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Working in Six Research Areas: A Multi-Field Sociological Career

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Abstract

I have written a strictly autobiographical essay about the half dozen areas or fields in which I have done sociological research during my career. One reason for writing the essay is to encourage students to become what I call a “multi,” there being too few in sociology, as in other disciplines in which most researchers do their work in one field. I hope the essay demonstrates that working in many fields can make for a satisfying and productive career. Research across areas also encourages comparative work, and if enough young people become multi-field researchers, sociologists might then develop more interdisciplinary skills, which is even now desirable and may one day be necessary for all the social sciences. My essay also describes how and why I became interested in my six fields and how I moved between them during my career. The rest of the paper describes my major studies and other activities in my fields as well as the institutional and other contexts that I believe have affected my work.

Every year, Columbia's sociology faculty members participate in a first-year colloquium in which they introduce themselves and their work to the new graduate students. My presentation in recent years has featured an analysis of research careers in sociology, beginning with a distinction between solo-field and multi-field researchers.

"Solos," I argue, typically choose one field in their discipline, sometimes as early as graduate school, and even though they may teach courses in several fields, they devote their research to that one field, often for their entire careers.

Most sociologists, like most researchers in other fields, are solos. They are also the mainstays of normal science; in addition, they often expand and sometimes transform their fields.

"Multis" are as the term implies, researchers who work in several fields. Some do so in several but closely related fields, others in unrelated fields; yet others conduct cross-field research that contributes to the entire discipline. Given that disciplines are organized for the solo majority, multis may sometimes be marginal figures, although at the same time they are often better known precisely because they are working in several fields.

This article describes one multi-field career: mine. Although it is mainly an autobiographical essay, it aims to be sufficiently sociological to identify the macrosociological and other forces and agents that helped me at various stages. In addition, I hope my story will offer some guidance and inspiration to young sociologists whose interests and temperaments lead them in a multi-field direction. Believing that the discipline can use more of us, this account of my life in sociology may demonstrate that a career can be made outside the solo mainstream.

MY CAREER—AN OVERVIEW

My academic career has been unusual temporally. I spent my untenured years mainly as a researcher who taught one or two courses a year, thus giving me maximal opportunity to publish. From there I proceeded directly—a full 12 years after receiving my PhD—to a tenured full professorship. Moreover, before becoming

an academic, I briefly worked as a city planner. Although most of my research has been sociological, my PhD is in social planning, and I have always described myself as having been trained both in sociology and planning.

I was born in 1927, into an urban middle-class German Jewish family. We were able to leave Nazi Germany early in 1939, waited in England for the American visa for which my parents had applied years earlier, and arrived in the United States in September 1940. We came without any money into a country still suffering from the Depression, and my parents worked at menial jobs until World War II gave a boost to the domestic economy and they obtained white-collar work.

Like other immigrants, I tried to learn as much about my new country as possible and, discovering in high school that I enjoyed writing, thought I would become a journalist. Once in college, I discovered that the writing I enjoyed doing most was called sociology, so that even before graduate school I thought I would like to become a sociologist. However, I also wanted to remain a writer, and like some other professors, I ended up a writer-researcher who made his living teaching. I was fortunate also that the research and writing that interested me most did not require large grants and the proposal writing needed to obtain them.

Serendipity—and necessity—played a role in my career even before it began. My father decided that German refugees would have better job opportunities in Chicago than in New York, where most refugees had settled. We ended up in Woodlawn, a then mostly Irish low-income neighborhood of Chicago that happened, however, to be immediately adjacent to the University of Chicago. Because I had to live at home, I wound up at what I still think of as the neighborhood university, which was then and is even now often thought to be the most serious in the United States. One quarter's savings, a one-quarter scholarship, and later the GI Bill enabled me to obtain both a bachelor's and a master's degree at Chicago.

In the 1940s, those of us interested in teaching expected to wind up as high school teachers.

For this reason, and also because my college social science courses had made me a believer in a unified social science, I enrolled not in sociology but in an interdisciplinary MA program in the social sciences. The program was so new that it had not yet developed a long list of requirements, and although most of my courses were in sociology, I also took courses, generally of my own choice, in several other social sciences. I did not know it then but I was already training myself for a multi-field career.

Chicago was full of brilliant professors, four of which were most important to me as a student. One was Earl Johnson, a sociologist who had studied with Robert Park and John Dewey but who saw himself as a disciple of the latter and, like Dewey, was deeply involved in public policy issues. A second was Everett Hughes who trained me in fieldwork, taught us to think comparatively, and, in his quiet way, regularly came up with amazing insights. A third was David Riesman, a role model for multi-field sociologists if there ever was one, someone who treated even undergraduate students as his equals and encouraged them to pursue their personal research interests. But I probably learned the most from Martin Meyerson, a social scientist and planner who helped me learn how to combine sociology and planning. In addition, Meyerson supervised my MA thesis and, later, my PhD dissertation.

I also studied with W. Lloyd Warner who, in a discipline that then shunned the work of Karl Marx and was just beginning to translate Max Weber, was nearly alone in researching social class. I benefited greatly from fellow students; in sociology they included, among others, Howard Becker, Eliot Freidson, Erving Goffman, Joseph Gusfield, Louis Kriesberg, and Lee Rainwater. But there were many, many others, teachers, teaching assistants, colleagues, and of course authors who taught me how to think and analyze in social science ways. One author must receive mention: Karl Mannheim, whose *Ideology and Utopia* (1936) and its chapter on the sociology of knowledge provided me with the initial epistemological grounding for my subsequent work.

My GI Bill money ran out in 1950, and after receiving my MA that year I worked as—and in the process learned to be—a professional planner. Planning was then still a young field so that learning the basics did not require a degree, and besides, I worked mainly as a social researcher in planning.

Although I was not interested in urban design (or physical planning), I wanted to do what I considered to be socially useful research and to use it to help shape public policy. By 1953, I realized that I did not belong in the bureaucratic organizations in which planners had to operate, and because academic opportunities were beginning to surface, I returned to graduate school. I had a difficult choice: full scholarship and full-time job offers from both Columbia's Sociology Department and the University of Pennsylvania's Planning Department, and I finally chose the latter. As a Chicago student, I had read Merton's writings on functionalism, mainly because they attended to the consequences of social action, essential knowledge for anyone interested in public policy. Subsequently, I sent Merton my MA thesis, and he invited me to study with him at Columbia. The Sociology Department had too many PhD students already, however, and I felt I would not receive sufficient individual attention. Moreover, I continued to be unhappy about sociology's reluctance to confront policy and political issues.

