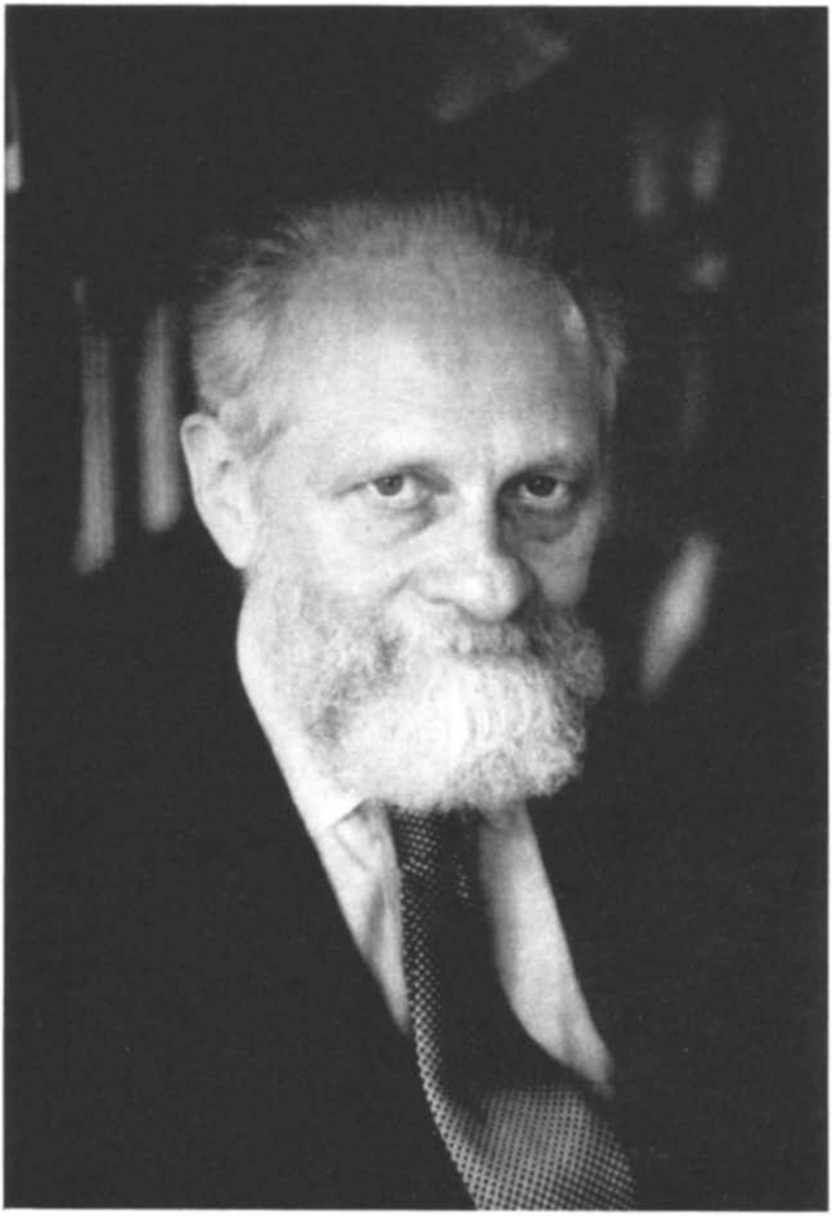

Peter M Blau

Annu. Rev. Sociol. 1995. 21:1–19

A CIRCUITOUS PATH TO MACROSTRUCTURAL THEORY

Peter M Blau
12 Cobb Terrace, Chapel Hill, North Carolina 27514

KEY WORDS: bureaucracy, formal organization, memoir research, social exchange theory

ABSTRACT

I introduce this memoir about my academic career by describing the fortuitous incidents involved in my coming to this country and becoming a sociologist. In graduate school my sociological orientation changed under the influence of Merton and Lazarsfeld from grand theories to systematic theory grounded in research. My dissertation was a field study of bureaucracy in terms of Weber's theory, which led to a book on exchange theory. Next I collaborated with Duncan on a nationwide study of occupational achievement and mobility, for which I learned regression analysis, reluctantly at first, but later becoming converted to it. During the next decade I conducted a research program on bureaucracy, specifically of quantitative studies of various types of formal organizations, from which I developed a limited organizational theory. The limitations of this theory prompted me to construct a formal macrostructural theory of population structure's influences on intergroup relations, which was subsequently tested in empirical research on the 125 largest metropolitan areas in the United States.

INTRODUCTION

My becoming a sociologist was doubly fortuitous. To go to college and then to graduate school was for me the result of a combination of incredible coincidences, and my majoring in sociology was an accident, since when I started college I did not know what sociology was.

In November 1939, I arrived in New York as an immigrant from Vienna, though not by a direct route. After the Nazis had taken over Austria in 1938,

0360-0572/95/0815-0001$05.00

I fled to Prague, where I spent a year, during which the Nazis also invaded Czechoslovakia. When I finally received my American visa in August 1939 and traveled to France to embark for the United States, World War II broke out and the ship on which my passage was booked did not sail. Thousands of people—Americans returning home as well as refugees immigrating—waited in Le Havre for passage, and this turned out to be fortunate for me. While waiting in Le Havre for news about sailing, I met and passed the time with some Americans, one of whom was the graduate of a midwestern protestant college. He told me that students at his college had collected a fund for a refugee scholarship, but for this scholarship they had no candidate. He asked me whether I would be interested. I could not believe my ears, and I did not believe him, honest as he seemed (and was), but I told him I would be very interested.

