely in essay form, as is the work of Erving Goffman and Howard S. Becker. Perhaps the greatest influence on my own thinking about human behavior is found in the essays of Herbert Blumer. If some of the best minds of my discipline employ this style of writing and thinking, then surely I cannot be faulted for trying to imitate it. As Coser suggests, there is a freedom about this path. It seems to me that such freedom is largely missing in the (undeniably useful) ritual of scientific research which follows a rigorous procedural formula to pursue an extremely circumspect problem and permits little creative departure from a predetermined path.

Editing

The essays reproduced in this volume are almost exactly as they appeared originally. Editing is by its nature a revisionist activity—well beyond the usual "corrections" of grammar and spelling. I had, in the spirit of an essayist, intended to do what I could to improve the style of these pieces and thus to pretend that I wrote somewhat better at the time than is the fact. Actually, I made few changes in the previously published material. Nevertheless, this caveat may protect me from appearing deceptive. I have also omitted segments which are repetitive or otherwise unnecessary, sometimes because they appear elsewhere in this volume. For the benefit of readers who suspect conspiracy, deletions are indicated by ellipses and cross-references; expansions or updating are specified in footnotes and sometimes bracketed in the text. It is always clear when and where they occur. References to all previously published papers are of course specified.

I had originally intended, in the best postmodern tradition, to rewrite history by obliterating the sexist language of my earlier writings. My mind was changed when I saw a television interview with a Mark Twain scholar. She argued convincingly that the language Twain put into the mouth of Huck Finn, including the word "nigger," was not only an accurate reflection of the way in which his characters would have spoken at the time but also a reflection of the racist culture in which they were enmeshed. Twain exposed that injustice with the very language that perpetuated it. Up until the mid-1970s, sexism was endemic in the male world of scholarship and embedded in the language which it employed. To change my thoughtless language of the time to something currently acceptable, would be like having Huck refer to his friend as "African-American Jim." So, the painful and offensive sexist language is not removed.

Selection and Organization

I have chosen to reprint (or publish) those essays which seem to me to bear on the practice of sociology and, in my opinion, the practice of social science generally. Basic theoretical and methodological pieces are omitted as are most empirical research reports and observations on the discipline. In more conventional language, I might have referred to part 1 as "Theory" (and offended most theorists) and part 2 as "Methodology" (and offended most methodologists). The titles I employ seem to me to more accurately reflect the content. All of the chapters in part 3 deal with evaluation research and all of them raise questions about some things evaluators seemed at the time to take for granted. Perhaps that has changed over the years. I hope so.

Part 4 contains chapters on nine of the substantive areas which captured my interest between 1953 and 1993. Some of the others are touched upon elsewhere in this book. As a precocious graduate student I published an essay deploring the increasing specialization in sociology (Deutscher 1958). I have not altered this view. It is not only that I rapidly became bored with reviewing the literature in one substantive area, but more importantly that it seems to me the best way to understand a process is to distance oneself from it by studying a similar process as it occurs under other conditions. When I recently asked a staff member of the American Sociological Association why such obscure scholars were nominated for high office that year, he responded that these people only appeared "obscure" to me because of the increased specialization in the field. I was told that no sociologist can any longer be well known across the board. That is, of course, hogwash. There are any number of scholars whose contributions are of sufficient importance to be noted by nearly all members of the discipline, as witnessed by the illustrious list of presidents up to this time (1997).

As for part 5, there are those who consider polemics wicked. I do not. There is no issue on which I am informed and about which I am concerned, that I can pretend to display neutrality. To me, that would be wicked. This organizational procedure obliterates chronology. Even within the parts of this volume, I have paid no attention to the order in which the essays were originally written. Those dates are duly noted and the concerned reader may take account of them.

Consistent Themes

I planned to say something about central themes which pervade these chapters, but had to wait until I had reread them in order to discover what, if any, there might be. Although it has always seemed to me that there were some things wrong and some things right with social science as I found it, for most of my career, I deluded myself into believing that I would use whatever methods, tools, logic, or procedures seemed most appropriate to the problem. Delusion it was, since eventually it was pointed out to me by both students and colleagues that there was a certain consistency to my work. As it turns out I discovered a few themes in these chapters, although Emerson to whom the aphorism, "A foolish consistency is the hobgoblin of little minds," is attributed, would be proud of me.

Chapter 1 sets a tone which appears repeatedly throughout the book. These chapters are all problem-oriented or action-oriented or policy-oriented. This is true of the earliest essays on petty offenders (chapter 20) as well as recent ones on Indian tribal peoples (chapter 24) and the evaluation of Project Head Start (chapter 15). It is also true of everything in between. I use the word "useful" a great deal in referring to sociological work. I also display an unfashionable disciplinary chauvinism. Sometimes it is attributed to Durkheim and his "social facts" while at other times I cite Mill's distinction between "private troubles" and "public issues." In pressing for the importance of a sociological perspective in dealing with any social issue, I do not intend to malign other disciplines: multidisciplinary work is effective only insofar as each disciplinary perspective maintains its peculiar integrity.

I was surprised to find that for a long time and to a great extent, I have been taken with experimental logic as a methodology, that is, a logic of procedure. This has nothing to do with technique and has little to do with that static procedure of vapid hypothesis- testing some "scientists" refer to as "experiments." My prime example of experimental logic in demonstrating social causation is Max Weber's *The Protestant Ethic and the Spirit of Capitalism (1948).* True experimental thinking forces the researcher to consider changes through time (before and after) and to simultaneously make comparisons between groups (experimental and comparison). Weber does this with historical data. It can be done with any kind of data. It can also be done on less grand a scale. My concern for methodology—the logic of procedure—appears repeatedly in the form of little logics as in my effort to rationalize the use of loaded questions as a device to discover stereotypes (chapter 5) or the heavy dependence on "secondary data" in order to pursue an analysis involving crossnational comparisons (chapter 8).

There may well be other more or less consistent themes in this book and I suspect that readers—both approving and disapproving—will discover them. I searched for them in part because I was curious and in part to provide the reader with some clues to the nature of the book. Certainly, the most pervasive theme seems to me to be my insistence that reality is created by those who define it and even those who do not agree with them must live with the real consequences of the definition (I have a vague recollection of someone else having defined the social situation in this manner). This theme is everywhere in this volume. It is visible for example in my early interest in the consequences of public images of nurses (chapters 5 and 7). It is explicit in the definition of delinquency (chapter 22) and in the explanation of who does and does not get into public housing (chapter 16). It appears in the self-righteous self-definition of the deserving rich (chapter 17). It appears more subtly in the discussion of "success and failure" (chapter 10). It is in fact ever-present in this collection.

What about Theory?

The semantics of the word are impressive. It reeks of importance and it is done by very important people. I find myself insisting on its pertinence in many of these chapters. The fact is that I remain unclear about what the term refers to. The earliest essays tend to reflect the kind of theory testing that is thrust upon graduate students. My master's thesis leaned heavily on the ideas of Robert K. Merton and Edwin A. Sutherland and even on the emerging work of Talcott Parsons.[1] As an infant professional, I felt compelled to couch my civil-rights activism in the now archaic framework of small groups theory (chapter 21). When I came up with the title "Fun and Profit in a Disaster," my current mentor, Arnold Rose thought it too frivolous. I returned with "A Functional Analysis of a Disaster." Rose was even more disdainful of a title which dignified what he insisted was a conservative deterministic throw-back to social Darwinism. But the editors of *Social Forces* liked it and it stayed (chapter 19). After all, functional theory was really "in" in those days. My current dislike for some of these stuffy titles is reflected in my revival of the more descriptive ones for chapter titles in this volume.

