

Preface to the Classic Edition

Introduction

Organizations and Environments embodies three goals that have guided my writing over the past forty years. First, I have sought to write not only about theory, but also about *organizations themselves*. I have tried to address the universe of contemporary organizations—not just large, publicly traded firms. Second, I have studied the *emergence* of organizations, populations, and communities, rather than simply examining their structure. Third, I have used an *evolutionary approach* to explain the process through which new social units emerge.

In the 1979 preface to this book, I wrote:

In trying to write a book on organizational sociology for both students and colleagues, I decided there was no point in either reviewing all of the “perspectives” advanced by theorists in the past two decades or in re-creating the seemingly endless debates over measurement and method that have plagued the field. Rather, I have attempted to present a perspective that integrates concepts and research findings from all social science disciplines studying organizations, while retaining the gains made by historically and politically sensitive investigators in the United States and abroad. With a slight shift of emphasis from an original investigator’s intentions, I found that a great deal of the literature in economic history, industrial economics, the social psychology of organizations, organizational sociology, and political sociology could be integrated into an encompassing framework.

That statement still fits today: I seek an overarching framework for investigating organizational change. I use an evolutionary approach because it is a

generic framework for understanding social change. The approach is applicable at multiple levels of analysis and directs our attention to the processes of variation, selection, retention, and struggle that jointly produce patterned change in evolving systems.

The Genesis of My Perspective

The seeds of my evolutionary approach to studying organizations were planted in the 1960s at the University of Michigan, where I earned my Ph.D. in sociology. Although I majored in sociology, I minored in political behavior and social psychology, also taking courses in the economics and mathematics departments. The 1960s were fantastic years to be in graduate school at the University of Michigan, as most of the school's social science departments ranked in the top 10 in their fields, I believe. I had many opportunities to explore and learn from great professors such as Dan Katz, George Katona, Phil Converse, and Marshall Sahlins.

Courses and departmental brownbag seminars were awe-inspiring experiences, given my peers who were attending Michigan then; Rosabeth Kanter, Paul Hirsch, Bob Hauser, David Featherman, Don Black, Bill Roy, Rafe Stolzenberg, and Jeylan Mortimer were just a few of the people in the program. Not all of the action was in the classrooms. We had the first draft board sit-in in the United States; several sociology faculty participated and were arrested. Some of my friends were founding members of the Students for Democratic Society, and the Vietnam War was a palpable presence on campus.

I began working on a dissertation project that would eventually encompass a natural experiment driven by civil disorders in 1967 and April of 1968. Under the direction of Al Reiss, Director of the Center for Research on Social Organization, we had begun a study in the summer of 1966 of approximately 800 businesses and organizations in three US cities: Boston, Chicago, and Washington, DC. All three cities were heavily affected by the civil disorders. When we went back for a second wave of data gathering in 1968, I saw first-hand the turmoil that led me to title my dissertation "Organizations in a Hostile Environment." Eventually, the results of that two-wave panel study and an additional two waves became the case example of environmental instability and organizational failure in Chapter 5 of this book.

After I took a position in the New York State School of Industrial and Labor Relations at Cornell, I discovered a great deal of support for international and comparative research. Accordingly, I used a Ford Foundation grant to spend the 1975–1976 academic year in Berlin and London, trying to replicate research that I had begun in the United States. In England, I worked with several social and urban geographers on a similar three-city study. In Berlin, at the International Institute of Management, I had the chance to participate in a team organized by Alfred Keiser and John Child on a project comparing German and

British corporations. During that year spent in Europe, I made many connections with scholars in the Netherlands, France, Italy, and other countries which led to further research. For example, in 1977 Lena Kolarska at the Institute for Philosophy and Sociology, Warsaw, and I began a project on exit, voice, and loyalty that became the basis for Chapter 9.

A summer spent at Stanford University in 1973 contributed significantly to my emerging perspective on organizations. Dick Scott invited me to be the second visiting scholar to participate in the Research Training Program on Organizations and Mental Health. I taught an organizational theory course to a class that included Chuck Snow, Kaye Schoonhoven, and a number of Mike Hannan and John Meyers' students. I suspect that I learned as much over those three months as did the students in my course. Moreover, it was through the Stanford connection that I met Jane Weiss and we began a collaboration that was cut short by her premature death in 1981. Jane catalyzed my interest in a more comparative and political view of organizations, as reflected in the papers we published on class analysis (Aldrich and Weiss, 1981) and world systems theory (Weiss and Aldrich, 1977).

