

EVOLUTIONARY PERSPECTIVES ON STRATEGY

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We advocate studying strategic management from an evolutionary perspective: using dynamic, path-dependent models that allow for possibly random variation and selection within and among organizations. We argue that this perspective directs our attention to some of the most interesting problems in strategic management. The papers in this special issue are summarized, along with some of their implications for the advancement of an evolutionary perspective on strategy. Collectively, the papers draw on various theoretical rationales, illustrating how an evolutionary perspective can help to integrate the diverse and otherwise separate theoretical traditions that meet within the field of strategic management.

Most strategy research offers some rationale to account for performance differences among organizations or to account for strategic differences that presumably have performance consequences. For instance, a better-performing organization may be in a market position that is protected from competition (Porter, 1980), may have unique capabilities that enable it to innovate or differentiate (Wernerfelt, 1984; Barney, 1991), may occupy a powerful position in a network of organizations (Pfeffer and Salancik, 1978; Burt, 1992), may have a structure or strategy that fits well with the challenges offered by the market (Scott, 1975; Venkatraman and Prescott, 1990), may be efficiently designed so as to minimize transaction costs (Williamson, 1991), or may have outwitted its rivals in strategic interaction (Dixit and Nalebuff, 1991; Saloner, 1991)—to mention just some of the more popular rationales. A common belief among these various schools of thought is that a theoretical rationale can be expected to correspond to empirical patterns observable at any given time. In this belief, strat-

egy researchers typically look for cross-sectional correlations in data at a single point in time, or sometimes even in a single case at a single time. Such evidence generally is accepted as a test, or at least an illustration, of a theoretical rationale.

But through what mechanisms do these predicted results come about? We beg this question when we focus our rationale and research on what exists at a point in time, without specifying the dynamics through which these outcomes develop. As Carroll and Harrison (1994) observe, such thinking is based implicitly on what March and Olsen (1989) call the assumption of 'historical efficiency'. By making this assumption, we expect that the cause-effect relations in our rationale will play themselves out to steady-state equilibrium quickly, uniquely, and independently of the particulars of the development process. Under this assumption, 'evolution' is a rapidly optimizing force—one that brings about empirical regularities as if by a design consistent with our theoretical rationale (Nelson, 1994).¹

Those who take an evolutionary perspective

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¹ This 'as if' line of reasoning is rarely used explicitly (cf. Friedman, 1953), but rather is left implicit in theories that do not consider the development process.

on strategy, by contrast, explicitly question how strategic outcomes develop, and in so doing treat the assumption of historical efficiency as part of the research agenda. This approach has several important consequences for our research. First, it requires that we specify a *dynamic* model. This means constructing theory that can predict patterns of change, including rates of change (the speed at which change occurs) and alternative paths of change (particular sequences of events). Dynamic models may predict convergence toward a steady state, several possible steady states, or possible ranges rather than states (Tuma and Hannan, 1984; Anderson, Arrow, and Pines, 1988). But regardless of their treatment of equilibrium conditions, evolutionary models attend to the *pace and path of strategic change*. For instance, we might model how quickly—and along which paths—organizations will grow, change their performance, or experience strategic events such as birth, restructuring, product innovation, merger, technological change, or failure. Of course, such changes are what pique the interest of strategy researchers. Thus taking an evolutionary perspective directs our attention to those occurrences that are most interesting to the field of strategic management—and yet are the least understandable through static theories and cross-sectional research designs.

Second, an evolutionary perspective allows for *variation* in the possible strategies that organizations pursue. Most theories in strategic management take the 'strategy space' of possible variants as a given and then predict which would prevail if organizations pursuing the different possible strategies were to enter into competition. But how do new strategic variants develop? How do organizations search for and learn about strategic options, especially given well-known constraints on organizational rationality (Cyert and March, 1963; March, 1981)? How adaptive is this process of search (Levinthal and March, 1981; Mezias and Lant, 1994)? These questions invite us to study the rate and path of innovation among and within existing organizations, when organizations grow (Ijiri and Simon, 1977; Penrose, 1968), when strategic initiatives are launched within firms (Burgelman, 1983a; Garud and Van de Ven, 1992), or when new jobs are created (Miner, 1990). These questions also suggest that we study the degree to which innovations are brought by existing organizations vs. through the founding of new organizations (Freeman, 1995).

In either case, an evolutionary perspective allows that many variations arise essentially at random—a possibility sometimes built into evolutionary models (Cohen, March, and Olsen, 1972; Padgett, 1982; Levinthal, 1991; Nelson, 1994). More commonly, random development represents a baseline model, serving as the null hypothesis. Theory is then challenged to explain variation or selection beyond that which arises stochastically.