A PhD in sociology from Columbia would probably have trumped a PhD in planning from the University of Pennsylvania in the job market, but the Penn department was small and would give me a chance to work once more with Martin Meyerson, specifically on his ambitious research project to bring social science to bear on planning decisions. As luck would have it, I was Penn's first and only PhD student in planning, and because no PhD-level courses were yet being taught, I was once again free to choose my own courses across the social sciences. In sociology, I still remember especially my courses with Digby Baltzell and Marvin Bressler.

I received my PhD in 1957 and an assistant professorship in planning at Pennsylvania as well, but as already noted, I spent the next

dozen years as a researcher there and elsewhere in research institutes, although always teaching a course on the side. I did not plan it this way, but research money was beginning to flow generously so that I could concentrate on research and writing. Furthermore, I was already becoming a prolific writer and one who also published outside the academy, which resulted in a fruitful exchange for me and my employers. Through my writing, I provided visibility and publicity for them; in exchange they gave me time to do my own work and to participate in the antipoverty and other public policy activities of the 1960s, which I describe below.

In 1969, with three published books and about 75 academic and nonacademic articles in my curriculum vitae, I decided it was time to move into full-time teaching. I was also ready to become a sociologist, but after I was denied tenure at Columbia for what I have been told were political reasons, I accepted a professorship in planning at the Massachusetts Institute of Technology. I soon realized, however, that a social planner like me did not belong in what was then a purely physical planning program, and in 1971, after Columbia's post-1968 trauma had ended, I joined its Sociology Department, where I have been ever since.

I did not give up social planning, however, but just found a new and sociologically acceptable term for it: social policy. My major lecture course was called Urban Sociology and Social Policy, and I also led graduate seminars on social policy. I have always most enjoyed helping students undertake empirical and policy research, and as often as possible I have taught graduate and undergraduate research seminars and in the fields that have most interested me. Although I became an emeritus in 2007, I still teach the course on Field Research Methods I have given since 1975. The rest of the time I write.

MY SIX RESEARCH FIELDS

No one ever told me to find a single research field and then to stay with it. Perhaps no one yet thought this way in the post-World War II

years, or else I was far away from the disciplinary mainstream where, I have been told, such suggestions are made. However, thus unencumbered, I have always followed my research nose, the issues coming up in the world outside, and the questions that I wanted to answer through research and felt competent to address.

Nevertheless, looking back on the research and writing I have done so far, I see my work as falling into six fields:

1. Community Studies—and Urban Sociology
2. Public Policy
3. Ethnicity—and Race
4. Popular Culture, the Media, and the News Media
5. Democracy
6. Public Sociology

Six fields may seem like a large number, but some of my research and writing straddles fields, and almost all the fields are related. Moreover, some—and in a few cases much—of my work in each of the first five fields is about inequality and equality, economic, political social, and cultural. The last field is the exception: It arrived on my intellectual doorstep when the 2004 president of the American Sociological Association (ASA), Michael Burawoy, resurrected a term I had used when I was president of the association in 1988—but more about that below.

However, the six fields have something else in common: I became interested in some aspect of each before I entered graduate school, and in some cases even earlier. Perhaps they all go back to a poor immigrant's initial curiosity about the United States. The incentive and other structures shaping the academy and sociology since I began graduate school have played a role as well.

Although I believe my empirical research itself to have been value-free—or rather to apply the values shared by empirical researchers—the topics I have chosen for study, the ways I framed them, and the implications I have drawn from my findings have reflected my values. I have never hidden the role of such values and, when

relevant, have always indicated that I see myself as a left-liberal on many issues.

Conversely, I do not think of myself as an intentional follower of any sociological or other school, instead letting the topics of my research and their framing guide me toward whatever concepts and theories seem relevant to the analysis. Like everyone else, I was of course concurrently shaped by all the usual sources of influence, but identifying them is a task for someone else. This is an autobiographical essay (for earlier autobiographical essays, see Gans 1990b, 2003b).

The remainder of this article discusses each of the six fields: how I came to them or they to me, how they are connected, both to each other and to my life, and what I tried to contribute to them. The discussion follows the above order, but in my actual work, I moved between fields, and I still do. In retrospect, some of my colleagues must have thought that I parachuted in and out of fields, and I have often wondered how the solos among whom I landed felt about it and me. They could not have been too unhappy because ultimately I received lifetime awards in several fields in which I have worked, and in 2006, the ASA's highest one, the Career of Distinguished Scholarship award.

1. COMMUNITY STUDIES AND URBAN SOCIOLOGY

Some people think of me primarily as an urban sociologist, but my initial field is more accurately described as community studies. This field came to me: As a student in Everett Hughes's classic field methods course, I had to choose a Chicago census tract and study an institution in it. As a result, I produced a small community study, and then another one doing preliminary work for Morris Janowitz's study of the community press (Janowitz 1952).

Equally important, I was turned on by field methods, more properly called participant observation and now also called ethnography. Like all other research methods, it has its shortcomings, and it is suitable only for some research

questions, but I still think it is the most scientific of the available sociological research methods because it brings researchers into close and continuing contact with the people they are studying and the groups and institutions in which they are embedded.

As I suggest in detail in discussing Field 5 (Democracy), I was also interested in politics, especially political participation. David Riesman, although on the Chicago faculty, was then in New Haven working on what would become *The Lonely Crowd* (Riesman et al. 1951). He was also interested in political participation, and because we came at it from different angles, we corresponded about it extensively. Fortunately for me, he was as compulsive a letter writer as I.

For my MA thesis, I decided to study political participation in a local community. David Riesman told me to contact Martin Meyerson, a young professor in Chicago's Planning Department, to help me find a suitable community for study, and led me to the new town of Park Forest, Illinois. It later became famous as the research site for *Organization Man* (Whyte 1956).

Park Forest was a planned new town, then 1.5 years old, and as I watched it become a community, I decided that I would someday move into another new town as its first resident and watch that process from the very beginning. The research also made me a sociological expert on new towns, maybe then the first and only in the world, but a few years later, I was offered a position with a Chicago architectural planning firm that was planning three new mining towns in northern Minnesota and northern Michigan. Because the houses had to attract and recruit workers, I interviewed area miners who were building their own houses about their design-related preferences and priorities and sent my findings back to the Chicago architects designing the houses for the three towns. Subsequently, I put some of my new town research findings to good use in helping to plan the new town of Columbia, Maryland (Gans 1964b).

