



L. A. Gar

A SOCIOLOGIST'S ATYPICAL LIFE

Lewis A. Coser

Distinguished Professor of Sociology Emeritus, State University of New York—Stony Brook, and Adjunct Professor of Sociology—Boston College, Chestnut Hill, Massachusetts 02167

KEYWORDS: functions, marginality, atypical career, outsiders, insiders

Abstract

This paper depicts the career of the author as a complicated journey from European beginnings to American destinations. It emphasizes crucial turning points that finally led the author from antifascist radical intellectual to established member of the American sociological community. It attempts to show lines of divergence but also lines of continuity in his thought and contributions. This essay attempts to show the extent to which his background influenced the themes and orientations of his sociological work.

The paper may be read as a contribution to the sociology of knowledge from the autobiographical point of view. It stresses the various stages of the author's career and attempts to show the extent to which these stages are reflected in his writings and general orientations. The paper closes with a bird's eye view of the recent past and the probable future of American sociological thought.

INTRODUCTION

Throughout my career I have been something of an outsider professionally, but I also often have had the advantage of participating in the inside life of sociology. I have been actively engaged in our discipline yet I have never been totally committed to any of the various approaches that have dominated our field in my many years as a sociologist.

Beginnings

My beginnings foreshadowed my subsequent career. I was born in Berlin, Germany in 1913 and left high school shortly before the Nazis came to power in 1933. I had been active in various leftist groupings in my last years in Berlin and I was Jewish. Under these circumstances, I was told that I might soon fall into the clutches of the Nazi police or of the storm troopers. I left Germany early in 1933 to live in Paris, while my parents, who had no political involvement, stayed on, hoping like so many members of the Jewish upper strata, that Hitler's regime would soon collapse.

During my first few years in Paris I lived, like most of my comrades, in miserable and marginal ways. My parents were not allowed to support me financially outside Nazi Germany, and the French government did not allow foreign exiles to take regular jobs in the tough job market of the depression years. I worked as a travelling salesperson for several wholesalers, and somewhat later I was privileged to work as a personal secretary to a Swiss author and journalist. I had a good number of other jobs, some of which have slipped from my memory. Only after the popular front government led by Léon Blum came to power in 1936 was I finally given a working permit which enabled me to work for the French branch of an American brokerage house until the outbreak of war.

My early years in Paris I was able to study at the Sorbonne because tuition was free and attendance at lectures was not mandatory as long as one showed up from time to time and passed the requisite examinations. When I first decided to go to the Sorbonne as a student, I was by no means clear as to what I was going to study. After some flirting with modern history, I finally decided upon comparative literature mainly because I was in command of French and English in addition to my native German. I did well in my first few semesters, so well in fact that I was not surprised when one of my professors, Jean Marie Carré, asked me whether I had already made plans for a dissertation. I told him that I wasn't sure as yet but felt I might attempt a comparative study of the English novel in the Victorian age, the German novel of roughly the same time, and the French novel of the same period. My strategy would be to study the various ways in which the differing social structures of these countries influenced various patterns of growth of the novel. Professor Carré threw up his hands in horrified surprise and almost shouted: "Social structure, my friend, is not a subject of study in comparative literature, that is something to be studied in sociology." I took his advice. It is under these somewhat unusual circumstances that I became a sociologist.

French sociology in the interwar years turned out to be a somewhat narrow enterprise. At the Sorbonne, the field was still dominated by the Durkheimian school, and it was taught by men such as Paul Fauconnet and Célestin Bouglé,

who had been Durkheim's disciples and had not deviated from a straight Durkheimian path. I am still able to summarize the content of this or that chapter in, say, *Suicide*, when awakened in the midst of profound sleep. I don't wish to complain about this Durkheimian emphasis. I learned a great deal from my teachers and the various Durkheimian authors that we were required to read. But I also realized that there were a number of major sociologists from Germany and elsewhere that were simply ignored. There was a rumor that a brilliant young sociologist, Raymond Aron, had gone to Germany for an extended study period to work on Max Weber. We knew that Bouglé had once been a student of Simmel in Berlin. But Bouglé chose not to lecture on Simmel, and Aron was not yet a member of the faculty at the Sorbonne. I could have listened to Marcel Mauss at the Collège de France across the street from the Sorbonne, but I was not particularly interested in the highly technical subjects in anthropology on which Mauss lectured in those years. Halbwachs joined the teaching staff at the Sorbonne only after I had to restrict the number of hours given to sociological work when I was employed by the American brokerage firm.

