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## ORGANIZATIONAL ECOLOGY: PAST, PRESENT, AND FUTURE DIRECTIONS

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**We situate the Special Research Forum on Organizational Ecology in the program of ecological research on organizations. We begin with a broad description of organizational ecology's theoretical and empirical development based on the contents of prior collections of work in the field. We then highlight key issues facing ecological research, outline how the articles in this special research forum are linked by common threads, and discuss their contributions. We close with suggested directions for future research.**

When it rains, it pours; this adage is an apt description of developments in organization theory in the late 1970s. The three-year span between 1975 and 1978 saw the introduction of four new, influential perspectives on organizations. In 1975, Oliver Williamson published *Markets and Hierarchies*, which redefined transactions cost analysis. In 1977, John Meyer and Brian Rowan published a foundational article on institutionalized organizations, and Michael Hannan and John Freeman published their seminal article on the population ecology of organizations; both works appeared in the *American Journal of Sociology*. In 1978, Jeffrey Pfeffer and Gerald Salancik published their book on resource dependence theory, *The External Control of Organizations*.

The development of these organizational theories and the interplay between them is far beyond the scope of our introduction to the Special Research Forum on Organizational Ecology. Nonetheless, an understanding of the history of organizational ecology is necessary to appreciate the circumstances and issues leading to this special research forum. To this end, we will first sketch the broad outlines of organizational ecology's theoretical and empirical development since 1977. Thereafter, we highlight key issues in ecological research that have emerged from discussions within and outside of the specialty. We then describe the goals of the forum and common threads connecting the articles and close with suggested directions for future research.

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We appreciate the light touch of Angelo DeNisi in shepherding this special research forum to completion.

Rather than describe the development of ecological theory chronologically or topically, we have chosen to situate this special research forum by using prior similar forums as benchmarks of the development of ecological research. As collections of a number of related (and important) pieces of work in a single place, special forums are attempts to influence the development of an area of inquiry as well as to showcase its accomplishments. Special forums can be organized in journals or in edited books. To date, three collections of ecological research have been published.

### SPECIAL COLLECTIONS AS A WINDOW ON ORGANIZATIONAL ECOLOGY

The first collection was *Ecological Models of Organizations* (1988), edited by Glenn Carroll. This forum contained the results of ten empirical studies and two theoretical works. What does it tell us about the first decade of ecological theory? First, it highlights the primacy of vital rates (entries and exits). Carroll stated that in the beginning "organizational theorists were ignorant about founding and mortality processes" (1988: 2) and that during the decade preceding 1988, knowledge had increased but researchers were still "a long way from any real understanding" of either phenomenon (1988: 2). Thus one of the primary contributions of the collection was a focus on greater understanding of vital rates. Carroll argued that "the chapters in this book will move us along considerably in that quest. For the most part, they report empirical research on the topics of organizational founding and mortality" (1988: 2).

Organization-level change was, at that time, beyond the frontier of ecological theory. In his discussion of adaptation and selection, Carroll pointed out that "organizational ecology is the one new theoretical perspective that does not subscribe to the adaptation model of organizational change . . . adaptive change is not impossible, or even rare, but it is severely constrained" (1988: 2). As a consequence, from a societal viewpoint "most organizational change is the result of processes of organizational selection and replacement rather than internal transformation and adaptation" (Carroll, 1988: 2). Thus, at the end of the first decade of the burgeoning of organization theory, a clear demarcation is made between ecological theory and adaptationist theories, such as resource dependence and strategic management.

The line distinguishing ecological theory and transactions cost theory was, however, somewhat blurred. Carroll argued that "efficiency and effectiveness may drive some organizational selection processes (such as profit-making firms in competitive industries), they may be totally unrelated to others" (1988: 3). Although transactional (or another) efficiency may, in some situations, be consistent with ecological thinking, only "a vulgar interpretation of the ecological selection model holds that efficiency and ef-

fectiveness are the only criteria that might account for why one population of organizations replaces another" (Carroll, 1988: 3).

The demarcation between ecological theory and institutional theory had, at least from the ecological perspective, largely disappeared. Thus, as Carroll noted, instead of efficiency and effectiveness, "Political, social, cultural, and institutional criteria can account for many selection processes among organizations. In what may be the most convincing demonstration of this point to date, the chapters that follow pursue these types of sociological arguments with great force and attention to detail" (1988: 3–4).

A second noteworthy feature of the first collection is its methodological consistence. During the decade preceding publication of *Ecological Models of Organizations*, it was recognized that ecological theorizing required longitudinal data and models for dynamic analysis. There was a convergence on a particular technique—hazard function or rate models. Although a few of the empirical studies in the collection used time series models of the type found elsewhere, the majority utilized rate analyses. In fact, there was early convergence on a particular software package, the RATE program developed by Nancy Tuma at Stanford University (Tuma, 1980).