Then I was interned as an enemy alien for a couple of months in France, owing to my German passport. Before I was taken to the internment camp, the American gentleman volunteered to take most of my baggage to New York and leave it there with a friend, where I could pick it up once I arrived. When the French authorities approved my petition to let me embark for New York (another piece of good luck) and had a soldier escort me to a ship leaving for New York, I finally sailed. On arriving, I called the number of my friend's friend to inquire about my baggage and about the possibility of the scholarship ("possibility" because I hardly believed it would materialize). This man told me that I was (again) lucky because the young faculty adviser of the scholarship committee happened to be in New York for a conference and I could meet him. I did meet him (and got my baggage), and after consulting with the committee he wrote and offered me the scholarship. (Paul Lehmann—for that is who he was—later became a well-known theologian at Princeton, Harvard, and Union Theological Seminary. Paul and his wife remained my best and oldest friends until his recent death.)

I accepted the offer, of course, and went to Elmhurst College, near Chicago. I wanted to major in psychology, having been inspired about the subject by my gymnasium teacher, Fritz Redl, but the college had no regular psychology department and I was advised instead to major in sociology. I did not know what sociology was, but after exploring it I found it interesting and did major in it. In good part, my interest was the result of an excellent sociology teacher, Fritz Henssler, also an immigrant. Henssler had a law doctorate from Germany but only an MA in sociology from Northwestern (undoubtedly the reason that he could not get an appointment in a university).

My major sociological interest was in theory, but my conception of theory then was very different from what it is now, having changed several times in the five decades since. In college, I admired broad theoretical systems that provide sweeping interpretations of people and social life, theories such as those of Marx, Freud, George Herbert Mead, Cooley, Thomas, and some neo-Freudians, notably Fromm (1941), whose *Escape from Freedom* had just appeared.

My theoretical conception then differed from mine today in another dimension as well. Despite my interest in Marxian theory, my theoretical orientation was not at all structural but was sociopsychological. I considered value orientations that motivate people essential for interpreting social life, and I was critical of approaches, such as demographic studies or Watson's behaviorism, that confined themselves to objective conditions. Both aspects of my theoretical orientation are reflected in my bachelor's thesis. Numerous books and articles published around 1940 studied relations between Marx's and Freud's theories; this gave me the idea of analyzing for my AB thesis the relationship of the theories of Mead and Freud.

GRAND THEORETICAL SCHEMES AND RESEARCH-GROUNDED THEORIES

After graduating from college in 1942, I joined the army, where I spent three years, half of it in combat in Europe. Although I wanted to get an advanced degree in sociology, I had not thought I would be able to afford graduate school, but the GI Bill, providing tuition and a small living allowance to veterans, made it possible. A couple of months after my discharge I entered the sociology department at Columbia University, in February 1946. I was primarily attracted there by Robert Lynd, whose books I had read in college, and with whose progressive ideology I had great sympathy. I took courses and seminars of his and enjoyed my conferences with him, but my academic interests gradually changed in new directions.

At first I continued to pursue my major interest in grand social theory, and initially I disparaged survey research as mere fact finding. I took virtually all theory courses from Abel, MacIver, and Merton, and my reading concentrated on the classical theories. I read nearly all the major works (translated or in German) by Weber, Durkheim, Pareto, and Simmel as well as American theorists, including the first book of the man who would dominate theoretical sociology for some decades, Parsons' *The Structure of Social Action* (1937).[1] However, I also took most of Lazarsfeld's methodology courses, not only the

[1]Although I no longer consider the complex conceptual frameworks that characterize Parsons' monographs proper social science theories, I do admire the systematic and insightful distinctions his theoretical schemes make, and I consider such conceptual schemes, and specifically his, to make important contributions to the development of theories. Indeed, Parsons (1951: 536) himself notes in the concluding chapter of *The Social System* that the theoretical analysis presented "is not an attempt to formulate a theory of any particular concrete phenomenon, but is the attempt to present a logically articulated conceptual scheme."

required ones, because I was increasingly interested in research and in the sophisticated procedures Lazarsfeld had developed for analyzing empirical data.

The sociology department of Columbia University at midcentury was an exciting place to be. The intellectual academic atmosphere that permeated it engendered the feeling in many of us that we were on the verge of new developments and advances in sociology. This atmosphere had its source in a challenging faculty and an exceptionally large student body with disproportionate numbers of very bright students.[2] Many of them had been held back by the war and were now eager to make up for lost time by learning quickly and starting to make contributions of their own. The outstanding established sociology faculty was epitomized by Lynd, whose two books, written in collaboration with his wife, on Middletown (Lynd & Lynd 1929, 1937) are still classics. The faculty had recently been augmented by two young assistant professors, Paul Lazarsfeld and Robert Merton, and soon thereafter by a third, C. Wright Mills. The growing reputation of these new faculty members more than justified their appointments.

Significant numbers are necessary to produce a vigorous atmosphere in a population, and the backlog of graduate students from the war years produced this critical mass. The Columbia sociology department had substantially more than 100 students in those years, a much larger number than in preceding or following years. Merton's popular classes were filled to standing-room capacity, though they were held in a room with 96 chairs and numerous window seats. Not only were there a large number of students, there also were strong social ties integrating them, despite the inevitable competition. The many veterans among them had become emancipated from their parents and so came to Columbia without strong social ties. Most were thrown together for sociability and companionship. This created a camaraderie rooted in common academic interests and manifested in frequent discussions of academic issues. These discussions stimulated an interest in sociological questions in our regular social intercourse.