I wrote the prospectus for this book in May 1974, and delivered the finished manuscript in August of 1977. Over those three years, my thinking evolved in a number of ways, as seen from the change in my anticipated title. Initially, I proposed to call the book *The Organization and Its Environment*, but I realized that using the definite article (the) implied a homogeneity and singularity in which I no longer believed. Instead, I turned to plural words—"organizations" and "environments"—as a way of expressly recognizing heterogeneity and diversity.

Battle of the Coasts

The brand of organization theory that emerged from the 1960s emphasized structure at the expense of genesis and process. Thompson's (1967) book *Organizations in Action* appeared at about the same time as the Aston group (Pugh et al., 1968) was beginning to publish the results of its research. For a book with the word "action" in its title, however, actors were strangely absent. Passive verbs prevailed, and Thompson described organizations bereft of social life. Similarly, the Aston group described organizations in the language of structures, as opposed to actions. Like Thompson, they also showed no interest in how organizations came into existence. Instead, they focused on what I have called "retention mechanisms," such as formalization and centralization of authority.

In the 1970s, a paradigm shift challenged these earlier approaches, as theorists on both coasts developed distinctive ideas about organizational analysis. Looking back after almost three decades, everything now seems remarkably clear. But at the time, I don't think anybody had an idea of what would happen next. On the West Coast—in a very short span of time—resource dependence,

population ecology, and “new” institutional theory views of organizational analysis emerged. Pfeffer had begun working on resource dependence ideas when he was at the University of Illinois with Salancik, and he brought those ideas to Berkeley and then Stanford. Hannan and Freeman knew each other from their days at the University of North Carolina, and when Freeman moved from Riverside to Berkeley, they were just down the road from each other. Through his training program, Dick Scott had recruited excellent students with whom he was working out the institutional perspective, and both he and Hannan had John Meyer as a colleague. As Paul Hirsch has reminded me, the “old institutionalist” departments on the East Coast showed remarkably little interest in hiring scholars to teach about the “new institutionalist” views, and vice-versa. Matters have changed a great deal since then, however.

On the East Coast, Oliver Williamson pursued his transactions cost economics approach at the University of Pennsylvania and then at Yale. But, eventually, Williamson returned to Berkeley, where he had begun his academic career. My colleague at Cornell, Karl Weick, followed his book on the social psychology of organizing with ideas about loose coupling and enactment. Weick and I shared a number of doctoral students, and I served as his sole Associate Editor for several years when he began editing the *Administrative Science Quarterly* (ASQ). (Gerry Salancik eventually joined us.) My views were clearly influenced by him, and I think he kept me in touch with the social psychological ideas that I had first heard in my courses at the University of Michigan.

Of course, while these battling approaches were developing on both domestic coasts, additional approaches to organizational analysis were flowering in other parts of the world. As I discovered during stints at various European universities, Marxist scholars of organizational change saw organizations as a critical battleground between capital and labor. Scholars analyzed organizational structures in terms of struggle, conflict and domination, and exploitation, rather than in the neutral terms found in American organizational texts. Scandinavian approaches to organizational analysis were much more problem-focused and pragmatic, and scholars were willing to use whatever conceptual principles they could to shed light on specific issues. Differences in methodological presuppositions generated a great deal of tension between US and European scholars, and I often found myself defending one group to the other, leaving me uncertain of where I stood myself.

I believe the domestic tensions between the coasts and the international tensions of the time fostered an era of ferment that kept diversity alive in organizational studies. Despite some calls for a unified perspective and paradigm integration (or, more likely, the triumph of one approach over all others), the disparate theory groups kept their views alive through various institutional mechanisms, such as conferences, edited books, and training grants. For example, the European Group on Organizational Studies (EGOS) was founded in the early 1970s and was remarkably successful as a countervailing force to U.S. theoretical hegemony. Martin Ruef and I (Aldrich and Ruef, 2006) have

explained at length the differences between the diverse perspectives that developed in the 1970s and I refer interested readers to that treatment.