The second collection was *Organizational Evolution: New Directions* (1990), edited by Jitendra Singh. Although the two collections were published only two years apart, the differences are substantial. One important difference surfaced by the second paragraph of the later book. Whereas Carroll posited a focus on vital rates rather than organizational change as a distinguishing characteristic, Singh defined the central thrust of organizational ecology as "the investigation of how social environments shape rates of creation and death of organizational forms, rates of organizational founding and mortality, and rates of change in organizational forms" (1990: 11). The "frontier" of ecology had shifted so that organizational transformation was grist for the ecological mill, and three of the empirical studies in this collection involved analyses of organization-level change, including changes in product-market strategy.

However, the title of the book reveals a more substantial difference. Instead of examining how "changes in organizational populations are largely attributable to how environmental conditions influence the demographic processes of entry and exit in populations," the book "emphasizes organizational evolution, a broader theme" (Singh, 1990: 11). The nature of the empirical studies in the collection and the relative proportions of empirical and theoretical work illustrate this shift in focus.

The empirical studies showcased in the collection are still primarily focused on organizational demographics, analyzing differential entry and exit rates. However, compared with the studies in the first forum, these show greater variability in content and method. The variability in content stems from an emphasis on alternatives to established models—these included a refined risk set alternative to the liability-of-newness model and mass dependence as an alternative to the density-dependence model—and from aug-

mentations of established models (such as the addition of technological and market dynamics to population dynamics). The variability of method is seen in the use of simulation techniques and the reintroduction of conventional methods like tabular analyses and goodness-of-fit tests.

A more visible shift in focus can be seen in the increased proportion of theoretical work to empirical work; not including commentaries, close to half the chapters in this second collection are theoretical. With the benefit of hindsight, we believe that the increased emphasis on theoretical development was a necessary consequence of the shift from a purely ecological perspective to an emphasis on evolution. This shift can be seen in the nature of the theoretical pieces, which concern speciation, strategy making, community ecological, and evolutionary economics. At the time of the second special forum, few of the established models in organizational ecology provided a theoretical base for these new directions.

The shift to evolution from ecology was greatly facilitated by a shift in participants. On the surface, there was a substantial overlap of authors between the Carroll and Sinah volumes: of the 22 authors in the second collection, 10 had contributed to the first collection. However, a number of writers in the second book were not established ecologists. Economists, scholars from strategic management, and even a physicist contributed, whereas the majority of authors in the earlier volume were sociologists, many of them students of Michael Hannan or John Freeman.

The boundary between organizational ecology and institutional theory had shrunk by the time of the first special collection. By the time of the second, many of the boundaries separating ecology and other areas of organization studies had faded as well. The relationship between adaptation and selection had been incorporated as an important theoretical issue. This produced a potential overlap of ecology with not only transactions cost theory and resource dependence theory, but also with strategic management.

The third special collection was *Evolutionary Dynamics of Organizations* (1994), edited by Joel Baum and Jitendra Singh. Baum and Singh pointed out that "the principal focus of this volume is the hierarchical nature of organizational evolution . . . . Accordingly, the four main sections of the book address *intraorganizational*, *organization*, *population*, and *community* evolution" (1994c: vii). The second collection introduced a shift from ecology to evolution, but the third collection elaborated evolutionary thinking and began a serious attempt to integrate processes at different levels of analysis.

The first shift in focus involved an increase in theory over empiricism. The elaboration of evolutionary work also meant giving attention to theoretical issues, particularly in the newest areas of research. Sections three (organization) and four (population) of *Evolutionary Dynamics* were predominantly empirical, with seven of nine chapters devoted to empirical analyses of one kind or another. These sections were devoted to core areas with very well developed models and problems. Section five (community) contained five chapters, three of which were theoretical. Community-level

evolution had received some attention in both of the prior special collections (and in the wider stream of research). The newest addition to evolutionary theory was intraorganizational evolution. This section contained three chapters: two were theoretical, and the other was a simulation.

The contributors to this 1994 special collection were both more numerous and more diverse than the first two collections' contributors. More than two-thirds of the 1994 contributors had worked in areas other than organizational ecology, including evolutionary economics, strategic management, institutional theory, and the management of technology and innovation. The extension of the frontiers of organizational ecology was clearly accompanied by a broadening of the affiliations of the people involved.

What does our examination of earlier special forums suggest? Organizational ecology was initiated at the same time as three other influential theories of organizations, and the early emphasis was on differentiating ecological theory not only from its predecessors but also from its competitors. As Carroll pointed out in the first special collection discussed here, "Organizational ecology is the one new theory that does not subscribe to the adaptation model of organizational change" (1988: 2). A focus on vital rates was the distinguishing feature of ecology, and most efforts were devoted to theoretical models of vital rates and the empirical testing of these models. Longitudinal analysis of vital rates produced methodological demands (techniques for analyzing rates of events over time) on ecology unlike the demands faced by the competing theories, and ecology was soon distinctive methodologically as well as substantively. This combination of distinctive theory and method led Carroll to argue that, compared to the other three theories, organizational ecology showed the greatest generality and that "advocates of the other theories would do well to take heed" (1988: 6).

However, by the time of the first collection, there were also considerable differences of opinion and emphasis within ecology, revolving primarily around institutional theory and levels of analysis (Carroll, 1988: 6). Ecological theory had incorporated portions of institutional theory, albeit only as determinants of vital rates. This produced differences of opinion about the relative roles of competition and institutional constraints as determinants of vital rates. There was also divergence about how much emphasis to place on the community and, to a lesser extent, organizational levels of analysis.