Lazarsfeld and Merton did not align themselves with the other methodologists and other theorists, respectively. Rather, they joined forces and established a coalition that made the first, and quite possibly most successful, research-cum-theory team in sociology. This cooperation of two faculty members presumably from opposite ends of the sociological spectrum had much impact on graduate students. Increasing numbers, though by no means all, of

[2]The high quality of the students is illustrated by the outstanding reputation a considerable number achieved. The two years I was in residence, Selznick, Lipset, and Gouldner were completing their dissertations; Rossi, Inkeles, Rose Coser, and Wrong were in my cohort; and the next included Lewis Coser, Coleman, Trow, and Katz.

the students became interested in combining research and theory in one way or another. Given this new direction, Merton and Lazarsfeld came to dominate the department's direction.

Merton in particular played an important role in my conversion to an interest in linking theory to research; he helped to wean me from a concern with grandiose philosophical theories remote from empirical research. To be sure, I also became greatly intrigued by the imaginative methods and procedures Lazarsfeld developed, illustrated by his elaboration scheme (Kendall & Lazarsfeld 1950). Merton influenced my change in orientation more, however, because I knew him as an impressive and insightful theorist, which made his emphasis on the need to ground theory in empirical research more effective, given my theoretical predilection.

To represent this new sociological emphasis on linking theory closely with research, Merton later coined the term "middle-range theory." I disagree with the connotations of this term, which imply a mixture of empirical and theoretical elements. Genuine theory cannot be middle range, for it must be distinct from research in two fundamental respects: Its concepts must be abstract and its propositions general, lest it be not testable since it has no new or different implications. An explanation of gang warfare or of some criminal behavior, be it rape or robbery, that deals only with that specific crime cannot be tested by making predictions for other conflicts or controversies.

Indeed, Merton's middle-range theories are really not middle range. Thus, in his article with Kitt (Merton & Kitt 1950), he uses reference group theory to explain a surprising finding from a survey of soldiers in World War II. Promoted soldiers were more likely than privates to be satisfied with the Army's promotion system, as one would expect. Independent of their rank, however, members of the military police, where promotion chances were much worse than in the air corps, were more satisfied with the promotion system than were members of the air corps—contrary to expectations.

The theoretical explanation advanced in terms of reference group comparisons, however, does not refer to some differences between military police and air corps, but to those between any groups that differ in the chances of obtaining some rewards. This is not the middle ground between research findings and theory but a true theoretical generalization, albeit one derived from research findings, yet testable with new empirical predictions, as the article itself in considerable detail indicates (Merton & Kitt 1950:43–45).

If I am correct in saying that the term "middle range" is inaccurate for precise theories as distinguished from vague and untestable ones, it is nevertheless a fruitful error. [Merton himself coined this term for Durkheim's apparently false claim that (anomic) suicide rates increase during periods of increasing prosperity as well as during those of economic depression.] The latent function of the concept of middle-range theory is to narrow the gap between theory and research and to bring them together. The collaboration of Merton and Lazarsfeld repeatedly accomplished this, not by developing specific theories about particular empirical subjects but by constructing general theories based on a series of related empirical results.

THE DYNAMICS OF WORK GROUPS IN BUREAUCRACY

Becoming emancipated from pure grand theory for its own sake surely does not mean completely ignoring it. Neither does it mean confining oneself to purely descriptive empirical studies devoid of theoretical conceptions and implications or to speculative interpretations of texts removed both from the brilliant theoretical ideas of the major classical theorists and from any confrontation of the speculative textual interpretations with empirical reality.

Ignoring the brilliant insights of the major classics is surely not what Merton taught, nor what he practiced in his own work. The issue is that one should not confine oneself to the conceptual level of the classics and merely interpret their concepts or relate them to one another. Rather, one should learn either to use their great ideas in empirical research or to refine them by drawing out their implications for new substantive problems and making their propositions more precise in the process. This is well illustrated in what is one of Merton's best known, if not his most famous, paper—"Social Structure and Anomie"—published originally in 1938.

This paper, extending a major concept of Durkheim's in new ways, was revised in published form repeatedly by Merton in the next three decades, quite aside from the numerous intervening oral refinements in lectures or "oral publications," the oxymoron he himself used. (Moreover, Merton recently [1994] published a long manuscript elaborating these extensions of his anomie theory.) Here—as well as in several other cases—Merton, far from rejecting the ideas of a classical theory, is so captured by them that he keeps returning to his own first elaboration of them to improve it and thus further refine them.

I, too, have been inspired and stimulated by classical theorists in my work, particularly by Weber, Durkheim, and Simmel, and I have often used their ideas in my research and analysis. The first major expression of this was in the choice of my dissertation topic. Weber's theory of bureaucracy aroused my particular interest, because it is the prototype of the "iron cage," Weber's term for the increasing confinement of individual freedom by the growing rationalization and bureaucratization of modern life. More generally, bureaucracy dramatically illustrates structural constraints—the limits structural conditions impose on individuals' choices and opportunities—which I consider to be the central subject matter of sociology.

I decided to conduct empirical research on bureaucracies for my dissertation, using as a conceptual framework Weber's theory of bureaucracy. A popular research subject at that time was the empirical study of work groups in industry. The best-known example is the study of several groups of manual workers in one of the plants of the Western Electric Company—*Management and the Worker* by Roethlisberger & Dickson (1946). I had taken a course in industrial sociology with Conrad Arensberg, in which studies of work groups in industry were analyzed and a method for systematically recording the social interaction among their members was presented (Chapple & Arensberg 1940). I decided to undertake an equivalent study on bureaucracy. If the operations in a factory can be clarified by studying industrial work groups, one should be able to throw light on operations in a bureaucracy by observing groups of officials at work.

Early during my observation period I noticed that the officials in the federal law-enforcement agency I first studied often discussed problems in their cases with colleagues, although every official worked on different cases and it was officially proscribed to consult anyone but the supervisor (who might refer intricate legal problems to the legal department). This intrigued me because it was similar to the prohibited practices reported among manual work groups such as restriction of output. When I studied it further, however, I found that it was quite different and far more interesting. I discovered unofficial consultations, the informal stratification system it generated, and the diverse consultation practices by which people sought to protect or improve their informal status.