It was not until I was well into my research on the relationship between attitude and behavior, that I discovered the need to grasp the world from the perspective of ordinary people in order to understand that world. This is hardly an original perspective and it was surely influenced by Robert Habenstein's lectures at Missouri and later by the conversations during my weekly lunches with Herbert Blumer for the year I spent at Berkeley. Nor was my admiration for the work of friends like Howard Becker and Erving Goffman irrelevant. That symbolic interactionist or phenomenological viewpoint crystallized for me at about the time Harold Garfinkel began publishing his studies in Ethnomethodology. But, more than anything else, it was again the simple "definition of the situation" attributed to W.I. Thomas which initially shaped my notions of validity and reality: When people define a situation as real, it is real to them and it has very real consequences. The objective features of that situation may or may not have any relevance to human behavior. I refer to this perspective as a "situational sociology." I do not know whether this is a theory or not.

This is where I, along with some other social scientists, part with our colleagues who hold to a "scientific" image of the study of human behavior and social processes—"scientific" in the sense of how they imagine the physical sciences to operate. It was, however, my Dutch students at the University of Amsterdam who forced me to rethink this microsociology or social psychology. They correctly reminded me that situations are defined in a sociocultural and historical context and that different contexts may well produce different sets of definitions from which people choose. Eventually it was necessary to take account of social organization and social structure in their contextual roles in human behavior. My Dutch guide, graduate assistant, translator, and cultural indoctrinator—Cas Wouters—was instrumental in helping me understand these things.

Consistent with this theoretical perspective (if it is that) is my persistent nagging about validity as distinguished from reliability. If we are to be helpful in the efforts of practical people to deal programmatically with practical problems, then we must provide them with data which reflect their concerns. We are not free to define phenomena in terms of whatever convenient measure we may find. To the extent that we do this, we measure what interests us and not what concerns the external social world. That might be all right if it were not for the fact that we can mislead policymakers and social planners into believing that we are in fact studying their concerns. It is unacceptable to me when a social scientist explains the inappropriateness of his or her measure by claiming that "it is the best we have." What good is the "best" if it is irrelevant and thus misleading? These same social scientists are often satisfied if they can demonstrate the reliability of their measure, that is, its consistency through time and space. Is it really profitable to be consistently wrong? Frankly, it surprised me to discover that I was very much concerned with validity in some of my earliest publications (see, for example, chapter 5).

All in all, I suspect that the term "perspective" better expresses what sociology has to contribute to the world, than does the term "theory." Sociologists look at things differently from people committed to other disciplines. To me, the most important of these defining differences is captured in the word "skepticism." The sociologist questions what everyone else, scientist or otherwise, takes for granted. This is what part 3 of this volume is all about. In addition to those seven chapters, related discussions appear elsewhere about the legitimacy of social indicators (chapter 3), objectivity (chapter 5), synthesizing research (chapter 6), and newspaper reports as data (chapter 8).

Applied Research as Distinct from What?

The distinction between emphasis on reliability and emphasis on validity is a major key to the difference between what may be acceptable in academic research in contrast to applied research. The professor may fiddle with whatever data in whatever manner pleases her, with the ultimate consequence of an article in an obscure journal read by a handful of colleagues. This does little harm to the world at large. But contrast it to the evaluation researcher who, for example, uses educational achievement tests to measure the effects of Project Head Start. Since such "cognitive" dimensions have but a minor role in what Head Start programs attempt to accomplish, the results of such research are misleading and can have disastrous consequences for millions of children and their families. Chapter 15 deals exclusively with this particular example.

For much of my career I never thought of myself as an applied sociologist and in fact thought the distinction between applied and academic (pure?) research to be spurious. It seemed to me little more than a distinction devised by some academics to denigrate social scientists who worked outside of the hallowed halls and walls. With the exception of my first three years on the job in Kansas City, I have always had a university base and obtained tenure as a professor at all three of the universities where I worked full time. I did not pay serious attention to the notion of applied sociology nor take serious note of people who called themselves applied sociologists until the decade of the 1980s. This is then an important theme which emerges only late in this book, most clearly in the chapters in part 5. It will be seen that the convergence of certain labor market, economic, academic, and other factors brought this matter into sharp focus in the 1980s and it brought me into the fray on the side of those I thought to be put upon. This was not new to me. As a Young Turk in the early 1950s I devoted considerable energy to the fledgling Society for the Study of Social Problems which was designed as a haven for sociologists who were concerned with social problems to a greater extent than they were with so- called basic research and methodological and theoretical issues. By the 1980s the circle had completed itself. There is then, a modicum of consistency in this body of work.

Notes

1. I deliberately selected for chapter 20, an essay, which spells out actions which can be taken to deal with the problem of town drunks or "Petty Offenders." A more theoretical analysis derived from that same research appears in Deutscher 1954.

References

Coser, Lewis A. *Everett C. Hughes On Work, Race, and the Sociological Imagination* (Chicago: The University of Chicago Press, 1994).

Deutscher, Irwin. "Public Drunkenness and the Social Structure," *Midwest Sociologist* (Spring, 1954):16–21.

———. "Specialization in Sociology: A Logical Inquiry," *Midwest Sociologist* (December, 1958):34–38.

Hughes, Everett C. "Social Change and Status Protest: An Essay on the Marginal Man," *Phylon* 10 (first quarter 1949): 58–65.

Portes, Alejandro. "Portrait of the Scholar: Notes on Irwin Deutscher's *What We Say/What We Do*," *The Sociological Quarterly* 15 (Summer 1974): 457–461.

Weber, Max. *The Protestant Ethic and the Spirit of Capitalism* (London: George Allen and Unwin, 1948).

Webster, Noah. *Webster's New Twentieth Century Dictionary of the English Language*. Unabridged (New York: William Collins Publishers, 2d ed., 1980).

Part I

Toward a Useful Sociology

1

For Beginners: The Social Causes of Social Problems—From Suicide to Delinquency

In thinking about social problems and their causes it is easy to forget that social problems are indeed social—that, although they reflect themselves through the behavior of individuals, the sources, the origins, the causes lie outside of individuals. One may be able to cope with problematic behavior in an individual by treating that individual, but the problem will remain to reflect itself in hundreds of thousands of other individuals until the external pressures in the society which are creating the problem can be identified and modified.

Social problems will ultimately be solved when we learn where to intervene in the ongoing processes of the society. They will not be solved by treating the individual whose behavior is merely symptomatic of difficulties which transcend him and which are outside of him. This is what I mean when I say that social problems are social.

When we forget this, and think of social problems as personal or individual, we become enmeshed in the fallacious line of reasoning called reductionism. Thus, if we reduce the social problem (crime, delinquency, divorce, or what have you) to a personal problem and observe the individual as our unit of study rather than the society, why not continue? Why not observe only the brain rather than the whole complex individual? Or why not study just a brain cell? Or, better still, let us isolate a molecule of matter from that cell, put it under an electron microscope, separate out an atom, and then carefully observe

---

From Ephraim H. Mizruchi, ed. *The Substance of Sociology*, 2d ed. New York: Appleton-century-Crofts, 1973, pp. 293–304.

the nature of the atomic particles in order to really get at the ultimate sources of crime, delinquency, divorce, or what have you? This is reductionism carried to its extreme and its absurdity is apparent. Although less apparent, this same absurdity is present at any point in the process of reduction, including the first step when one reduces the social problem to a personal problem.

