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2009

LOST IN SPACE, OUT OF TIME: WHY AND HOW WE SHOULD STUDY ORGANIZATIONS COMPARATIVELY

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LOST IN SPACE, OUT OF TIME: WHY AND HOW WE SHOULD STUDY ORGANIZATIONS COMPARATIVELY

Howard E. Aldrich

Over the past few decades, there have been many calls for more “comparative research” in organization studies, and those calls have been answered so frequently that they have made possible this edited volume (Dobbin, 1994; Scott, 2001). I like to think of comparative research in terms of paying more attention to context, following Tsui’s (2006) definition of contextualization as meaning the incorporation of context “in describing, understanding, and theorizing about the phenomenon within it.” Indeed, scholars have offered so many possible dimensions of “context” that Von Glinow, Shapiro, and Brett (2004) proposed the term “polycontextualization” to cover all possible bases. In this chapter, I argue that “organizations” constitute a very heterogeneous set and that understanding the contextual effects of time and place gives us some leverage in explaining such heterogeneity.

I focus on just two dimensions of context, albeit big ones: time and space. I chose these two because of the substantial progress that’s already been made in incorporating them into organization studies and because there is still so much that can be accomplished. I begin with a critique that McKelvey and I offered 25 years ago of the field’s inattention to context,
especially the scope conditions surrounding research findings. I note that our critique fell short because it devoted insufficient attention to time and space as critical aspects of organizational environments. Despite considerable progress in the ensuing 25 years, the problem we pointed out persists, with many investigators still indifferent to reporting important contextual information about their projects.

Echoing the arguments of others in this volume, I then offer a brief defense for paying more attention to time and space in studying organizations and suggest how we might study them. I offer an amendment to the argument that McKelvey and I made, with the benefit of hindsight. With regard to time, I suggest a life course perspective and an historical perspective. With regard to space, I suggest looking at regional and national differences. Let me be clear about my purposes.

First, I am not making an argument for yet another taxonomy of organizations and organizational forms. Regardless of the taxonomy used, it must take account of time and space. Second, I am not making an argument for any radical changes in research designs. Instead, my arguments turn on making more effective use of the designs already available to us by explicitly taking account of time (clock time and constructed time) and space (local, regional, and national). Clearly, we have many examples of projects within the field of organizational studies that have emphasized "context." For example, the core idea of institutional theory is that cultural–institutional environments affect organizational practices and policies. Nonetheless, I believe that we have not paid enough attention to the temporal and geographical conditions that may account for the observable differences in organizational practices and policies. Consequently, we may have put too much faith in generalizations from our research projects.

THE ORIGINAL CRITIQUE OF MCKELVEY AND ALDRICH

In our 1983 paper, McKelvey and I (McKelvey & Aldrich, 1983) took the field of "organization science" to task for not paying sufficient attention to the scope conditions under which research findings are valid. (Today I would argue that the field also had not paid sufficient attention to matching theoretical ambitions with research designs.) We argued that the field fell short on three critical criteria: classifiability, generalizability, and
predictability. We noted that samples of organizations were so poorly
described that classifying them was impossible, that generalizations were
being carelessly drawn, and that the predictive power of most theories was
extremely weak.

With regard to classifiability, we noted that researchers mostly collected
data on organizations conceptualized as legal entities, most of which were
large firms. With regard to generalizability, we noted the extreme
heterogeneity of many samples. Samples contained organizations from
diverse industries, sizes, locations, and so forth, and we wondered whether
generalizations could safely be drawn to some larger population. With
regard to predictability, we noted that researchers were often content with
finding weak patterns in their analyses.

When we looked more closely at the literature, we discerned two broad
approaches investigators seemed to be using in designing their research:
(1) treating organizations as all alike, or (2) treating organizations as all
unique. Most studies followed the “all alike” approach, reporting their
results as if one organizational form characterized all organizations. This
tendency was evident in the haphazard way samples were drawn and in the
expansive way generalizations were offered. We noted that a conservative
conclusion from a typical project would be that generalizations applied only
to these kinds of organizations. In spite of this, we found an implicit
assumption that the results applied to all organizations, with no obvious
qualifying statements. Lack of concern for representative samples was also
evident, as was an overall cavalier attitude toward organizational differences
and representativeness.