The Levittowners

In 1956, while I was studying at Penn, William Levitt, the builder of two previous Levittowns, announced that he would build his third in New Jersey, about 20 miles from Philadelphia. I bought a house there (I was buyer number 25), arrived in October 1958, conducted three years of fieldwork and in-depth interviewing, and published the findings a half dozen years later (Gans 1967).

Community studies are an intensely stimulating genre because the fieldworkers not only obtain answers to their initial questions but, by being there day to day, also learn a great deal about a humongous number of subjects—findings that are both broadening and that come in handy for decades after. The Levittown study showed me how a set of strangers come together to create a functioning community, but I also learned about the origins and operations of a variety of community, neighborhood, and block institutions, formal and informal, as well as about most of the topics covered in introductory sociology courses.

I also observed the extent to which class and class conflict saturated community life, and much of *The Levittowners* was in fact a case study of young lower middle- and working-class Americans. In addition, I studied whether and how people's lives changed in the move from the city and discovered that, beyond achieving many of the goals for which they moved to the suburbs in the first place, the changes were much fewer than commonly believed. That also suggested the limited effects of space and the built environment on people's lives, a theme about which I am still writing (e.g., Gans 2002).

David Riesman liked to argue that one of sociology's functions was to debunk what, thanks to John Kenneth Galbraith, we now call the conventional wisdom. Levittown was a fertile site, rich in empirical data to debunk the stereotypes that demonized the new postwar suburbs. I found that Levittown was not a sterile, conformity-ridden, and homogeneous aggregation of unhappy young urbanites forced by government to move to the suburbs. My

debunking helped to evoke nonscholarly interest in the book and reviews in some general media and, because few sociologists studied suburbs, turned me into a suburban expert for quote-hunting journalists. In addition, Levittown supplied some raw materials for a critical and often reprinted analysis of Louis Wirth's *Urbanism as a Way of Life* (Gans 1962b).

The Urban Villagers

When Levitt had to postpone the start of Levittown, New Jersey, for a year, he and serendipity provided me with an extremely lucky break: the invitation to do a fieldwork study in the West End of Boston. The invitation came from the National Institute of Mental Health (NIMH), and specifically from Dr. Leonard Duhl, a social psychiatrist who funded several of NIMH's sociological studies in those days, but David Riesman, who would play a continuing role in my career for many years, had recommended me.

NIMH was supporting a larger study, by the psychiatrist Erich Lindemann, of how people react to disaster, and the West Enders were about to face one: the loss of their neighborhood to slum clearance. The main study was a before-after displacement interview study with a 500-person sample, and my role was to provide the interview study with a sociological analysis of the community and institutions the West Enders were about to lose.

In October 1957, my then wife and I moved into a fifth floor but comfortable tenement apartment in the West End. Following the lead of Helen and Robert Lynd's work in Muncie, Indiana (Lynd & Lynd 1929), and particularly William F. Whyte's (1943) study in the North End, the neighborhood immediately adjacent to the West End, I examined the everyday life of the area, mainly the working-class Italian Americans who constituted its most numerous population, as well as the institutions that served, underserved, and controlled them and the other West Enders.

However, when I wrote up my findings, I also added observations and reflections on a

variety of theoretical and other issues, including several that had preoccupied me in graduate school. Among the book's main themes were the structural and cultural differences between the poor, the working class, and the middle classes; in addition, I argued that class position was more important than ethnicity in understanding the West Enders. The last two chapters of the book described the then forthcoming destruction of the West End, debunked the claim that it was a slum, and offered a critical analysis of the slum clearance program, with recommendations for how to reform it so as not to victimize the people whom it displaced.

To my great surprise, *The Urban Villagers* (1962a) became a success, selling over 180,000 copies, especially, I am told, as a supplementary text in a variety of sociology courses. I still meet people who tell me that the book inspired them to become sociologists. My critical analysis of the West End's destruction, beginning with an article in the planners' professional journal (Gans 1959), initially made me an outcast among mainstream planners and others who favored urban renewal. However, as a result of Jane Jacobs's (1961) influential general critique of planning and the recognition that urban renewal brought about Negro removal, liberals and even some planners began to oppose the renewal program. Consequently, by the mid-1960s, I was frequently invited to testify, consult, talk, and write about urban renewal and participate in antirenewal planning and related activities (e.g., Gans 1965a).

2. PUBLIC POLICY

My formal involvement in public policy began in 1950 when I started to work as, and to become, a planner, but informally it began much earlier. Growing up as a Jewish child in Nazi Germany and spending the start of World War II in England must have started the process, but it took a particular turn during my adolescence when I learned, in a distinctive fashion, to think about issues of equality and democracy.

In the early 1940s, I spent two summers at Jewish camp near Chicago that helped the local

truck farmers harvest their crops as part of the war effort. The camp itself was first and foremost an educational venture in labor or democratic socialist Zionism. There, I first learned about the Israeli kibbutzim, the agricultural collective settlements in which all property was commonly owned and which were governed by direct democracy, the entire community meeting weekly to make the needed decisions.

Much impressed, I thought of moving to Israel after high school to spend my life on a kibbutz. Indeed, during my college years, I was part of a study group that intended to go to Israel for a year to study the kibbutzim before deciding to move there. Although I was never able to do more than visit some of these communities, my youthful library research about them initiated a lifelong interest in egalitarian policies.

My interest in and knowledge of public policy were enriched and, equally significant, legitimated when, as a graduate student, I took Social Science 2400, the University of Chicago's mandatory year-long course that aimed to teach us how the social sciences could help the newly founded United Nations make the world a more prosperous, democratic, and in other respects better place for everyone.

My first academic public policy project was my dissertation at the University of Pennsylvania, one of the three planned volumes in the Meyerson project to use social science concepts and methods in planning. My study was entitled "Recreation Planning for Leisure Behavior: A User-Oriented Approach," which argued among other things that public recreation planning needed to pay more attention to the usage patterns of its users and the avoidance practices of the nonusers. However, I also wrote about the suppliers of recreation: the social movements that lobbied for more parks and public playgrounds and proposed a recreation planning program that responded to the wishes, values, and interests of users, suppliers, and the larger community. My topic was framed broadly enough to enable me to include findings on leisure behavior and connect them to recreation planning. That gave me a chance also to bring

in popular culture and the mass media, subjects that were more interesting to me than recreation planning.