Domain defense soon turned into domain expansion. Although the research program of organizational ecology had been successful, other theories had also generated bodies of work demonstrating their efficacy. The main barrier to expansion was the exclusion of organization-level transformation as a significant source of change in the composition of organizational populations. This barrier could be removed by a theoretical elaboration: the theorem excluding adaptation is replaced by hypotheses on the *relationship* between adaptation and selection. These hypotheses require greater attention to theoretical development (which occurred in the second collection) and, we would argue, substantive interaction with scholars from other domains, which also occurred.

A second change was the increasing attention paid to community ecology. The barrier to expansion in this domain was less theoretical than empirical. An appreciation for community dynamics had long existed in organizational ecology, and some theoretical models were available as foundations for empirical analyses. However, the data requirements were imposing. It is difficult enough to gather comprehensive longitudinal data on a single population of organizations. The task of collecting such information on multiple, interacting populations is intimidating. It is not surprising that most work has been (and continues to be) theoretical.

The most recent collection shows the beginnings of a division of labor. The broad focus is on evolutionary change across multiple levels of analysis, with much attention given to integrating processes at different levels. Each level continues to develop on its own, with researchers' paying attention to core problems. Thus, population ecology continues to develop and test models of vital rates, but not in isolation from the organization and community levels.

### KEY ISSUES IN ORGANIZATIONAL ECOLOGY

The impressive volume of research conducted and published since Hannan and Freeman's (1977) foundational piece on population ecology has been accompanied by critiques of ecological research by sociologists outside the specialty of organizational ecology and reflective discussions of gaps in the field by organizational ecologists.

Early criticisms of ecology centered around its neglect of powerful organizations (Perrow, 1986), but those criticisms have become obsolete as ecologists have modeled the fates of powerful organizations by analyzing size dependence in death rates (Carroll, 1984; Wholey, Christianson, & Sanchez, 1992), dominance in technological systems (Barnett, 1990), and size-based segmentation of populations (Amburgey, Dacin, & Kelley, 1994). Young (1988) assailed organizational ecologists for using unclear definitions, weak measurement, and a narrow focus on competition and concluded that population ecology was not a useful paradigm. However, as Brittain and Wholey (1989) noted, ecological research has not only benefited from systematic replications in diverse populations, but also from broadened models of competition. Pfeffer (1993: 613) opined that, of the various specialties in organization studies, population ecology has the highest degree of consensus; he pointed to the consistency in methods dependent variables, works cited, and valuation of problems across studies.

More recently, neoinstitutional theorists and organizational ecologists have joined together in critical assessments of the program of research on density dependence in organizational populations. At the same time, ecological researchers have also reflected on gaps in the study of organizational foundings, mortality, adaptation and selection processes, and the problems posed by diversified organizations. Below, we outline each of these issues to

set the stage for delineating the contributions made by the articles in this special research forum.

### **Density Dependence**

Much like the Phillips curve showing the inflation-unemployment trade-off in macroeconomics, density-dependent models of founding rates and death rates have inspired considerable debate. Drawing on neoinstitutional models according to which sheer prevalence makes social arrangements taken-for-granted, Hannan (1986) originally proposed that legitimation and competition underlie a U-shaped relationship between population density (the number of organizations) and the failure rate of organizations and an inverted U-shaped relationship between population density and the founding rate of organizations. Studies of several industries have supported the predicted relationships between population density and failure and founding rates (Hannan & Carroll, 1989; see Hannan & Carroll [1992] for a detailed review).

Neoinstitutionalists such as Zucker (1989) have questioned whether density is a credible proxy for cognitive legitimacy and urged the use of more proximal surrogates. Barnett and Amburgey (1990) pointed out that density dependence presumes that all organizations have an equal impact and urged the use of mass dependence to account for the impact of larger organizations. Peterson and Koput (1991) simulated density dependence in death rates and suggested that it is an artifact of unobserved heterogeneity. Delacroix and Rao (1994) noted that empirical support for density dependence in death rates was weaker than the support for density dependence in founding rates, and they urged that density effects be unbundled because they mix the impacts of track record, vicarious learning, and the development of an infrastructure.

Building on the idea that the ability to make use of institutional infrastructures influences mortality, Baum and Oliver (1992) reported that that relational density—the number of formal relations between the members of a population and key actors in the population's environment—diminished death rates and increased founding rates. In related analyses, Baum and Singh (1994a, 1994b) differentiated between overlap density and nonoverlap density to measure potential competition and potential mutualism. They reported that overlap density dampened foundings and increased death rates, whereas nonoverlap density increased foundings and diminished death rates. More recently, Baum and Powell (1995) suggested that if an institutional ecology of organizations is to be cultivated, ecologists need to use non-density-based alternatives to incorporate the effect of sociopolitical legitimacy.