Today, we might characterize this “organizations are all alike” approach
as one of extreme decontextualization. If organizations are all the same and
if researchers are studying their own special samples, no detailed attention
to “description” is required. I believe this is analogous to what Heath and
Sitkin (2001) described as the “behavior is behavior” approach to organ-
izational behavior. That is, it simply does not matter if the particular
behavior someone is studying happens to be inside an organization. If
investigators can ignore context and assume that “variables” mean the same
thing across all contexts, then they can bring very high-powered results to
bear on the data. However, is that a safe assumption to make?

The “all unique” approach was found less often in our 1983 survey, but
we did find some studies reporting their results as if every organization
possessed a unique form. Explicit statements of this position were hard to
pin down but were nonetheless implicit. In particular, case studies of single
organizations often seemed to follow this logic, as researchers carefully
avoided generalizing their results to other populations. Today, we might characterize this approach as “extreme contextualization.” We see clues to this approach in studies that essentially report no unexplained variance, instead offering very tidy explanations in which everything is fully accounted for.

As a remedy for the ills we diagnosed, McKelvey and I prescribed a population perspective, grounded in evolutionary theory, that emphasized research methods that improved the description and classification of organizational forms, defined more homogeneous groupings for purposes of improving generalizations, and focused on improving the power of explanations. We noted that change would not come easily, as the old ways were embedded in a research paradigm widely shared by organizational researchers. Nonetheless, we were cautiously optimistic.

Looking back, what we failed to note – but should have – was another feature of the evolutionary approach: all explanations of organizational outcomes must pay attention to organizations and their environments. Instead, we focused almost entirely on organizations and neglected to take account of the extent to which organizational outcomes are shaped by two aspects of their environments: time and space. By “time,” I mean both “clock time,” measured in days, weeks, months, and years, and “socially constructed time,” measured in units that are meaningful to actors within a particular organizational and social context. For example, in high reliability organizations, “time” may be thought of in terms of how long it has been since an accident, rather than by a daily or weekly rhythm of work (Weick & Sutcliffe, 2001). By “space,” I mean differences due to location in particular geographic units, such as cities, regions, and nation states.

As a thoughtful reader pointed out, there are several complementary definitions of time and space effects. In this chapter, I often link space to a bounded political jurisdiction, such as a city or nation, and thus my focus is on laws, regimes, and other institutional structures and practices. However, another way of thinking about the importance of time and space is in terms of resource constraints (Freeman & Audia, 2006). Hawley’s (1950) classic treatise on human ecology noted that time and space impose considerable limits on what organizations and populations can do, as overcoming the friction imposed by time and space constraints requires resources. For example, in Galaskiewicz’s study of children’s use of urban facilities, time and space are important not only because of simple cost factors, but also because getting to places that offer activities for children requires overcoming the friction of distance caused by time and space constraints (Galaskiewicz, Inouye, & Savage, 2008).
Time and space, as discussed above, are important for the modeling and data analysis portions of our research projects and should be incorporated into our data collection and research designs. However, as my examples will reveal, they are often proxies for other processes, structures, and patterns that we theorize and that themselves vary across time and space (norms, laws, culture, and specific historical conditions). We won't be well served if we simply incorporate time and space as “variables” into our models. Instead, I will argue that we need to use the context of our cases to theorize the processes, structures, and patterns that vary across time and space.