The analysis of unofficial consultation and the informal status structure it produced gave me the idea for the theory of social exchange I later elaborated (1964). Even before I did so, Homans (1961) developed a theory of social exchange based on substantially different assumptions, in which he also used my analysis of unofficial consultation as the prototypical illustration of social exchange processes. Paradoxically, in my field study of bureaucracy, based on Weber's macrosociological theory of it, I had discovered the kernel of a microsociological theory.

Before either exchange book appeared, my dissertation was published after several revisions (1955), and Homans (1956) reviewed it. His review was gratifyingly favorable, but he first criticized the title (*The Dynamics of Bureaucracy*), saying that the book was a study not of bureaucracy but of small groups of officials. I had to admit that he was right. I did not study the concepts in terms of which Weber analyzed bureaucracy—large size, division of labor, administrative hierarchy, impersonal decisions—but the informal social processes and status structures in work groups. This is why a micro and not a macro theory emerged from it. But how would one study the issues Weber poses—for instance, whether the attributes he considers to characterize bureaucracy do in fact occur together in formal organizations? Answers would require data on many cases, but how can one survey many organizations if data collection on a few work groups takes a full year? This question had a sleeper effect on me. Right then I could not deal with it, as I was involved in another empirical study.

ANALYSIS OF A NATIONAL SURVEY

I was appointed assistant professor at the University of Chicago in the fall of 1953. The sociology department had just lost four senior faculty members: Burgess and Ogburn had retired, Wirth had unexpectedly died, and Blumer had accepted the chair of the sociology department at Berkeley. Everett Hughes, the new chair, rebuilt the department by hiring mostly recent PhDs, among them several from Columbia University. I was followed by Peter Rossi, Elihu Katz, and James Coleman. There were several other young faculty members, notably Otis Dudley Duncan (the only one already in residence when Hughes started to chair the department), Fred Strodtbeck, and Anselm Strauss. The diverse faculty—newcomers and old-timers, quantitative and qualitative sociologists—created a stimulating academic atmosphere, albeit one with some conflict, which, however, contributed to the vigorous climate.

My interest in Marxian theory of class differences and my socialist background were reflected in some academic interest in stratification and mobility. Although I had written only a few papers on the subject, I was a member of the International Sociological Association's Research Committee on Stratification, which in the 1950s had as its objective to encourage its members to conduct national surveys on stratification and mobility in their respective countries. Such surveys had been conducted in Britain under Glass's (1954) direction and in Sweden by Carlsson (1958); one on Denmark was in progress by Svalastoga (1965).

Despite the prominence of US scholars in survey research, however, no national survey on stratification and mobility had been carried out in this country. I was encouraged to undertake one, and I reluctantly decided to try. I was fully aware that I did not have the statistical competence to analyze a quantitative national survey adequately on my own and that I needed a collaborator who had the necessary methodological skills. I asked Duncan, who did (and who became one of the best quantitative sociologists), whether he would join me in conducting such a survey, and he agreed to do so. To carry out this large project, we needed both the cooperation of the US Bureau of the Census for data collection and the necessary financial support from the National Science Foundation.

Although completing these essentially preliminary steps took several years, we were finally successful in obtaining approval for our grant proposal and agreement from representatives of the Census Bureau to collect data for our study. These data were obtained in a self-administered supplement to their monthly "Current Population Survey" in March 1962.[3] Since the information on individuals is confidential, we did not have direct access to the data, but the Bureau of the Census prepared multivariate cross-tabulations for us from which all analysis was carried out. Duncan was very ingenious and foresightful in requesting tables that included all the data we would need for regression analysis.

The book in which the analysis of our research is published—*The American Occupational Structure*—is generally known, and there is little point in summarizing it here. Suffice it to say that our objective was to obtain data on American stratification and mobility comparable to data from other nations. We wished to ascertain major characteristics that influence differences in people's occupational achievements and in their opportunities to move up from their social origins. For this purpose, we not only analyzed the conventional mobility matrix, cross-tabulating the occupational origins and current occupations of respondents, but we also used another procedure with three new elements.

These three new statistical procedures were introduced by Duncan. First, we used regression analysis, which had been used only very rarely before in sociological surveys except in demography. Second, to use regression analysis we had to convert (detailed) occupations into a measure of occupational status that could be treated as a continuous variable. The measure used was Duncan's (1961) socioeconomic index, based on the prevailing education and income of the members of each detailed occupation. Third, Duncan (1966) introduced path coefficients, employed by Sewall Wright (1960) in his biometric work, into sociological analysis; these enabled us to trace the influences from every independent variable, via possibly various intervening variables, to the dependent variable. After our book's publication in 1967, regression and path analysis became widely used in sociological research on other topics as well as on occupational mobility.