Third, evolutionary inquiry asks how *selection* processes affect, and are affected by the pace and path of strategic change. Research on selection among organizations has proliferated within the research project of organizational ecology (Hannan and Freeman, 1989), with a strong emphasis on processes of organizational founding and failure. In this volume several of the studies model organizational failure rates. These studies report several findings that appear inconsistent with the assumption of historical efficiency. Selection in the auto industry favored different strategies at different points in the organizations' development (Carroll *et al.*); selection among hotel chains worked against those that were the most locally adaptive (Ingram); selection worked against software firms that relied on once-benificial alliances (Singh and Mitchell); selection eliminated money market fund organizations that appeared to engage in the greatest amount of strategic search (Makadok and Walker); and selection among retail banks depended on the historical path of competition (Barnett and Hansen). Overall, these results add to mounting evidence that selection processes often do not function as a smoothly and rapidly optimizing force (Barron, West and Hannan, 1994; Carroll and Harrison, 1994; Barnett, 1996)—contrary to the assumption of historical efficiency.²

Oddly, some recoil at an emphasis on organizational failure, preferring to focus instead on instances of well-planned, sustained, excellent performance. This preference is seriously flawed on scientific grounds. One cannot adjudicate cause and effect when analyzing only today's survivors—a problem of sample-selection bias made all the worse if we focus on only the best of those survivors. Furthermore, this problem is

² In general evolutionary theory, little support remains for the idea that selection can be relied upon as an optimizing force (Gould and Lewontin, 1979; Sober, 1984; Casti and Karlqvist, 1995).

compounded when we retrace the histories of successful organizations with our theories in mind. Such research invites retrospective rationality, as illustrated by notorious cases where strategic analysis consisted of post hoc rationalizing of events that, in fact, developed over time in unexpected and unplanned ways (Weick, 1995). Rather, in order to understand strategic success, we must study both the winners and losers—as we do in the systematic analysis of organizational failure.

Selection processes take place within organizations, as well as among them, as illustrated here by Noda and Bower, Doz, and Burgelman. A central idea of this work is 'strategic context': the process through which new (existing) strategic variations are internally selected (deselected) and retained (abandoned) through an amendment of the firm's concept of strategy (Burgelman, 1983a, 1986). This work builds on the variation-selection-retention paradigm of cultural evolutionary theory (Campbell, 1969; Aldrich, 1979; Weick, 1979), which keeps it general enough to be applicable in various cultural contexts (Burgelman, 1988a). Other work in this vein integrates ideas from organizational ecology and strategic management (Burgelman and Singh, 1987; Burgelman, 1990). For instance, this research analyzes strategy making within firms as an intraorganizational ecological process (Burgelman, 1991, 1994; Burgelman and Mittman, 1994), where internal selection can substitute, to some extent, for external selection. A central proposition of this line of work is that external and internal selection, together, determine the fates of organizations. Those that continue to survive have an internal selection environment that reflects the relevant selection pressures in the external environment and produces externally viable new strategic variations that are internally selected and retained (Burgelman, 1988b; see also Hrebiniak and Joyce, 1985). Similarly, work on punctuated-equilibrium organizational change notes that whether organizations survive depends on how they manage through sequential cycles of reorientation and convergence (Tushman and Romanelli, 1985; Gersick, 1991; Romanelli and Tushman, 1994).

In summary, taking an evolutionary perspective on strategy means developing dynamic, path-dependent models that allow for possibly random variation and selection within and among organi-

zations.³ To contribute to the evolutionary perspective, it is not necessary for a study to satisfy all the components of this definition. Most careful research looks at only one or another aspect of strategic evolution, as when a study looks only at failure rates or only at variations due to innovation, but all work in this vein studies strategic dynamics. Each of the papers within this volume deals with variation and strategic search or with selection, and most have to do with both.

We organize our review beginning with the papers by Stuart and Podolny, Makadok and Walker, and Doz, which deal primarily with strategic search and organizational learning. Then we review the several papers that deal primarily with selection processes, including those by Ingram, Singh and Mitchell, Carroll *et al.*, Barnett and Hansen, Noda and Bower, and Burgelman. These papers represent a wide variety of approaches. Methodologically, they include intensive case studies, continuing a stream of process research well established in the strategy literature (e.g., Bower and Doz, 1979; Burgelman, 1983b). They also include analyses of large data sets used to obtain estimates of dynamic statistical models. Regardless of methodology, however, the papers in this issue each report an empirical analysis. This choice reflects the belief, on our part, that the greatest value of an evolutionary perspective comes in its use as a lens that can identify interesting regularities in empirical settings.

STRATEGIC SEARCH

How do organizations search for new strategies? This clearly is an important question, but research on strategic search is hampered by the fact that it is very difficult to measure. Stuart and Podolny make considerable progress on this problem in their study of local search in technology strategy.

Local search

Stuart and Podolny study the development of technological variation among Japanese semicon-

³ The problem of defining what constitutes evolutionary theory in general is not resolved (Sober, 1984). In the social sciences, most working definitions include the use of explicitly dynamic models and an allowance for randomness, variation, selection, and sometimes retention (Nelson, 1994; Aldrich, 1979).

ductor companies. They propose to measure a firm's 'technological niche' according to the inventions on which an organization builds its own inventions. This method then allows firms to be described as either close or distant to one another in technology space, depending on whether they build on similar or different inventions. By aggregating these differences, one can characterize firms at a given time according to their relative distance from other firms in technology space. This technique permits the identification of clusters of technologically similar organizations, of organizations that are unique technologically, or of organizations that stand somewhere between different technological groupings. For instance, among the largest 10 Japanese semiconductor companies, the authors discover a cluster of technological leaders and a cluster of firms with a technological base geared toward consumer electronics. This analysis then is repeated using 'egocentric' data—a subset of the data including only firms that have at least some technological overlap with a given organization. Using this approach, the authors conduct a competitor analysis in technology space—in this case identifying the technological competitors of Mitsubishi.