Searching for a Synthesis

In many respects, this book reflects my search for an integration and synthesis of the several perspectives on organizations that emerged in the 1970s in the United States and Europe. Although my work became identified with the “population ecology” tradition, I was actually trying to develop an approach that blended what I saw as the best features of the emerging perspectives. I was heavily involved in the genesis of the resource dependence perspective, and many of the arguments in this book explicitly draw on resource dependence principles, especially in the later chapters. I rather carelessly used the label “population ecology” interchangeably with “natural selection” throughout the book, but my views would have been better described as a sociological approach, strongly informed by evolutionary principles. I will briefly expand upon these points, indicating how I saw my views as adding value that moved beyond the perspectives on which I drew.

First, my papers in the early 1970s drew heavily upon the concept of resource dependence. For example, in our 1975 paper in *ASQ*, Sergio Mindlin and I wrote that “the major axiom of the resource dependence perspective on the study of organizational behavior is that organizations must be studied in the context of the population of organizations with which they are competing and sharing resources” (Mindlin and Aldrich, 1975: 382). Following up my earlier papers, we laid out the premises of a resource dependence model, building on Emerson (1962) and Blau (1964). Note that we combined the ideas of “populations” and “resource dependence” and thus our model was unlike that subsequently proposed by Pfeffer and Salancik.

Second, many of the explanations in this book explicitly invoked resource dependence principles, beginning with my proposal in Chapter 3 that environments can be defined as resource controllers. In Chapter 5, I made the point that “selection” is not just the working out of an impersonal invisible hand, but rather that power and domination were the keys to understanding organizational change. I wrote that we needed to understand the distribution of resources in environments and the terms on which they were available to organizations. In Chapter 11, I drew on Blau’s reworking of Emerson’s ideas concerning the association between dependence, autonomy, and power. In Chapter 12, I used resource dependence ideas to argue that organizations use interorganizational relations to manage their interdependencies. Thus, this book is thoroughly infused with resource dependence concepts and principles.

Third, in the early 1970s, guided by Paul Siegel’s comprehensive course on human ecology at the University of Michigan, I was using ideas from Amos Hawley and other ecologists to frame organizational issues in population terms.

For example, in a 1971 paper that was published in *Human Relations*, I argued that an “organization-environment” perspective would correct a common problem with traditional approaches that were “using single organizations instead of *populations* of organizations as the frame of reference” (Aldrich, 1971b: 280; emphasis added). I argued that organizational properties had been “investigated without regard to their contributions to fitness in varying or diverse organizational environments” (Aldrich, 1971b: 281-282). I championed population-level thinking at many points in this book, drawing on many of the themes I developed with Pfeffer in our 1976 collaboration (Aldrich and Pfeffer, 1976).

Fourth, although I knew about Dick Scott’s research program and John Meyer’s projects on institutional analysis, they did not figure prominently in my preparation for this book. At that time, I saw the emerging neo-institutional perspective primarily as a vehicle allowing mainstream sociologists to insinuate fundamental sociological concepts into the curricula of management departments in business schools! Weick and Williamson played a larger role in this book, as evidenced by my discussions of loose coupling, viewing environments as information flows, and market forces affecting interorganizational relations.

Thus, over the decade of the 1970s, I sought ways to integrate and synthesize the insights from work emerging on both coasts, as well as from what I was learning from my European colleagues. I also identified issues overlooked by the new views and sought ways to improve upon those perspectives. For example, neither the resource dependence nor the population ecology perspective paid much attention to organizational boundaries and organizational creation. But, in this book, I argued that we could not take organizational creation and persistence for granted. Similarly, I saw others’ views as lacking historical depth and found that they focused on the “American case.” In contrast, in this book you will find over 70 separate mentions of examples from non-US nations, with many extended examples illustrating specific points. For instance, in writing about political revolution, I devoted a page to the Communist revolution in China. I saw other views as falling short on the linkage of micro-dynamics with macro-dynamics, as they focused either on organizations *or* on populations. In contrast, I wrote about interdependence and autonomy within organizations (chapter 4), about managerial strategic choice (Chapter 6), and about the persistence and transformation of entire organizations (Chapter 8).

With regard to how to label my work, I am convinced that it is best to use the terms “evolutionary perspective,” “evolutionary approach,” or “evolutionary theory” because evolutionary thinking in the social sciences has matured and shed some of its earlier unwarranted connotations. (For more on this issue, see my discussion in Chapter 2 of the political as well as theoretical controversy stirred up by the term “evolution.”) The fundamental axiom of this approach is that explanations for organizational change must explicitly incorporate organizational properties and environmental characteristics. Neither organizational nor environmental attributes, by themselves, are sufficient grounds for building explanations.