All of this is not to deny such things as idiosyncratic individual or personality problems. I am simply stressing the point that social problems are social and individual problems are individual and that the distinction should be kept clear. It is no more reasonable to expect to eliminate social problems by treating individuals than it is to expect to eliminate some kinds of personality problems by treating the society. Historically, man has always attempted to place blame or responsibility on the individual. At one time we thought he was taken with evil spirits or the devil, and that explained why he behaved so badly. Later we decided he had bad genes, and that explained why he behaved so badly. More recently we have decided that he is "sick," and that explains why he behaves so badly. I can see no basic difference in defining, say, a delinquent boy, in terms of the devil, genes, or mental illness. The shift in words tends only to make us more condescending, more tolerant, more "understanding" of the deviant behavior. Whatever you call it, it remains reductionism.

This perspective is hardly original with me. I am standing on the shoulders of giants who stood on the shoulders of other giants before them. The giant who most clearly formulated this position, both in terms of its logic and in terms of its empirical demonstration, was a turn-of-the-century Frenchman named Émile Durkheim. As a concrete example of how a social problem can be understood in terms of social phenomena, let us look at a classic study conducted by Durkheim.[1] The problem he chose to explore was suicide—on the surface one of the most individualistic, personal, nonsocial kinds of acts a person can commit.

In one of the first attempts to work with mass statistics, Durkheim was able to demonstrate convincingly that suicide rates were very stable. In places and among groups where the rates were high they tended to remain high over a considerable period of time, while in places and among groups where the rates were low they tended to remain low over a considerable period of time. In addition, he was able to show that when these rates were related to external events, changes were consistent no matter where they occurred. For example, under conditions of sudden economic or political upheavals suicide rates declined. Durkheim actually demonstrated that suicide rates were more stable than mortality rates. Thus, one could predict the expected number of suicides in any segment of the population with greater accuracy than one could predict the number of deaths.

Durkheim did not stop with a simple description of variation in suicide rates. The uniformity and regularity he found in those rates suggested to him that suicide was not a whimsical individual matter, but rather was a reflection of the kind of society in which people lived. He went on to demonstrate that wherever people were collectively enmeshed in something bigger than themselves, wherever they were well incorporated into one or more major institutions of the society, wherever they felt themselves bound by strong external controls, wherever they lived under the shadow of what Durkheim called a "collective conscience"—under these conditions people were much less likely to take their own lives. And where these conditions did not exist, the suicide rates were higher. For example, with the breakdown of family solidarity by widowhood or divorce, suicide rates went up: for married men in Prussia the rate was four per 10,000, for widowed men it was fifteen, and for divorced men it jumped to nineteen. For women it went from one to two to three. And the figures were remarkably similar in other European countries.

But the family is not the only major social institution. Looking at religion, he showed that where religious ties were strong and collective, as with Catholics and Jews, the rates of suicide were considerably lower than where religious ties were either weak or individualistic, as among Protestants. The same held when community ties were considered; in the French provincial rural villages the rates were not nearly as high as they were in the burgeoning prefectures of Paris. Durkheim's general conclusion, based on both his data and a logical critique of other explanations, was that suicide was a social fact and that it could be explained in terms of other social facts. He concluded that when people felt that they were an integral part of a larger collectivity and that collectivity maintained a degree of reasonable order in their world, then there would be significantly less alienation from the society. And what could be a more perfect index of alienation than the rate at which people deliberately took their own lives? When people cease to believe that there are any rules worth abiding by because it doesn't do any good when you do abide by them, then suicide can be expected to increase.

There are some holes in all of this. Some of them were recognized by Durkheim and handled by him. Others are the result of more extensive knowledge that has accumulated in the half-century since *Suicide* was written. One of the big problems which Durkheim recognized was that the word "suicide" was a popular, or at best, a legalistic term which tended to be either loose or arbitrary. He tried to clarify the concept by defining it as the voluntary taking of one's own life, but realized that even this included some very different kinds of behavior which resulted from very different motives. As a result of his historical research he classified suicide into three distinct types, to which he gave the names "anomic," "altruistic," and "egoistic."

Briefly, the anomic suicide is most prevalent where people find themselves frustrated by the kind of society in which they live. It occurs when people perceive disharmony between the desired goals of life and the right or proper means of achieving those goals. The potential anomic suicide has the desire to get somewhere but no matter how hard he tries he cannot make it. This is indeed a nightmare situation. The egoistic suicide occurs when people, rather than seeing anything wrong with the society, see themselves as outside of it. They do not want to get anywhere. The egoistic suicide has nothing to live for; he is completely malintegrated in what may be a well-integrated society. The altruistic suicide is the polar opposite of the egoistic. It occurs when people are so well integrated that the demands and loyalties of the society are more meaningful than life itself; for example, the man who dies for his family, his church, or his country. He may deliberately and knowingly give up his own life, but his "suicide" is indeed in a different category from the others.

I mention all this not because I think we should be more familiar with esoteric sociological treatises, but because Durkheim has provided us with a model that gives a new perspective to the understanding of social problems in general. Let me illustrate by attempting to apply the model to a second case: juvenile delinquency.

Durkheim addressed himself to three basic questions in his efforts to understand suicide: (1) What is the logical status of popular causal explanations of the phenomenon? (Although I have not discussed this, he analyzed the weaknesses of arguments explaining suicide in terms of psychopathy, race, heredity, imitation, etc.) ; (2) Can the phenomenon be described as a social fact? (If it can, we must avoid the reductionist fallacy and pursue understanding in terms of other social facts) ; (3) Is the phenomenon actually unitary or does the concept need to be broken down into distinct classes which require different explanations?

What is the logical status of popular causal explanations of delinquency? Pick up your daily newspaper, talk to a neighbor, read a magazine, or ask your best friend and you will discover the cause of delinquency. Depending on whom you ask or where you look, it may be television, "bad" schools, inability to read properly, poverty, lack of recreation facilities, comic books, alcohol, divorce, working mothers, mental illness, mental retardation, school dropouts, habits of dress, or even physical stature—to mention only a few. Although it is probably true that at some time, in some place, each of these factors has been a major contributing element in the delinquency of some child, it is also true that no one of these factors is associated with most of the behavior which we label "delinquent."

The late Edwin H. Sutherland,[2] noted criminologist, liked to tell the story of the two slum youths who were being chased by a policeman after committing an act of vandalism. One was a tall fellow with long legs who ran fast, leaped over a high fence, and escaped. The other was a short fellow whose stubby little legs just wouldn't carry him fast enough or far enough. He was caught by the policeman and sent to the reformatory. For our short-legged fellow, this was the beginning of a long career in crime, while the long-legged fellow continued in school and eventually became a priest. Moral of the story: short legs cause crime!

The story, although absurd, points up the weakness of the causal logic associated with all arguments relating specific factors to delinquency, whether the factor is physical stature, television, or whatever. Television provides a good general example. Certainly there were delinquents before TV was invented; most children who watch television do not become delinquents; there is no evidence that most delinquents are addicted to television.