The only major sociologist discussed outside the Durkheimian magic circle was Karl Marx. Not that our sociology teachers paid much attention to him, but there were active Marxist discussion groups, some led by Communist Party members, or by various other lecturers representing left-wing sects and communities. I had already been familiar with Marxian currents of thought while still in Germany, and I now studied Marxist writings with considerable assiduity. The Marxist allegiance also allowed me to distance myself from most of my professors whom I could always ignore since, after all, I considered that what they taught was "bourgeois sociology." And yet, in that lively intellectual socialist milieu, questions were being raised in discussions with my French and German friends. Special mention must be made of Henry Jacoby, a refugee from Hitler's prisons, whom I came to consider my closet intellectual companion in Europe and later in this country. He opened vistas for me that I would not have had without our long discussions over many years. He freed me from a Marxist orthodoxy which threatened for a while to rigidify my thoughts. All in all, when I arrived in the United States as a refugee in 1941, I still was an unorthodox Marxist with strong admixtures of Durkheimian thought and a somewhat vague acquaintance with German and British social scientists whom I had read in the 1930s.

Coming to America

When I came to the States just a few months before the outbreak of the American phase of World War II, I worked for various government agencies, from the Office of War Information to the Department of Defense. Toward the end of the War, I needed to focus on a personal life plan for the future.

I had once hoped to make a living as a journalist, a kind of leftist Walter Lippmann, but I soon realized that there was no chance of being successful in that line for a recent arrival on these shores. Together with my friend Travers Clement, I edited a left-wing magazine, *Modern Review*, for a while, but I soon left because of political disagreements with the financial sponsors of the magazine. I also began to write book reviews for periodicals such as *The Nation*, *The Progressive*, and *Partisan Review*, but it was rapidly apparent that this was not a way to make a living.

Rose Laub, whom I married soon after I met her, was the person who had worked on my “case” at the International Relief Association when I was a candidate for a special visa as a political antifacist refugee. Let me anticipate here and say that she has been my partner by now for over 50 years. She has had so intimate a part in my intellectual and emotional life that I find it almost impossible to sort out ideas of hers that later cropped up as mine. Ours continued to be an ideal collaboration of mind and a common sensibility that nothing could shake throughout the common joys and trials of half a century. After we had our first child, Rose decided to continue her studies, which had begun at the Ecole Libre des Hautes Etudes—a branch of the New School for Social Research. She sought advice from other young socialists in New York City, people like Seymour Martin Lipset and Daniel Bell, and they suggested that she continue her studies at Columbia. Through her I met the young Robert K. Merton, whom she much admired, and Robert Lynd, who was not intellectually stimulating but provided much needed support. Given such encouragement she applied and was accepted at the university. Because I was, as almost always, disposed to respect her judgment, I decided in 1948 also to enroll as a graduate student at Columbia.

In the spring of that year I received a call from Nathan Glazer, another member of the radical student coteries in New York, asking me whether I knew David Riesman. I had never heard of him but was told that he had been a brilliant young lawyer, the last law clerk of Justice Brandeis, and until recently he had taught at the university law school at Buffalo. Riesman had decided a short time ago to shift from law to a career in sociology, I was told, and so he joined the Social Science staff at the College of the University of Chicago. He was in New York to recruit additional staff. I expressed great interest and was delighted when Riesman, after an hour or two of intense conversation, asked me whether I was available to teach at the College of the University of Chicago. When I asked what I would be expected to teach I was told: American History. I could hardly believe my ears. Why on earth would a university in the “Wild West” hire someone from Berlin, and Paris, to teach American history to what I thought were “corn fed” youngsters reared in Midwestern small towns. (I didn’t know at the time that a high proportion of the University of Chicago undergraduates came from the East and