Issues Raised for Organization Theory by the Evolutionary Perspective

Evolutionary theory provides a general framework for understanding social change. It explains patterned change in social systems, of any type, by focusing on the processes of variation, selection, retention, and struggle. Looking into the future, I've identified four interesting issues raised in this book that offer fertile grounds for further study.

Selection: Identifying Units of Selection

Chapter 5 argued that environmental selection occurs at multiple levels: subgroups, organizations, organizational forms, and possibly other levels. Chapter 8 explained why some organizational forms persisted whereas others were selected out. Today, we still face the questions of how to specify units of selection and whether we can identify the conditions under which different units of selection are most likely to be favored by selection forces (Sober and Wilson, 1998).

Evolutionary accounts rest on identifying the selecting forces that interact with particular variations to produce organizational and population change. Compared to person-centered accounts, selection arguments can seem mad-deningly indirect and impersonal. Possibly the most difficult premise to convey is that selection derives from the consequences of action, not the intentions of actors. (Indeed, this unpopular idea caused great difficulty for many reviewers of this book!) Individual differences across actors are clearly still important, as some persons are more highly skilled than others at judging, envisioning, and reshaping selection environments. Nevertheless, the consequences of action are what count. Because social scientists often overlook this feature of evolutionary models, it bears repeating.

Theorists today disagree over the relative significance of different units of selection: routines and competencies, bundles of routines and competencies, organizations, and various supra-organizational entities. In this book, I was largely silent on this question, moving across these units without comment on the difficulties involved. For example, in Chapter 4, I wrote about "tasks" and "innovations," but then subsequently gave most of my attention to organizations and populations.

In the intervening two decades, evolutionary theorists became much more conscious of the need to be explicit about what is being selected by selection forces. Using the lens of evolutionary theory, we can now identify the implicit choices made by theorists from various perspectives. For example, TCE theorists, by focusing on implied decisions involving specific transactions, have encouraged a "routines and competencies" view of what is selected. Organizational sociology has been more ambivalent in this regard. Resource dependence and institutional theorists have often treated organizations as units of analysis. However, because they have typically favored interpretations based

on adaptation rather than selection, their explanations have been very similar to organizational learning accounts: organizations change, rather than being selected out of a population. By contrast, organizational ecology has been relentless in its treatment of organizations as units of selection, although it has also encompassed studies of organizational change.

Regrettably, we have only limited empirical evidence with which to evaluate the various claims regarding the most important units of selection. The emerging synthesis of ecological and institutional theory seems to present us with socially constructed entities that constitute units of selection. However, such entities appear to lack internal diversity on which selection pressures can operate, because researchers do not give us an explicit model of how organizations are sustained, which was one of the themes in Chapter 8. Does their coherence depend on unique routines and competencies, or bundles of them? Alternatively, does it depend upon organizations' abilities to draw upon some mix of population-wide routines and competencies, with only some unique to particular organizations? Do external pressures or internal retention mechanisms sustain an organization's complexity?

Adopting an explicitly evolutionary perspective will raise such issues to the top of our research agenda. Are organizations inseparable bundles of routines and competencies on which selection forces operate, selecting entire entities? Such a view would largely negate the notion of population-level knowledge, for how could surviving organizations contribute anything to it? Alternatively, are organizations merely carriers of population-level routines and competencies, and thus temporary repositories of the real stuff of organizational evolution? From this perspective, changes within existing organizations, as well as foundings and disbandings, play a major role in evolution. Selection forces could affect the mix of routines and competencies in organizations as well as their distribution in populations.

Variation: Within- and Between-Nation Rates of Entrepreneurship

The emergence and early growth of organizations is still under-theorized and under-researched. Similarly, even though population and industry contexts are now routinely invoked as explanations for organizational phenomena, not enough work has been carried out on the genesis of new populations. In Chapter 7, I examined the conditions under which new organizations and new organizational forms are most likely to emerge and devoted most of the chapter to economic, political, and social conditions that vary across nations. My focus was mainly historical, but I spent some time on factors such as direct government support, ideological legitimation, and technological innovation. Implicit in this discussion was an assumption that rates of entrepreneurship varied across nations, but I had no reliable evidence to document that claim.