Methodological Problems Persist: Time and Space in ASQ

Back in 1983, McKelvey and I were harshly critical of the skimpy information offered in the methods section of articles published in top journals, in addition to calling into question the choices made by investigators in designing their research. For this chapter, I examined all 128 empirical papers published in the *Administrative Science Quarterly* (ASQ) between 2000 and 2007. For each article, in keeping with my “space” theme, I noted whether more than one nation was sampled, and if so, whether that nation was the United States or some other country. For my “time” theme, I examined whether the authors reported when they had collected their data. I also noted if they collected data from more than one point in time and if so, I noted the length of time covered by the study. My results are shown in Table 1.

Somewhat surprisingly, given the increasing internationalization of the field of organization studies, as reflected in the proliferation of international journals and an increasing international representation at Academy of Management meetings, over 90% of the papers covered only a single nation. Not so surprisingly, 88% of the single nation studies were conducted in the United States. One might well ask whether organizational life is the same in the United States as in the rest of the world, but there are larger issues involved.

One possible effect of this single nation focus would be a failure to put research into an historical context, as we know that many organizational forms have diffused across nations. Thus, at any particular historical conjuncture, forms within a nation may be at different stages of development and implementation. Another effect of this single nation focus could be a misattribution of causal effects. Investigators could be led to argue that some features of organizations within a nation are “cultural,”
when in fact they are very similar for organizations of the same form in other nations. Lacking a comparative context, investigators may well be tempted to make an inappropriate cultural argument about what they have discovered.

Only about two-thirds of the papers published in ASQ over this time actually reported the dates covered by the research design. In about one-third of the papers, it was simply impossible to discern the years covered by the study. (Note that this was not a problem for authors who explicitly bounded their study interval with specific years, regardless of when they actually collected the data.) Indeed, given that my investigation began in the year 2000, I could even argue that we could not tell what century is covered by one-third of the papers.

What is the implicit claim being made by authors who do not tell us the years to which their data pertain? Does “time” make no difference to the generalizations being offered? Is the structure or process being investigated one that truly “stands the test of time” in the sense that it is invariant over the years, decades, and centuries? Although this proposition sounds absurd on its face, perhaps there are some principles of organizational life that change so slowly that we can safely ignore time. For example, the proposition that complexity increases with organizational size may well be the same in the 19th, 20th, and 21st centuries. However, that is a proposition that should be explicitly stated and defended, perhaps through multiple attempts at replication.

**Table 1.** Space and Time as Reflected in Eight Volumes of the *Administrative Science Quarterly*, Volumes 45–52.

<table>
<thead>
<tr>
<th>Author Report</th>
<th>Percent</th>
<th>N (By Row)</th>
</tr>
</thead>
<tbody>
<tr>
<td>1. More than one nation in sample</td>
<td>9</td>
<td>128</td>
</tr>
<tr>
<td>2. If one nation only: USA</td>
<td>88</td>
<td>118</td>
</tr>
<tr>
<td>3. Dates of data collection reported in methods section</td>
<td>65</td>
<td>128</td>
</tr>
<tr>
<td>4. Data collected from more than one time point</td>
<td>65</td>
<td>128</td>
</tr>
<tr>
<td>5. If data collected over time, time period covers:</td>
<td></td>
<td></td>
</tr>
<tr>
<td>0–4 years</td>
<td>27</td>
<td></td>
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<tr>
<td>5–9 years</td>
<td>19</td>
<td></td>
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<tr>
<td>10–19 years</td>
<td>30</td>
<td></td>
</tr>
<tr>
<td>20–29 years</td>
<td>11</td>
<td></td>
</tr>
<tr>
<td>30–49 years</td>
<td>4</td>
<td></td>
</tr>
<tr>
<td>50 years or more</td>
<td>9</td>
<td></td>
</tr>
<tr>
<td>Not ascertained</td>
<td>1</td>
<td></td>
</tr>
<tr>
<td>Total for panel 5</td>
<td>100</td>
<td>81</td>
</tr>
</tbody>
</table>
When I have presented these results at conferences, more than a few critics have suggested that for laboratory experiments, small group studies, and other investigations inside an organization, it may not matter if we are explicitly told in what year the data were collected. Although this comment sounds plausible at first, it rests on two strong assumptions. First, we must assume that the article was published within a few years of the data being collected and thus knowing the year of publication gives us the “period” for the behavior or structure under investigation. Second, we must assume that the “half-life” of the article is very short and that it will outlive its usefulness shortly after being published.

