



# Who Wants to Be an Evolutionary Theorist?

## Remarks on the Occasion of the Year 2000 OMT Distinguished Scholarly Career Award Presentation

HOWARD E. ALDRICH  
*University of North Carolina–Chapel Hill*

**H**eedless of Madonna's warning, Paul Hirsch urged me to use this occasion as an opportunity to preach a bit. The Organization and Management Theory (OMT) division gave me a great platform from which to put forward my sense of where we are, what we've accomplished, and where we have fallen short. Three decades ago, I was the first associate editor of the *Administrative Science Quarterly*, and Paul thought that I might have some ideas on where we might go over the next decade or so. In short, Paul asked me to give a "Leadership Sermon." I'm assuming that he also released me from the scholastic requirement that I must thoroughly document the intellectual history and empirical rigor of all my claims! I plead for a temporary suspension of disbelief while I make my arguments.

Having just published my book, *Organizations Evolving* (1999), I have given some thought to what's missing in OMT, as well as to what it would mean if we took the evolutionary approach seriously. What differ-

ence would evolutionary thinking make in the way we think about theoretical issues and design our research? As it happened, I came up with a mixture of complaints and challenges. I want to celebrate what we've accomplished, but also to note where more needs to be done. In particular, I want to encourage people to pay more attention to process-oriented theorizing and research. As space does not permit me to review the fundamental features of evolutionary theory itself, readers desiring more information will need to consult my book.<sup>1</sup>

Four questions constitute the organizing themes of my talk. First, how can we build a more realistic OMT? Currently, our theorizing and research seriously misrepresent the actual shape of the organizational landscape. Second, what's wrong with outcome-driven research, and why should we focus more on event-driven research? Many of us fall too easily into the trap of explaining outcomes by working backward in time. Third, does OMT have rhythm? The theme of

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the 2000 Academy of Management meeting was time, and I agree that timing is everything. Our theories and research designs often leave the timing and pacing of change imprecise or ambiguous. Fourth, in our research and theorizing, we need to ask ourselves more often, "What happens next?" We need to face the fact that all empirical generalizations are about the past, and begin thinking about building models from our theorizing and research that help us understand what is likely to happen in the future.

### TOWARD A MORE REALISTIC OMT: THE TRUE SHAPE OF THE ORGANIZATIONAL LANDSCAPE

Because you're reading this, you probably also read the major OMT journals and maybe even write for them. Accordingly, we might expect that you'd be fairly knowledgeable about the basic facts regarding the organizational landscape. To confirm my assumption, let's play a little game: "Who wants to be an evolutionary theorist?" Take out a piece of paper and number it from 1 to 9 or, if you're reading your own copy of *JMI*, just write the answers in the boxes in Table 1 (no fair looking ahead—if this is going to work, you must play the game honestly!). Now, answer the nine questions in Table 1. All questions are about businesses in the United States. Sorry, Jean-Claude!

I'll make it easy for you. Here are the answers, all of which are in my book: 20,000, 5,478,000, 0, 14,000, 7,300,000, 21,000,000, 510, 20,000, and 4,300,000. Of course, this list is out of order! I considered putting the questions in order by the magnitude of their answers, but that would make the quiz much too easy. The answers are in the appendix.

When I ask these questions in seminars around the world, I routinely find that most people get most of these answers wrong by at least one order of magnitude, and sometimes more. Because the audience for my OMT talk consisted of the more enthusiastic and committed members of the profession, they got many more correct than most of my previous audiences.<sup>2</sup>

If your answers were typical, you overestimated the number of large firms, initial public offerings (IPOs), mergers and acquisitions, and publicly held firms. By contrast, you underestimated the number of new firms and total business entities. You may have even thought that personality differences matter for entrepreneurs. I'm no longer surprised by such esti-

mates, as they simply reflect what the business press emphasizes, as well as the topics and research designs found in academic journals. Should we be concerned? Consider the following parable.

At a cocktail party, academics from across the campus of Beserkely University meet and commiserate about their declining standard of living, unworthy students, and the results of the recent presidential election. Their host has invited colleagues from many different colleges and departments, and so you're meeting scientists, humanists, and even a few coaches. The president is out of town on a fund-raising trip.

A balding, middle-aged man in a tweed jacket, blue jeans, and wearing a black belt with brown shoes introduces himself: "Hello, I'm a botanist. You know, the study of plants?" You suddenly revise your estimate of the evening's potential payoff, as you see a chance to get expert advice on a problem that's been bugging you. "Oh, I'll bet you can help me. I've been battling some type of really nasty weed in my lawn for the past 3 years. It's really tough. I keep planting new rye grass, using fertilizer, and pouring enough weed killers on the lawn to wipe out the songbird population of my neighborhood, and still the stuff persists. I can't stamp it out. What is it? How can those things survive these brutal winters? What can I do?"

The botanist looks surprised at your question. "Oh, I'm sorry. I really have no clue. You see, I only study redwoods." Disappointed, you move on.