Stuart and Podolny generalize this distance measure to include differences over time, both among firms and within a single firm's history. It makes a powerful tool for the evolutionary analysis of technological change, allowing one to measure the 'localness' of search by the relative distance that a firm travels in technology space over time. Using these generalized distance measures, the authors discover surprisingly stable *relative* technological positions among these companies over the period 1982–92—even though the Japanese semiconductor industry experienced extreme *absolute* change both quantitatively and qualitatively over that period. Furthermore, their analysis draws our attention to the companies that have experienced more extreme relative changes in their technological base or that have followed unique technological trajectories. Mitsubishi, for instance, shifted from the consumer electronics cluster to the technology leader cluster—a result, the authors report, of a strategic change by Mitsubishi during the study period.

One of the most attractive characteristics of Stuart and Podolny's method is that it allows one to depict technological distances both numerically

and graphically. The numerical result is the familiar Euclidean distance score, measured over technology space both at a point in time and over time. Of course, the advantage of such a numerical measure is that it can be used as an independent variable in a predictive model. The distance scores can also be arrayed in two (or three) dimensions with standard techniques of multidimensional scaling, making it possible then to describe the relative technological positions of firms graphically. The plots generated by Stuart and Podolny offer compelling evidence of technological clustering, which they then corroborate by regressing the multidimensional scaling coordinates on several variables representing aspects of technology strategy. Lines bisecting these regressions clearly separate the group of technology leaders from the group based on consumer electronics technology.

Stuart and Podolny use their results to investigate whether the technological positions of these firms affect their involvement in strategic alliances. They find, interestingly, that most alliances involved firms within the group of technology leaders—developing either among leaders or between leaders and the more technologically peripheral firms based in consumer electronics. They also find that alliances especially involve firms that have changed their relative position over time.

The Stuart and Podolny study represents a considerable advance in the evolutionary study of technology strategy in particular and of strategic search generally. As the authors observe, evolutionary theories frequently emphasize—but almost never measure—relative, local change among organizations. This omission has impeded the advancement of our knowledge in this area. Using the ideas and method of Stuart and Podolny, one can now empirically model firms' time paths of search and explicitly study the relative development of technological trajectories. What's more, this approach may be able to bring empirical definition to the often-elusive 'resource base' of organizations. The authors note that, to the extent that an organization's technological position reflects its strategic capabilities, this analytical approach allows us to measure an organization's position in resource space distinct from its behavior in the market. Consequently, the authors' approach to evolutionary analysis may represent a way to study the link between stra-

tegic resources and market competitiveness without falling prey to tautological or *ex post* definitions of competence. As Stuart and Podolny suggest, future work on strategic evolution would do well to employ the ideas and methods developed in this paper.

Search and selection

Makadok and Walker investigate the selection consequences of strategic search by the 233 money-market fund organizations that existed from the inception of the industry in the U.S.A. in 1975 through 1991. Strategic search is not directly observed in this study. Rather, search is inferred from an analysis of each organization's 'growth system', comprising its size, scope, performance, and cross-product subsidies—modeled so that each of these variables is allowed to affect the development of the others. Organizations with strong estimated effects among these variables have especially responsive growth systems, evidence of effective strategic policies. For instance, some organizations are more effective than others at parlaying good performance in one period into increased demand and growth in the next period. By estimating each organization's growth system for each of several time periods, Makadok and Walker are able to trace the path followed by each firm as its growth system becomes more or less responsive—presumably reflecting its search for an effective growth strategy.

The authors then speculate about the selection consequences of search and use their estimates of each firm's growth system to test two main hypotheses. First, they note that search may be adaptive, as argued in some theories of organizational learning and evolutionary economics. If search is adaptive, then the authors expect to see organizational failure rates decline as firms discover more effective growth strategies. Operationally, this would mean that firms with more responsive growth systems—those with higher estimated coefficients in their growth system—will have lower failure rates. This result should hold, moreover, after one controls separately for the *level* of each variable in the growth system. That is, it is not simply that large, broad, good-performing, well-subsidized firms are expected to survive. Rather, it is that after controlling for size, scope, performance, and subsidy, failure rates should be lower for firms with stronger

estimated effects among these variables—since these are presumably the firms that have discovered more effective growth strategies. Thus Hypothesis 1 is tested by including the time-varying estimated coefficients of each organization's growth system as independent variables in a model predicting organizational failure.

Makadok and Walker are skeptical of Hypothesis 1, however, noting the plausible counter-argument that search is not adaptive. They draw on Bowman's (1963) idea that managers typically oversearch for better practices, as optimum practices are unlikely to be much better than practices near the optimum—yet attempts to reach the optimum are extremely costly and are plagued by random disturbances that prevent convergence on the optimum. Firms with the very best growth strategies are unlikely to be more viable than those with less effective growth strategies—contrary to the logic of fully adaptive search in Hypothesis 1. In fact, Makadok and Walker do not find support for Hypothesis 1—failing to reject the null hypothesis that an organization's growth system coefficients do not improve (collectively) on a failure model without these effects.