The project was not designed to make a theoretical contribution but to provide a baseline for future trends in the American occupational structure and for comparisons of it with those in other countries. To be sure, we interpreted a number of our findings in theoretical terms. Thus, we pointed out that while we observed no vicious circle of poverty for people generally, we did see one for blacks, as indicated by their cumulative disadvantages in

[3] Glass originally suggested that I might get the necessary data free of cost by asking the Bureau of the Census to add a single question to one of their regular population surveys—occupation of father. As it turned out, the data we needed required an entire supplementary questionnaire, and the cost the Census Bureau charged for it was a large part of the entire grant from the National Science Foundation.

comparison to whites. At each step of their careers, blacks are handicapped even when they have overcome earlier handicaps. Thus, statistical analysis shows that even if blacks had the same educational opportunities as whites, as they do not, they would get less good jobs; and even if they had the same occupations as whites, which they do not, their earnings would be lower.[4] In another connection, we derive some inferences from several findings about expanding universalism in American occupational life. But such ad hoc interpretations do not contribute to a general theory that is testable in research on other subjects.[5]

The main contribution of this study, which made it so popular and influential, consisted of the methodological innovations Duncan introduced. Our long collaboration on this research, which involved repeated disagreements owing to our very different sociological orientations, also furthered progress on my own serpentine path to a macrostructural theory. Used to the Lazarsfeld tradition of statistical analysis based on cross-classifications, I was not familiar with regression analysis; initially I tried to convince Duncan that we should use cross-tabulations instead. But he resisted, and since he was more knowledgeable about quantitative procedures, I had to give in. After I became used to regression analysis, however, I was completely sold on it, owing to its ease of examining multiple influences, their polynomials, and their contingent (interaction) effects. I continued to employ it in my subsequent research, including that testing macrostructural theory.[6]

RESEARCH ON WEBER'S THEORY OF BUREAUCRACY

After we completed the mobility study, I returned to thinking about the problem Homans posed in his review when he suggested that a field study of work groups in government bureaus is not a study of Weber's theory of the formal structure of bureaucracy. To refine, let alone to test, a theory of formal organizations, information on many of them is necessary. As I carefully reread Weber's analysis of bureaucracy with this caveat in mind, it occurred to me that his theoretical scheme does not deal with the personal daily behavior, attitudes, and social interaction of officials at all. His concepts are exclusively concerned with the formal characteristics of bureaucracies: their large-scale operations, division of labor, administrative hierarchy, different subunits of

[4]I use the present tense although the data referred to are from 1962, because despite some improvements these statements are apparently still correct.

[5]Although based on a sample of more than 20,000 respondents, from the structural perspective our research is a case study of a single social structure.

[6]Ironically, at the end of his career Duncan started using log-linear procedures, which, though greatly refined, ultimately rest on Lazarsfeld's old-fashioned cross-tabulations.

various kinds and on various levels, official decisions and relations governed by impersonal rules. These conditions are assumed to promote administrative efficiency.

It dawned on me that data on all these conditions can be obtained from records and informants and do not require surveys of all or even a sample of members of the organization, let alone direct observation for months of every kind of groups of officials. (To be sure, such data would provide interesting additional information on various conditions Weber did not include in his theoretical scheme, but they are not needed for the empirical analysis of the concepts of this scheme.) To use an absurd example, to find out the number officials as an indication of scope of operations, one would not interview all of them and then count the interview schedules; one would simply obtain their number from records or informants.

The case for other data is not quite so obvious, but the same principle applies. The division of labor, the proportion of administrative staff, the number of hierarchical levels, the number of officials with different ranks, the number of subdivisions of various kinds and on various levels, the volume and specificity of written operating rules—all these and many other conditions in a bureaucracy can be ascertained from records, the table of organization, and informants. Much of this information can be obtained from a detailed table of organization—not from the official one, which is often out of date and always insufficiently detailed, but from a new one constructed by having the official one corrected and expanded in interviews with appropriate informants.

If these considerations are not completely mistaken, a research assistant or two should be able to obtain a bureaucracy's administrative structure in a visit of a few days. I decided to try to carry out such a design and thus wrote an application to the National Science Foundation for a grant to support some pilot studies. The major one was an investigation of all state Employment Security Agencies (ESA) to obtain information on their administrative structure. To my own surprise, considering the unusual research design, I obtained the grant; I also obtained the cooperation of the US Bureau of Employment Security for such a study. After some preliminary pilot studies including some at ESA regional headquarters, three research assistants visited every state headquarters of the ESA at each state capitol for a few days, collecting extensive information on the formal structure of the entire agency and much more limited data on that of its larger local offices.

Thus, we had detailed data on 53 state agencies (in addition to those in the 50 states, data were collected on three of the four other ESAs—the District of Columbia, Puerto Rico, and the Virgin Islands, but not that in Guam). We also had much sparser data on 1201 local offices of these agencies. This [53] is a small number for regression analysis, but it is essentially the entire universe of ESAs.[7] We also analyzed 387 major divisions as cases, and the analysis of local offices was based on data from 1201. During the next decade, quantitative research on a variety of public and private organizations was conducted, each project confined to a single type. Examples of the public bureaus studied are public personnel agencies and finance departments. The studies of private and nonprofit organizations included department stores, manufacturing concerns, hospitals, and universities and colleges.

I developed a theory to explain the regularities observed in this research on public employment agencies (which are also responsible for unemployment insurance). The basic findings incorporated in the theory are the following: First, the large size of organizations increases their differentiation in various dimensions at decelerating rates. This is the case whether the division of labor, vertical levels, horizontal subdivisions, or other forms of differentiation are examined. For every form of differentiation, in other words, the size of an organization is positively related to the extent of differentiation, but all these correlations are most pronounced for smaller organizations and become increasingly attenuated for those in the larger size range. In mathematical terms, the influence of size on differentiation is indicated by a polynomial with a positive main and a negative squared term.

Second, large size reduces administrative overhead (the proportion of administrative personnel), which implies an administrative economy of scale. Third, degree of differentiation, which entails greater structural complexity, is positively related to administrative overhead. Finally, large size directly reduces yet indirectly (mediated by its influence on differentiation) increases administrative overhead; but the direct negative exceeds the indirect positive effect on administrative cost; this produces the net negative effect that finds expression in the administrative economy of scale.

The theory seeks to explain why the rate of differentiation with the increasing size of organizations declines for larger organizations. The inference made is that the feedback effect of the rising administrative cost of increasing organizational differentiation, and hence complexity, with growing size are responsible. To sustain the economy of scale in administrative cost from which large organizations benefit, they must not become so differentiated that the administrative cost of complexity absorbs this economy of scale; this is effected by dampening the influence of expanded size on enhancing differentiation and complexity.