Today, research projects that cover large numbers of nations, such as the Global Entrepreneurship Project (GEM), confirm that attempts to found busi-

nesses vary more between nations than between demographic groups within nations. Possible explanations include labor market conditions, cultural values, governmental rules and regulations, and a nation's position in the global economy. Projects explicitly designed with cross-national comparisons in mind will enable us to sort out these various explanations. When nations constitute an important level of analysis, multi-national research teams are called for, and I expect that cross-national research teams will become increasingly common.

Historical Analysis and the Pace of Evolution

Historical analysis informs evolutionary models in two ways. First, theorists are concerned with the manner in which the past appears in the present. Second, theorists today build synthetic models of organizational change from the results of studies conducted over past decades. For ecological and institutional researchers, statistical models may include information from many decades. From an evolutionary perspective, how reasonable are such aggregations?

As long as retention mechanisms anywhere preserve routines and competencies, they are potentially available as raw material for evolutionary processes. For example, social movements can make salient once again a dormant or under-appreciated organizational model (Clemens, 1997). What implications does this have for organization studies? Currently, organizational researchers devote an immense amount of attention to discovering what works. Perhaps more attention should be devoted to finding seemingly worthless or irrelevant organizational knowledge. Many large organizations in the 1990s allocated some of their transformation efforts to re-engineering their processes, which meant finding and discarding routines thought no longer useful. What mechanisms in organizations and populations retain routines and competencies that are simply being held, rather than used?

The second question I posed concerns the long-term stability of retention mechanisms, within organizations, populations, and communities. Organization theory needs to make some assumptions about environmental stability so that it can claim generality for its findings. For example, what features of environments have remained unchanged for the past few decades? What features have changed? Unless we can confirm such stability, research would be of purely historical interest, in the best sense of that term. However, scholars have allocated little effort to this task.

If our research practices reflected an evolutionary sensibility, our journals would be filled with meta-analyses testing the stability of results obtained in different periods. The issue is not simply one of external validity, because the term "validity" presumes that results should be the same. By default, however, researchers seem to have adopted an assumption that empirical generalizations hold over all eras and epochs. As I noted in Chapter 7, this assumption is unwarranted.

Ecological and institutional researchers have been most active in building data sets that span great sweeps of history. Roaming over decades and inserting period effects into their models, ecologists and institutional theorists have compiled impressive empirical generalizations. Many studies, however, focus on the most basic demographic outcomes—foundings and disbandings—because of data limitations. More attention should be paid to the evolution of organizational forms or the content of organizational processes, such as in Lippmann's (Forthcoming) study of the emerging radio broadcasting industry during the 1920s.

Using an historical lens, we can raise a more general question about retention mechanisms. Have improvements in the speed and scope of information technology increased the pace of innovation to such an extent that societies are abandoning their histories more quickly than before? The rapid commercialization of electronic commerce over the past decade on an unprecedented scale has caught many established organizations and populations off guard. Although they scrambled to catch up, many have found themselves in a recurring cycle of costly adaptations to continuous technological improvements. To discover whether the pace of organizational evolution is speeding up, we need projects that systematically document the emergence of new populations. Through attending conferences and workshops over the past few years, I've learned of many projects focusing on new populations. Many of them have been designed by graduate students, giving me hope that a future book on organizations and environments will be thick with descriptions of emerging population and evolving communities.

Collective Action at Multiple Levels

In Chapters 11, 12, and 13, I described collective action, both purposive and unintentional, as a potent evolutionary force. In this respect, social evolutionary theory for organizations differs from other applications of evolutionary thinking. Most of the examples I have offered involve purposive collective action by individuals and organizations coalescing around shared interests for a limited period. I emphasized the advantages accruing to organizations that created vehicles for collective action and then used them to acquire more resources in competitive situations.

Over the past century, formal organizations have squeezed out individuals and informal organizations as significant actors. Are collectivities of organizations now beginning to squeeze out single organizations? Organizations are assuredly the central actors in all facets of social, economic, and political life. Throughout the book, I presented examples of their roles in many sectors of society, from shaping individual identities to influencing the course of political decisions. Today, over 90 percent of the population in the United States works for an organization rather than themselves, and organizations employing 100 or more people now contain over half of the workforce (Small Business

Administration, 2004). Relatively few goods and services are still home-produced, as formerly family-based activities such as recreation and leisure activities are now produced by organizations and acquired on the open market.