The authors also investigate a second main hypothesis, that strategic search is maladaptive, as suggested by Hannan and Freeman's (1984) structural inertia theory. This argument is based on the premise that organizations are expected to be reliable and accountable. Frequent and rapid changes in strategic policies imply reduced reliability and accountability, leading to social sanctions and ultimately to an increased likelihood of organizational failure. Makadok and Walker operationalize this idea as the cumulative change in each organization's growth system coefficients over time. Organizations with greatly changing growth policies are expected to show a great deal of cumulative change in the coefficients of their growth systems. Measures of the cumulative change in each organization's growth system, therefore, are expected to be associated with higher rates of organizational failure.

Correctly testing Hypothesis 2 requires the authors to isolate the survival implications of change *per se*—of the change process—apart from the implications of the content of organizational strategy. It is conceivable, perhaps likely, that organizations experiencing a great deal of cumulative change in their growth policies end

up with extremely responsive growth systems. Nonetheless, the hazards of structural inertia come from the process of strategic change, and they threaten organizational viability apart from whatever improvements in strategic content they may have yielded. Consequently, in order to empirically model the maladaptive consequences of the change process, one must separately control for the consequences of strategy content (see Barnett and Carroll, 1995). Makadok and Walker do this in their failure models by controlling for the time-varying coefficients of each organization's growth system and then estimating the distinct effect of the cumulative amount of change in those coefficients. This procedure gives an estimate of the survival implications of change *per se*, holding constant the responsiveness of the growth policies that resulted from this change. With this model, the authors find strong support for Hypothesis 2.

Learning and initial conditions

How corrigible are organizations? On the one hand, we know that initial conditions continue to have enduring consequences, and yet we also see organizations learn. Doz's study looks at this tension in the context of strategic alliances. He investigates the extent to which firms alter their collaboration in an alliance in response to feedback, and how this process is constrained by the initial design and objectives of the alliance. Doz asks under what circumstances initial conditions foster or block interpartner learning in collaborative projects.

Doz documents change at the project level among six strategic alliances involving six companies. The emerging picture is complex. Partners start the collaboration process with a given set of initial conditions. They improve their knowledge in areas that have bearing on each of the initial conditions and re-evaluate whether the alliance should continue. The re-evaluation is based on whether the alliance appears to be efficient, adaptive, and equitable. This learning has cognitive and behavioral aspects that may or may not support one another. In some cases, cognitive learning is accompanied by behavioral learning that leads to mutual adjustment, making the initial conditions less salient. But in other cases, it is not. If cognitive and behavioral learning support each other, the alliance is likely

to become stronger. Such learning seems to be facilitated when the task definition and the interface design remain somewhat open-ended at the outset so that they can change. If cognitive and behavioral learning are not mutually supportive, the alliance is likely to wind down and disband.

One especially interesting finding in Doz's study concerns the use of organizational routines. It is well known that organizations typically respond to new problems by using existing routines (March, 1981; Nelson and Winter, 1982). Doz finds a similar response in these strategic alliances: organizations tended to activate their own routines when dealing with one another, exacerbating the potential for misunderstanding, conflict, and distrust. In a related finding, Doz observed that the strategic context established by top management caused alliances to suffer if it was either extremely deterministic or extremely permissive: the former does not allow taking advantage of unanticipated strategic opportunities; the latter may lead partners to doubt their mutual commitment to the success of the alliance.

Doz's longitudinal-processual field research is a good example of how an evolutionary lens helps us to see the constraints faced by managers and to see that these constraints are often the result of previous adaptive efforts. His study identifies interesting phenomena such as the exaggerated use of organizational routines in the interface between organizational partners and the tension between determinism and permissiveness in setting the strategic context. The title of Doz's paper asks 'Initial conditions or learning processes?' But the paper's answer is initial conditions *and* learning processes. Its findings underscore the role of managers in recognizing inertial forces and, rather than denying them or simply wishing them away, taking action that alleviates or redirects them.

STRATEGY AND SELECTION

Many thousands of organizations fail each year, often in the heat of competitive 'shake outs.' Despite its ubiquity, natural selection among organizations still is only rarely studied by strategy researchers. Yet scholars of many perspectives rely implicitly on selection processes to bring about their predicted outcomes. On closer examination, selection processes often generate

unexpected and sometimes counterintuitive consequences. The environment often confronts organizations with conflicting selection pressures, making it uncertain which strategies will succeed. Several of the studies in this issue illuminate this more complex view of strategy and selection.

Adaptation by parts vs. wholes

A classic example of conflicting selection pressures occurs when an organization operates in more than one market. In this case, the organization faces a trade-off between highly localized adaptation and system-wide coordination. If it takes the localized-adaptation strategy, then the organization will be structured into independent units—each conforming to the demands of its own market. By contrast, if the organization takes a coordination strategy, then reliability and uniformity are preferred across an entire system.