specifically from New York). I thanked David Riesman but told him that I would not teach American history. I was most astonished when the Dean of the College called me a week or two later to talk about an appointment at the College. I told him that I had already declined the offer to teach American History. "OK," he said, "we have simply shifted a young sociologist into the history section so that you can now teach in the social science/sociology field." Thus, just as I had shifted to sociology in France more or less by accident, so it was again by accident that I came to get my first American teaching position in sociology.

Beginnings in Sociology

My years at Chicago were exciting. While I had suffered in France from the narrow approach to social sciences at the Sorbonne, I was now exposed to a wide variety of intellectual stimuli at the Chicago College. My colleagues varied in their interests and careers. The majority were sociologists, but there were also young historians, and even a young poet. But it wasn't only the backgrounds of my new colleagues that were exceedingly varied. The materials that we were supposed to teach were also quite eclectic. We lectured on Weber's *Protestant Ethic*, and Durkheim's *Division of Labor* but, we also discussed Margaret Mead. We presented historical approaches but also structural anthropology, Myrdal's *American Dilemma*, and Freud's *Civilization and Its Discontents*. There was a surfeit of honey. What was most exciting were weekly meetings in which the whole staff participated and presented their varied views on the materials to be taught the next week. I am not sure how much my first-year students profited from my lectures and discussion groups, but I am sure that I learned an immense amount during the two years I taught at Chicago.

Nevertheless I left the University of Chicago after two years of teaching there to return to New York, to devote at least one full year to an intense study of various branches of Columbia's graduate sociology offerings. The department at the time was largely, though not wholly, dominated by young teachers who saw themselves to be on the forefront of the development of sociology and who claimed allegiance to a brand new theoretical approach: structural functionalism. Robert K. Merton, Bernard Barber, and Kingsley Davis had been students at Harvard under Talcott Parsons. Other members of the faculty, such as Paul Lazarsfeld and Robert Lynd, while not claiming allegiance to the newly developing branch of theory, were at least willing, so it seemed, to let it have a trial run in the department. With graduate students such as Suzanne Keller and Hanan Selvin, we followed in the steps of people like Philip Selznik, Al Gouldner, Marty Lipset, Peter Blau, Zena Blau, Pete Rossi, Alice Rossi, and Rose Laub Coser. Not all were orthodox functional-

ists, but all in different degrees were deeply marked by the teaching of Robert K. Merton and his colleagues.

What struck me forcefully at Columbia was the sense of working on the frontiers of sociology, building the foundations of what was sure to become the most exciting subject matter in the social sciences for a long time to come. After the staleness of the teaching at the Sorbonne, after what sometimes seemed the excessive eclecticism of the Chicago College offerings, Columbia seemed to me to have found a most productive approach.

And so this erstwhile Marxian cum Durkheimian sociologist began to ally himself with the structural-functional school. Even though I was somewhat older than most of my fellow students, I felt like a youngster who is suddenly offered a cornucopia of riches, whose existence had not even been known to him before. These were the years in which most of the Columbia staff reached or approached the height of their productivity; that saw the publication of some of the brilliant papers by Robert K. Merton, when the first structural functional text books such as Kingsley Davis's *Human Society*, were published, and when Talcott Parsons was at the apogee of his powers. Some other Columbia teachers, who were not rushing toward functionalism, nevertheless were willing to give to this approach a tolerant and benevolent hearing. It was a joy to be alive in the Columbia atmosphere in the late 1940s and the early 1950s.