As a matter of fact, while working as a volunteer probation officer I encountered a case where TV might have had the opposite effect. Orville, my twelve-year-old charge, had been apprehended for breaking into the homes of five neighbors. Counting the fountain pen he ruined, the footstool he damaged while standing on it in search of treasure in high places, and the thirty-five cents he discovered in Mrs. Polanski's sugar bowl, Orville did damage and stole property amounting to $8.35. But the point of this story has to do with the technique of entry rather than the amount of loot. In his confession, Orville stated that he entered the first home by using the key to the back door. The key was obtained by inserting a newspaper under the door and pushing the key (which had been left in the keyhole on the inside) onto the paper. The paper was then removed along with the key.

When I asked Orville where he had acquired this clever technique, he replied that he had seen it on TV. Evidence that television is a cause" of delinquency? Probably not, because what really got Orville into trouble wasn't the first door; it was the other four. Since the householders in these cases did not leave keys inside their back doors, Orville had no alternative but to take an axe to those doors. In assessing restitution against Orville's father, the court ordered that the following amounts be paid (Orville's father earns fifty-five dollars a week on the shipping dock of a downtown department store and supports a wife and two other children in addition to Orville):

DAMAGE INSIDE OF PREMISES	$ 8.00
CASH STOLEN	.35
REPLACEMENT OF FOUR DOORS @ *$78.50*	*314.00*
TOTAL	$322.35

Orville was to remain on probation until this amount had been fully paid at the rate of ten dollars per week—approximately eight months. The technique he had picked up from television saved him nearly eight weeks probation and considerably reduced the risk of failure to meet restitution payments and the institutionalization which would probably result from such failure. Had all of the neighbors been so thoughtful as to leave keys inside of their back doors, Orville might well have gotten off with a simple reprimand after his father paid the $8.35. I could go on and remind you of the thousands of boys who saw the same TV program and did not attempt to demonstrate the technique to their own satisfaction. I might even suggest that the hundreds of boys who get caught breaking into homes each year would be better off had they seen this program.

Again, although true, the story is told with tongue in cheek. The serious conclusion is that it is difficult to defend the position that television causes delinquency. The same is true of comic books, which I want to mention briefly before moving on to other things. Although the same arguments hold in reference to comic books that hold for all of the other attempts to explain delinquency in terms of simple factor causes, there is an additional argument that can be made in this case; that is, that things are no different than they have ever been in this respect.

I once attempted to introduce my four-year-old to classic children's literature. I thought *Cinderella* would be a nice starting place and obtained the original version of Grimm's. I began worrying right from the start, and by the time we got to the part where one of the wicked sisters was whittling her heel off with a kitchen knife in order to make the slipper fit, I closed the book on the pool of blood and eventually managed to calm my horrified daughter. *Gulliver's Travels* didn't work out much better; fortunately, I read a little ahead and managed to suggest something else before we got to the part where Gulliver tries to drown the cute little Lilliputians in his urine. *Jack and the Beanstalk* worked the final cure. This story of a deceitful boy who spends the family's last penny for a colored bean, who betrays the kindly and generous Mrs. Giant by stealing everything of value in her home, and tops it off by murdering her husband when he attempts to retrieve what is rightfully his—this story convinced me that my daughter was better off with Disneyesque comic books, and certainly no worse off with other types where at least right is clearly delineated from wrong and justice always prevails.

Enough said concerning the logic of currently popular causative arguments. Turning to Durkheim's second question—is there evidence that delinquency is a social fact which shows some regularity in the rate of its appearance, as well as exhibiting consistent rate differences between various collectivities in the society? Or is it an idiosyncratic phenomenon, whose range of appearance varies as widely as the range of personality structure? I could tell you of the hundreds of studies which show that urban rates are higher than rural ones, and incidentally relate to you the problems of my colleague, Professor Jerome Himmelhoch, who undertook a delinquency study in the state of Vermont and after three years is still frantically trying to find a delinquent. Or, I could describe the classic study of delinquency areas conducted twenty years ago in Chicago by Clifford Shaw and Henry McKay.[3] This study showed that delinquency rates were always high in one section of the city regardless of which ethnic group happened to be migrating through it at the time. Poles, Italians, Irish, Negroes—it made no difference what the nativity of the population was; that area always had high delinquency rates.

But rather than describe ancient studies conducted in far-away places, let's take a look at Syracuse, New York, right now. Professor Robert Hardt of the Youth Development Center staff has been gathering and analyzing data on delinquency for Syracuse and Onondaga County since 1957. He has published three *Delinquency Profiles*; one reporting the distribution of apprehensions for 1957–1958, another for 1959–1960, and the third for 1961.[4] These data reveal that for the county as a whole the rates have been stable over the five-year period, varying between 18 and 22 apprehensions per thousand children. When we separate the city from the rest of the county we also find stability, but of a different order. Apprehension rates in the city vary, not between 18 and 22, but between 28 and 34 apprehensions per thousand children. And what of the rest of the county? Here too we find stability, not in the range of 28–34, but with only 10 to 13 children out of every thousand in the county apprehended each year. Actually, the difference between city and county appears to be even more constant than the relatively stable year-to-year picture: apprehensions in the city remain very close to two-and-a-half times greater than those in the county every year.

There are differences by sex, with boys being apprehended four to five times more frequently than girls every year. There are differences by age, with the rates for fourteen and fifteen year-olds being eight times greater than those for seven to nine year-olds. Putting these two facts together, we find that for each seven- to nine-year-old girl apprehended, 73 fourteen- to fifteen-year-old boys are apprehended. There are also differences within the city—very marked and very stable differences. Comparing economic areas of the city, we find that no matter what the year, the rates consistently go up as the average income of the area goes down. In the highest income area the rates of apprehension are from 10 to 14 per thousand, while in the lowest they are from 74 to 79 per thousand. For boys only in the lowest income area the rates range between 113 and 123 over the years, and for fourteen- to fifteen-year-old boys in that area they are a phenomenal 243 apprehensions per thousand. Contrast this with girls in the high income area, where apprehension rates vary only between one and four per thousand.

Since practically all of Syracuse's non-white residents are concentrated in the lowest- income ghetto, the only comparisons which can be made by race are within this low-income area. In the urban slum—roughly the Fifteenth Ward—where the rates are high for everyone, there is no difference between Negro and white boys. Today 75 percent of the children in that area are Negro. Yet a study conducted twenty years ago, when only 13 percent of the children were Negro, shows that this area had the highest delinquency rates in the city at that time, too. Apparently what Shaw and McKay discovered in Chicago is also true of Syracuse, and there is little doubt but that it is true of most cities. There are areas of the city which consistently reflect high rates of delinquency as well as a wide range of other sociopathic behaviors.

It is, of course, clear that migrant Negro families do not move out of these ghettos in a generation or two, as did the Germans, Poles, Italians, Greeks, Irish, and others. To the extent that a Negro is compelled by external constraints to remain in such an area generation after generation, the Negro will manifest all of the symptoms created by the pressures of this kind of living and will manifest those symptoms generation after generation: crime, delinquency, vice, gambling, high rates of infant mortality, illegitimacy, desertion, dependency—the whole range. When a community refuses to allow this particular group the same opportunities to move up and out which other groups had before it, then the community should be aware of the great price to be paid for such a luxury—both in the economic and in the social sense. It is not my purpose to preach about race relations; I just want to remind you that, like the man says, "you pays your money and you takes your choice." What can be done about this situation is a problem to which we will return shortly.