A young woman dressed in what looks like a safari suit introduces herself: "I'm new here, in the zoology department. Just got back from some fieldwork and am looking forward to my first faculty meeting." You realize she *must* be new. She had yet to sit through a department meeting where colleagues spent an hour arguing over whether the minutes for the last meeting must show the names of *all* those who seconded a motion, or just that it *was* seconded.

"Oh good, so you study animals? I have a question for you. My lawn is infested with these little rodent-like creatures. Every spring, I notice raised mounds of earth all over the place, and after the dog digs them up, I catch the mower's wheels in the holes. When Teddy actually catches one of the creatures, they look like blind hairy rats. Between them and the weeds, they're ruining my lawn. What are those things, anyway? What makes them so hardy? What can I do?"

Another startled look, accompanied by what looks, for a moment, like a condescending smile. "Oh, I'm

Table 1  
*Who Wants to Be an Evolutionary Theorist?*

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1. Approximately how many business entities have filed tax returns with the Internal Revenue Service in recent years?
  2. Approximately how many businesses had paid employees in the most recent reporting year?
  3. About how many incorporated businesses are there?
  4. How many business startups were attempted the past year? (Feeling lucky? How many were left after 1 year?)
  5. About how many firms, per year, were acquired in mergers and acquisitions in the first half of the 1990s?
  6. Approximately how many publicly traded firms are there? (On all the stock exchanges and over the counter.)
  7. About how many firms employ 500 people or more?
  8. How many Initial Public Offerings (IPOs, for those of you in Finance) were there in 1999?
  9. On how many personality traits do successful entrepreneurs differ from unsuccessful ones?
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terribly sorry. I have no idea. I only study elephants." You glumly head back to the kitchen for another glass of Chianti, wondering how all these damned narrow specialists ever got jobs at Beserkely, supposedly a truly diverse university.

### Redwoods and Elephants: OMT's Dilemma

Our field's claimed domain is organizations, but, like the scientists at the Beserkely University cocktail party, in reality we fall far short of universal coverage. Our journals are filled with studies of mostly large organizations or the surviving members of much larger cohorts of all kinds of organizations.<sup>3</sup> Most members of our potential study populations exited prior to when the study was carried out, and our data sets consist of the remnants of selection processes we've overlooked. Many variables we'd like to use in our research are only available for large, publicly held firms. Consequently, we miss the true extent of variety, diversity, and heterogeneity in the organizational landscape.

Of course, we've known about this problem for years. For example, researchers interested in financial performance measures must limit themselves to publicly traded firms. Privately held firms don't have to make public reports of their financial performance, and they're also notoriously idiosyncratic in their accounting practices. Thus, strategy and finance researchers, and others looking for standardized financial accounting measures, have found themselves limited to just 20,000 publicly listed firms, constituting a tiny fraction of the organizational population.

Left truncation is another example of the problems that limit the representativeness of our data sets. Our understanding of the association between organizational "age" and various kinds of transformation has been seriously distorted by selection bias. If we exam-

ine only surviving firms, we don't observe organizations over the full range of ages during which they're at risk of transformation.

### Summary

Limiting our studies to only a small fraction of the organizational world means that we ignore much of the historical process that generated such organizations. Most, after all, began small (Aldrich & Auster, 1986). We miss their aging and their evolution through periods when competitors were eliminated. We don't see the distinctive differences that made surviving organizations hardier than their peers. In research that showed the importance of taking account of historical periods, for example, Jones (in press) traced the evolutionary differences within and between technology and content firms as they battled for dominance in the film industry between 1911 and 1920. Such historical details are lost if only the oldest and largest firms constitute our samples. Moreover, by ignoring the smallest and most fragile organizations, we overlook the source of diversity in the organizational landscape and the pace of its reproduction. Indeed, our field's multidisciplinary nature is threatened if we ignore the incredible diversity of its subject matter.

The skewed nature of the research that appears in our journals constitutes the longer-term issue facing us, as does the skewed nature of the populations that scholars choose to study. Skewed samples, in turn, bias the kinds of theory that we do. The people who are writing theory are inevitably basing it, in part, on what they choose as empirical generalizations from the research literature. If those empirical generalizations are based on a very small subset of the universe, then theorizing is also inevitably skewed toward those larger organizations.

For me, one of the most exciting areas of strategic leadership is figuring out how millions of small firm

owners manage to keep their organizations intact from 1 day to the next through a variety of vexing circumstances. By contrast, the leadership literature mostly focuses on people who either manage very big firms or, worse still, people who are the CEOs of very big firms, not middle managers. Because they focus on huge established firms, researchers ignore the much larger pool of people who are also leaders by most definitions but who don't get any attention. Is there still life in the strategic leadership literature after Jack Welch retires?

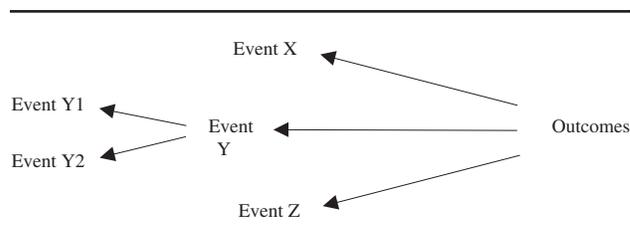
### WHAT'S WRONG WITH OUTCOME-DRIVEN RESEARCH?