An instance of this trade-off appears in Ingram's study of alternative naming strategies in the U.S. hotel industry. Ingram documents naming differences among all U.S. hotel chains that ever existed from 1896 to 1980, predicting and finding that this difference helped to determine which ones survived or failed. Throughout this period, U.S. hotel chains faced a choice, either to allow each of their establishments to identify with its particular locale or to adopt the name and image of the chain. Hotels pursuing the local-naming strategy were free to adapt their identities to whatever was most appropriate in their own locale—an advantage denied to hotels that adopted the name and image of a chain. By contrast, chain-named establishments had advantages due to their identification with a larger system of hotels. By adopting the common-naming strategy, a hotel changed its transactions with customers from one-time, spot market exchanges to repeated transactions. Ingram argues that this shift to repeated transactions made credible the hotel's commitment to providing valuable service, as it allowed customers to punish the hotel chain in future transactions for failing to do so. Locally named hotels, by contrast, suffered from a lack of credibility because their transactions with buyers typically were one time only, and so did not permit buyers to discipline the hotel for renegeing on the contract for quality service.

Ingram predicts that this credibility gave the common-naming strategy a selection advantage

over the local-naming strategy, and so he predicts lower failure rates for hotel chains adopting the common-naming strategy. Supporting this prediction, he finds those chains experienced failure rates 36 percent lower than did chains employing the local-naming strategy. This effect held despite his controls for various other independent variables, and it strengthened when aspects of organizational size were controlled in estimates from a subsample of the data.

More generally, Ingram's study nicely illustrates how one can turn the unit of selection 'problem' into an interesting research topic. An important question in theories of evolution concerns the unit that is selected or deselected. This issue is especially difficult when we study the evolution of complex organizations, as their nested, hierarchical structure makes it possible to study selection of products, divisions, establishments, or departments, as well as of entire corporations (or even networks of organizations). Ingram's approach is to allow the whole organization—the hotel chain—to be characterized by the strategies taken by its constituent establishments. In this way, he finds a compelling operationalization of the trade-off between establishment-level advantages of local adaptation and system-wide advantages of reliability.

Ingram's study compares the selection consequences of two different strategies used by a particular organizational form—in this case the multi-unit organization. An interesting, alternative comparison is to see how different organizational forms fared when using the local-naming strategy. Did locally named establishments within hotel chains have a selection advantage over single-unit hotels? Both forms of organization pursued the local-naming strategy, but the members of hotel chains conceivably benefited from their affiliation with the larger chain. Alternatively, single-unit hotels may have been individually vulnerable, but as a *population* these hotels may have been strong competitors because selection processes would be especially effective in weeding out weak variants (Barnett, 1996). Furthermore, a population of stand-alone, single-unit organizations might arguably produce greater variation to begin with than would the many members of relatively few chain organizations. Ingram's novel comparison of strategies could be extended to other comparisons where strategy and organization together affect selection processes as industries evolve.

The liability of collective action

Strategic moves taken at one point in time have ongoing implications for an organization's fate. This process is illustrated by Singh and Mitchell's study of collaborative commercialization relationships in the U.S. hospital software systems industry from 1961 to 1991. In particular, they note that, once formed, alliances imply increased dependence between firms, as they come to rely on one another's capabilities. This dependence, in turn, might become hazardous if the future brings unexpected changes—the 'two-edged sword' of increased access to, and loss of control to, another organization (Selznick, 1949). Singh and Mitchell study two ways that this loss of control can make organizations more likely to fail: when a firm loses a partner because the partner fails, and when a firm's partner forms a relationship with another firm.

In the first case, losing an alliance partner to failure means losing access to the capabilities of the partner. Singh and Mitchell predict that this loss will increase a firm's failure rate—unless it can replace the failed partner with another. Here the loss of a partner is arguably an unexpected shock to the organization. At the time of their formation, no doubt such alliances are seen by all parties as beneficial. Yet by depending on these benefits, the organization makes itself vulnerable in the event that its partner fails. The empirical results support this prediction.

In the second case, where a firm's partner finds a new partner, a hazard is predicted because of a consequent change in the relationship. The firm's partner improves its negotiating position by forming a new alliance—changing the terms of trade to its benefit by reducing its dependence on any one relationship. Furthermore, Singh and Mitchell argue that if resources are constrained, then the formation of a new alliance may cause the partner to underinvest in the first alliance—harming the firm that became dependent on that alliance. In these ways, the authors expect that when a firm's partner forms new alliances, the firm's failure rate will increase. The results support this prediction, at least in specifications that allow for a time lag in the effect.

More generally, the Singh and Mitchell study suggests the usefulness of analyzing strategies with an eye for possibly adverse evolutionary consequences of policies that appear to be adapt-

ive at the time they are implemented. By and large, our understanding of strategic alliances has remained strongly functionalist, with theorists proposing various advantages that are presumably explanations of the existence of alliances. No doubt these advantages are noted at the time of alliance formation, but it is important for us also to understand the liabilities that may result from collective action among organizations (Barnett, 1994). Singh and Mitchell offer evidence of two ways that a firm's hazard of failure might increase as a result of its past decisions to enter into alliances. Future work should continue to look into additional ways that alliance formation generates a liability of collective action.

Selection and initial conditions

The strongest form of evolutionary argument holds that current organizational fates can be traced to causes at the time of founding. In their paper, Carroll, Bigelow, Seidel, and Tsai note that two popular ideas in the strategy field can be usefully thought of as this sort of 'founding conditions' argument, with contradictory implications. On the one hand, resource-based theory states that laterally diversifying firms can leverage capabilities in order to perform well in new markets (Hamel and Prahalad, 1994). This argument implies that new entrants that come from some other industry ('De Alio' entrants) will perform especially well. By contrast, theories of entrepreneurship argue that brand new 'De Novo' firms are especially adaptive to new conditions, because they are free from established routines developed for different times and places. Carroll *et al.* set out to study both of these ideas together by modeling organizational failure among all 2197 firms ever to have produced automobiles in the U.S.A. from 1885 to 1981.