Studies such as those done in Chicago and Syracuse indicate that there are forces external to the individual which act consistently and uniformly in a manner which is either encouraging or permissive of delinquency. Delinquency is indeed in large part a social fact. I must emphasize the "in large part" because, low though they may be, there are delinquency rates in all parts of the city and county, in both sexes, in all age groups, and in all economic areas. Where rates are low, as where rates are high, we have a social fact which must be explained by other social facts. But other levels of explanation may be required to understand why we find some delinquent youngsters in low-rate groups and some nondelinquent youngsters in high-rate groups. Such aberrations do not appear explainable by social facts.

The Durkheimian model would lead us to seek explanation in the various agencies of social control, those constraining institutions in the society which imbue young people with the norms and values of the large community: the family, religion, the neighborhood, the school. The Durkheimian model would lead us to suspect that these agencies are either ineffective in their socializing functions (that is, in a state of breakdown) or are effectively operating counter to the norms of those in the larger society. Thus, on the basis of his Boston studies, Walter Miller [5] argues that the lower class represents a cultural solidarity in itself and what is defined by agents of the middle class as "delinquency" is not viewed as antisocial behavior by the lower class family and neighborhood. Richard Cloward and Lloyd Ohlin[6] consciously adopt the Durkheimian framework in their analysis of delinquent gangs in large cities. They suggest that such gangs represent various types of adaptations to what Durkheim called a state of *anomie* in the society—a condition under which the normative structure has deteriorated to such an extent that people see little hope of achieving success by legitimate means. Cloward and Ohlin recommend that the apparently blocked opportunity systems be opened for these youngsters so that they can see themselves as having the same chances for success as do middle-class youth. Of special importance are the opportunities provided for meaningful education and for work.

Turning to Durkheim's third question—is the phenomenon just one thing or does the concept need to be broken down into distinct classes which require different explanations? Even a casual observer can recognize the lack of precision in the notion of "juvenile delinquency." In a sense it refers to any kind of behavior on the part of a young person which older people strongly disapprove. If we try to pin delinquency down to legalistic terms, the situation remains messy. Not only does the legal definition of a young person vary from one jurisdiction to another and from one time to another, but the kinds of acts included in the definition present a spectrum of misdeeds, many of which bear no relationship to each other. For example, the sharp differences in rates between sexes noted above may be explained in part by the fact that for girls, the external controls operate with greater force and provide fewer opportunities for delinquency. But it is also true that what is called delinquency among girls is generally not the same phenomenon as what is called delinquency among boys. In Syracuse the great majority of apprehended girls are defined as delinquent because of behavior, which is thought to endanger their own morals. The great majority of boys, on the other hand, are apprehended because of threat or damage to other people or their property.

As another example of the inadequacy of legal definitions, let me point out that New York State will have a large decline in the number of adjudicated delinquents beginning 1 September 1962. This decline has nothing to do with the changing behavior of young people, only with the changing behavior of legislators. The revised Family Court legislation defines a delinquent as a juvenile who commits an act which would be a crime if committed by an adult. The older law included many other things in its definition: habitual truancy, ungovernable behavior, willfully deporting oneself in such a manner as to endanger the health or morals of oneself or others, and so forth. With such items as these excluded from the court's delinquency jurisdiction, not only will rates go down all over the state but adjudicated girl delinquents will practically disappear. (Under the law, youngsters involved in these behaviors are defined as juveniles in need of supervision.) I suppose it can be argued that this is one way to solve the delinquency problem.

We must be continually aware that "delinquency" is really more than one kind of thing and therefore will require more than one kind of explanation. Cloward and Ohlin have applied Durkheim's theory with some success to an explanation of juvenile gang behavior in large cities. But it is unlikely that such an explanation is appropriate when we try to understand why nice Johnny Jones from that respectable Jones farm family shot his father's head off with a shotgun. This is a different phenomenon in spite of the deceptive sameness of the label. Even the same overt acts are not necessarily a result of the same causes. Some boys steal cars for joy rides and then abandon them; others steal cars, strip them of everything removable, make contact with a fence, and sell their loot. These are not the same kinds of behavior.

I believe that the perspective on social problems that I have been discussing enables us to take a more level-headed look at social problems. I also believe that as this perspective is more systematically applied to contemporary problems, with the sophistication of contemporary techniques, we will begin to learn considerably more about the causes of such problems. There have been recent efforts: Ohlin and Cloward in the field of delinquency and Charles C. Hughes[7] and his associates in the field of mental health provide two examples. Perhaps the most promising aspect of this perspective is that it strikes at the roots of the problem rather than at the symptoms which manifest themselves in the behavior of one individual or another. If we can learn how to alter the agencies of social control, to reinforce the collective conscience, to intervene in the organization of the society, then we will have cut off the mainsprings of the problem and automatically relieved the pressure on many little dams we must currently build in the form of individual treatment and therapy.

Most people would agree that if we could find a way to do away with delinquency, this would be better than finding a way to cure delinquents. Further, most would agree with the assertion that our current therapeutic methods are designed to deal with people who are psychologically disturbed in one way or another. However, many, if not most, so-called delinquents do not fall in this category. When the problems are not internal—that is, not personality problems—but are the result of external pressures—that is, social problems—then personality treatment is inappropriate. In addition, there is no conclusive evidence regarding the extent to which and the conditions under which current therapeutic methods are effective. Finally, even assuming for the moment that individual treatment is effective, it must be a losing battle since trained psychotherapists, psychiatrists, and caseworkers cannot be produced at nearly as rapid a rate as can delinquents. This problem is further confounded by the slow process of such treatment—it may take many months or even years to complete a therapeutic program with one delinquent boy. At best, treatment and correction programs provide only a holding action. Those agencies which are fighting this discouraging holding action should be encouraged and supported, but we know that the war is not to be won on that battleground. It is for such reasons as these that I prefer to think in terms of prevention.

When we state that we are concerned with prevention, we have made a policy choice and, I think, a wise one. The alternative is to be primarily concerned with programs of treatment, therapy, correction, cure, or whatever you want to call it. These represent two distinct and different kinds of problems. Prevention means getting at the roots of the problem, intervening in the processes that lead to it, providing young people with the stimulus, encouragement, and opportunity for healthy productive adolescent careers *before* delinquent careers crystallize. Prevention means keeping people from becoming delinquent; treatment means working with delinquents in an effort to redirect their life careers.

If we are going to "prevent," where do we begin? It seems that a prevention target area and target population will emerge from a careful study of delinquency rates in almost any city. But having identified such target areas, what does one do with them or to them? If you will agree that the delinquency problem in such areas—as well as many other perceived problems—is a social problem, that it is a consequence of external pressures in the areas themselves rather than of internal pressures which arise within individual young people, then you will also agree that prevention means somehow altering the nature of the external pressures in those areas rather than altering the personalities of individuals. Unlike the skid row to which homeless and frequently alcoholic men tend to gravitate, the high delinquency area is one that creates the problems which characterize it.