Dynamic designs might compensate for some of the problems I mentioned, but a high proportion of OMT research is still cross-sectional and static. At times, we seem almost wedded to single-administration questionnaires and surveys. Evolutionary explanations focus on processes and are event driven, with events followed prospectively to outcomes. By contrast, nonevolutionary explanations are outcome driven, with outcomes followed backward to their preceding events. Outcome-oriented researchers ransack organizational histories for formative events that might have led to the observed outcomes.

#### Outcome-Driven Explanations

Outcome-driven explanations are built backward, from an awareness of observed outcomes to prior causally significant events. Two related problems are introduced with this strategy. First, it often leads to investigators selecting on the dependent variable, a well-known research bias. Second, even though we might include all organizations—those that have experienced the event and those that have not—we still observe them at only one point in time. Figure 1 gives a graphic example of an outcome observed at Time 1 that is then linked backward to events occurring earlier.

For example, in a survey study, we might ask respondents about their level of commitment to an organization, and then ask about previous events hypothesized to have affected their commitment. We might ask about promotions, experiences with coworkers, or lateral transfers within the organization. In another example, while doing an organiza-



**Figure 1: Outcome-driven explanations**

Source: Adapted from Elder, Caspi, and Burton (1988).

tional ethnography, we might conduct semistructured interviews and examine organizational archives to reconstruct relevant previous events.

Dutton's (1997) review of research on building strategic agendas mentioned several studies of major decision making that were outcome driven. For example, Hickson, Butler, Cray, Mallory, and Wilson (1986) studied 150 top decisions, Mintzberg (1978) studied 25 strategic decisions, and Eisenhardt (1989) examined eight firms in high-velocity environments by beginning with "endings" and then reconstructing the processes leading up to them. In each study, researchers conducted interviews with participants and also examined documents from company archives.

What difficulties might befall us in constructing outcome-driven explanations? First, of course, not everyone who was at risk of experiencing the event in the past is still around, thus introducing sample selection bias into our design. Organizations may have failed, disbanded, or otherwise gone out of existence. Even when studying large organizations, researchers will find that some have exited the field if the time span covered is lengthy. Individuals who took part in making key decisions are often no longer around for interviews. Second, relevant records from the past may have been lost or destroyed, leaving us unable to build a trail back from the outcome to relevant events. For example, bureaucracies keep records for administrative purposes, not research, and they often discard them after a set period of time (Reiss, 1992).

Third, humans are remarkably good at retrospective reconstruction, as recent incidents of "recovered memory" have shown. People like to put themselves at the center of the action—inflating their importance, excusing their mistakes, and settling scores with those no longer around to defend themselves.<sup>4</sup> Knowledge of the outcomes inevitably frames the explanations people offer when asked about the past (Kahneman, Slovic, & Tversky, 1982). Fourth, humans are also incredibly myopic creatures. Events whose impor-

tance was unrecognized at the time simply remain unacknowledged and left out of explanatory accounts subsequently offered.

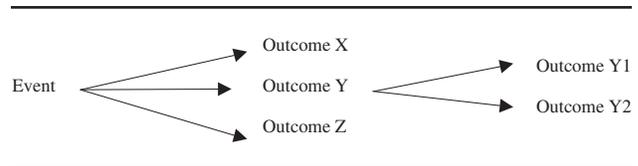
I am not arguing against choosing research problems on the basis of observing significant outcomes.<sup>5</sup> Most of science is about studying what leads up to or causes outcomes. Much of the philosophy of science and epistemology is really about how to avoid making causal attribution errors, given outcome-based research. Problems arise when researchers forget that their underlying goal is to build event-driven explanations for outcomes, rather than to celebrate the outcomes.

### Event-Driven Explanations

In contrast to outcome-driven explanations, event-driven explanations are built forward, from observed or recorded events to outcomes. Researchers pick certain kinds of events a priori and then record their occurrences over time. No simple rules exist for such designs, and some events can figure in more than one narrative. Moreover, most events we observe probably have no obvious consequences, thus requiring that researchers have strong a priori theoretically grounded notions of the expected causal process. Figure 2 gives a graphic example of events observed over time, which are then linked forward to outcomes occurring later. Note that later outcomes are themselves events with subsequent consequences.

For example, several event-driven survey research projects have been funded over the past three decades with notable success. Back in the 1940s, sociologists and political scientists studying voting behavior pioneered the study of political behavior over time. In 1940, three researchers studied 3,000 people in Erie County, Ohio, and followed about 600 of them with monthly interviews between May and November: "Interviews were spaced about a month apart to fit best the natural course of campaign events" (Lazarsfeld, Berelson, & Gaudet, 1944, p. 5).

Based on that groundbreaking study, many other panel studies followed. The Panel Study of Income Dynamics (PSID), initiated by John Lansing and Jim Morgan at the University of Michigan in the 1960s, followed people over many years, with interviews repeated at various intervals. Reports from the PSID, especially by authors such as Duncan and Coe (1984), allowed researchers to gain important new insights into the dynamics of economic status, especially poverty and welfare. Similarly, the National Longitudinal



**Figure 2: Event-driven explanations**

Source: Adapted from Elder, Caspi, and Burton (1988).