To reconcile these competing stories, Carroll *et al.* go far beyond the claim that founding conditions matter, specifying detailed patterns of dynamic effects implied by both ideas. First, they expect that the resource advantages of De Alio firms will give them an initially lower failure rate, and they predict a similar advantage for De Novo firms that experience a 'preproduction' period in which the organization prepares to do business. Both of these predictions are supported by their empirical analysis.

Second, Carroll *et al.* then model the advantage

of De Novo firms by specifying separate patterns of change in the failure rate for De Novo firms as compared to De Alio firms and to preproduction firms. They predict that as time passes, the advantage of existing routines and resources for De Alio and preproduction firms will become disadvantages due to inertia. De Novo firms with no preproduction experience are free from this liability, by contrast. Consequently, the initial disadvantage of the De Novo entrant is expected to reverse, so that it becomes less likely to fail. They then find evidence of this pattern—although the reversal is significant only compared to preproduction firms.

In addition, Carroll *et al.* investigate the survival implications of a De Alio entrant's industry of origin. Although the resource-based theory is not yet developed enough to make general predictions in this vein, the authors note some particular, potentially important differences among three common industries of origin: engine manufacturers, bicycle manufacturers, and carriage manufacturers. They suggest that the received wisdom among industry experts is that engine manufacturers would have an advantage as De Alio entrants into automobile production, but in fact they find the opposite—that carriage and bicycle manufacturers are the most viable De Alio entrants. The authors then explore several possible reasons for this finding.

The hypothesis tests of Carroll *et al.* are conducted with the well-developed 'density-dependent' model of organizational ecology (Hannan and Freeman, 1989; Hannan and Carroll, 1992). Carroll *et al.* use that model as a baseline, so that the basic evolutionary processes of legitimization, competition, and founding conditions are controlled. In particular, they find that failure rates of each kind of entrant fall with initial increases in the numbers of that kind of entrant—evidence of increasing legitimacy of that strategy. At high numbers, however, the effect turns competitive, so that additional increases in a given strategy predict an increase in the failure rate. Also, in addition, the number of competitors in an organization's year of founding is included as a covariate, and it predicts a higher lifetime failure rate for organizations born in a year with more competitors. This effect, known as 'density delay,' is evidence that organizations set up during scarce times suffer ongoing hazards as a result.

Carroll *et al.*'s use of a well-established model

to test a strategic hypothesis is exemplary for several reasons. First, it shows that their hypothesis tests hold even after they control for processes that are known to affect organizational evolution. Second, this approach yields results that are comparable to those in other studies. Third, they study a new set of ideas within a generalizable modeling framework. This approach makes their findings more compelling than if they were to use ad hoc specifications, and it makes their novel ideas testable on other data sets. Researchers can attempt to replicate and advance their findings simply by estimating or extending their model in other organizational settings. Empirical modeling of this sort can go a long way to increase the accretion of knowledge in the strategy field.

The Red Queen

Barnett and Hansen study how exposure to competition affects organizational survival, using a synthesis of organizational learning theory (March, 1988) and organizational ecology (Hannan and Freeman, 1989). They propose that an organization exposed to competition is likely to learn as a consequence (Barnett, Greve and Park, 1994). Assuming that learning is adaptive, the organization becomes a stronger competitor, triggering search and learning in its rivals. This response, in turn, strengthens competition from rivals felt by the first organization, starting the whole process over again. This reciprocal system of causality has been dubbed 'Red Queen' evolution by the biologist Van Valen (1973)—a reference to Lewis Carroll's *Through the Looking Glass*, in which Alice observes that she appears to be standing still even as she is running a race, and the Red Queen replies that in a fast world one must run just to stay still.

Barnett and Hansen argue that the Red Queen probably is very important in strategic evolution because, like an 'arms race' model, it is self-reinforcing. Even if each incremental adjustment is minor, over time this mutual incrementalism could conceivably add up to a very large difference. The authors also note, however, that it is potentially difficult to detect the consequences of this process, as each organization becomes more viable but its competitors become stronger too. As a result, net measures of performance or survival may lead us to believe wrongly that

nothing has changed even when a Red Queen exists.

To overcome this problem, Barnett and Hansen model organizational failure rates as a function of two distinct, simultaneous effects. Each organization's own competitive experience is included in the model, because organizations with more competitive experience will be more likely to survive. At the same time, each organization's survival is allowed to depend on its *rivals'* competitive experience. Organizations with more experienced rivals are expected to be less likely to survive. Although descriptive statistics would confound these two opposing effects, Barnett and Hansen's multivariate model of organizational survival separates them into distinct terms. The key to separating these effects is in operationalizing 'competitiveness' as a property of organizations, rather than markets, allowing organizations to be stronger or weaker competitors as revealed by their effects on other organizations' viability (Barnett, 1993, 1996).