I was willing to pledge allegiance to the school of functional-structural school. And yet, there remained some major reservations that prevented me from claiming full adherence to traditional functionalism. Columbia was a special place where the traditional functionalism of Malinowski and Parsons was, under the leadership of Robert Merton, subjected to critical appraisal. Soon I found myself again a heretic within the edifice of a mother church. Above all in the work of Talcott Parsons but to a degree also in the work of many of his students, one could observe a strong bias in favor of social harmony, common norms, stable equilibria, and commonly accepted frameworks of social action. This seemed rather unpersuasive an approach to someone who had seen the rise of facism in Europe, the ravages of the second World War, the Stalinist terror and murderous violence in the USSR. The world of Hiroshima and Auschwitz seemed hardly the same world as that of such benevolent liberals as Parsons and some of his first and second generation of disciples. Where in their writings were the horrors and inhumanity of the world that we had just lived through? So it came to pass that the dissertation I wrote under the guidance of Merton, and the subsequent publication of my first book, *The Functions of Social Conflict*, constituted my attempt to make social conflict the subject of a functional mode of analysis. I attempted to join the exploration of the largely tabooed subject—social conflict—to the

current stress on functional analysis. I tried, in other words, to join concern with the causes and consequences of social conflict to an exploration of its functions within specific historical or theoretical structures.

The dissertation was written under the supervision of Robert K. Merton, traces of whose thought can be found on almost every page. But it was also deeply influenced by the sociological approach of Georg Simmel. I had read a few of Simmel's writings in Paris, but at the time I had not been much impressed. The Marxian heritage had made me tone-deaf to Simmel's formal approach to sociology. But while preparing for the dissertation at Columbia, I reread Simmel and found him one of the most fascinating sociologists I had ever encountered. I was so impressed, in fact, that I proposed to Merton to make the work of Simmel the subject of my dissertation, but Merton was not encouraging. He felt that beginners in the field should devote themselves to a less ambitious subject and work on a more delimited problem. I was discouraged when I went home after my lengthy talk with Merton and was prepared to move to some completely different subject when I looked again into Simmel's *Soziologie*, most of which had not as yet been translated into English. I reread his chapter on conflict, "Der Streit"—and suddenly resolved that I would devote the major part of the dissertation to a discussion of the conflictual rather than the harmonious aspects of social phenomena. I cannot say that I was aware at the beginning how I would structure the dissertation, but I still vividly remember sitting in my study and suddenly having an intuition of what later became the dissertation and the book. Together, of course, with Merton's preponderant influence, the dissertation was written under the aegis of Georg Simmel.

This is not the place to deal in any depth with this study. Suffice it to say that the dissertation, and the book published subsequently, was my first major attempt to bring about a strange marriage between functional analysis, then the vanguard of American sociology, and Simmel's approach.

Intellectual Influences

The work of Talcott Parsons assumed an important place in my intellectual development. I had enormous respect for him and no sympathy for many of his critics who attempted to picture him as a reactionary servant of the American power elite. As a social thinker he was, of course, quite consistent with New Deal types of social thought. I felt indeed that Parsons and I, given our divergent backgrounds and personal histories, often had different opinions, but I had high respect for his work, above all his magnum opus, *The Structure of Social Action*. I told Parsons later that I was indeed often in another ideological political camp than he, but that I was in effect what the British call His Majesty's Loyal Opposition. An opposition, in other words, that refuses to

accept many of the premises of a given set of orientations but always stresses that there is a great deal of common ground between contenders in the political and social arena. Parsons smiled when I told him that, and I venture to think that he found such a train of thought much to the point I once spent a whole summer reading and rereading *The Structure of Social Action*—and I have never regretted it. Even though my first academic publication in America was a critical review of Parsons's *Essays*, I have never changed my opinion that Parsons and Merton are the two towering figures in twentieth-century American sociology. Only Erving Goffman might be considered also to have as exalted a rank among modern American sociologists.

However, I felt a gulf between the Parsonian approach to the social sciences and my own emerging sociological stance. Some though probably not all of this can be explained, I believe, by the different milieux in which we had our roots. It seems fairly obvious that a person brought up in the quiet milieu of a college president in a small Midwestern town in the twenties would tend to have a different ideological and political stance in regard to his country and its social structure from that of somebody who had grown up in the turmoil of the Weimar republic, among the stresses of a culture that was breaking down under one's eyes. Such persons could hardly be expected to share a similar *Weltanschauung*. In subsequent years, when I taught at Brandeis and lived in a Boston suburb, I came to know Parsons fairly well, though not intimately. My respect for him and his work never faltered.