We know, then, what part of the city it is necessary to focus our efforts upon and we know that those efforts must be directed at altering the community rather than altering individual personalities. But the question remains, what does one do to such a community to alter its complexion—to change its impact on the behavior of the people who live in it? I suggested earlier that the Durkheimian model leads us to look at the sources of the collective conscience—at those agencies in the community which exercise constraints upon their members. Behavioral constraints are conveyed to the individual and enforced by a limited number of entities in the community which we call social institutions. Among these are the family, the neighborhood, the church, the school, the political establishment, and the economic order. These, in effect, make a community what it is. Each is responsible for conveying to young people certain standards of behavior, and further, for conveying the consequences of not living up to those standards.

It follows, then, that efforts must be directed at altering these social institutions, not toward altering the physical quality of the area. Altering social institutions is seldom a simple matter. Because of its difficulty and complexity, I would suggest that if it were to be attempted attention be directed toward those institutions over which there is some collective control by the larger community: the school and the neighborhood. Through public schools, public housing, urban relocation, and slum clearance we find an opening wedge—a point at which we can exert some leverage. But to build a new school building is no more the answer than to build a new public housing project. These are things of brick and mortar, not social institutions. In effect, we have used such brick-and-mortar construction to ease our middle-class consciences while tightening the noose around our ghettos. The slum school is a demoralized, custodial institution wherever it is found and regardless of how pretty it may appear. Public housing does indeed provide people with the opportunity to move out of crowded, rat-infested, unsanitary housing into clean, new, and relatively roomy living facilities, but the social world is the same—a world of gin mills, numbers rackets, whore houses, and economic exploitation from outside as well as from within. It remains the social world of the slum and the rates of sociopathic behavior do not change.

In wrestling with the problems of such areas, I would like to think that there is some way of helping to mobilize the apathetic, frustrated, dependent, defeated people who live in them to organize themselves—to stand up on their own collective legs and demand to be heard. In this way, regardless of what the issue is, they might establish a degree of hope, of collective strength, of collective self-respect. We have several members on the staff of the Youth Development Center who think this is possible and are willing to make the effort. But it may be that the task of reorganizing such areas is too gigantic. It may be that the best public policy is one which permits urban renewal to move in the direction of converting these demoralized areas into uses other than residential. The alternative is to destroy them. It may be that the most effective public policy is one which enables the people of the slum to be assimilated into the larger city, as other generations were before them—before the doors were closed.

Why, for example, should we continue to build low-income public housing in such areas when we know what it does to people to live in such areas? Why, for that matter, should we build big, expensive public housing projects anywhere when it would probably be much cheaper and certainly more effective to move potential tenants into scattered houses, purchased on the open market wherever they might appear in the city? In effect, why not absorb the eight or nine percent of the population who live in high-delinquency areas into other areas of the city where the collective conscience is supported by existing strong institutions and where rates of delinquency as well as other sociopathic behavior are not high? What would happen? There is evidence that the behavior of these people would begin to approximate that of their neighbors. Alan Wilson[7] and Robert Hardt[8] have both reported data indicating that children from low-income families who attend schools in middle-income areas tend to approximate middle-income children in school performance and aspirations for college education. In Rochester a limited experiment in scattered-site housing shows a drastic decline in police calls, marital conflict, and drunkenness on the part of problem slum families who were placed in the scattered houses. As you can see, I have shifted ground from an emphasis on strengthening indigenous institutions to a suggestion that the slum be allowed to die—to be voided of its people. At the same time opportunities must be opened up to permit those people to be incorporated into the larger social world of the American city where they can benefit from the same collective conscience, which supports, constrains, and motivates the rest of us.

Notes

1. Émile Durkheim, *Suicide: A Study in Sociology*, edited by George Simpson (Glencoe, IL: The Free Press, 1951).
2. Edwin H. Sutherland, *Principles of Criminology*, edited by Donald R. Cressey (5th rev. ed), (Philadelphia, PA: J.B. Lippincott Company, 1955).
3. Clifford R. Shaw and Henry D. McKay, *Juvenile Delinquency and Urban Areas* (Chicago: University of Chicago Press, 1942).
4. Robert H. Hardt, *A Delinquency Profile of Syracuse and Onondaga County, New York, 1957–1958*; vol. 2, 1959–1960; vol. 3, 1961 (Syracuse, NY: Syracuse University Youth Development Center).
5. Walter B. Miller, "Lower Class Culture as a Generating Milieu of Gang Delinquency," *Journal of Soclal Issues*, vol. 14, no. 3, 1959, pp. 5–19.
6. Richard A. Cloward and Lloyd E. Ohlin, *Delinquency and Opportunity: A Theory of Delinquent Gangs* (Glencoe, IL: The Free Press, 1961).
7. Charles C. Hughes et al., *People of Cove and Woodlot* (New York, Basic Books, 1960).
8. Alan Wilson, "Residential Segregation of Social Classes and Aspirations of High School Boys," *American Sociological Review* 24 (Dec. 1959): 936–45.
9. Robert H. Hardt, "The Impact of School Milieu on Pupils' Educational Plans." Paper read at the annual meeting of the Eastern Sociological Association, New York, 1961.

2

A Short and Selective History of Evaluation Research in the United States

with Susan Ostrander

Lewis Coser cites a pair of what he calls "baroque aphorisms." On the one hand the mathematician Niels Abel argues: "It appears to me that if one wants to make progress in mathematics, one should study the masters and not the pupils." On the other hand, Alfred North Whitehead suggests that "a science which hesitates to forget its founders is lost." Abel and Whitehead appear to proffer contradictory advice. Abel's aphorism might apply to such comparisons as John Dewey and the American educators who interpret him or the differences between Marx and some Marxists or Freud and some Freudians. Although Whitehead's aphorism appears to advocate the reverse of Abel's, let us consider. Whitehead's advice to "forget" our founders implies that we have the knowledge to forget. One does not forget what one never knew. To the extent that science is cumulative in fact (as it claims to be in fancy) we build upon the foundations of the past—and then forget them. Without knowledge of the past each of us is compelled to discover everything anew. On these grounds evaluation research is deserving of more historical attention than it has received.

I submit that evaluation researchers are not very sensitive to his-

---

This chapter was originally read at the annual meetings of The Midwest Sociological Society, Minneapolis, Minnesota, April, 1981. I am grateful for the assistance of Nancy Dukes in locating obscure references and Peter Rossi for a detailed critique of an earlier draft. Much of this material was later incorporated into an article co-authored by Susan Ostrander and myself (1985).

tory; some are downright contemptuous while others are simply naive. Caine and Hollister, for example, insist that "traditional methods in science . . . are seldom models that can be directly copied" (1972:133). A view of history as "copying" is naive. It neglects the research function of the self-defeating prophesy so clearly understood by demographers and labor-market analysts.

For example, if a shortage of engineers is projected, it is recommended that the production of engineers be increased; if a surplus of Ph.D.'s is projected, it is urged that we reduce our production of scholars. The self-defeating prophesy is the basic rationale for the study of the future.[1]

Insensitivity to the history of their own activity appears in Freeman and Sherwood's thankful conclusion that "the influence of visionary clergymen, guilt-ridden do-gooders, and political radicals—dedicated to projecting their own humanitarian views in the guise of scientific inquiry—has pretty well diminished" (1965: 205). They cavalierly dismiss the historical antecedents that largely account for the existence of contemporary evaluation research. Missionary-type clinical idealists played a critical role in the development of modern evaluation research (Levine and Levine 1974 and 1976). Furthermore, this position neglects not only the role of clinicians and idealists in the development of modern evaluation research but also the historical and continuing political quality of all program evaluation. Without "do-gooders" and politicians it is unlikely that any program would be initiated or any evaluation undertaken, much less acted upon.