Otherwise, consider the consequences: imagine that several decades into the future, a scholar wants to examine the possible effects of economic downturns on the likelihood of “associative homophily” at parties in New York City involving business executives (Ingram & Morris, 2007). The scholar finds an article with an innovative research design published 20 years before and wants to replicate it, but because it does not indicate in what year the study was conducted, cannot decide if it was done before or after a major economic crisis that erupted around the time the paper was published. Does the desire to seek out others of the same race at business gatherings increase in turbulent times or is it a constant preference?

Just to be crystal clear: we either must assume that the patterns of attitudes, behaviors, structures, or other phenomena analyzed in an article are time invariant or that papers are published for immediate consumption only and have no value for future generations of scholars. If, by contrast, editors accept papers on the assumption that scholars will still be looking them up in the decades to come, checking to see whether anything has changed, then readers must be explicitly told when the data were collected.

In contrast with my discovery concerning missing information with regard to when data were collected, I was heartened to find that almost two-thirds of the studies reported data for more than one observation point. Approximately 27% used a narrow window of four years or less and another 19% followed cases between five and nine years. One quarter of the studies conceptualized the passage of time in terms of decades, following their cases for 20 years or more. At the extreme, 9% covered at least half a century.

Choosing a time frame for data collection requires investigators to consider the rhythm and periodicity of significant changes in what is being studied. For some processes, such as organizational failures, a decade or less might be sufficient. In contrast, for large-scale institutional changes, decades might be needed. Implicitly, then, the data in Table 1 show that few investigators were thinking in terms of large-scale changes.
My analysis of empirical papers published in ASQ since 2000 suggests that many of the concerns McKelvey and I expressed in 1983 are still relevant today. Investigators are still making implicit assumptions about the time and space invariance of the processes and structures they are studying, with most papers exemplifying a single nation short-term time orientation. Almost a third of them evidently feel their results are time-invariant.

Why Study Organizations Comparatively?

Perhaps my position is already clear from my review of what McKelvey and I argued and from my critique of what I found in ASQ, but let me make two points concerning why we need to pay attention to context in terms of scope conditions and theory testing. First, investigators need to make clear the scope conditions they claim for their generalizations. Adding time and space to one’s research is not simply a way to explain more variation in organizational outcomes. That is a fool’s errand. Instead, I believe that incorporating time and space into our comparisons improves the reliability and validity of our generalizations. Omitting a comparative context from our research can be a substantial threat to validity. In addition, studies of diffusion and population dynamics often chose empirical settings that “are biased toward large or otherwise significant industries, organizational populations, or phenomena” (Denrell & Kovács, 2008). Such biases threaten internal and external validity.

Second, with regard to theory and model testing, unlike the state of the field in the early 1980s, our theories today heavily emphasize organizational environments. So, whereas it made some sense to neglect time and space in 1983, it makes no sense now, when almost all our macroorganization theories concern time and space. As McKelvey (2002) has argued, our goal should be to improve each leg of the three-legged stool on which our field rests: theory, model, and data. We should use our theories to build models that we then test with data. Feedback from those tests allows us to fix the model and try again. Research is conducted, then, to set plausible parameter values for our models, rather than to “test theory.”

Indeed, in the social sciences, it would be an extraordinary project that actually was able to challenge the fundamentals of a good theory. Instead, the best we can hope for is that repeated failures of models to be verified with data eventually call into question the theories. One excellent tool in this regard is simulation, such as through agent-based modeling. March (1991) made good use of this technique in his widely cited paper on exploration and exploitation.
Consider the role of time and space in two very popular approaches to organizational analysis. Organizational ecology research rests on the bedrock assumption that population dynamics depend on where a population is in its growth trajectory and also on the scale at which competition and cooperation are occurring. It is impossible to fully test ecological models without taking into account the full history of a population. Institutional organizational research at the population and field levels rests on the assumption that the diffusion of practices and forms over time depends on opportunities and constraints at multiple levels of analysis. It is impossible to fully test institutional models without taking into account the speed and shape of the diffusion curve over time and over diverse locations. Thus, from the viewpoint of setting the scope conditions and testing the models implied by our theories, we need to take time and space into account.