I worked directly under Merton's influence and had a much closer intellectual companionship with him than I ever had with Parsons. It might be said that this was because I studied at Columbia rather than at Harvard, but I don't think that this is the key "reason." I felt closer to a teacher who had grown up under circumstances and trials and tribulations that were more nearly similar, at least to a certain degree, to my own. But above all, I was seduced—if this is the right word—by Merton's intellectual style. I held with him that the task of the new sociology that was growing in all the major intellectual centers in America should be limited to developing theories of the middle range, rather than to pursuing the chimaera of a huge structure that would likely be useless to the generations of young sociologists following the first generation of structural-functional sociologists.

I have no space to discuss here the various teachers at Columbia who influenced me at that time. Let me only mention that I was ideologically close to C. Wright Mills, and we had for a while a fairly close companionship, but I was never much influenced by him in sociological matters. The only teacher who influenced me in something like the same degree as Merton was Kingsley Davis, another young structural-functional analyst. I was never as close to him intellectually as to Merton, but my debt to him was considerable.

First Publications

My Columbia degree was conferred in 1954, and the book based on my dissertation, *The Functions of Social Conflict*, appeared two years later. The fate of the book might be of interest. When first published, it was by and large well received. I recall only one wholly negative review, and this was by a person who specialized in nasty reviews. The book, though received with respect, did not create much of a stir in the 1950s. As I recall, some 3000 copies were sold at the time. But in the 1960s things changed dramatically. The civil rights movement, the student movement, and the multifaceted conflicts that marked those years quite naturally led the young students and activists to ask what sociological work might be helpful to understand a social scene that seemed hardly understandable within the fixed categories of classical structural functionalism or any other sociological system. To be sure, Max Weber or Karl Marx was able to provide some guidelines, but could contemporary sociologists contribute to their enlightenment?

The work of C. Wright Mills was widely read in those years, but Mills had died in 1962. There were, or so it seemed to many young budding sociologists, only three recent books that not only analyzed social conflict but rejected the otherwise pervasive stance that saw conflict as a social disease, a dysfunctional and destructive element. These were Ralf Dahrendorf's *Class and Class Conflict in Industrial Society*, Max Gluckman's *Custom and Conflict in Africa*, and my own book. I don't know the sales of the other two books, but my book suddenly became a bestseller and a "must" book in departments of sociology and in neighboring departments both here and abroad. My book was translated into seven or eight languages, was a mainstay for graduate student instruction, and became one of those books that a young sociologist simply had to read. I tell this story mainly in order to point to a more general lesson. A book's success or lack thereof is only weakly related to its content; it is largely determined by whether it finds an audience and times that uphold it.

I published another book on the sociology of conflict, *Continuities in the Study of Social Conflict*, a decade later. This book, though treated with respect, never approached the sales of *The Functions of Social Conflict*, partly because it advanced ideas mainly supplementary to those in the first book, but also partly because the social scene had reverted to what was then referred to as "normalcy." In any case, I was by that time getting bored with the study of conflict. I felt that I had "had my day" and I didn't wish to be known as "Conflict Coser." I then turned to writing several books that explored sociological approaches to the world of ideas. *Men of Ideas* discussed the

relation of intellectuals of various types in institutional settings. *Greedy Institutions*, a study of institutions that “devoured” their members, was well received but didn’t have an impact comparable to that of *The Functions of Social Conflict* partly at least because it had no direct link to what was then happening on the social and political scene.

Politics and McCarthyism

Rather than itemizing still other books and papers I wrote or coauthored, I want to change gears and talk about another aspect of my writings, which, at least on the surface, has little connection with my sociological work.