Study of Youth followed children from their early years in school up through adolescence, again with repeated interviews. By asking questions about the same events in successive surveys, researchers can link prior events to subsequent outcomes, such as getting a job or losing it, paying back a loan or welshing on it, and getting ahead or being held back in school.

In the field of entrepreneurship studies, Reynolds and his colleagues (Reynolds & White, 1997) have used multiwave panel studies to study the behavior of nascent entrepreneurs. A nascent entrepreneur is defined as someone who initiates serious activities that are intended to culminate in a viable business startup. Beginning with studies in several states, Reynolds (2000) and a large team of collaborators developed a method for conducting panel studies of the business startup process covering the entire United States. The model has been extended to Norway, Sweden, Canada, the Netherlands, Greece, and Argentina, and has been modified for cross-national comparisons of entrepreneurial activity and national economic growth in the Global Entrepreneurship Monitor Project (Reynolds, Hay, & Camp, 1999).

Event-based explanations can be built on archival records as well. We can seek evidence on events that have occurred by searching the files of organizations, using commercial directories, and tapping other publicly available sources of information. For example, many people are familiar with ecologists who use event history analyses based on archival data. Because of their dependence on archival data, they've been stuck with just a few events that can be studied that way. One of them is disbanding, whether through failure, bankruptcy, or another form of exiting a population. Another event recorded in company files and announced publicly by large firms is CEO succession.

Often, archival information will yield much of what we need. But there are other kinds of things that we are not going to find in the archives because the archives were collected for administrative purposes. Record-keeping bureaucrats in organizations keep records for themselves, not for researchers. Administrators keep records because they need to make decisions of some

kind, such as whether to tow your car to the city pound because you're a blatant traffic ticket scofflaw or to just put a notice on your windshield because you're an amateur offender. They don't care about your socioeconomic status or whether your parking problems stem from your attempt to hold down two jobs at opposite ends of the city. Thus, it's not so easy to take what they have in their archives and mold it to our purposes.

Research on interorganizational relations has been especially hampered by a lack of available information on events. Relations between organizations are often informal and undocumented, as Barley, Freeman, and Hybels (1992) discovered when they were trying to collect information on strategic alliances in biotechnology. When proprietary information is involved, organizational participants see researchers as one more source of technology leakage (Teece, 1980). In short, they may not trust us to keep our mouths shut. Indeed, in keeping with my first theme, privately held firms are loath to give up any sensitive information, leaving researchers to fall back on what they can find in public reports and government documents (e.g., Securities and Exchange Commission filings).

### Summary

Cross-sectional designs and outcome-oriented data collection hamper our ability to construct evolutionary explanations. Dynamic designs enable us to separate the random noise in our data from the true underlying structural time trends (Dooley & Van de Ven, 1999). We need dynamic and event-oriented designs that leave room for unexpected historical conjunctures, blind alleys, and dead ends. Moving backward from organizational characteristics to "reasons" why it must have developed that way encourages the construction of arbitrary explanations.

For example, Fischhoff (1982) showed that people couldn't disregard what they already know about something when it comes to constructing an explanation about why something happened in the past. Once they know the outcome, people build stories that lead, inevitably, to that outcome. Fischhoff and his colleagues designed experiments in which they altered historical outcomes, using cases most people don't know much about. They took real historical data and simply changed the outcome of some series of events. When they asked people to estimate the probability with which they could have successfully predicted the

outcome of the events, given knowledge only of the past, they consistently overestimated their abilities. Moreover, in writing up stories that justified their predictions, they were able to put together very coherent and compelling stories. Of course, they were wrong.

Moving forward with our explanations, using reliable information consistently collected on similar kinds of events allows us to avoid outcome-driven explanatory biases. However, we face another danger. Investigators may discover that their event-accounting schemes from early periods lose their explanatory power when circumstances change. When powerful period effects exist, our explanations will be historically contingent in the extreme (Aldrich, 1999, chap. 8). We need more dynamic research designs and fewer cross-sectional ones.

Growing interest in issues of organizational identity and continuity fits nicely into a concern for event-driven explanations (Whetten & Godfrey, 1998). Whether individuals develop a strong identification with an organization might depend on when they join and that organization's position in its life course. For example, we might expect that people who have experienced the ambiguity and struggle of a firm's formative days would identify closely with it. Indeed, they might even consider it a mirror of their own personal identities. Later arrivals might not feel the same.

Thus, depending on when they joined, employees may have very different images of the firm. Throughout its life course, an organization's identity and the emergent culture that shapes the selection and socialization of new employees remain contested. Looking forward, it may be very hard to predict which of many possible versions of firm identity will prevail. Unknown future events and contingencies create a selection environment through which some identities are likely to dominate others (Aldrich, 1999, chap. 6).