In addition, the dissertation, which came to nearly 800 pages, also devoted itself to theorizing the social science–planning interface that Meyerson, my fellow PhD student John Dyckman, and I were attempting to develop. My theorizing and other parts of the dissertation are summarized in Gans 1968a (chapters 6–9).

When I received my PhD in planning in 1957, I was fully credentialed to work in public policy, but my initial contribution was my 1959 critique of slum clearance, mentioned above. However, then serendipity and the early signs of dramatic changes in American society intervened and sent me in an additional direction.

The Urban Villagers was published about the same time as Michael Harrington's *The Other America* (Harrington 1962), which helped to inspire the War on Poverty. Not long after my book appeared and received favorable reviews, I began to be viewed as a sociologist of urban poverty—or at least someone who could make sociological presentations on poverty. Although I insisted that most of the West Enders I wrote about were earning moderate incomes, I met enough poor ones to satisfy the sudden demand for knowledge on the subject. In addition, I could talk about antipoverty policy.

Consequently, I spent part of my time in the 1960s writing and talking about poverty. The writing was mostly for nonacademic publications. The talking took place in a variety of committees, workshops, conferences, and seminars in which a number of us analyzed federal proposals and tried to develop alternative ones that we thought had a better chance of helping poor people to escape poverty.

In many of the meetings, I sat with the young professionals, organizers, activists, and political staffers, several working for one of the Kennedys, who actually invented the programs associated with the War on Poverty. But I was hardly the only sociologist. S.M. Miller was perhaps the most active sociological convener of policy discussions, and Frances Piven and

Richard Cloward were the most active in developing new antipoverty policies. Among many others were Lee Rainwater, as well as Peter Marris and Frank Riessman. Through a policy committee affiliated with the Congress of Racial Equality (CORE), then the most liberal of the civil rights organizations, we also worked with the black leadership around Martin Luther King, including James Farmer, Bayard Rustin, George Wiley, and, among the policy-oriented academics, Kenneth Clark and Hylan Lewis.

The antipoverty policy analysts were not always in agreement; some, with Michael Harrington in the leadership, called for jobs and full employment as the policy centerpiece; others, most vocally Frances Piven and Richard Cloward and the welfare rights movement that they helped to found and lead, believed that broader and more generous welfare benefits for poor families, regardless of the number of parents, were more urgent.

I never thought of it as a dichotomous policy issue, and the basic theme of much of my writing and talking about poverty proposed that poverty could not be eliminated until enough secure and decently paid jobs were available for the poor who could work. Income supports had to be made available to the rest, including the mothers who should stay home to raise their children until they began to attend whatever pre-kindergarten schooling was available to them.

In those days, we still believed in the possibility of full employment, although I had resonated to the automation scare of the 1960s and began to suggest that new technology (still mostly on the horizon) would increasingly affect work time, either increasing joblessness or reducing work hours (Gans 1964a).

Twenty years later, I received a German Marshall Fund fellowship to go to Europe and study early experiments with reductions in the workweek and worksharing to increase the total number of jobs (Gans 1990c). Although Western European workweek reductions have not yet created as many jobs as expected and are now being revoked in response to employer demands, I still think that they will someday be

necessary everywhere, including in the United States.

My concurrent activities in antipoverty policy and antirenewal activities led, among other things, to an invitation in 1966 from Senator Ribicoff of Connecticut to testify before his Committee on the Crisis of the Cities. That gave me an opportunity to describe that crisis as a by-product of economic and racial inequality and to try to connect antipoverty and urban policy (Gans 1966b). A few years later I became a consultant for the National Advisory Commission on Civil Disorders, for which I wrote a memo on the ghetto rebellions and subsequently drafted chapter 9 of the Kerner Report (Gans 1968b).

Struggling Against Inequality

At the start of the 1970s, the programs of what many activists had since the mid-1960s called the Skirmish on Poverty were, with some notable exceptions, about to go on the chopping block. I reacted in two ways. The first was, strangely enough, optimistic, for I chose to see the decline in poverty brought about by Lyndon Johnson's War on Poverty, the economic growth that improved the lives of the working and middle classes, and the socio-cultural trends set in motion by the hippies, yippies, and other radical young people as a sign of a forthcoming equality revolution. My conclusion was much too hopeful, but I assembled a set of essays, some which had earlier appeared in the *New York Times Magazine* on that theme and titled them *More Equality* (Gans 1973).

Several of the other social scientists working on antipoverty policy were also egalitarians, and although we did not then stress it in our writing, many of us believed that the reduction of poverty had to be accompanied by a drastic shrinkage of the economic and other gaps between the poor and the nonpoor. That gap was modest in comparison to today's much grosser inequality, but gross enough to us even then. We also began to speculate that poor people suffered not only from poverty but also

from the social and psychological effects of inequality, an idea that has by now received some empirical verification.

My second reaction to the end of the War on Poverty was an article entitled "The Positive Functions of Poverty," which appeared in the *American Journal of Sociology* (Gans 1972). The article argued that poverty persisted in part because it was functional, i.e., useful, for the more affluent classes. The body of the article identified 15 such functions, including supplying a labor force ready to do the society's dirty work at low wages; providing neighborhoods that could be torn down as slums; and, because the poor tend not to vote, making the actual electorate economically more conservative than it would otherwise be.

Because the article's tone was, despite my best efforts, ironic, it was at first dismissed as a satire, but over the years it has been reprinted more than two dozen times, including in anthologies used in English writing courses. In retrospect, I suppose I was saying—even if unintentionally—that we are all implicated in the persistence of poverty. The article had a second agenda: to present a radical functional analysis so as to rebut the charge that functionalism was inherently conservative.

The Culture of Poverty and the Underclass

One of the people in the then tiny circle of social scientists concerned with poverty that I met at the start of the 1960s was a dynamic young Labor Department official named Daniel Patrick Moynihan. He had a PhD as well as experience in New York state politics, and in 1965 he became famous as the author of "The Negro Family: The Case for National Action," now commonly known as the Moynihan Report.

Like its author, the Moynihan Report was both liberal and conservative. Its liberal parts advocated greater equality and discussed the negative effects of joblessness, but the chapter discussing the report's title argued that single parenthood and other problems of the black family were major causes of the perpetuation

of black poverty. Needless to say, that chapter received most of the political and media attention.