As I explained earlier, I became a democratic socialist still in my adolescent years and I have never waived from this allegiance. I shed a great deal of my Marxian heritage, and I have perhaps lost much of the optimism of my earlier years, as for example the belief that the kingdom of socialism was just around the corner. But I have never abandoned my conviction that a socialist commonwealth, a social utopia, must animate our concern with the future of humanity, if we don’t wish to see the world slowly regimented and bureaucratized in an iron cage. I still believe, even though this is hardly popular at the present, that a better and more just egalitarian democratic society should be high on the agenda of contemporary political thought, recent evidence to the contrary notwithstanding.

This being the case, I asked myself, soon after coming to America, in which ways a socialist could spread his ideas even if only through relatively narrow channels. I wrote essays and reviews for such publications as *The New Republic*, *The Nation*, *Partisan Review*, *Politics*, and *Commentary*—which was not then a central organ of the neoconservatives. In those early years Dwight Macdonald, a superb radical writer and editor who published *Politics* during the war years and after, taught me whatever writing skills I possess. Those writings seemed to differ from my sociological writings, or so critics averred. And I will readily admit that there was indeed a different style and terminology in writing for the two different audiences. But I would also contend that in all my writings there are common themes that do not differ from my basic stance about the social scene as much as might at first seem the case to the more casual reader. I played two types of social roles—that of “disinterested” and “value-neutral” sociologist, and that of the social commentator on the radical left—for a good number of years without experiencing any difficulty in combining these differing commitments. But things changed rapidly in the late 1940s and 1950s with the coming of McCarthyism to the public scene.

In those dire days, the McCarthy influence held sway over a good part of American life, be it in the academy or in the forum of general public debate. In the academy, in particular, the McCarthy wave of red-baiting led not only

to some academics losing their positions, but, more frightening, it led to many academics falling silent and adopting an Aesopian language in order to escape the McCarthy attacks. To put it differently, frightening as it surely was, the impact of censorship by demagogues and political authorities was finally not as detrimental to the free play of the mind as was the self-censorship of so many timid souls. Given this state of affairs many of my friends and I found it more and more difficult to write on so-called controversial matters for liberal or left-of-center publications.

Dissent

Sitting one afternoon in the faculty lounge of Brandeis University where both of us taught, my friend Irving Howe and I expressed our unhappiness with this state of affairs. One of us threw out the idea that if the existing publications were no longer inclined to publish our writings, we should perhaps create our own magazine. After a while, I suggested *Dissent* as the title of our new venture.

We resolved tentatively to call a small gathering of political friends to explore whether they were willing to support such a magazine, be it with money or ideas for papers and reviews. To our astonishment, some 50 people gathered in New York and urged us to go ahead. The first issue of the magazine appeared in 1954. Howe and I were sure at the time that the magazine would have but a short life—we thought in terms of one to three years. Yet, to our amazement, *Dissent* turned out to have a long life; indeed, it continues still to be published after almost 40 years. The magazine profited from the contributions of a good many sociologists, but it was not a sociological publication.

I felt at the time that while it was desirable to separate social scientific writings and advocacy, there was no reason that a particular person could not play both roles and play them well. I, for one, have tried to do so. There have, of course, been occasions where the sociological soul in one's breast clashed with the socialist soul, but these difficulties have been surprisingly few.

By and large *Dissent* became a respected voice on the left, and I dare say that I often run into social scientists of various orientations who assure me that the mixture of left radicalism and social science has been over the years a quite sustainable combination. All in all, *Dissent* has been a continued source of joy and satisfaction for me. My friendship with Irving Howe and our close collaboration on *Dissent* for many decades have shaped a good deal of my ways of thinking and feeling about America and beyond. I look with equal enjoyment at the yellowed pages of *Dissent* and, say, the *American Journal of Sociology*, both of which contain contributions from my pen.

Dissent prints some 10,000 copies each quarter. This is, of course, a small

circulation compared with the mass publications, but it is doubtless read by many more people in libraries and elsewhere. I haven't written as much for *Dissent* in recent years as I have in the past. This was partly because I haven't been in New York City, where Irving Howe, Michael Walzer, and most younger editors are located, but it is also because I felt that my work for sociological audiences might at times have a wider impact than my political papers. But my somewhat reduced output in *Dissent* by no means indicates a falling off of interest.