These critical assessments have also inspired rebuttals in defense of density-dependence theory. Carroll and Hannan (1989) defended density as a proxy for the prevalence of an organizational form because it had the



advantage of generalizability. In a rejoinder to Peterson and Koput (1991), Hannan, Barron, and Carroll (1991) simulated density dependence in death rates and reported that under controls for age dependence, unobserved heterogeneity produced spurious density dependence in three out of ten simulations. In a reply to Baum and Powell, Hannan and Carroll (1995) proposed that sociopolitical legitimacy may not be an exogenous variable but can be a consequence of population processes, and they defended density-based approaches to legitimation as providing a general and parsimonious account of vital rates.

The debate about density dependence has directed attention to the measurement of legitimacy; however, one issue that has received less attention is the distinction between the legitimacy of a form and the legitimacy of individual organizations. The density-dependence thesis makes cognitive legitimacy a collective good that is freely accessible to all organizations within an industry rather than an organization-specific endowment in the form of reputation that is inaccessible to rivals. Rao (1994) showed that victories in road races enhanced the survival of automobile producers after controlling for density and interpreted this effect to mean that victorious organizations enjoyed a higher status and were able to benefit exclusively from the Mathew effect (that is, they were rewarded disproportionately because of higher status). However, this study spanned only the early history of the automobile industry, from 1885 to 1912, and it could not test the density-dependence thesis, which requires data on a form over its complete life history.

One useful way to extend ecological research on density dependence is to simultaneously assess the effects of form-level legitimacy stemming from density and organization-level reputation. Do low-status organizations with poor reputations die at a faster rate than high-status organizations, even if they are protected by form-level legitimacy? Does the reputation of high-status incumbents deter foundings even when form legitimacy is on the rise? Alternatively, does the reputation of individual organizations generate spillover effects that are beneficial to all instances of a form? Research on these and other related issues is sorely needed to illuminate the boundary conditions of density dependence.

### **Organizational Foundings**

The proliferation of ecological analyses of foundings (see Aldrich and Wiedenmayer [1993] for a review) has also occasioned reflective discussions of gaps by ecologists. One limitation of ecological research on foundings is that it understates organizational diversity because it includes only the outcomes of successful founding attempts and overlooks unsuccessful founding attempts (Delacroix & Carroll, 1983). Owing to the dearth of data on preorganizing processes, organizational ecologists rarely distinguish successful events from nonevents in the founding process. Instead, ecological

researchers concentrate their attention on the times between the appearance of operational start-ups—that is, successful new entities that begin to produce goods and services. A sample selection bias ensues because many emerging organizations fail before they start operations: some potential founders fail to incorporate, and newly incorporated entities may be unable to commence production (Aldrich, Rosen, & Woodward, 1986; Hannan & Carroll, 1992). Hence, organizational diversity is understated because selection processes winnow out emerging organizations and lead to the retention of a few operational start-ups. Moreover, selection pressures may be more intense for emerging organizations than for operational start-ups because emerging organizations may lack formal goals, clear boundaries, and unambiguous technologies.

Ecological researchers seldom differentiate the subprocesses of announcement, incorporation, and operational start-up or analyze differences in their causal structure (Hannan & Freeman, 1989: 148). Consequently, they model the arrival of operational start-ups (successful foundings) as an event recurrence process and represent it as a point process; a timed counter of events with the set of integers representing the state space of the process. Since each value of the counter of events is a state, the overall rate of the occurrence of the event is the object of study, and time dependence between events is treated as a nuisance function.

However, the founding process can also usefully be described as a state transition process. Unlike an event recurrence process, a state transition process has a small state space; as a result, each state is substantively interesting, and transitions between states become the objects of study. For example, potential organization builders with prototypes can incorporate and then start operations; alternatively, they can start operations and then incorporate. Incorporation entails the establishment of a corporate actor and increases the ability of entrepreneurs to act as one body despite fluid membership. Operational start-up means that a new organization has commenced the production of goods and delivery of services to its customers and therefore implies that a founding attempt has come to fruition. A few studies have usefully analyzed the transition from incorporation to operational start-up (Roberts & Hauptman, 1987; Schoonhoven, Eisenhardt, & Lyman, 1990), but incorporation can also succeed operational start-up in a given industry.

Because of the dearth of research on the subprocesses of incorporation and operational start-up in single organizational forms, researchers know little about the antecedents of successful organizing attempts. We do not know much about the relationship between the duration of the gestation or preorganization phase and the rates of incorporation and operational start-up. Moreover, we also do not know if delayed incorporation dampens operational start-up rates. Similarly, there is little evidence on whether delayed operational start-up diminishes incorporation rates. Furthermore, we do not know how lateral entries by established organizations influence the speed of incorporation and operational start-up. Research on the structure of the

founding process can also direct attention to the impacts of prior start-up experience, prior social ties among founders, and the social capital of founders on the pace and success of organizational-building efforts (Eisenhardt & Schoonhoven, 1990).

Including potential organization builders, incorporations, and operational start-ups as actors in the drama of founding can shed light on the microfoundations of ecological research and address the following questions: Is the macrolevel model of density-dependent legitimation and competition compatible with a rational actor model of entrepreneurs? Do macrolevel processes of legitimation and competition affect entrepreneurial beliefs about the viability of ventures? Is the density-dependent selection of emerging organizations a special instance of how environmental feedback shapes the actions of decision makers? The detailed study of these issues is essential to expand knowledge of organizational diversity and shift attention from entrepreneurs to the organization-building process as an object of sociological inquiry.