Commonweal, the liberal Catholic weekly, asked me to comment on the report, and I wrote a carefully balanced review (Gans 1965b) that attempted, between the lines, to persuade the author to rethink his analysis of the black family. Afterward, Moynihan invited me first to the planning conference for the 1966 White House Conference to Fulfill These Rights and then to an 18-month long seminar on poverty sponsored by the American Academy of Arts and Sciences. The seminar, which met monthly, discussed most of the major empirical and policy issues of the period, but I now remember it mainly as an endless debate with Oscar Lewis over the culture of poverty.

A majority of the seminar members, including me, were critical of Lewis's analysis because he provided little empirical evidence for it. Some of us were also concerned that the political opponents of antipoverty policy, including academics, were using Lewis's concept to blame the poor for their own poverty and that Lewis did not protest their misuse of his work. Eventually, several of us wrote papers critical of the culture of poverty for the book that came out of the seminar (Moynihan 1969).

The culture of poverty continued to be the major blaming concept for another decade, but in the late 1970s, it was replaced by a much more graphic one: underclass, which unlike Lewis's concept made the black poor its major villain. Like the popular version of the culture of poverty, it viewed the poor as lazy, stupid, impulsive, promiscuous, abusive, criminal, and given to family breakdown and other pathologies, a list that had not changed significantly for many centuries. Myrdal (1963) introduced the term as a color-blind concept to describe a new social stratum generated by the ever more capital-intensive economy that was likely to be permanently stuck at the bottom of, and even under, the existing class hierarchy. About a dozen years later, Myrdal's term was racialized and redefined to blame the poor, especially the black poor, after the 1970s ghetto disorders,

the spread of crack cocaine, and the increase in street crime.

Having seen Lewis's originally scholarly concept turned into a political weapon against the poor and noticing that Myrdal's underclass was following the same path, I became curious about the process by which this took place and especially of the roles that social scientists and journalists played in it. In 1989, I received a Russell Sage Foundation Visiting Scholarship to do the research, fortunately for me at the same time as Michael Katz, the historian of poverty who had just published a book on *The Undeserving Poor* (Katz 1989). My book, *The War Against the Poor* (Gans 1995), identified the mixture of journalistic and social science inputs that had transformed Myrdal's term and the foundations and other institutions that then put it into wide play in the popular press and elsewhere.

I have not done any significant research or writing on poverty and antipoverty policy since this book was published, but I have continued to pursue public policy questions. My most recent book, *Imagining America in 2033* (Gans 2008), although subtitled "a utopian narrative," is also a book about desirable and feasible future public policy. I used the book, and its futurist standpoint, to further explore policies I had written about earlier and to suggest scenarios by which they might be implemented. However, I also wrote about several—for me—new policy areas: the larger economy, war and peace, energy and global warming, the family, and education, among others.

Although the book is new, my plan to write it was not. That was hatched when I first read Edward Bellamy's influential but also highly implausible utopia *Looking Backward* (Bellamy 1888) in high school or college and thought I would like someday to attempt a more plausible utopia. I first tried to write the book in the early 1970s, perhaps partially in despair about what the Nixon administration was doing to the United States at the time, but I still had too many empirical projects to begin or finish first.

Even my interest in the future is not recent. Although I know that prediction and merely

projecting trends is dangerous and should be avoided by social scientists, I was also trained to think about long-term planning. Although I am not a futurist, my list of publications includes nearly a dozen articles with the word future in the title, beginning as far back as the mid-1950s.

Connecting Policy and Politics

Growing up in Nazi Germany meant that politics was always in the air, and this probably stimulated an early interest in the politics of my new country. Going to the University of Chicago in the 1940s made me and others the constant target of conversion and recruitment attempts by Stalinist and Trotskyist student activists, which added ideology to my ongoing political education. However, my campus experiences convinced me early on that I was most comfortable with social democratic or left-liberal political values, and that has never changed.

Living in a poor neighborhood in Chicago and under the city's already well-functioning political machine, I learned early on that money makes the world go 'round, and politics helps to allocate and distribute it. As a planner, I also learned that policy and politics are forever intertwined, and over the years my regular but marginal involvements in various kinds of political organizations have given me a political education that has often been useful in my policy work.

Toward the end of my student days, I did some political work for my local congressman and joined the local branch of Americans for Democratic Action (ADA), the then influential liberal organization. While studying at Penn, I became active in its Philadelphia branch. In the 1960s, I was elected to the national ADA board and participated in an effort to push the board in an egalitarian direction on domestic economic policy. Along with some other social scientists, I also served on the board of the League for Industrial Democracy (LID), a small but influential democratic-socialist organization that was bitterly anticommunist and thus intensely hawkish on the Vietnam War. The LID was

considerably more constructive on civil rights and racial equality, which gave me a chance to work with Bayard Rustin, Michael Harrington, and the young people around them, particularly on A. Phillip Randolph's "Freedom Budget" before I resigned in protest against the LID's support of the war.

The LID was very much Old Left, and most of my fellow board members were bitterly opposed to the New Left. I was not and had in fact become friendly early on with the leaders of the Students for a Democratic Society (SDS), including Tom Hayden and my now Columbia colleague Todd Gitlin. I served mainly as an occasional and informal kibitzer-adviser on public policy and on the uses of sociology to several New Left organizations (Gans 1965c, 1966a) and thereby added a distinctive course to my political education. I took what turned out to be a much longer course when several of the so-called New York Intellectuals (many of whom I had met when I was still a graduate student and again as a regular contributor to *Commentary*, the then politically liberal Jewish magazine) began to turn neoconservative.

These and other political activities and experiences have influenced both my policy-related research and my teaching. Politics changes all the time, and policy analysis must confront these changes. Although the academy still often tries to be above politics, students, even in basic sociology courses, must obtain as much instruction about the political process, past and present, as we can give them.

3. ETHNICITY AND RACE

The census tract that I chose in Everett Hughes's 1947 fieldwork course included a section of the Jewish area in Hyde Park, the neighborhood in which the University of Chicago is located, and part of my final report included findings about the Jewish community. My choice was not accidental; like other Jewish adolescents, I had spent part of my teens exploring my Jewish identity (to use today's language), and first research projects are almost always motivated in part by personal curiosities.

W. Lloyd Warner was once more an influence, in this case with his book on ethnicity in Yankee City (Warner & Srole 1945). Later, I became very interested in the two authors' analysis of social and cultural assimilation. My interest in assimilation was so strong that originally I thought about writing my master's thesis on the treatment of assimilation in the Yiddish theater. Instead, I later studied several Jewish night club entertainers, watching them guilt-trip their assimilating audiences for deserting the culture of their immigrant parents (e.g., Gans 1953).