**How do we Build Time and Space into Comparative Organizational Research?**

In an unjustly overlooked celebration of the contemporary relevance of the Chicago school of sociology, Abbott (1997) argued that “no social fact makes any sense abstracted from its context in social (and often geographic) space and social time . . . Every social fact is situated, surrounded by other contextual facts and brought into being by a process relating it to a past context.” He argued that we should keep social facts in their contexts, describing the temporal and spatial structures of social contexts or “locatedness,” to use his term. Abbott felt that a failure to take time and space into account made our work boring and reflected an unwillingness to take intellectual risks. Certainly, rising to the challenge posed by Abbott will be a major undertaking for most scholars. Nonetheless, it is a challenge worth meeting.

**Time in Comparative Research: Two Scales**

We can include “time” in our comparative analysis at several scales, and I will focus on two: at the level of the life course of specific organizations, and at the level of organizational populations. I focus first on the life course of organizations as a response to the question that Dave Whetten asked at our conference in Sundance, “what happens if we only focus on big and old versus new and small organizations?” In examining this question, I will treat time on a micro scale.
In my studies of entrepreneurship and organizational startups over the past several decades, I have become acutely aware of the difference between the mundane and the improbable. In the most recent decade, about 7 million startups per year have been launched by potential entrepreneurs in the United States. Within about six years, a little more than 2 million are left. During that time, only about half of those have managed to employ at least one person. Of the ventures that become sufficiently organized to qualify as truly viable businesses, only about 1 in 10 actually grow. Of those that grow, about 1 in 10,000 is lucky enough to have an initial public offering (IPO), at which time the founders are able to cash in on their success. Recently, the number of IPOs has been dwindling, with the number of high-technology IPOs at less than 50 per year. These 50 constitute the “black swans” (Taleb, 2007) of the entrepreneurial world. That is, they are highly improbable, have a high impact, and are predictable only after the fact.

Were we to focus only on the black swans, rather than comparing them with the mundane initial population of startups, or with any of the other population subsets I mentioned earlier, we would miss the critical feature of entrepreneurship in the United States: entrepreneurial success is difficult to attain. From a comparative point of view, we must consider two possible explanations for the process revealed by these numbers (Haveman & Rao, 1997). From a selection perspective, the data paint a picture of initial heterogeneity winnowed by time. The initial organizations in the population are simply overwhelmed by forces beyond their control, leaving a small set of lucky survivors. Note that testing this explanation requires we capture as much of the startup group’s initial heterogeneity as possible. If we are studying startups within an existing population, identifying population members will be fairly straightforward. However, if we are farsighted and fortunate enough to recognize the potential development of a new population, not yet recognized by data gathering agencies and bureaus, then bounding the population to study will be a daunting task.

From an adaptation perspective, the story is one of organizations learning at each stage of the process, with some learning well and others not learning at all. Those that learn survive to go on to the next point in the life course, whereas most of those learning slowly or not at all are eliminated. From this perspective, the initial group of startups might still be heterogeneous, but with respect to their capability for learning. Plus, as in testing implications from the selection perspective, investigators must still capture as much of the initial heterogeneity as possible. Heterogeneity arises from multiple sources, including founders’ goals and strategic preferences. For example,
businesses started by families may not follow the same path as spin-offs from large corporations. Many owners of family businesses profess a desire to remain small, although circumstances may conspire against this wish. Conceptualizing “heterogeneity” thus requires us to have a deep understanding of what we are studying.