[7]I had originally thought that this ESA project was the first quantitative study of formal organizations, but I was wrong. The first one was probably Woodward's (1958) analysis of British industrial firms. Another British one was published by a group at Aston University in Birmingham, about the same time as our ESA study, in a series of articles in the *Administrative Science Quarterly*; the first of the series analyzing research (an earlier one was a literature review) is Pugh et al (1968).

This theory has been corroborated in the research on other organizations. Whatever type we examined, whether public, private, or nonprofit, large organizations were more differentiated in various dimensions than were small ones; their administrative overhead was less than that of small ones, despite the fact that their differentiation (and hence large size, indirectly) raised administrative overhead. This strong supportive evidence for the empirical findings strengthens confidence in the inference derived from them that the dampening effect of the administrative cost of complexity on the effect of size on differentiation can account for the *decline* in the rate at which differentiation increases as size does.[8]

The importance of abstract concepts clearly distinct from any empirical variables implied by them is well illustrated by this theory, as is the importance of theoretical generalizations beyond their empirical manifestations. To be sure, the theory is derived from empirical data and their relationships. But theoretical insights often emerge from empirical research, sometimes unexpectedly (what scientists refer to as serendipity) and at other times from exploratory research designed to search for new theoretical ideas. Although generalizations directly based on research findings may make some theoretical contribution, however, they also have some shortcomings, a limitation like that of middle-range theories, that is, they do not imply new empirical tests, though more abstract theoretical hypotheses inferred from them would.

The concept of differentiation is truly an abstraction from diverse manifestations of it, like division of labor, hierarchical levels and ranks, branch offices in different locations, and diverse subunits of various kinds on different levels. But the case of administrative overhead is different. Whereas it may be defined to include only a variety of staff personnel or to include also the organization's administration (its management on senior levels), in either case it refers to an empirical segment of the population and not to an abstraction. The distinctive significance of management should be noted. Only senior managers have the authority to organize the work and to order others to perform it. In short, senior managers have power over the life chances of workers, which the support staff has not.

The only thing an organization's administrative management and staff have in common is that they are not production personnel; they do not directly perform operations that contribute to the organization's objectives. A theoret-

[8]Cross-sectional studies by others of these relationships in organizations generally support our findings (based on cross-sectional studies), but studies of changes within organizations were inconsistent and did not support them. (See Cullen et al 1986, who also summarizes other studies.) Longitudinal studies that pool organizations, rather than examine their internal changes, found that growth reduces administrative overhead, in accordance with my theory, but decline in size does not raise it, contrary to what is implicit in the theory (Freeman & Hannan 1975).

ical abstraction, however, must refer to an underlying common denominator of diverse empirical variables, not simply to nonmembership in a given population segment. Surely, that one is not a ditch digger or not a Supreme Court Justice is not a theoretical abstraction. Whatever the definition of the administrative component, therefore, it is not an abstract theoretical term, but rather one of two alternative ways of defining empirically a component of the personnel of an organization.

Whereas the theory has been supported by a variety of organizations, they all were formal organizations. The reason is not merely that our research program studied only various organizations. It is also that the theory is not testable in groupings of people that have no designated administrative component. Ethnic groups, social movements, families, corner gangs, social classes, age cohorts, neighborhoods, educational categories, and innumerable other subdivisions of society can be characterized in terms of their differentiation in various respects, but they do not have administrative components, unless they establish or become formal organizations, as social movements that become political parties do.

Generalizations about diversity, inequality, or other forms of differentiation could be advanced for and tested in these other groupings, but it would not make sense to test generalizations about a non-existing administrative component. In short, the theory based on Weber's scheme is confined to formal organizations, though not to public bureaucracies, which is a limitation for a sociological theory. Can sociological theories apply to all populations? I seek to answer this question with a case in point.

A THEORY OF POPULATION STRUCTURE

Before presenting a brief synopsis of the macrostructural theory at which I have arrived on my twisted route through more than half a century of work in sociology, I want to note what I consider the function of theory to be and how my conception is related to and differs from those of the philosophers of science who influenced me most.

A theory's function is to explain some phenomena, which may be empirical regularities or themselves lower-level explanations or theories. An explanation, as Braithwaite (1953:348–49) put it, "is an answer to a 'Why?' question which gives some intellectual satisfaction." This is also true for common-sense explanations; the difference between them and scientific explanations is that the latter require greater logical and methodological rigor. Specifically, "[a] scientific system consists of a set of hypotheses which form a deductive system, that is, which is arranged in such a way that from some of the hypotheses all other hypotheses logically follow. The propositions...[are] arranged in an order of levels..." (Braithwaite 1953: 12).

Not all explanations are theories. Simpler ones merely furnish the *explicandum's explicans.* One can answer a "Why" question about a particular occurrence or regularity by specifying its cause, the antecedent that produced it, either as a necessary condition for it or as one of several influences on it. But this answer raises another question: Why does *x* cause or help bring about *y*, whether as a necessary condition or as one of several possible influences? To answer this question requires a generalization of which the *explicandum* is a particular instance or manifestation, and if the connection between these two is logically sound, it is the first step in theorizing.

Only the first step, however, because other generalizations may also logically imply the same cause-and-effect relationship. Whether there is one theoretical generalization or two or more alternative ones that can account for the original relationship, a genuine generalization has numerous implications that are entirely different from the original proposition it was advanced to explain. These new implications of the generalizations for diverse empirical relationships make it possible to test them.[9] Although this is an idealized image that rarely can be fully realized in sociology, and most of my theoretical analysis has not realized it, I believe that the macrostructural theory adumbrated below comes, at least, close.