Then, at the start of my thesis fieldwork in Park Forest, I learned that some of the Jewish residents were actively trying to establish a synagogue and Sunday school. These institutions were intended to persuade their children, who were growing up with Christian peers and wanted to celebrate Christmas, to instead celebrate Chanukah. In fact, watching a group of strangers trying to create Jewish institutions in Park Forest first gave me the idea that later led to my study of Levittown, New Jersey.

Nathan Glazer, whom I had met through David Riesman when they were working together on *The Lonely Crowd*, was then editing a sociological section in *Commentary*. When I told him what I was doing, he asked me to write an article about my Park Forest findings. The resulting article (Gans 1951) gave me my first, and totally unexpected, minutes of recognition. It also emboldened me to undertake a two-article analysis of the larger Jewish community in the United States (Gans 1956a,b) and what I saw as its assimilating and thus changing culture. In the second of those articles, I suggested in a footnote (1956b, p. 561) that Jews and other ethnic groups were undergoing a "more or less straight line of cultural and social assimilation," as a result of which I have often been credited with and blamed for inventing a "straight line theory" of ethnicity.

By the 1970s, ethnicity was a rapidly growing sociological field. Reading the new literature about non-Jewish ethnic groups—and doing the informal fieldwork in New York City's multiethnic community that becomes second nature to every fieldworker living in that city—

I began to suspect that my observations about American Jewry in the 1950s might hold true for the second and third generations of other ethnic groups. I had already noticed in Park Forest that the Jewish parents I was studying were more interested in feeling Jewish—and using Jewish cultural and religious objects and symbols to do so—than in participating in the organized Jewish community or practicing the ethnic and religious laws and customs of their ancestors. At the time, I called this phenomenon symbolic Judaism, but in the 1970s I elaborated on it and renamed it symbolic ethnicity (Gans 1979b). Since then, sociologists studying other ethnic groups have found the concept relevant, and I suspect it will be applicable to the descendants of the continuing post-1965 immigration as well.

In the 1980s, students of ethnicity had begun to recognize that unlike the poor and white (or at least swarthy) European immigrants of the 1880–1924 wave about which they had been generalizing, the new immigrants were predominantly nonwhite and multiclass and included a significant number of professionals.

The field of ethnicity had by now been renamed immigration, and many of the younger researchers, themselves members of the post-1965 wave, argued that the then hegemonic acculturation and assimilation model did not apply to a multiclass and multiracial immigrant population. Some members of my cohort of researchers disagreed, arguing that while racial and class discrimination would get in the way of social assimilation into the white community, the children of the new immigrants would nevertheless assimilate culturally (or acculturate) and absorb the mainstream American cultures of their time much like their predecessors of the old European immigration (e.g., Gans 1999b, but the definitive statement is Alba & Nee 2003). Conversely, I have also argued that some children of poor immigrants might acculturate so completely that they would reject immigrant jobs that in bad times might result in their downward economic mobility. I called this phenomenon second-generation decline (Gans 1992b); Portes & Zhou (1993) called it

downward assimilation, and our pessimistic assessment has stimulated a considerable amount of significant research.

Partly as a result of the arrival of Asian American and Latino graduate students at Columbia University, I began to teach and write once more about race (e.g., Gans 1999c, 2005).¹ However, I continue to argue, as I did in *The Urban Villagers*, that the analysis of the subject remains incomplete until the role of class is factored in.

4. THE MEDIA AND THE NEWS MEDIA

My work in the media, which began with the entertainment media and popular culture but then moved into the news media, actually started immediately after we arrived in Chicago. The landlady in the Woodlawn rooming house where we lived the first year had stored a year's worth of *Chicago Tribune* Sunday comics in her basement, and I spent endless and entranced hours going through them. German popular culture for children was dominated by movies, but by 1935 the Nazis had banned American films and so completely politicized German movies that we stopped going to the movies. America's Sunday comics offered me a means for satisfying my immigrant curiosity about my new country, and I suppose indirectly it led to my various activities in the media field.

At the University of Chicago, I discovered a fledgling communications program that was buried in the Library School and run by its dean, Bernard Berelson. The University of Pennsylvania, like other schools, offered only a couple of communications courses, but after receiving my PhD I taught a course on mass communications and popular culture. I also joined interested colleagues to lobby for a mass communications program, which eventually became a

reality as the Annenberg School of Communication. Many years later, at Columbia, I joined another lobbying group that helped to bring about the Journalism School's PhD program in communications.

In 1949, I wrote a term paper entitled "Popular Culture and High Culture" for a University of Chicago graduate seminar taught by Elihu Katz and David Riesman. I later turned it into a short article, then several ever longer ones, and finally into a book (Gans 1974). *Popular Culture and High Culture* described five "taste publics" and "taste cultures" stratified by class and laid out the aesthetic values and cultural choices of these publics in various media and arts. However, as the book's subtitle pointed out ("an analysis and evaluation of taste"), I was concerned not only with the analysis of taste but also with its evaluation. I criticized the advocates of high culture who claimed that only its aesthetic values were acceptable and argued for cultural democracy on the grounds that people's aesthetic values and cultural choices reflected their level of education and socioeconomic position. Until people had access to the income and schooling of the professional cultural elite, they could not even be asked to choose high culture.

My defense of popular culture earned me a number of angry reviews, especially from critics in the humanities. A decade later, my book was literally swamped by Pierre Bourdieu's *Distinction* (1984), which came to roughly the same empirical conclusion but was based on a subtler and more wide-ranging analysis and ended up on the side of high culture. However, I gather my book continues to be useful for stimulating discussion in undergraduate media and culture courses.

But I was always pursuing empirical research as well. It began with the studies of Jewish entertainers, mentioned above, which were followed later by an article about Hollywood movies (Gans 1957) in which, as in my dissertation, I once more made the case for the users and analyzed the indirect role of the eventual audience in the movie-making process. That year, I also paid a brief visit to Hollywood with the

¹I was inspired particularly by the Asian American and Latina students whose dissertations I sponsored or cosponsored, notably Margaret Chin, Jennifer Lee, Sara Lee, Ayumi Takenaka, Norma Fuentes, and Cynthia Duarte.

idea of undertaking an ethnographic study of the making of a movie. I am still sorry that I never got to carry out such a study.