In his Progressive treatise, Herbert Croly (1909:6) argues that Americans "may never have sufficiently realized that this better future . . . will have to be planned and constructed rather than fulfilled of its own momentum." All of this was to occur by applying the new social sciences to public policy.[2] The tradition of applied sociology has been described as a "venerable" one which "emerges from the deepest taproots of the discipline" (Gouldner 1957:1022). The discipline has, of course, not always been comfortable with that tradition. Richard LaPierre has written of the quest for scientific respectability that began between the two world wars and of the concurrent rejection of an image of sociologists as moralists (see LaPierre's letter in Deutscher 1973:36–38). Such efforts to sever sociologist's reformist roots accompanied by the projection of the discipline as an objective science, persist in contemporary applied sociology.

In their review of the history of social science and social policy, Scott and Shore comment on the truncated analyses which appear in the literature: "The heavy reliance on examples of efforts to use sociology in policy that have occurred in the past quarter-century has resulted in a highly misleading and incomplete picture of applied sociology in American society."[3] They conclude that, "the record of accomplishments in applied, policy-relevant sociology is seriously distorted and the explanations for them are misleading when we label 1950 as the approximate date when the first serious attempt to use sociology in policy began" (Scott and Shore 1979: 78). Although their observations on the distortion of history apply as well to the narrower area of evaluation research as they do to the broader area of social policy, there are differences. I will discuss below, the quantum leaps in evaluation research that have occurred in recent decades and I will note important works in evaluation during the first half of this century (e.g., Chapin and Dodd), that are overlooked by Scott and Shore as a result of their concentration upon the broader realm of policy research.

Perhaps the major source for the neglect of and even contempt for history in much contemporary social science is the essential ahistoric position of experimental research. Those contemporary disciplines that strive most diligently to emulate what they view as a scientific method tend to see causation as known only in the here and now—only as a consequence of the experiment that does or does not establish the link. Such an ideology fails to distinguish between experimental technique as it is carried out in the laboratory and the experimental logic which establishes the connection. I am as bound by my times as any other social scientist in my commitment to the causal logic of experimentation. What is not always recognized is that historical evidence is as amenable to that logic as any other kind of evidence. It is apparent in Durkheim's ruling out of plausible alternatives in his suggestion that suicide is a social fact. It is visible in Max Weber's causal linkage between the Protestant ethic and the spirit of capitalism. It may serve more than idle curiosity then, to consider some of the history of program evaluation.

The Early Twentieth Century

It is possible to trace our origins back to the Enlightenment, to the development of the scientific spirit, to the beginnings of rational appraisal of previously mystical phenomena. One might trace our roots to the evolution of a capitalist ideology which was offended by the expense incurred by the loss of labor from such populations as the mentally ill and the cost of maintaining these peoples on public funds (Levine and Levine 1976). But that is too grand a task. Nevertheless, evaluation research as we know it today has existed at least since the 1890s. Francis Caro has uncovered an early example in an 1897 article by J. M. Rice, a teacher who compared the pupils in his one room schoolhouse when they were subjected to spelling drills and when they were not. His pre- and post-test design including study and control groups was in fact an experiment that revealed no difference in spelling ability as a result of rote drills.[4]

The late nineteenth century experimental efforts by Rice (and probably a few other social reformers) may have stimulated what Odum calls "the sound and the fury" of methodological discussions among sociologists in the 1920s. He reports, however, that "no one did much empirical research . . . " and it was not until the 1930s that more rigid methods began to take hold" (Odum 1951:221).

It is unlikely that Rice's spelling experiment had any influence on educational practices. The sociology of knowledge suggests that he was ahead of his time, perhaps in the same way that Galileo had been a few centuries before. But one sociologist, with the protection and resources of a great university was to begin a remarkable chain of events with a publication in 1917. In that article Stuart Chapin considered the trial-and-error nature of nineteenth-century experimentation in utopian movements and suggested that scientific experimentation be attempted. By 1935 Chapin was able to report on some of the theses his students had completed, evaluating school programs, social agencies, and community organizations. He also proposed refinements in the methodology of field experimentation. Examples of this work are Christiansen's thesis on school progress and economic adjustment based on 1926 data (1935) and Mandel's thesis on Boy Scout tenure and community adjustment (1938). Chapin describes these as *ex post facto* experiments in contrast to the projected experiment.

Chapin and Stuart Dodd were among those said to be responsible for the transformation of sociology from a reformist social movement into an empirical and statistical science (Oberschall 1972: 230). Chapin's plea for controlled experimentation was not ignored within the discipline. In his last book, Franklin Giddings decried evaluative research that proposed as evidence testimonials from satisfied agency clients. For Giddings, this was an example of "bad social science" (1924:38–9). In its place he called for evaluations with "measurements and accountings" (1924:42). Giddings proceeded to outline the contribution of statistical inference in approximating experimental control, claiming that, "it is by application of these procedures . . . that we may hope in time to build up a scientific criticism of the enormous mass of loose inferences which we now encounter relative to the consequence of countless societal experiments" (Giddings 1924:181).

In 1931 Stuart Dodd (1934) conducted a pioneering projected field experiment. He evaluated the effects of an educational campaign on the attitudes of remote Syrian villagers. The design consisted of an isolated experimental village and several comparison villages matched on a number of factors. The educational campaign focused on mental hygiene." Dodd is attentive to both problems of reliability and those of validity. He checked the consistency of his instruments within families, between samples, among interviewers, and among coders. Independent verification of his measures appears in high correlations between them and statistics on mortality, morbidity, and longevity.[5] Dodd's criticism of his own findings of "no difference" attributable to the program, is basically one of contamination. He suspects that the villages were not as isolated from one another as he had originally believed. Chapin, on the other hand, focuses his critique on the inadequacy of the controls and he proceeds to encourage his M.A. students to work on problems of matching and distributive equivalence in their experimental theses.

Apparently no one entertained the possibility that the educational program made no difference in attitudes toward mental hygiene. This is a reversal of the tendency among contemporary evaluation researchers to minimize the possibility of contamination or poor controls and their inclination to attribute a lack of statistical significance to program ineffectiveness. Yet the problems of maintaining randomization and avoiding contamination in field experiments are as persistent today as they were in the 1930s.[6]

Although Dodd's experiment is remarkable, it was not the only effort of its kind, nor was it the first. Greenwood (1945) reviews a number of others including the National Tuberculosis Association's use of Framingham, Massachusetts as an experimental community for treatment in community health from 1918–1924. In 1925 Bruce Melvin simulated laboratory methods in the study of rural social problems. Maurice Taylor reports an experiment in 1926 on localized services in social work. A year later Clarence Senior reported an experiment in Cleveland on community organization for adult education. In 1930 Pitrim Sorokin (then at Minnesota with Chapin) did an experiment with nursery school children on the efficiency of work under varying controlled conditions. This is but a sampling of the extensive experimental evaluation research undertaken prior to the Great Depression.