Single organizations may become increasingly isolated and vulnerable in the face of growing multi-organizational units. Supra-organizational entities such as alliances, coalitions, trade and professional associations, and inter-organizational networks figured heavily in my analysis, especially in the later chapters. For example, in Chapter 13, I noted the critical role the National Collegiate Athletic Association played in creating standardized conditions at the national level for college athletics. Long-term collective action by supra-organizational entities substantially complicates the problem of evolutionary analysis. Researchers constantly struggle with the problem of endogeneity in their models: which factors are external and which are internal to the phenomenon they are trying to explain?

From an evolutionary perspective, the ability of organizations to reshape their environments via collective action poses major demands on theorists and researchers. The division between exogenous and endogenous factors may only make sense in the short term or in static descriptions. Over the longer term, the cumulative effects of organized action may render such distinctions wholly artificial. Organizations are changing their environments even as they are being shaped by them. Moreover, global forces affecting organizational environments have fundamentally complicated our ability to understand and analyze organizational change.

In this book, I argued that other organizations were the key factor that organizations must take into account in their actions. In 1999, I argued that collective action by other organizations is now the key factor in shaping an organization's life course (Aldrich, 1999). Today, I would add that collective action by organizations on a global scale is not only reshaping the organizational but also the natural landscape, as well. Some of the results have been catastrophic (Diamond, 2004). Although in this book I argued that "the state must surely be the major force affecting organizational formation in the 20th century," it seems clear now that non-state actors' influence dominates world events. Whether organizational scholars will keep up with this development remains to be seen.

Acknowledgments

In the prefaces to the original version of this book and my 1999 and 2006 titles, I've acknowledged contributions from dozens of people, so I won't repeat them here. However, a few people contributed specifically to bringing *Organizations and Environments* back into print and I want to thank them. The original inspiration for making this book available again came from a young scholar with a PhD from a prestigious West Coast university, who told me that

he had been commissioned to write a chapter on an issue covered extensively in this book. When I asked him if he'd thought of using *Organizations and Environments* as a guide to the "older" literature, he seemed puzzled. It was then I realized that he'd never heard of my book in graduate school! My editor at Stanford University press, Margo Beth Crouppen, enthusiastically resurrected a project I thought was dead and pushed me to make this preface more inviting. Paul Hirsch and Martin Ruef gave me extremely valuable suggestions on what to put into the preface, and Andy Van de Ven slipped into his "Lake Wobegon" persona to provide words of encouragement.

I dedicate this book to my four grandchildren for the laughter and love they've brought into my life with Penny: Gavriel Tzvi, Jackson, Yaakov, and Yehudis.

Howard E. Aldrich

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PREFACE

I could never understand why sociologists studying organizations were apparently content in focusing on single organizations and ignoring the societal context of what they observed. Perhaps things began to go awry when organizational sociology slipped out of mainstream sociology in the 1960s and drifted aimlessly during a period of great ferment in other sociological specialties. Failing to ground their studies in a political and historical context, organizational sociologists lost touch with developments in other disciplines as well. While grand theory was superseded by historically-informed and period-specific explanations in many social scientific investigations, quite a few organizational sociologists were drawn into "systems theory" and "contingency theories" of dubious historical relevance. While European theorists were delivering body-blows to American theories of stratification, social mobility, and the role of the state in advanced industrial societies, much of organizational sociology remained curiously parochial. The promise of organizational sociology, as revealed in the work of Philip Selznick, James Thompson, and others was not entirely lost, however, and during the 1970s a renewed sense of excitement crept into the field.

In trying to write a book on organizational sociology for both students and colleagues, I decided there was no point in either reviewing all of the "perspec-

tives" advanced by theorists in the past two decades, or in re-creating the seemingly endless debates over measurement and method that have plagued the field. Rather, I have attempted to present a perspective that integrates concepts and research findings from all social science disciplines studying organizations while retaining the gains made by historically and politically sensitive investigators in the United States and abroad. With a slight shift of emphasis from an original investigator's intentions, I found that a great deal of the literature in economic history, industrial economics, the social-psychology of organizations, organizational sociology, and political sociology could be integrated into an encompassing framework. Of course, achieving such integration has its costs (as I'm sure critics of the book will point out).

From beginning to end, this book is focused on a central unifying theme: organizational change. Each chapter deals with a different aspect of the question, "Under what conditions do organizations change?" Theoretical integration across the chapters is achieved through the population ecology model of organizational change. It captures the essential features of sociology's emerging concern for relations between organizations and their environments, and stresses the inseparability of mainstream sociology and organizational sociology.