### **Organizational Mortality**

Despite numerous ecological analyses of organizational death relying on diverse populations (see Baum [1995] for a review), researchers' understanding of dissolution, be it through merger, absorption, or outright failure, is limited by the dearth of studies that treat financial performance as a predictor of mortality. Do poorly performing organizations tend to persist, as Meyer and Zucker (1989) argued, or do they fail faster than others, or do their desperate tactics precipitate the failure of proximal organizations? Some writers have suggested that poor performance excites a spiral of high risk taking and poor performance and noted that unpredictability of a firm's income stream jeopardizes the explicit and implicit commitments of an organization to its stakeholders (Bowman, 1984; Bromiley, 1991). Therefore, an issue deserving attention is whether risk jeopardizes survival and how it interacts with performance and constrains the life chances of organizations.

Ecological research on mortality has tended to overlook how existing organizations are relationally embedded in social networks; exceptions include the studies by Miner, Amburgey, and Stearns (1990) and Baum and Oliver (1991) on the impact of interfirm links on death rates. Board interlocks knit top managers of companies into an intercorporate network (Pfeffer & Salancik, 1978), and firms can be connected to each other through strategic alliances and joint ventures. A testable proposition meriting scrutiny is whether centrality in a network enhances autonomy and confers survival advantages to organizations (Pfeffer, 1987). However, structural embeddedness, as Granovetter (1985) noted, need not always promote social order; it can also generate disorder. Therefore, social ties can also serve as conduits for the transmission of hostile influences and hasten death.

In a related vein, the death of network organizations is also a topic that has received scant attention in ecological research. Organizations may belong to a corporate network, such as a Japanese *keiretsu*, or be controlled by one business family, as in a South Korean *chaebol* or a Taiwanese *jituanqiye*. Unlike individual organizations that fail by liquidation or absorption, enterprise groups cease to exist when the links between the central unit and constituent organizations are severed. The linchpin organization may be forcibly closed, reorganized, or dissolved, and the constituent organizations may drift apart to join other groups or form new networks. Comparative ecologies of the decline of different enterprise groups could help to delineate the boundary conditions of the mortality of network organizations. For instance, one could compare the failure of the Kaiser group in the United States with that of the Suzuki family of organizations in Japan. A related issue of interest concerns the fates of constituent organizations after the collapse of the central organization in a business network. Constituent organizations may cohere to form new groups, be attached to other enterprise groups through force or consent, or cease to exist. At least two questions emerge: When are such organizations likely to be attached to other groups? When are they likely to form a new network organization? Research on these issues can extend our knowledge of organizational birth trajectories and affiliative dynamics.

### **Adaptation and Selection**

A central issue in organization theory concerns whether organizational change consists of organizations' adapting to their environments (Lawrence & Lorsch, 1967; Thompson, 1967) or whether instead organizations are inert and unable to undertake reorganizations of goals, authority, technology, and market segments (Hannan & Freeman, 1984). Ecological research on organizational change has analyzed the effect of interorganizational links on the rates and effects of change (Baum & Oliver, 1991; Miner et al., 1990). Studies have also assessed the dynamics of the propensity to change and the dynamic effects of change on the survival prospects of organizations (Amburgey, Kelly, & Barnett, 1993). Extant studies contrasting adaptation and selection have tended to neglect the sources and effects of changes in goals, authority, and technology on the life chances of organizations and their financial performance (Barnett & Carroll, 1995). Moreover, although a commonplace proposition in organization theory is that organizations learn from experience, there is also a dearth of research on how organization- and population-level learning processes facilitate adaptation and diminish mortality.

If organization-level analyses routinely treat change and death as competing risks for individual organizations, the rise of network organizational forms makes it necessary for ecologists to model change and foundings as competing risks. As microcommunities, network organizations can be viewed as incubators for creating new organizations and adapting existing

subunits. Hawley (1986) suggested that functional differentiation in communities can come about by changes in existing units or the addition of new units. Hence, mutual fund families can either change the objectives of existing individual funds or create new funds. Similarly, a business unit of a Japanese *keiretsu* or a South Korean *chaebol* can diversify into an area; or alternatively, a new business unit can be established to secure a beachhead in a new marketplace. In such cases, adaptation and foundings become competing risks for the network organizational form. It is also possible that an existing unit can enter a new niche and the entry can be spun off into a separate entity; thereby, adaptation can precede spin-off.

Because of their focus on organization-level events, organizational ecologists have not devoted much attention to whether industry evolution operates through replacement of one unchanging organizational form by another or through mutation of the members of one organizational form into another (Haveman, 1995a; Haveman & Rao, 1996). Prior researchers have examined the founding, transformation, and failure of individual organizations without determining the impact of these processes for entire industries. Studies that examine how microlevel events (foundings, transformations, and failures) cumulate into a macrolevel process (the replacement of one form of organization by another) are essential to determine how change in industries comes about. Such analyses are also essential to fulfilling the broadest goals of organizational ecology research—understanding “how social conditions affect the rates at which new organizations and new organizational forms arise, the rates at which organizations change forms, and the rates at which organizations and forms die out” (Hannan & Freeman, 1989: 7).