The extent to which an organization's identity affects its strategy depends, in part, on the trail of events established by its history. For example, research on top management teams has shown that a firm that experiences poor performance is more likely to replace a retiring—or ousted—CEO with an outsider, someone whose outlook has not been imbued with the firm's identity.<sup>6</sup> These outsiders are, in turn, more likely to make substantial strategic changes than are insiders. Poor performance events may thereby lead to broken connections between firm identity and strategy. In contrast, long periods of acceptable performance may lead to succession by long-time insiders and an unbroken marriage of firm identity and strat-

egy. A tight bond between identity and strategy, however, does not guarantee a firm's survival. Strong identities can either blind organizations to changing environments, rendering them unable to adapt, or give them the cohesion they need to survive challenges brought on by change (Fiol, in press).

### WHO'S GOT RHYTHM?

Time was the theme of the 2000 Academy of Management meeting, and in evolutionary theory, timing is critical. Theories of organizational change often leave the timing and pacing of change imprecise or ambiguous. We don't know whether the posited changes are accomplished in days, weeks, months, years, or decades. Research designs often further confound the issue, leaving a mismatch between a theory's implicit time frame and actual empirical indicators. For example, few theories of organizational change specify that change only occurs once a year, and yet researchers who rely on archival data often only have data available in 1-year chunks. As I've noted, researchers using archival data are at the mercy of administrators, whose demands for data are driven by organizational needs, not a theory of why things are happening.

#### Model Time Explicitly

Investigators should develop explicit models that specify the intervals of time during which transforming changes or events occur (McKelvey, 1997). What is the posited time trajectory? For example, how long should it take for new regulations to disrupt the established order of competition in an industry—weeks, months, or years (Haveman, 1992)? Why does it take that long? Are the regulators underfunded, incompetent, or in collusion with those they are supposed to regulate?

When investigators spell out a theory of the timing of events and outcomes, they can then decide on the frequency of observations needed and the time interval between them. For example, Lazarsfeld et al. (1944) decided that voters probably didn't change their minds about candidates more often than monthly, and then designed their data collection around that assumption. In an organizational example, Sastry (1997) showed how incorporating the notion of pacing into models of organizational change enriches our understanding of how organizations

adapt to environmental change. To do so, however, requires that we have an explicit theory of pace and duration.

#### Waiting, Pacing, and Duration

Time can be measure in clock time, but it is also embedded in people's interpretations of their situations. We can distinguish between two dimensions of time: pace and duration. Pace is the number of events in a given amount of time, whereas duration is the amount of time elapsed for a given event. Variations in pacing reflect the operation of cultural norms in evolutionary processes.

Merton (1949) coined the term *socially expected duration* to explain human's tendencies to base their decisions on how long they think a particular relationship or event will last. For example, officers in voluntary associations are usually elected to 1-year terms, and they plan their activities accordingly. In another example, normative expectations regarding career progress milestones are built into universities. Some universities have a reappointment clock in which faculty members have an initial 3-year appointment, and if they are renewed they come up for tenure in the 5th or 6th year. Those are socially expected categories in the sense that your career is now set up in 3- and 6-year chunks (for other examples, see Lawrence, 1997).

Gersick's (1991) research implies that if you give people a 3-year or 6-year duration in which they are meant to be working, they would typically take that expected duration and halve it. They would then time what they do with respect to the midpoint of that interval. So, for example, if a department gives a junior faculty member a 3-year appointment, we would expect that after 1.5 years, some kind of panic would set in. Assistant professors will begin to recognize that they have lost half of the time they've been allotted to prove themselves. Their behavior will change substantially in the second half of that period, compared with the first half.<sup>7</sup>

We can use the idea of expected duration to interpret business startup processes. For example, we can explain what happened in April 2000 as a change in investors' socially constructed duration expectations about dot-com firms. Many of the entrepreneurs who started dot-coms were thinking about their enterprises within a very long time frame. In fact, it's not clear that a closed-ended time interval was even salient to many of them. They might have been dreaming of an IPO, which would have meant that they were

thinking, "Well, I probably have 5 or 6 years or so from startup time, to when venture capitalists get involved, to when we go public." They thus fantasized that they had 5 or 6 years to prove themselves. For them, 1 month, 6 months, or 1 year was really not a conspicuous time unit. Thus, they felt no pressure to do anything in a hurry, and they wouldn't have felt any pressure until they were halfway through that lengthy period.

In addition to duration effects, we must also be attentive to period effects—changes produced by historical events and forces that have a similar effect on all organizations, regardless of age (Aldrich, 1999, 201-216).<sup>8</sup> In April 2000, a classic period effect occurred. Suddenly, the perceptions of investors changed from "we are waiting for an IPO in 6 years" to saying that "we want to see results more quickly." The entrepreneurs who had been thinking about time in half-decade chunks were forced to think of time in terms of months: "I'm a new venture, and all of the sudden my investors say they want to see some positive cash flow inside of 18 months."

Nothing changed insofar as the fundamental issues about building a business were concerned. What changed was the expected duration during which people expect the building process to occur. The definition of a successful startup was no longer, "Take 6 years, build the company, and have a public exit event." Now, the normative expectation for the duration of a successful startup was, "Within 18 months we want to see positive cash flow, or at least we want to see a movement toward positive cash flow, maybe even profitability."