Going beyond these baseline effects, Barnett and Hansen also consider the condition under which learning may be maladaptive. Two historical constraints are considered. First, drawing on Levitt and March's (1988) idea of a 'competency trap,' Barnett and Hansen propose that competition-driven learning in the distant past is likely to have taught organizations outdated lessons. Consequently, they predict that exposure to competition in the distant past is maladaptive, making organizations both more likely to fail and weaker competitors. Whether Red Queen evolution is adaptive or maladaptive should depend on historical timing: recent experience is predicted to increase survival and competitiveness, whereas distant-past experience is predicted to have the opposite effects.

A second constraint arises when organizations compete against many different cohorts of rivals. An organization facing a single cohort of rivals shares with them a single sequence and timing of incremental adaptations. When a new cohort enters, the organization may also adapt to the challenges of this new competition, but it is constrained by adaptations made in the past to established rivals. In the same way, adaptations made in response to the new cohort of rivals constrain what can be done in response to established rivals. This pattern suggests that we should attend to the *variance* as well as the amount of

competitive experience had by an organization. Organizations with their experience spread across many different cohorts of rivals—those with high variance among their competitive relationships—are more constrained in their ability to adapt to any one cohort. As these constraints increase, adaptations are less likely to exceed the costs of search and learning. Consequently, the authors predict that organizations with high variance among their competitive relationships are more likely to fail.

Barnett and Hansen empirically model these arguments together by specifying each organization's *experience distribution* in models of organizational survival. Number of competitors represents just one aspect of an organization's experience distribution: its *number of competitive relationships*. Beyond this, their arguments suggest that they also model (1) the amount of competitive experience (the *mean duration* of relationships), (2) the *historical timing* of these relationships, and (3) the *variance* in durations of these relationships. They also control for the effects of selection that might otherwise lead to spurious evidence of organizational learning. Only by modeling all of these effects together, they argue, can one detect both the adaptive and maladaptive consequences of Red Queen evolution.

The authors estimate their model using data on all 2970 retail banks ever to operate in this century in the state of Illinois (excluding Chicago). Until recently, bank branches and holding companies were prohibited in Illinois. With only unit banks operating, each of the 650 communities within Illinois was a distinct and independent local market. These data provided ample differences in the competitive histories of organizations and their rivals—a requirement for identifying the Red Queen model. They found support for their predictions in estimates of the organizational failure rate among these banks.

Several conclusions come from the Barnett and Hansen study. First, the Red Queen model finds strong support, suggesting that a dynamic model of competitive strength may be a much better predictor of organizational success and failure than are models of static competition, which typically look at only the numbers and size distribution of competitors at a single point in time. Second, their approach is based on the idea that 'competitiveness' is a property of individual organizations, not of markets, as is usually

thought to be the case. This innovation should be extremely useful for the field of strategic management, where much of our theory is based on the idea that some organizations are more competitive than others. Third, their study demonstrates that evolutionary processes have both maladaptive and adaptive consequences. Finally, Barnett and Hansen's model allows for strategic interaction among competitors, and at the same time it acknowledges that organizations are limited in their ability to strategize. The explanatory power of their model demonstrates the usefulness of basing our models on realistic assumptions when we describe the evolutionary consequences of strategic interaction.

Selection, initial conditions, and managerial discretion

One of the ways in which an evolutionary perspective on strategy can be helpful is by identifying constraints on managerial action. Some of these constraints come from outside any particular organization, such as industry structures, laws, or consumer preferences. But other constraints come from within a firm, arising over its history—such as the initial conditions and organizational routines highlighted by Doz's paper. These internal constraints may limit managerial discretion in important ways.

Such internal constraints are revealed in the study by Noda and Bower. They begin their study of BellSouth and U S WEST with an interesting question: why would two organizations having similar initial market positions, similar competencies, similar structures and routines, and similar management talent embark on different courses of action when a new business opportunity arises for both? The authors then describe the different internal constraints that shaped these firms' very different strategies in cellular telephony during the period 1984 (after the breakup of AT&T) through the early 1990s.

Noda and Bower's paper shows ways in which corporate context affects the pattern of new business development with these firms. The authors use the Bower-Burgelman (B-B) process model (Bower, 1970; Burgelman, 1983c) to conceptualize the strategic decision-making processes concerning cellular telephony in BellSouth and U S WEST, and to highlight the differences between these processes in both firms. Their

paper is the first to examine the usefulness of the B-B process model in a comparative study at the firm level.

Like Doz, Noda and Bower show that initial conditions associated with the corporate context are important in the strategy-making process. Top management sets the structural context, in particular the resource allocation rules. Top management also sets the initial strategic context, which reflects their 'crude strategic intent' regarding particular areas of business. Structural and strategic contexts, together, define the playing field for middle-level and operational-level managers. Managers below top management pursue business activities that give substance to the strategic context.