### The Diversified Organization

The emergence of large, diversified firms is a well-established change in the world of organizations that presents a substantial challenge to organizational ecology. As Hannan and Freeman (1988: 9) pointed out, the crucial element in population ecology is the population of organizations, and the first assumption is that organizational populations can be defined so that they have a unitary character: the members must have a common standing with respect to the processes of interest. One way to express this problem is to ask the question, To what organizational population does the General Electric corporation belong?

One technique for addressing diversified organizations is to examine mixtures of freestanding organizations and subunits of diversified organizations. Although such examinations can certainly reveal interesting phenomena, they are not without problems. On the face of it, such an analysis is the organizational equivalent of defining all organisms that eat grass as belonging to the same population. If, in fact, subunits of diversified organizations and freestanding organizations do not have common standing with respect to the processes of interest (creation, dissolution, and transformation), then this

practice does not withstand the first assumption of the theory. Unfortunately, there are theoretical and empirical reasons to believe that subunits and organizations do not have common standing (Freeman, 1990).

The question of common standing aside, this approach involves a version of methodological individualism; in effect, its assumption is that the diversified organization is a congeries of essentially independent elements. Thus, all relevant information can be obtained through matching subunits with freestanding populations. Although this may be an accurate representation for some diversified organizations (e.g., conglomerates based on unrelated diversification), it ignores the interrelated value chains and economies of scope used by others.

If a piecemeal inclusion of diversified organizations into ecological analysis is problematic, what about defining a population that consists of diversified organizations? This approach is also not without problems. On the face of it, it is the organizational equivalent of defining all omnivores as belonging to the same population. What level of diversification constitutes a birth into this population? Do organizations of equal diversity but unequal size or scale have common standing with regard to vital processes?

Organizational ecology has produced significant advances through the analysis of populations of organizations that can be easily differentiated from others and in which common standing with regard to vital processes can be demonstrated. It can be legitimately argued that this approach is very broadly applicable; there are undoubtedly more independent restaurants than there are *Fortune* 500 firms. Nonetheless, large, diversified firms are an important part of the world of organizations. A way to incorporate them into the domain of organizational ecology would be a major advance.

## ARTICLES IN THE SPECIAL RESEARCH FORUM

The preceding issues motivated us to organize the Special Research Forum on Organizational Ecology to extend ecological research on the vital rates of organizations—foundings, mortality, and adaptation—and to strengthen bridges between ecological theory, neoinstitutional theory, strategic management, organizational economics, agency theory, and entrepreneurship.

As noted, Pfeffer (1993: 613) wrote that, of the various specialties in organization studies, population ecology had the most consensus. We agree with Pfeffer's characterization of organizational ecology and with his argument about the importance of consensus in the development of cumulative knowledge. However, consensus and consistency should be dynamic; new concepts and techniques that arise need to be incorporated to the extent that they address central issues. This presents a conundrum: How can organizational ecology maintain the *dynamic* consensus necessary for the cumulation of knowledge? We want population ecology to be an area in which "those who study organizations energetically seek out ideas, perspectives,

and techniques," but we do not want a situation in which "fundamentally any theoretical perspective or methodological approach is as valid as any other" (Pfeffer, 1993: 615).

Our view was that a focus on *central problems* was crucial; if a phenomenon is defined by the ecological literature as significant (births, changes, and deaths within organizations, populations, and communities are examples), and new concepts and techniques can be fruitfully incorporated from other theoretical perspectives while consensus is maintained. In the call for papers, we tried to outline some areas of overlap between ecological theory and other perspectives, such as neoinstitutional theory, strategic management, organizational economics, agency theory, and entrepreneurship. We also took Pfeffer's comments on how social structure maintains consensus to heart; the reviewers for the special forum represented a variety of different theoretical perspectives, although ecological scholars were the most numerous.

We believe that the articles presented here extend the frontiers of organizational ecology both theoretically and methodologically. Below, we briefly describe some commonalities among the articles and highlight the importance of the extensions provided by each. We close with some suggestions for future research.

Four of the five works concern organizational foundings (Bruderer & Singh, Baum & Oliver, Usher & Evans, and Lomi & Larsen) and establish new links between organizational ecology and the field of entrepreneurship. Similarly, four articles analyze organizational mortality (Bruderer & Singh, Usher & Evans, Lomi & Larsen, and Swaminathan) and enlarge the contact between ecological theory and neoinstitutional theory and strategic management. Three (Bruderer & Singh, Usher & Evans, and to a lesser extent, Swaminathan) examine the relative roles of adaptation and selection in evolutionary change and extend understanding of the relationship between transformation and selection processes. As this inquiry proceeds, the overlap between ecological theory and other perspectives, such as neoinstitutional theories, strategic management, and institutional economics, becomes substantial. Three of the five articles (Baum & Oliver, Lomi & Larsen, and Usher & Evans) involve shifts in levels of analysis or geographic aggregation, shifts away from a unitary conception of organizational populations and niches and toward a more detailed treatment of intrapopulation heterogeneity and the "local" environments of organizations. Ecological theorizing has often incorporated organizational heterogeneity within a population (i.e., distinctions between specialist and generalist forms, or the varying age of organizations). Environmental heterogeneity, in the sense of different local environments rather than a single common environment, occurs much less frequently in ecological models than organizational heterogeneity. As ecological theorizing encompasses local environments and the microstructure of organizational niches, the interplay between strategic management and organizational ecology will increase in importance.