An Overview

Over the last 40 or so years, I have been active in the American sociological community. I have served as a President of The Eastern Sociological Society and of the American Sociological Association and also served for some ten years on the Executive Committee and later the Council of the American Sociological Association. I was active in the Society for the Study of Social Problems and served as its President. This cumulation of honors is probably due to the fact that I never completely pledged allegiance to any one school of thought and therefore lay outside the fray of contenders.

The reasons for the fairly rapid decline of the functionalist ascendancy in the sixties and after have not been fully elucidated. But one of the reasons, fairly widely admitted by young neofunctionalists such as Jeffrey Alexander, has been the failure of Parsons and his disciples to come to terms on a theoretical level with the central importance of social conflict in human affairs. This being the case, I was spared some of the "poisoned arrows" directed at the functionalists for their neglect of social conflict. Even though I occasionally came under fire for having authored a book on the *functions* of social conflict, I was generally given some measure of grace because I had written a book on the functions of social *conflict*.

As readers of these pages know, the last 20 or so years have been years of turmoil, distress, and dissension in the ranks of American sociology. The dominance of structural functionalism has come to an end. But no other single theoretical tendency has taken its place, so that one is tempted to compare the current state of affairs in the American sociological field to a "Tower of Babel." One finds now a great number of different tendencies in sociology, and many spokespersons of these tendencies are now unable to understand, let alone speak, the language of their theoretical opponents. Given the incomprehension by some theorists when it comes to evaluating some of their theoretical antagonists, much of what passes nowadays as theoretical discourse is expressed, in my opinion, in the esoteric language that only the members of this or that theoretical sect understand. It is, however, futile to complain about sectarian tendencies if one's own intellectual products may also have a sectarian character in the eyes of some antagonists.

What is needed today, it seems to me, is not only tolerance for the eclecticism that dominates the sociological battlefields but also, to use a phrase of Robert Merton, disciplined eclecticism. We are now clearly experiencing a crisis in American sociology. In fact we seem to be so insecure when it comes to envisioning sociology's future that we are tempted for the first time since the days of Herbert Spencer or August Comte to hanker after foreign products said to be superior to the homegrown variety. Although, given my background, I am happy that the Age of Nativist Theorizing came to an end a fairly long time ago, and although we have learned a great deal from Juerген Habermas, Anthony Giddens, Pierre Bourdieu, and several others, we need not fall prey to a kind of inverse snobbism which ranks foreign products as necessarily superior to homegrown offerings.

Some years ago, I wrote a book entitled *Refugee Scholars in America* in which I tried to assess the contributions of refugee scholars to the work of their various American scholarly disciples. I concluded that such contributions varied considerably among scholarly and intellectual disciplines, but they surely had an immense influence on American cultural life, in general. It might not be too far fetched to compare the present enhanced influence of foreign authors to the impact of refugees in the 1930s and 1940s. In both cases, European thinkers have exercised considerable influence in the various branches of the social sciences, but they became enduring contributors only if they became part of the multistranded fabric of indigenous work.

Some special comments seem to be needed about the current ascendancy of Marxist thought in America. When one thinks of the not too distant past in which Marxism was tabooed almost everywhere on the American sociological landscape, one cannot but be delighted to see how Marx's thought has not only infiltrated but has been absorbed in American social thought. We have gained immensely from finally abandoning the nefarious "Know-Nothing" attitude with which Marxism was persecuted in the recent past. We have witnessed over the last 20 or so years a flourishing Marxist literature that is truly astonishing. Yet there is something curious in the recent Marxist renaissance in America. While Marx stressed in all his teachings the close link between thought and action, the current American scholarly Marxism is, well, very academic indeed. The writings of many younger Marxist scholars seem aimed to integrate them in the academy rather than to result in social action. In some cases, these Marxists in quest of tenure are, it seems to me, rather inauthentic persons. The key Marxian idea of false consciousness has some application in a cultural milieu, the modern American academy, of which Karl Marx surely never dreamed.