Thus, to understand the evolution of firms, we need to understand the social expectations they face regarding pacing and duration and how expectations change over time: What are the community or population expectations? Viewed historically, the definition of time is very context specific (Bartunek & Necochea, 2000). In particular, how time is defined in a particular era affects the urgency with which people carry out what they are doing. People slow down or speed up, depending on how close they are to expected timelines and guideposts. In describing the e-commerce world of the late 1990s, Kanter (2001) noted that "in the everything-faster e-world, where innovation is improvisational theater, opportunities become themes before the need is fully documented, the actors start the play while the producer is still finding backers, and the team celebrates milestones while the ending is still undetermined" (p. 280).

One interesting area of inquiry is the extent to which firms, when they experience a shift occur like the one in April 2000, have the ability to adapt and adjust to changes in social expectations. In the fall 2000, many observers predicted that something like 80% of the dot-coms would no longer be around by 2003. Observers were not talking like that before April.<sup>9</sup> Indeed, we heard few mentions of impending firm mortality. We knew that mortality was a likely ending event for many of these firms, but there was no sense of urgency. Now, in 2001, predictions of doom are being echoed over and over again, increasing the pressure on entrepreneurs to shorten their time horizons and not think about time in terms of half decades or even years.

Timing thus represents a critical part of the selection logic in evolutionary models. Survival often depends on small differences in the co-occurrence of several events (Carroll & Harrison, 1994). Rapid changes in selection environments can change the terms on which resources are available, thus altering the fate of organizations and populations. For example, in some technology fields, a new system or device introduced in September may garner few sales, although the same technology introduced the previous January might have come to dominate its market for some time. At the population level, in their study of San Francisco area hospitals' survival from 1945 to 1990, Ruef and Scott (1998) showed that powerful period effects can lead to historically contingent explanations. Outcome-based explanations and cross-sectional data thus put our empirical generalizations at risk by ignoring the importance of timing.

## Summary

How can we study pacing and duration? We need information on when changes were initiated and completed, and on key events within the changes. Such information must be collected at the appropriate level and in a timely fashion. However, collecting timely information from archival and publicly available data is difficult. Often, such data can tell us when an event was completed, but not when people or organizations began working on it. So, we know the outcome but not the sequence and pacing of events leading up to it. For example, most corporate merger and acquisition negotiations begin in secret, and many never come to fruition. If we study only successful mergers and acquisitions, we might gain a false sense of the pacing and duration of the process. If we rely on public

announcements, we also obtain only a partial picture of the process. Obtaining timely information will require that we develop alternative strategies for collecting it.

Fieldwork and real ethnography—the kind that goes beyond “data collection by walking around the office floor”—is costly, but it is critical to discovering the proper intervals for subsequent large-scale observation and data collection (Stewart, 1998). Fortunately, we have a number of exemplars of how to conduct rigorous process-oriented field studies and ethnographies, from Barley’s (1990) intensive field immersion studies to Van de Ven, Polley, Garud, and Venkataraman’s (1999) dynamic quantitative research. Barley spent months observing technicians in two Massachusetts’ hospitals, and Van de Ven and his colleagues tracked the development of innovations in real time and in their natural field settings.

I recognize that investigators face resource constraints on the kind of information that they can collect. If we had unlimited resources, we would obviously choose representative samples and follow them over long periods of time. We would have an army of people documenting everything that happens in the organization. We don’t have that available to us, so the question is, What kinds of compromises do we have to make? Van de Ven et al. (1999) and Barley’s (1990) research shows us that questionnaire- or survey-based research is not the only way to think about the problem.

In planning survey-based research, researchers usually think about large *N* studies and of the need to compile sampling lists based on some known universe. But in event-driven research, we need a lot more information about micro-events whose consequences aren’t yet known. That probably means spending more time in a smaller set of organizations, using other methods than survey research. Undoubtedly, our sample size will be smaller and more time will be required for that project than a survey. I recognize the inevitability of such constraints.

That’s why, in my preconference seminars at Academy of Management meetings about the obligations of senior scholars, I’ve said that we don’t just want to say to junior scholars, “Okay, we haven’t done very well at this, and we haven’t succeeded. Write us off and you do it better.” Junior people could justifiably look at us and say, “Why didn’t you do this stuff when you had the chance? If you didn’t do it, why do you expect us to do it?” I recognize this contradiction, and

thus I’m also speaking to the more senior scholars who still have the energy to gear up for one last dash to the research frontier.

### WHAT HAPPENS NEXT?

Picture a typical empirical article packed with regression coefficients or other measures of one variable’s effects on another. If the authors were graphically inspired, their argument is probably illustrated by something like Figure 3, showing an arrow between several circles or boxes.<sup>10</sup> The figure and accompanying words in the text make an explicitly causal argument, although the time period represented by the arrow’s flight is not usually spelled out. Looked at from an evolutionary point of view, the results merely describe a moment in time, captured as the people and organizations in the study were on their way to new destinations. I’m often puzzled by how the results are discussed and why several questions, in particular, are left unanswered.