Three articles (Bruderer & Singh, Lomi & Larsen, and Usher & Evans)

depart from the research designs and analytic techniques commonly used in ecological research. Simulation techniques offer analytic leverage when empirical data of the type necessary to examine a particular problem are not available. Few areas of organization studies are as empirically demanding as organizational ecology. Simulations, particularly when they are linked to other empirical work, offer a new set of tools to ecological researchers. The study by Usher and Evans is not a simulation, nor is it a large-scale event-history analysis—it is a descriptive historical account of organizational evolution. As such, it illustrates the process of organizational evolution in a very accessible fashion. Finally, two of the articles (Baum & Oliver and Swaminathan) are comparative analyses of distinct populations; the former analyzes for-profit and nonprofit sectors whereas the latter compares Argentine newspapers and American breweries.

Although the articles in this forum share some commonalities, each makes its own unique contribution. Bruderer and Singh's work, "Organizational Evolution, Learning, and Selection: A Genetic-Algorithm-Based Model," takes research on the relationship between adaptation and selection in a completely new direction. As noted earlier, much of the work on adaptation concerns the effect of changes on organizations but says little about the impact of organizational learning. Bruderer and Singh demonstrate that the differing ability to learn new routines changes the selection regime (in their case, from a fitness landscape without intermediate levels of fitness to one that has intermediate levels of fitness). In a related vein, "Environmental Conditions at Founding and Organizational Mortality: A Trial-by-Fire Model," by Swaminathan, suggests that failures of organizations under high adversity may facilitate collective learning of a vicarious sort and enable survivors to develop successful recipes. He tested whether adverse environmental conditions at the time of founding burden organizations with an initial or permanent liability or whether a "trial by fire" reduces mortality by enabling survivors to learn from the experience of failures. Analyses of American breweries and Argentine newspapers show that with increasing age, organizations founded under high adversity experience a lower death rate than those founded under low adversity and thereby provide support for the trial-by-fire hypothesis. The article by Usher and Evans, "Life and Death along Gasoline Alley: Darwinian and Lamarckian Processes in a Differentiating Population," contributes not only in substance and technique, but also in exposition. The substantive contribution lies in the analysis of adaptation and selection processes within a population (the same problem addressed by Bruderer and Singh). However, in some respects, the more important contribution is in technique and exposition. Usher and Evans provide a qualitative, descriptive account of organizational evolution rather than the quantitative event-history analysis that typifies empirical work in organizational ecology. In our opinion, this divergence makes the substance of their research much more accessible to organizational scholars who are not specialists in organizational ecology.

Lomi and Larsen's work, "Interacting Locally and Evolving Globally: A



Computational Approach to the Dynamics of Organizational Populations," examines the relationship between local, microlevel processes and aggregate, population-level outcomes. As noted earlier, despite the substantial support in favor of the density-dependence model of population dynamics, little is known about its microfoundations. A number of scholars have noted that organizational populations (and environments) are heterogeneous and have asked the question posed by Lomi and Larsen: How can the behavior of individual organizations responding to their local environments give rise to the consistent aggregate pattern so widely observed? The simulation presented here not only indicates how fairly simple rules for local behavior can produce the aggregate outcomes frequently observed in empirical studies, but also suggests how different microprocesses can produce the differences in aggregate outcomes that have been observed.

Finally, in "Toward an Institutional Ecology of Organizational Founding," Baum and Oliver compare the strength of the effects of ecological and institutional characteristics of niches on the founding rates of day care centers. As noted earlier, one limitation of ecological research is that organizations' relationships with other organizations have been overlooked. Baum and Oliver develop the construct of nonoverlap intensity (the degree of resource nonoverlap among potential competitors) to describe the potential for cooperation among competitive organizations. They show how the effects of nonoverlap intensity, relational density, overlap, and nonoverlap density vary within and between nonprofit and for-profit organizational sectors as well as across levels of geographic aggregation. These results expand ecological research by illuminating the links between for-profit and nonprofit sectors and the play of ecological and institutional processes across local and wider levels of analysis.

In our view, the articles published in this forum extend the frontiers of organizational ecology by exposing innovative approaches and to a lesser extent, enlarging dialogue between ecological theory and other perspectives on organizations. We also believe that much work remains to be done to advance the dialogue between ecological theory and other specialties in organization theory.