In the summer of 1957 and just before moving into the West End, I went to Great Britain to try to discover why an overwhelming majority of the British movie audience, like most other Europeans, preferred American movies over domestic ones (Gans 1962c). I discovered that British movies were made by and for the educated upper middle class, in which the lower classes were often villains or targets of humor. As a result, the then largely working-class audiences flocked to American movies, whose heroes looked classless to those British moviegoers.

My other media activity was an active if pre-professional practice as a fledgling journalist and essayist. I wrote features and news stories for and then coedited the high school paper, served as a stringer for Chicago's City News Bureau, contributed regularly to a *Chicago Daily News* sports column—an early form of blogging—and was a working journalist for part of my military service. I also wrote for the college humor magazine at Chicago. My specialty was short satires, a genre that I continued to pursue over the years. Some were published in various not-so-mass media, and a few I have included in two of my books of essays (Gans 1968a, 1999a).

Since my graduate student years, I have also written for nonacademic publications. I will discuss that work, which is now called public sociology, in the final section of this paper.

My serious interest in news media research began in 1962, during the Soviet-American nuclear crisis over Cuba, when I was finishing my fieldwork in Levittown. Although I wondered whether its completion would be preempted by a nuclear holocaust, I also became curious why the news media appeared to egg on those calling for war. I decided that if they and I survived, I would undertake an ethnographic study of some newsrooms to satisfy my curiosity.

First I had to finish writing *The Levittowners*, which took much longer than *The Urban Villagers*. However, I was also able to work on a

related news media study before I could begin my own. After John F. Kennedy's assassination, Paul Lazarsfeld obtained funding to study how network television, then still inventing its news programming, covered the assassination, and I saw this as my opportunity to gain entry for my future study. As the most senior interviewer for the Lazarsfeld study, I was assigned to interview anchormen and top executives, although when I started my own study, I had no trouble gaining entry even in news organizations in which I knew no one.

When I finally began my fieldwork in 1966, my project had grown into a more general analysis of how the national news media go about deciding what is news, and I spent many months in the last half of the 1960s in the newsrooms of two newsweeklies and two television network evening news programs. That gave me a chance to compare print and electronic news media, although the similarities, most imposed by a basic journalistic method that has not changed much in the past century, were much greater than the differences resulting from technology.

I also discovered that the news sources, and indirectly the audience, play as great a role in the determination of newsworthiness as the journalists and that between the pressure of deadlines and the imperatives of the journalistic method, the journalists have less autonomy than is commonly thought. My research included a content analysis of the news programs and magazines, and the resulting chapter on journalistic values has received more attention over the years than the structural analyses in the rest of the book.

Although I may have been the first sociologist to do extended fieldwork in the news media, by the time *Deciding What's News* was finally published (Gans 1979a), others had also undertaken ethnographic studies, notably Gitlin (1980) and Tuchman (1980). Unfortunately, our and concurrent other work on the news media did not evoke new interest in media sociology, and while journalists are still writing books about the news media, no American sociologist has yet published a book-length ethnography on today's news media.

In 2003, I published a second book on the news media, *Democracy and the News* (Gans 2003a), which asked what journalists could, could not, and should do to strengthen American democracy. I was fortunate to receive a year's fellowship in Everette Dennis's Gannett Center for Media Studies and later another year at the Freedom Forum Media Studies Center. Although my study involved no fieldwork, I met with many journalists, political writers, and news executives there. My research suggested that while the news media are necessary for democracy, they are not sufficient to preserve it, and journalists can do much less on its behalf than they believe they do.

5. DEMOCRACY

Democracy and the News was also a sequel to *Middle American Individualism* (Gans 1988), which despite its title was actually about the future of liberal democracy. Written during the Reagan era, the book analyzed the differences between the popular individualism of working- and lower middle-class America and the individualism of corporate America, suggesting that middle America did not favor corporate individualism and sought instead to retain the New Deal and the Great Society and the welfare state that these federal programs had brought about.

However, my concern with democracy originated during my adolescence when, as I noted earlier, I discovered the direct democracy of the Israeli kibbutzim. In graduate school, I became interested in what role citizens could and should play in representative democracy, and my master's thesis research on political participation in Park Forest also dealt with this subject.

The empirical data to support the argument in *Middle American Individualism* came from polls and surveys, not from ethnography, but the book was a change of direction in other respects as well. For one thing, it was a return to the participation issues I had grappled with in my master's thesis; for another, it was a reaction to the various and often successful attacks on liberal democracy since the 1960s. However, the last and longest chapter was an

exercise in what might be called political policy making: a set of proposals for the future of liberal democracy.

For that chapter, I resurrected the notion, from my dissertation, of user-friendliness and argued that if the users, i.e., the citizenry, did not want to come to politics and government, perhaps a user-friendly government, with bureaucracies that would serve and even come to the citizenry when the latter needed it, might initiate a greater interest in politics. I advocated the establishment of new citizen lobbies to represent the citizenry, thereby ending the corporate monopoly on lobbying; subtler polling that enabled politicians to get a more complete understanding of their constituencies; and pluralistic news media that better informed and represented the variety of interests in the so-called mass audience.

The penultimate and longest chapter of my new book, *Imagining America in 2033* (Gans 2008), takes one more stab at this topic, this time looking at it in terms of political and other processes that could, over the coming decades, make American democracy more representative. At its analytic level, the book argues that the long-term erosion of jobs, work time, and thus purchasing power will force government to rescue both the consumers and the economy that depends on them. While the fierce political battle to bring about this rescue is taking place, government might have to become more representative.

Force never operates without agency, however. For example, that political battle could require the citizen lobbies I wrote about in my earlier books to become significant political players. Other essential changes proposed in the book include a basic but broad education in politics for everyone, as well as the democratization of the Senate and other political institutions, which will eventually require amending the Constitution.

6. PUBLIC SOCIOLOGY

In 1987, I was elected president of the ASA, and as a result of this gratifying honor, I

became involved in one more field. My first impulse in choosing the theme for the 1988 annual meeting was to do the conventional thing: pick one of my fields that could benefit from an annual meeting's programmatic input and publicity. However, I remembered that, as the 1973 president of the Eastern Sociological Society, I gave an address on equality that fell flat and decided that as ASA president I would choose a theme that would or should appeal to the entire discipline: sociology itself.

My theme was "Sociology in America: The Discipline and the Public," and my program committee and I scheduled a set of thematic sessions on how sociology was serving and could better serve the public. My presidential address (Gans 1989) offered my own observations and recommendations. The edited volume then required of ASA presidents (Gans 1990a) included the most relevant of the meeting papers and some others.