My conception of theorizing has been much influenced by Popper ([1934] 1959) as well as by Braithwaite, although I disagree with some of Popper's criteria of proper theorizing. However, I am in general agreement with his and Braithwaite's method of falsification for testing theories and the conception of theory on which it rests. A theory is a system of logically interrelated propositions on different levels of generality, which logically imply empirical predictions that make them testable and falsifiable. Theories can never be verified, owing to their generality (even if all known tests support it, a future one may not), and possible alternative theories may explain the same phenomena, but theories can be falsified. A theory is falsified if it is ascertained that one of its empirical implications, properly derived from it, is not confirmed in research. (This statement will later be modified.) Theories that have not been falsified in repeated empirical tests are tentatively accepted as corroborated, though they may still be falsified in the future.

One criticism I have of Popper's scheme is his rigid and emphatic assertion that only deduction is relevant for scientific theorizing and that induction is entirely irrelevant (1959:27–30, 34–39). To be sure, deducing a less from a

[9]If different theories have the same empirical implications and thus explain—logically imply—them equally well, an empirical subject must be found for which they have opposite implications, which makes it possible to decide between them by ascertaining which makes the right prediction. If no such subject can be found, the theories must be considered to be essentially equivalent.

more general statement is the logically correct procedure, and inducing a more from a less general one is a logical fallacy. But how do scientists ever discover theoretical generalizations? Popper is aware of this problem but simply dismisses it by stating (p. 31) that "inventing a theory... is irrelevant to the logical analysis of scientific knowledge." Although this may well be true, it merely reveals the irrelevance of rigid epistemology for the most important task of practicing theorists, namely, how to discover new explanations and be guided in the thoughtful processes of meditation and exploration that precede formalization of theories.

Whereas there are no strict epistemological principles for deriving theoretical generalizations from various empirically observed recurrent relationships or less general theoretical propositions, nevertheless, scholars and scientists have developed procedures to help them to advance general theoretical hypotheses that subsume, and thereby explain, diverse empirical findings and limited theories on different subjects. The best illustration is reconceptualization, which involves discovering common elements in a variety of research results or narrower theories and then reconceptualizing the apparently disparate elements in abstract and theoretical terms that subsume the underlying common denominator they share. To be sure, this is easy to say but most difficult to do, which is why sciences mature slowly.

I agree with the refinement of Popper's theory by Lakatos ([1970] 1977), in which he distinguishes naive from sophisticated falsificationism. The former rejects a theory on the basis of a single falsified prediction, the latter does not. Lakatos credits Popper for having moved, at least implicitly, from a naive, in his early work, to a sophisticated falsificationism in his later work, and Lakotos's own analysis expands this change and makes it explicit. His first major point is that theoretical generalizations not only cannot be verified, neither can they be falsified [implicit in Popper's (1959: 53–54) remark that tested theories must not be rejected without good reason]. He provides several grounds for their not being falsifiable, notably that "theories are normally interpreted as containing a ceteris paribus clause" (p. 101).

Lakatos's central point is that a theory is not rejected for any falsified prediction unless another better theory has been developed. Complex theories have many diverse implications, and often most of these implications are corroborated in research while one or a few are falsified. If this occurs for competing theories, as is not rarely the case, it indicates that they are inconsistent, though it does not indicate which one is false. In this situation, Lakatos suggests, attempts are made to make them consistent by finding false assumptions or propositions and replacing them. Only when a new consistent and improved theory has been created are the old ones rejected. What are the criteria of a superior theory? It has a wider range of diverse implications than any other and thus is more easily falsifiable [as Popper (1959: 13) already stressed]; and none or few of those empirical implications have been (possibly as yet) falsified while most of them have been corroborated in research.

My last disagreement with Popper is that I cannot accept his criterion of a theoretical generalization that it must be universally applicable. To be sure, a generalization must have no exceptions, but it can be confined to one field in a discipline and not to others. Indeed, most theories are so confined; the theory of optics does not explain nuclear energy. Popper often uses as an example of a universal generalization that all ravens are black, which I consider an empirical regularity and not at all a theoretical generalization. I adopt Braithwaite's criterion for theoretical terms that they must be abstractions from empirical observations and not merely combinations of operational variables.[10]

In conclusion, we finally arrive at the macrostructural theory I have developed in accordance with the criteria of theorizing outlined above. My central interest is the influence of the social structure of a population on people's life chances, not only the opportunities in their careers but also their other opportunities, such as their chances to make certain friends or marry certain spouses. Population structures are characterized by the population distributions in different dimensions, such as ethnic distributions or occupational distributions. Three generic population distributions are distinguished: heterogeneity, the distribution among nominal categories, such as ethnic affiliation; inequality, the distribution among graduated differences, such as education or income; and intersection, which is the opposite of the degree to which differences in various respects are highly correlated in a population.

Intersection corresponds to Simmel's ([1908] 1923) concept of crosscutting social circles. All three generic differences among populations are abstract concepts in Braithwaite's sense and also in Simmel's—pure social forms abstracted from their contents. There is no heterogeneity as such, only particular empirical manifestations of it, like religious heterogeneity or diversity in national background, just as there is no competition as such, only economic, political, or some other competition. I consider Simmel's forms to be theoretical abstractions in Braithwaite's meaning of the term—theoretical abstractions appropriate for sociological analysis. All three population characteristics are emergent properties which have no counterparts that refer to individual attributes, whereas a population's mean income, for instance, is not an emergent property but describes the population by an average characteristic of its members.

[10]In Braithwaite's (1953:76) own words: "A theory which it is hoped... in the future to explain more generalizations than it was originally designed to explain must allow more freedom to its theoretical terms than would be given to them were they to be logical constructions out of observable entities."