### **FUTURE DIRECTIONS**

Organizational economics is one area of overlap that remains to be explored. According to transactions cost economics and agency theory, selection processes shape the survival of organizational forms. However, both perspectives emphasize efficient monitoring and incentives as central to the survival of organizations (Fama & Jensen, 1983; Williamson, 1975). By contrast, ecological theorists hold that although efficiency issues affect organizational change, institutional processes such as legitimacy constrain their impact (Hannan & Carroll, 1992). In one study, Rao and Neilsen (1992) found that stock savings and loan associations outlived mutual savings and loan associations because of the former's superior monitoring, but they also re-

ported that deregulation eroded the advantage enjoyed by stocks. Additional studies of how population-level change is jointly shaped by efficiency considerations and institutional processes are sorely needed to enrich the dialogue among those positing economic, ecological, and institutional models of organizations.

The growth of network organizational forms that are between markets and hierarchies holds rich opportunities to shed light on the degree to which complementary assets, coordination problems, and legitimacy shortages underlie the rise and fall of intermediate organizational forms. One type of network is the Japanese *keiretsu*, the South Korean *chaebol*, and the Taiwanese *jituanqiye*. Another consists of organizational networks created through strategic alliances, joint ventures, and other forms of relational linkages (the blending of competition and cooperation into "coopetition").

Profitable opportunities exist for an interchange between organizational ecologists and students of issue evolution. Models of issue evolution that emphasize scarcity of public attention, issue competition, and the trajectories of issue development (Hilgartner & Bosk, 1988) also implicate organizations as significant actors in the drama of issue evolution. In a related vein, albeit with a different vocabulary, social movement theorists have suggested that the relationship between moderate and radical social movement organizations and the interplay between organizations championing a movement and organizations promoting a countermovement influence the fates of issues (McAdam, McCarthy, & Zald, 1988). Ecological models of niche width (Carroll, 1985; Freeman & Hannan, 1983) can be used to model how diffuse competition between moderate and radical movement organizations influences the rates at which they attract new members. Alternatively, diffuse mutualism might exist between radical movement organizations and moderates because of radical flank effects (Haines, 1984) whereby extremists make it easier for moderates to garner funds. Density-dependent models of evolution may shed light on the coevolution of movements and countermovements, and on how organizational dynamics underlie the rise and fall of issues.

Moreover, explicit attention to the coevolution of institutions and organizations can enhance the interchange between ecological and neoinstitutional theories (Scott, 1995). Although there is a growing body of research on the coevolution of technologies and organizations (e.g., Tushman & Rosenkopf, 1992), there has been very little work on the coevolution of institutions and organizations, even though organizations are a primary carrier of institutions. In a study of the California thrift industry from 1865 to 1928, Haveman and Rao (1996) showed that theories about thrift were primarily restructured through the differential birth of new organizations and death of old organizations, and only secondarily through the modification of established organizations. Research on the coevolution of institutions and organizations is needed to shed light on how institutional entrepreneurs deploy organizational forms to disrupt existing arrangements and shape new conventions (Clemens, 1993).

Future studies can also consolidate links between ecological research and study of the professions. Abbott (1990) suggested that interprofessional competition and intraprofessional differentiation play crucial roles in the development of professions. Ecological analyses can demonstrate how density-dependent legitimation and competition can influence professional dominance over a certain market or jurisdiction. Wholey, Christianson, and Sanchez (1993) showed that the formation of health maintenance organizations was shaped by density-dependent relationships between physician and corporate interests. In a related vein, ecological analyses can also delineate how organizational dynamics underlie the decline of professions; for example, the rise of educational technologist degree programs that treat library management as a minor area of study may not only have contributed to the replacement of librarians by educational technologists in budget-constrained schools but also led to the demise of library science schools.

A natural extension of modeling occupational dynamics is to strengthen the growing links between ecological research and the study of careers (Carroll, Haveman, & Swaminathan, 1992). Recent analyses of the effects of population dynamics on personnel mobility in the thrift industry (Haveman & Cohen, 1994) and the impact of industry dynamics on tenure distributions and turnover (Haveman, 1995b) point to the potential for fruitful cross fertilization. Ecological analyses of careers may also shed light on the distribution of gender and age inequalities in labor markets. Density-dependent processes and relational density stemming from the embeddedness of actors in social networks may enlarge our knowledge of how ethnic groups come to dominate an industry.

One final topic worthy of exploration is endogenous population change. March pointed out that one of the most important developments in evolutionary theory is "the emphasis on endogenous environments, on the ways in which the convergence between an evolving unit and its environment is complicated by the fact that the environment is not only changing, but changing partly as part of a process of coevolution" (1994: 43). The coevolution of populations is an important element of community dynamics but one not captured by the dominant modeling approach. Most work in community processes uses the Lotka-Volterra models of population growth and decline to capture the effects of one population on another. Although useful, this approach does not lend itself to analysis of coevolutionary processes that involve more than fluctuations in numbers of organizations.

Technological change is one such process. The technological innovations produced by firms within a population affect other organizations (such as venture capital firms) and the population itself in ways other than increasing or decreasing the size of the population. For example, technological innovation can produce changes in the selection regime so that different forms are favored (without changing net population size). It is likely that institutional coevolution has the same character.

Organizational ecology needs consensus to generate cumulative knowledge, but new concepts and information must be incorporated if ecological

theory is to remain vibrant. Among all of the various specialties of organizational theory, ecological theory is the most closely tied to *evolution*. It would be a bitter irony if ecological theorizing were to remain inert as the world of organizations changes.

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