As always, most of what we produced in the thematic sessions and the book sank quietly into disciplinary history, but one of the words I used in passing in my presidential address surfaced again 15 years later. Proposing that we recruit colleagues able and willing to make their research and writing salient for both their colleagues and at least the educated public, I suggested that they might be called public sociologists. I added that public sociologists had to be able to write in clear English and avoid the pitfalls of undue professionalism such as narrowness. Those so inclined should use their findings and insights to consider becoming social critics as well.

Michael Burawoy used public sociology again in his theme "For Public Sociology," for the 2004 annual meetings (Burawoy 2005), and thanks to his analytic skills, programming talent, energy, and outreach, a social movement for public sociology has developed in the discipline.

Even if the term public sociology was new in 1988, the practice was not; a number of American sociologists had written for the general public, and some of them had reached it very successfully. The list includes W.E.B. DuBois,

Robert and Helen Lynd, and in the second half of the twentieth century Daniel Bell, Nathan Glazer, C. Wright Mills, Robert Nisbet, David Riesman, Alan Wolfe, and many others.

I have sought to write both for the discipline and the general public from the beginning of my career, and, having started my writing as a journalist, I never thought much about doing anything else. Also, when I was in graduate school, both at Chicago and Penn, no one urged us to write in the technical journal style I call sociologese. More important, as there were few tenure track jobs back then, choosing between publishing or perishing was not yet required, and publishing in mainstream journals or with academic presses had not yet become essential routes to tenure. Besides, most of the ethnographers who were our role models in graduate school were able to report their findings and insights in clear English even as they were writing for their disciplines.

Writing for the general public became more urgent in the 1960s, when my policy and politics-oriented colleagues and I were eager to reach the public with our critical analyses of and proposals for public policy. For me, the incentive to do public sociology has never ended, originally because I thought sociological findings and ideas could counter the conservative attacks on antipoverty policy and the poor, and then because there were other policy and political issues to which sociology could make a contribution. But even as a student, I already thought that many of sociology's insights were significant enough to be shared with the general public.

My own outreach as a public sociologist has been mainly through my books, almost all of which have been published by trade presses. Except from the late 1960s to the mid-1970s when my articles appeared on the *New York Times* op-ed page and in its Sunday Magazine, my shorter work has been published mostly in smaller magazines and journals of opinion. These have included *Commonweal*, *Chronicle Review*, *Dissent*, *The Nation*, *The New Republic*, and *Social Policy*—for which I also reviewed more than 30 films during the 1970s.

In addition, like many other sociologists, I have long served as a regular supplier of quotes and sociological angles for telephoning journalists. However, perhaps unlike most others, I have also been writing to journalists when I thought a little sociological analysis could add to their reporting or when they made unfair or inaccurate comments about the discipline. As the author of articles and books on the news media, I felt qualified to write such letters. While most journalists do not respond, over the years I have maintained a periodic correspondence—now of course by email—with a number of them, including columnists and reporters in the major national news media.

Public Sociology and the Discipline

Public sociology can take many forms, but the need for sociologists to write books and articles for the general public is more urgent than ever. Given that the discipline's future depends directly and indirectly on public support, public sociology will contribute to its survival and growth.

Trying to reach the general public should help to make sociology more relevant and useful to at least some sectors of that public and of the larger society. We should especially aim to be relevant and useful to the people who most need social, economic, and political support—as well as more equality. If we can contribute sociological findings and insights to help achieve world peace and more global equality, so much the better. At the same time, we have to resist pandering and avoid overdramatizing our findings and hyping our work to appeal to media gatekeepers.

Still, we must make sure that our ability to contribute matches our good intentions to do so. Actually, aiming to produce first-rate public sociology may even help to produce more high-quality sociology. Despite the rigor of peer review, the journalistic and other gatekeepers who ultimately decide whether our sociology should become public are in some respects more demanding than our peers. They would of course like us to come up with headline-making find-

ings, but at the least they will reject work that is merely a sociological reframing of already common knowledge.

A relevant and useful sociology—and public sociology—could also give the discipline a leg up in the increasing competition between the social sciences. When I was a graduate student, we talked, somewhat pretentiously to be sure, of sociology as the queen of the social sciences, reifying it for its ability to theorize and study empirically any subject we chose. Today, that queen has lost her throne if she ever had one, for other social sciences feel free to investigate topics we thought were purely sociological.

Economists with an empirical bent analyze the same national and other large databases that were once virtually wholly owned sociological property. They and psychologists regularly run laboratory experiments on subjects once monopolized by sociologists, and anthropologists are finally doing fieldwork in the United States, thereby providing competition for sociological fieldworkers.

Last but not least there are book-writing journalists, some with at least undergraduate training in sociology and social science research methods. Although journalists working for daily and weekly media must meet deadlines that leave too little time for research, many book writers have as much if not more time for research than academic researchers. They have a better nose than we for the currently topical, and they are faster than we are in doing and publishing their work. And their prose will probably always be more lively than ours.

For Sociology

I am not overly worried by the competition and believe in fact that the discipline may ultimately benefit from it. Moreover, sociology retains a number of distinctive strengths that, if properly exploited, enable it to keep pace with at least its academic competitors. Let me mention just five.

First, we continue to be the irreverent and philosophically daring social science. Second, despite the cultural turn that the discipline

copied from the humanities, we still provide distinctive relational and structural analyses. Third, sociology has long specialized in going backstage. In an ever more scripted public sphere, we can therefore demonstrate the nakedness of various emperors and empresses better than the other social sciences. We also remain more active and effective debunkers.

Fourth, although all the social sciences depend to some extent on funding from the country's dominant institutions and other elites, sociologists continue to take an empirically grounded bottom-up perspective more often than other social scientists, subaltern theorists notwithstanding. Sociologists also study ordinary people more often, although we still con-

centrate on the poor and the victims and need to do more research among and on the affluent as well as the victimizers.

Fifth, we come closest to the people we study. Sociology conducts a larger number of interview and fieldwork studies than its competitors, even if such studies are not always as attentive to macrosociological factors as they should be. I do not mean to denigrate theorizing, historical studies, surveys, database analyses, or quantitative research, but sociology's emphasis on qualitative empirical work has distinguished it from the other social sciences, even anthropology.

If sociologists can develop these strengths further in the coming years, public and other kinds of sociology will surely flourish.

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²For brevity, articles reprinted in one of my essay collections [*People and Plans* (1968a), *People, Plans and Policies* (1992), and *Making Sense of America* (1999a)] are cited only in them.

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