The population ecology model explains organizational change by focusing on the nature and distribution of resources in organizations' environments, rather than on internal leadership or participation in decision making. The model provides a comprehensive framework within which organizational change and persistence can be studied, calling our attention to a level of analysis often overlooked in traditional approaches. Organizations are examined as representatives of types found in populations consisting of hundreds and often thousands of similar organizations, thus using an aggregate as opposed to an individual level of analysis. Investigators using the population ecology model cannot avoid dealing with the societal context within which organizations are created, survive or fail, and rise to prominence or sink into obscurity.

Studying variations over time in organizational forms requires not only longitudinal research designs but also knowledge of historical trends and changes in political systems, modes of economic production, law, patterns of international trade, and other topics often neglected in case studies or surveys of isolated organizations. Whenever possible, I have put illustrative case material into its proper historical context. For example, I examine the historical conditions under which new forms of organizations emerged during the rise of capitalism in the West and during industrialization in the United States.

Politically sensitive issues, often ignored in previous research, are highlighted throughout the book. The "environment" in population ecology studies, properly conceived, does not refer simply to elements "out there" beyond a set of focal organizations but rather to concentrations of resources, power, political domination and, most concretely, other organizations. There is no danger of a conservative "new orthodoxy" developing in organizational sociology, as long

as investigators remain sensitive to the issues raised in this book. One chapter, for example, deals with corporate ownership and control and the exercise of economic power, and another treats organizations as prized objects of intra-organizational struggles.

I prefer textbook authors taking a critical stand toward the literature they review, and thus I have critically reviewed others' research rather than merely summarizing it. I have sought to give students an appreciation of the conceptual and methodological issues facing organizational sociology, at the same time revealing to my colleagues where I stand. Taking my cue from other books I admire, I've included a great deal of descriptive material that illustrates theoretical principles (or occasionally just adds a bit of light to an otherwise tedious point).

Simple ideas are often stumbled upon quite slowly. In retrospect, the roots of the ideas in this book were laid down about a decade ago, when I sat through courses in organizational sociology and human ecology in successive terms at the University of Michigan. Reading James Thompson and Amos Hawley back-to-back (and then going on to political sociology) was a mind-expanding experience, and the possibilities of a synthesis have intrigued me ever since. I began teaching courses on organizations and environments in the early 1970s, and finally realized—as I suppose all authors must—that only by suppressing my monumental degree of ignorance in a host of areas would I gain the courage to undertake a book of this nature. Happily, I had long since discovered that ignorance is no bar to obtaining a hearing in the sciences (as a philosopher of science explained to me, we achieve progress only by rejecting false ideas), and so I plunged ahead.

Along the way, many friends and acquaintances were implicated in my effort, and to prevent them from denying any connection with this work, I've decided to give them due credit. Many people read a first draft of the book—some were paid to, some did it out of friendship, and others shared office space with me! In addition to reading the manuscript, Charles Perrow and Mayer Zald have been conducting unofficial tutorials for me for almost a decade, and they set high standards for an aspiring author. They disagree in significant measure with parts of my argument, and I've benefitted immeasurably from their criticisms. Bob Stern, Paul Hirsch, and David Knoke made extensive comments, suggested additional references, and contributed new substantive examples.

Joyce Rothschild-Whitt and Jane Weiss heightened my political sensibilities and pointed out passages where I strayed from the objective of stressing conflict and change. John Child and I have enjoyed an extensive correspondence over the years, and his criticisms sharpened my presentation of "strategic choice" and its limitations. William Clute, Jerry Hage, Dick Hall, Marshall Meyer, Mike Moch, Bonnie Payne, and Hans Pennings also took the time to make written comments and I am grateful to all of them.

Many of the book's ideas were initially developed in collaborative writing with others. Diane Herker co-authored a paper on boundary spanning roles with

me, and Lena Kolarska and I have written several papers critical of Albert Hirschman's work. Sergio Mindlin's re-analysis of the Aston group's data formed the basis for several joint papers on interorganizational dependence, and Jeff Pfeffer and I wrote a paper explaining how and why we viewed organizational change differently (in the guise of a comprehensive review article on organizations and environments). Albert J. Reiss, Jr., originally sparked my interest in the external conditions constraining organizational autonomy, while also teaching me a great deal about social science research. David Whetten and I worked together on a multi-community study on interorganizational relations, with special attention paid to the problems of aggregation and measurement.

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