It seems that most, though not all, American sociological research now contributed by young Marxists is to be found in the long neglected area of historical sociology. In addition, a number of historical approaches in

contemporary sociology have taken Max Weber as their guiding light. This is a positive development. It seems that the mutual suspicions and misunderstanding that for long hindered a profitable commerce between the disciples of history and sociology are now breaking down. Historians now read, discuss, and absorb in their own works the labors of such sociological scholars as Emmanuel Wallerstein, Theda Skocpol, and Barrington Moore.

The Status of Theory in Sociology

This leads me to add a number of ideas to the perennial debate on the status of theoretical thought in our discipline. All too often in the past, theory and empirical research have had very little connection in the sociological realm. Empirical researchers would politely bow to theoretical contributions in the first paragraphs of their research reports but would then happily proceed on their empirical pathways without ever coming back to the theoretical propositions. On the other hand, many theorists have tended to regard theoretical propositions as of value in themselves, so that they have felt no pressing need to validate theoretical ideas in the interplay of theory and research. I believe both of these stances are pernicious and detrimental to the further growth of our discipline. May I outline, by way of a homily, what I would consider a healthy development in sociology through a constant relationship between theory and research.

When there is some trouble with your kitchen sink or with your toilet that you cannot handle yourself, you are likely to call a plumber. When he arrives he displays a tool kit in which he has packed a variety of tools that he expects to be helpful in his attempts to fix your malfunctioning appliance. Why does he bring a large kit containing many tools when all that seems to be needed are a tool or two? When asked, the plumber is likely to answer that he does not know beforehand which tool will be of use in fixing your appliance. He carries a kit with many tools because he is initially ignorant of the character of your problem. Once he has surveyed the situation he is able to choose the appropriate set of tools.

I believe that sociologists should handle the problems of the relation between theory and research in roughly the same manner as the plumber. He may upon occasion be able to develop a theoretical tool without being concerned for the moment with its application, but in the long run, a tool will be of value only if it is applied in research operations. It is the dialectical interplay between tool and research problems that is, or rather should be, the essence of the sociologist's tasks.

To take just one more example, the assessment of the sociological contributions of Emile Durkheim. One ought to recognize that had he published only *The Rules of Sociological Method*, he would not occupy a major position among the select group of our ancestors. It is largely because

he showed us the uses of his theoretical and methodological work in such contributions as *Suicide* or *The Elementary Forms of Religious Life* that we continue to honor him. In these works, he used his theoretical tools to elucidate or analyze specific problems and so he became one of our most honored founders.

Even though Merton's papers on the relation between theory and research were published a good while ago, they seem as pertinent today as they were in the past. Merton argued, at least in part, against Parsonian positions, that our major concern must be with developing theories adequate to limited ranges of phenomena and that this task will be hampered if attention is centered on grand theory in the large. What he had in mind was, of course, that overall general theories were so far removed from a given field of research that there was no way of applying them to the relatively delimited research tasks of the moment. He argued that the relation of theory and empirical research should not be regarded as a one-way street. It does not suffice to apply a general theory to a particular research; it has to be realized that initial applications of theory to a research task are likely to react back to reformulate more adequately the particular problem at hand. In other words, the relation between theory and subject matter to be explained is never a one-way street but always involves a dialectical interplay between the subject to be investigated and one or more theoretical approaches.

Conclusion

Compared to the good years of the 1940s, 1950s, and 1960s, American sociology is currently undergoing a series of lean years, but one need not be a devotee of a dialectical thought to predict that sooner or later the "fat" years will return. We have no way of knowing precisely when this will be. But I am sure that there will again be a time in the life of the sociological mind when the high hopes of several decades ago will return. One can hardly predict the future of sociology with precision, but one may nevertheless express the conviction that social science without sociology would be a much diminished field of human endeavors.