Mention is due the experiments on worker productivity and morale by Elton Mayo in the early 1930s (1933) and toward the end of the decade by Rothlisberger and Dickson (1939). Chapin seized the opportunity provided by the social reforms of the New Deal to conduct "evaluative research on such topics as the effects of work relief compared to direct relief [shades of the income maintenance experiments of the 1970s], the effects of public housing on project residents, and the effects of treatment programs on juvenile delinquents" (Caro 1977: 7; Caro's source is Chapin 1947). Note that the movement away from social philosophy and reform and toward a more scientific sociology first appeared in applied research and involved leading sociologists.

This influence spread gradually to the academic community and did not become firmly entrenched until after the Second World War. It seems inaccurate to suggest, as Bernstein and Freeman (1975) have done, that the failure of evaluation research to seriously affect public policy is a consequence of its methods not having been sufficiently "scientific." To the contrary, history suggests that the practice of much evaluation research was *avant garde* in its adoption of experimental design and its use of quantitative techniques. Where then were the sociologists during the great surge of social reform stimulated by the Great Depression and implemented during the New Deal?

The Great Depression and the New Deal

In 1935 A. Stephen Stephan, glowing with enthusiasm and anticipation at the unprecedented opportunities for social science provided by the emerging programs of the New Deal, wrote of "Prospects and Possibilities: The New Deal and the New Social Research." He pointed out that the alphabetical agencies being erected by the federal government created "experimental laboratories for the social scientist. . . ." and that,

> these laboratories set up by the planning agencies of the New Deal permit a more effective use of the experimental method in the research projects of the social scientists. This research in turn would not only be an addition to science but would also be a form of social auditing for the planning authorities in noting and accounting the changes wrought by the programs. (Stephan 1935: 40)

Was such optimism justified? Rossi concludes that very little evaluation research was undertaken in the 1930s. He documents this observation with Moynihan's (1969) report that "at the time the War on Poverty was designed, a fruitless search was made through the archives for studies that would provide some assessment of the effectiveness of such programs as the Civilian Conservation Corps (CCC). . . . Little is known also about the other New Deal programs" (Rossi 1972:12). Further attempts to locate studies of this type were made under an Office of Economic Opportunity (OEO) contract in 1966 and met with equal failure (cf. Rossi 1972:12, n.3).

To what extent are Rossi, Moynihan, and Rosenbaum (the author of the OEO contract report) correct in their assessment that evaluations of New Deal programs were practically nonexistent? If they are correct how can the failure to grasp this opportunity be explained? Let us consider. Rossi does acknowledge that he had been informed that "the National Archives hold considerable documentary materials on the New Deal Programs" (1972:13, n.3), but apparently failed to pursue that lead in spite of Dentler's having provided a precise reference to the sources of the archived materials (Dentler, 1954). Rossi is also aware of research monographs published with the support of the Works Progress Administration (WPA) reporting on social conditions during the New Deal era. Drake and Cayton's *Black Metropolis* (1945), an analysis of Chicago's "black belt," provides one example. Such works are, however, not research on the effectiveness of New Deal programs, rather they are research supported by New Deal funds and dealing with contemporary social issues. Furthermore, since the style of these studies is sometimes (but not always) ethnographic and never experimental, it is possible that Rossi and the others define them out of evaluation research.

The solution to the puzzle of neglect may lie in the sociology of knowledge. Rossi explains (in a personal communication) that, although evaluation has always been necessary in decision making, there are many types of evaluating activities. He believes that the critical question is what sorts of information are needed and called for in different historical periods? It is necessary, for example, to distinguish between a descriptive monograph, a compendium of services, and people being served, and an assessment of the impact of a program. It is also necessary, for example, to distinguish among program monitoring, cost-benefit analysis, and experimental program impact evaluations. For most contemporary evaluators, it is probably the effort to obtain by experimental methods evidence of the impact of a program which constitutes evaluation research. What, in fact, did the New Deal social research consist of?

The bibliography of Dentler's thesis (1954) contains many references to New Deal research and data sources:

> When I used the national archives for my thesis, the shelves were *loaded* with evaluation reports of New Deal programs of every kind. There were weekly records on numbers of shovelsful of dirt lifted by WPA and CCC building crews, by state, by project, and by group, for example. The writers' project overall records were housed in 4,000 file boxes, for example. (Dentler 1981)

There were also the continuing experimental reports published by Chapin and his students. In 1939 Jahn published *A Control Group Experiment on the Effect of WPA Work Relief as Compared to Direct Relief upon the Personal-Social Morale and Adjustment of Clients in St. Paul* (Chapin and Jahn 1940). In that same year Chapin designed an experiment on the social effects of the new public housing in Minneapolis provided under the United States Housing Authority (Chapin 1940). That work was a follow-up of his earlier research on the effects of slum clearance and relocation (Chapin 1938) and it goes well beyond Secretary Ickes's notion of "concrete evidence of the social changes being wrought by the Roosevelt administration." Ickes was satisfied with enumerating the number of projects in the number of cities serving the number of people and the number of dollars entailed (Ickes 1934).

Howard Meyers of the Federal Emergency Relief Administration (FERA) reports an extensive research program under the auspices of that agency (1935). It was the function of FERA's research division to provide the social data necessary for it to pursue its responsibilities and it did so. By the summer of 1933 "the Division developed a system of monthly reports . . . for each county throughout the United States" (Meyers 1935:477). That division undertook extensive surveys on specific issues and employed comparative samples in its efforts to identify program effects.[7] By 1936 Howard Meyers was in the Division of Social Research of the WPA and reported on "Research with Relief Funds—Past, Present and Future."

This type of work lacks the sophisticated experimental designs employed by Chapin and others. Nevertheless it is social research designed to illuminate social policies. New Deal researchers were, alas, plagued with many of the same problems which haunt contemporary evaluation research. According to Landis, "New Deal research did not always produce information until the new social experiments had been launched. The research information obtained on CWA was analyzed after CWA had become history. Rehabilitation projects were begun before any research was available" (Landis 1936:598).[8]

In 1933 Harold Ickes created the National Planning Board whose responsibility was to develop comprehensive and coordinated plans for regional areas based upon scientific surveys and analyses of federal projects (Jones 1970:128). That board consisted of an economist, a political scientist—and the president's uncle (Charles A. Merriam, Wesley C. Mitchell, and Frederic A. Delano, chairman). In spite of open opposition from Secretary of the Army Dern and the Army Engineers, that board flourished for a decade. The National Planning Board was responsible for the gathering of massive data on the national condition and the publication of those data and their interpretation in a series of volumes entitled *Recent Social Trends in the United States* (President's Research Committee 1933). Other statistical reports emanating from the Board include *A Report on National Plannin*g (National Resources Board 1934) and their *Final Report* (National Resources Board 1934).

The opposition of the Army Engineers, mentioned by both Jones and Rossi (1972:17) seems to me to reflect what C. Wright Mills was later to call "the sociological imagination." The army was responsible for administering such programs as the CCC and conservation and reclamation programs. It approached the depression as a public issue and not as a congeries of private troubles. Its philosophy was that "there was nothing 'wrong' with the populations that were to be served: the problem lay in the economy. All that such programs as the CCC and WPA were supposed to achieve was the provision of employment and the accomplishment of certain public services, things which the economic system of the time was unable to achieve (Rossi 1972:17)." Having adopted the sociological position that it was the society which was flawed rather than the individuals who suffered from its disrup-