The theory developed deals with the influence of the population structure on chances of intergroup relations, defined as the rate of dyadic relations between persons whose social affiliations differ in any respect. The two basic assumptions refer, respectively, to the dependence of social associations on contact opportunities, and to the oft-demonstrated tendency for ingroup relations to be more prevalent than outgroup relations. The theory is exemplified here by three major theorems. The first assumption and the definition of heterogeneity imply the theorem that heterogeneity promotes intergroup relations. The same assumption and the definition of inequality imply that inequality promotes intergroup (status-distant) relations. (This seems implausible, but tests support it—as shown below in note 12). Probability theory is implicated in these two theorems. The second assumption and people's multigroup memberships, which Simmel emphasized, imply that intersections of social differences promote intergroup relations.

After it had been published, the theory was tested by comparing the population structures of the 125 largest American metropolitan areas in 1970 and ascertaining, as implied by the theory, their influence on intermarriage (Blau & Schwartz 1984). This is a severe test, since as profound and lasting a relation as marriage is less likely than casual relations to be influenced by sheer probability. The tests were conducted on numerous empirical manifestations of heterogeneity, inequality, and intersection, as they influenced intermarriage.[11] With rare exceptions, all tests supported the theoretical predictions, as did tests carried out with somewhat improved procedures on the same theorems and some tests that tested different theorems, for example, those on conflict.[12]

This project intersects two major interests of mine, as an academic and as a progressive. Ever since graduate school, I have been fascinated by the effects of the impersonal social structure, like a population's sheer composition, on people's opportunities, a point illustrated by this study. As a political animal, I have been horrified by the recent growth in poverty and inequality in this country and growing ethnic strife throughout the world. The study shows that multiple diversity of a population promotes tolerance, not merely casual contacts but friendships and even marriage between persons with different backgrounds, which may well portend improving social integration of society's diverse groups and strata.

[11]Virtually all those available in the PUS of the Bureau of the Census for 1970.

[12]To answer the implicit question raised above by the implausible finding on status-distance: Although inequality in education and socioeconomic status makes status more salient and thus indirectly discourages status-distant marriage, this indirect negative effect is overshadowed by a direct positive one, which makes status-distant marriage more likely owing to the greater average status-distance between any two persons implicit in greater inequality.

Literature Cited

Blau PM. 1955. *The Dynamics of Bureaucracy*. Chicago: Univ. Chicago Press

Blau PM. 1964. *Exchange and Power in Social Life*. New York: Wiley

Braithwaite RB. 1953. *Scientific Explanation*. Cambridge: Cambridge Univ. Press

Carlsson G. 1958. *Social Mobility and Class Structure*. Lund: Gleerup

Chapple ED, Arensberg CM. 1940. *Measuring Human Relations. Genet. Psychol. Monogr. XXII*

Cullen JB, Anderson KS, Baker DD. 1986. Blau's theory of structural differentiation revisited. *Acad. Manage. J.* 29:203–29

Duncan OD. 1961. A socioeconomic index for all occupations. In *Occupations and Social Status*, ed. AJ Reiss, PK Hatt, CC North, pp. 109–38. New York: Free

Duncan OD. 1966. Path analysis. *Am. J. Sociol.* 72:1–16

Freeman JH, Hannan MT. 1975. Growth and decline processes in organizations. *Am. Sociol. Rev.* 40:215–28

Fromm E. 1941. *Escape from Freedom*. New York: Farrar & Rinehart

Glass DV. ed. 1954. *Social Mobility in Britain*. Glencoe: Free

Homans GC. 1956. Review of *The Dynamics of Bureaucracy. Am. J. Sociol.* 61:490–91

Homans GC. 1961. *Social Behavior*. New York: Harcourt, Brace & World

Kendall PL, Lazarsfeld PF. 1950. Problems of survey analysis. In *Continuities in Social Research*, ed. RK Merton, PF Lazarsfeld, pp. 133–96. Glencoe: Free

Lakatos I. (1970). 1977. Falsification and the methodology of scientific research programs. In *Criticism and the Growth of Knowledge*, ed. I Lakatos, A Musgrave, pp. 91–196. New York: Cambridge Univ. Press

Lynd RS, Lynd HM. 1929. *Middletown*. New York: Harcourt

Lynd RS, Lynd HM. 1937. *Middletown in Transition*. New York: Harcourt

Merton RK. 1938. Social structure and anomie. *Am. Sociol. Rev.* 3:672–82

Merton RK. 1994. *The Legacy of Anomie Theory. Advances in Criminological Theory*, Vol. 6, ed. F Adler, WS Laufer. New Brunswick: Transaction

Merton RK, Kitt AS. 1950. Contributions to the theory of reference group behavior. In *Continuities in Social Research*, ed. RK Merton, PF Lazarsfeld, pp. 40–105. Glencoe, IL: Free

Parsons T. 1937. *The Structure of Social Action*. New York: McGraw-Hill

Parsons T. 1951. *The Social System*. Glencoe: Free

Popper KR. (1934) 1959. *The Logic of Scientific Discovery*. New York: Basic

Pugh DS, Hickson DJ, Hinings CR, Turner C. 1968. Dimensions of organization structure. *Admin. Sci. Q.* 13:65–105

Roethlisberger FJ, Dickson WJ. 1946. *Management and the Worker*. Cambridge: Harvard Univ. Press

Simmel G. (1908) 1923. *Soziologie*. München: Duncker & Humblot

Svalastoga K. 1965. *Social Differentiation*. New York: McKay

Woodward J. 1958. *Management and Technology*. London: HMSO

Wright S. 1960. Path coefficients and path regressions. *Biometrics* 16:189–202