Although Noda and Bower confirm that top management sets the corporate context within which new business development takes shape, their findings also show that top management finds it very hard to change the pattern of resource allocation once it has been set in motion. U S WEST's CEO intended to move away from regulated businesses and focus on businesses that would allow the company to generate net income as soon as possible. He did not anticipate (as many others did not anticipate) the potential of cellular. Even though the CEO and other top managers became aware that resource allocation and key premises in the strategic context were leading U S WEST to miss out on opportunities in cellular telephony within the U.S.A., they did not change the rules and premises to avoid this unanticipated outcome. At BellSouth, in contrast, where there was initially great skepticism about the business prospects of cellular telephony at the top management level, financial rules governing resource allocation were less short-term oriented than at U S WEST, and cellular was viewed as complementary to wireline telephony rather than just as one of many potential new business opportunities. The design of corporate context thus determined patterns of escalation (BellSouth) or de-escalation (U S WEST) of commitment on the part of top management as a result of the iterations of resource allocation. The finding that structural context—in particular the resource allocation rules—was very stable highlights an important constraint on managerial discretion.

Noda and Bower also find evidence of an intraorganizational ecology in which business activities compete for resources. Initial success measured in terms of the resource allocation rules

provides momentum. At U S WEST, the strategic context for new business development was less tied to telecommunications businesses than at BellSouth. Top corporate executives found that real estate and financial businesses were initially very successful and generated net income quickly. Incremental learning drove to expand nonwireless businesses (under the impulse of the managers associated with those businesses).

Finally, Noda and Bower's case data confirm that individual managers—'champions'—are important in getting a business initiative going and providing it with momentum. Their data also suggest, however, that once the business is taking shape, it becomes somewhat independent of particular individuals, as new managers replace the original champions. This result demonstrates that the unit of analysis for the B-B process model is the pattern of interlocking managerial activities, rather than the individual managers themselves.

Internal selection and managerial activities

How do selection processes operate within organizations? And what patterns of managerial activities are involved in internal selection? These questions are posed in Burgelman's paper on strategic business exit (SBE). The paper studies the pattern of managerial activities involved in Intel Corporation's strategic business exit from its core business in 1984–85, dynamic random access memory (DRAM), and the redeployment of some associated distinctive competencies in the more profitable erasable programmable read-only memory (EPROM) business and, especially, the microprocessor business. The pattern of managerial activities involved in SBE was identified by using the process model of internal corporate venturing (Burgelman, 1983b) to analyze the behavioral data generated by the SBE study.

At the business level, the combined activities of operational and middle-level managers caused Intel to decline from initial dominance in DRAMs to a losing position. Some middle-level managers who embodied some of the firm's most important distinctive technical competencies deployed these competencies inflexibly, despite the fact that the industry was changing. Other middle-level managers, responding to Intel's resource allocation rules, shifted scarce manufacturing resources away from DRAMs. Operational-level managers tried to reposition Intel as a niche player in

DRAMs, in an attempt to respond to internal and external conditions while taking advantage of Intel's distinctive competencies. This unsuccessful effort exacerbated Intel's loss of strategic position and reinforced the internal resource shifting and the concomitant de-escalation of commitment to DRAMs. These activities were intendedly rational, but they responded to incompatible internal and external pressures and so had the unanticipated consequence of setting Intel onto a course to exit from DRAMs.

Burgelman, like Noda and Bower, finds that at the corporate level the context set by top management had strong selective effects on the strategic actions of middle and operational managers at the business level. The resource allocation rules were a strong determinant of what the firm did, regardless of the rhetoric associated with official (or stated) corporate strategy. Like Noda and Bower in the case of U S WEST, Burgelman finds that Intel's top management did not change the resource allocation rules, even though the outcomes regarding DRAMs were not what top managers had in mind when they put the rules in place. The paper also finds that strategic business exit requires the *dissolution* of the strategic context of that business. Strategic context dissolution was found to be a complex process involving the combined but not always deliberately aligned activities of middle and top managers. By documenting the managerial activities involved in strategic context dissolution, the paper provides additional insight into the process of deinstitutionalization and a link between evolutionary and institutional perspectives.

Burgelman's paper shows some of the ways that internal selection may serve as a coordination mechanism. The paper also illustrates the intraorganizational ecology of strategy making, reporting the managerial activities that gradually decreased commitment to DRAMs and increased commitment to microprocessors. It also provides some evidence that strategic change that looks 'punctuated' at the corporate level of analysis may sometimes be the result of more gradual change taking place at lower levels in the organization.

CONCLUSION

Each of the papers in this volume takes an evolutionary perspective, looking at dynamic, path-

dependent processes and allowing for variation and selection within or among organizations. Each offers new insights and reveals important empirical findings. Taken collectively, they demonstrate that an evolutionary perspective may allow us to synthesize the many disparate theories now circulating in the field. The key here is that the evolutionary perspective is not inherently in contradiction with most theories of strategic management. Most rationales favored by a particular theory—efficiency, power, market position, distinctive capabilities, or whatever—usually can be understood in evolutionary perspective. In this volume, for instance, Ingram draws on economic rationale; Doz combines ideas from organizational learning theory and structural inertia theory; Singh and Mitchell analyze an asset specificity problem more often thought of as an issue for transaction cost economics; Noda and Bower as well as Burgelman combine ideas about economic incentives with an understanding of structural constraints; Stuart and Podolny use techniques and ideas from role theory in sociology; Carroll *et al.* synthesize ideas about strategic capability with structural inertia theory; Barnett and Hansen combine ideas from organizational learning theory and organizational ecology. What we advocate here is not a singular theory, but an *evolutionary perspective* that potentially can synthesize the many theoretical approaches now proliferating in the strategy field.

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