First, if the form, direction, and magnitude of the relations discovered in the analysis continue to hold, what will the organizations look like a few time periods into the future? We often remind our students that the results of a single study don’t constitute a sound empirical generalization. Only replications can tell us whether our theoretically based models have improved our understanding of the phenomenon. Overlooked in this formulation is the hidden truth that the coefficients in our research results are historical artifacts! Until we’ve tested the model on which the research is based in other periods, we don’t know the extent to which our results are dependent on the unique historical circumstances in which the tests were run.

All our results are, in a sense, about the past. Critics often chide me that “evolutionary theory is backward looking—it only helps us understand what has already happened.” They claim the higher ground and argue that they are engaged in a science of prediction. So I ask them, “When, exactly, did you collect your data? And when did you analyze it?” None of them ever claim to have written their results section before the data were collected. After all, building an explanatory model is easy once you know the outcome. But the strength of any claims about research results rests on our ability to project, using our understanding of the underlying process, what will happen

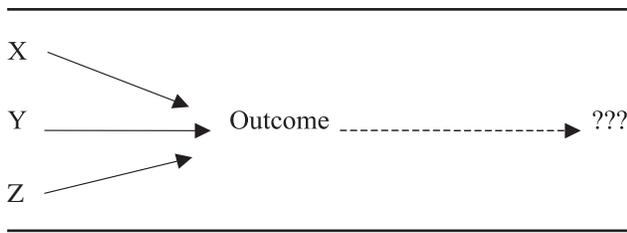


Figure 3: What happens next?

next in the organizations we have studied. To do that, we have to understand relationship dynamics, not just the presence of relationships.

Second, if a positive or negative association is found between two variables, can it be sustained into the future indefinitely? Are there inherent limits on how strongly the two variables can become linked? For reasons I don't understand, thinking about organizations as complex adaptive systems has just not caught on in OMT, despite its obvious utility in diagnosing flaws in causal models (Anderson, 1999). I've never forgotten Buckley's (1967) admonition to look for deviation amplifying and deviation dampening feedback loops in organizations evolving. Decades later, Senge (1990) used well-understood principles of feedback loops in dynamic systems to create simple but powerful models of learning organizations. Simply put, most organizations evolve not only through positive but also negative, moderating, and balancing feedback processes. Knowing where you have been doesn't necessarily tell you anything about where you might be going (Boal, Hunt, & Jaros, in press).

I'm puzzled by discussions of research results that implicitly assume equilibrium has been reached in populations of people and organizations. For example, if diverse weak ties give entrepreneurs an advantage in competing for resources, does that mean that surviving entrepreneurs will eventually have only weak ties with others? If strong ties give entrepreneurs the emotional support they need to persist in the face of discouraging results, does that mean that surviving entrepreneurs will eventually have only strong ties with others? Clearly, balancing and moderating forces affect the mix of ties that entrepreneurs can sustain, and a complete picture of entrepreneurial networking must make room for such subtleties (Aldrich, 1999, 81-88).<sup>11</sup>

People tend to forget things they haven't used recently. Organizations aren't immune to memory loss either, as Argote (1999) and her colleagues have shown in a series of creative projects. Knowledge that goes unused decays quickly, although the process

might be arrested if means are found to institutionalize it. For example, Argote estimated that up to 80% of what an organization learned could be lost in a year or so, without continuous reinforcement. In models of organizational evolution, organizational learning needs to be complemented with organizational forgetting and other balancing processes. We might throw in a little laughter as well (Kundera, 1980).

I'm very hopeful that simulation and computational or agent-based modeling (ABM) can give us the additional tools we need to test the dynamic implications of our research results (Carley, 1991; Lomi & Larsen, 2001). In criticizing current practice in OMT, McKelvey (in press) noted that model building is the missing element in the theory-model-data trilogy of the scientific realism approach. In the best of all worlds, data would never touch theory directly. Instead, theory informs the modeling process, which in turn implies and is informed by research design and results. Simulation and ABM give researchers the opportunity to see what happens next without being constrained by meager research budgets and incomplete data sets (Keister, 2000). For example, an extremely clever simulation, showing the consequences of exploration and exploitation strategies, was a major reason for the appeal of March's (1991) scheme. Sterman and Wittenberg (1999) used dynamic modeling to explore the evolution of scientific paradigms, formalizing the propositions in Kuhn (1970).

In many respects, criteria for evaluating computational models are no different from those used for any other type of social science model, as Alessandro Lomi (personal communication, March 2001) reminded me. A model has to be expressive and accurate. The term *expressive* means that we must capture the crucially relevant aspects of the process in our model, and not just metaphorically. Models based on selection logic must spell out how the selection process actually works. The term *accurate* means that what is true in (or can be proven about) the model has to be true for the process as well. Expressiveness requires theoretical abstraction, whereas accuracy requires empirical grounding. Achieving both in the same model can be difficult.<sup>12</sup>

## CONCLUSION

I've developed some of these themes at greater length in chapter 12 of my book, and so let me briefly summarize them here. First, we need to improve our

mental maps of the organizational landscape. Currently, academic and popular writing on organizations is skewed toward the largest and most prominent organizations, distorting our vision. A more realistic view should include the full range of diversity found in organizational communities, especially the organizations struggling to emerge from what Kaufman (1985) called the “primordial soup” of creation.

Second, we should be wary of outcome-driven explanations. Think forward, not backward, and build event-driven explanations and research designs. Use outcome-driven interests to spark inquiries, but don’t depend on them to build empirical generalizations. Third, we need to build time explicitly into our theories and models. Pacing and duration, when left unexamined, imply an equilibrium-based view and a static world. In this unrealistic scenario, organizations could not evolve, for nothing ever changes.

Fourth, we must recognize that all empirical generalizations are about the past. What differentiates an evolutionary view from others is an explicit recognition that the goal of organization studies is to build models of what happens next. The value of our research results depends on our ability to construct models of the underlying change process, use them to improve our theories, and then apply them to building better models. Accomplishing that goal requires that we understand the dynamics of relationships within an evolutionary framework.

## APPENDIX

### Answers to the “Who Wants to Be an Evolutionary Theorist?” Quiz

1. In 1996, approximately 23 million taxable entities—nonfarm sole proprietorships, partnerships, and corporations—filed business returns with the Internal Revenue Service (U.S. Internal Revenue Service, *Statistics of Income*).
2. About 5.5 million businesses (not establishments!) employed at least one person for enough hours in at least one quarter of 1996 to pay Unemployment Insurance and Social Security taxes (U.S. Census, *County Business Patterns*).
3. In 1996, 4.6 million firms were incorporated as legal entities (U.S. Internal Revenue Service, *Statistics of Income*). Most were quite small, of course.
4. In 1999, according to the *Global Entrepreneurship Monitor* (Reynolds, Hay, & Camp, 1999), about 7.3 million startup attempts were made in the United States (P. Reynolds, personal communication, March 2001). Reynolds estimated that about 2.3 million would

become “baby firms” in 12 months, based on previous results.

5. According to the Business Information Tracking Series (BITS) database—formerly called Longitudinal Establishment and Enterprise Microdata (LEEM) of the Small Business Administration—about 0.5% per year of the establishments with employees in 1990 were acquired by another firm during 1990 to 1994 (Aldrich, 1999, 261-264; Small Business Administration, 1998). In round numbers, that’s slightly fewer than 30,000 per year.
6. Aggregating across the NYSE, the AMEX, NASDAQ, and the various regional exchanges, shares of approximately 20,000 firms trade relatively freely in the public market.
7. In 1990, 14,023 firms in the United States employed 500 workers or more, employing more than 43 million people (Small Business Administration, 1994). At the top, 484 enterprises employed 10,000 or more.
8. In 1999, 510 firms had their Initial Public Offering, down from a high in 1996 of 758.
9. None. There are *no* thoroughly documented findings, based on rigorous research designs, showing that any personality traits consistently differentiate successful from unsuccessful entrepreneurs (Shaver, 1995). Gartner, Shaver, and Gatewood (2000) noted, “In short, the stereotype of the highly independent, financially-driven, risk-seeking entrepreneur may be nothing more than a distillation of the retrospective stories that entrepreneurs have told researchers in the past” (p. 10).

## NOTES

1. Additional revenues earned from readers actually purchasing my book to learn more about evolutionary theory will be donated to my youngest son, Daniel. He’s working on his Ph.D. in political science at Harvard and has become enamored of rational choice approaches, but is also finding that Theda Skocpol’s (1984) arguments for the comparative-historical path are quite appealing.

2. As I recall, Gerry Davis and Mark Mizruchi came close to getting them all right. But I think most people got between half and three quarters correct. Of course, I used the honor system in scoring the answers.

3. As Bill McKelvey reminded me, we focus on large firms—especially those of us in business schools—because that’s where the money is (e.g., in 1994, 9% of all corporations controlled about 97% of all corporate assets).

4. If you heard Joel Baum’s introduction of me at the Organization and Management Theory (OMT) session that led to this article, you’ll know what I’m talking about.

5. Bill McKelvey insisted I put this disclaimer into the body of the article.

6. I am indebted to Ted Baker for spelling this point out for me. I don’t think any of his firms ever did poorly, however.

7. Think "sleepless nights" and "take-out dinners" and you get the picture.

8. In complexity theory terms, the basin of attraction might change in the middle of the historical era being studied. We could no longer assume that a single equilibrium point attractor was controlling a stable set of phenomena. McKelvey (1999, in press) makes this all abundantly clear.

9. There's a rumor that Jack Welch knew, however.

10. Journal editors seem to like graphical models, for some reason, even though most do little more than show, with boxes and arrows, what the text says in words—as I've done in Figure 3.

11. For a more general discussion of the emergence and persistence of order in complex systems, see McKelvey (in press).

12. Alessandro Lomi (personal communication, March 2001) noted that he is "dissatisfied by the metaphorical use of models borrowed from other fields and designed to address specific problems. I think that any model can be useful but has to be embedded in a detailed understanding of the phenomenon at hand."

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