Notice several ways in which time figures into this narrative. First, to understand survival, we need to take a multiyear view, following organizations long enough to ascertain their fate. Second, because the factors affecting entrepreneurial success vary over time with changes in the economy, government regulation, the technological opportunity structure, and so forth, what enables some organizations to adapt in one era may be irrelevant in the next. Third, without following organizations over time, we cannot adjudicate between the selection and the adaptation models.

In keeping with my argument concerning the need for modeling, I note that “time” needs to be modeled explicitly when we are following organizations over time. At what scale should time be measured: minutes, days, weeks, months, or years? An examination of the papers published in *ASQ* and used for the analysis in Table 1 showed that very few were explicit about the units of time in which the investigators thought a process unfolded.

In his concept of “socially expected duration,” Merton (1949) gave us a clue for making such decision about units of time. He noted that humans tend to base important decisions on how long they think a particular relationship or event will last. For example, in her study of committee work, Gersick (1988) found that members dawdled and frittered away time until they perceived that they were about halfway through the time allocated to them to complete their work. At the halfway point, such as two weeks into a one-month assignment, they suddenly began taking the assignment seriously. Another example of the fragility of any particular expectation regarding expected duration can be taken from the example of the market for IPOs in the late 1990s.

Before April 2000, entrepreneurs starting new high-tech ventures and seeking venture capital thought about the time to grow toward an IPO in years, rather than months. Typically, they could expect venture capitalists not to pressure them to prepare for an IPO until five or six years had passed. After April 2000, with the crash of the stock market and, in particular, the drying up of the market for IPOs, high-technology entrepreneurs found that expectations regarding duration had changed drastically. Whereas they had become accustomed to thinking in chunks of six years, they suddenly found themselves forced to think in six month chunks or less. Venture capitalists
were asking for rapid results and were quick to pull the plug when entrepreneurs faltered. Thus, viewed from a socio-historical perspective, the very definition of time is context specific.

My second example concerns micro-level processes in which social interactions follow a daily rhythm and data collection and analysis must occur at the same scale. Black, Carlile, and Repenning’s (2004) reanalyzed ethnographic data from Barley’s (1986) study of the implementation of computerized tomography in two hospitals. Barley documented the impact of the new technology on role relations and changing patterns of social interaction between doctors and technologists. Barley supplied enough information to allow Black et al. to create a dynamic simulation of how the changes unfolded over the nine months of Barley’s observations. Their reanalysis provides an excellent example of a model in which time is conceptualized and measured at the level of the working day.

In generating the equations underlying their simulation, Black et al. modeled activities and interactions on a relatively fine-grained time scale of days. The resulting models, with their recursive interactions, revealed additional insights about the process of occupational boundary maintenance that were deeper and more complex than Barley’s original analysis implied. As in the example of IPOs after April 2000, modeling these processes required that investigators grasp the natural rhythm of the interactions, necessitating a profound understanding of the context.

Time 2: The Long Sweep of History
My example of the market for IPOs takes us into historical territory. Following the lead of population demographers, Aldrich and Ruef (2006) classified historical influences into three types of effects: cohort, period, and maturation or aging. Aging or maturation effects describe the secular process of aging, a process I referred to in the previous section. For example, organizations might become less adaptable as they age. A cohort effect occurs when historical events have a differential impact on younger versus older organizations (or vice versa). For example, high oil prices might seriously weaken younger organizations but have little effect on older ones. Stinchcombe’s (1965) classic paper on social structure and organizations developed this principle under the concept of “imprinting,” noting that organizations founded in the same era tend to resemble one another. Johnson (2007) drew on this notion in explaining why the Paris Opera today still shows traces of its roots from the 17th century. A period effect occurs when historical events have similar consequences for different age cohorts.
For example, high oil prices might actually force organizations of all ages to economize on their operations.

Back in the 1940s and 1950, scholars of national economic development created models that rested on static, cross-sectional comparisons of “under-developed” economies with those of the “developed” Western nations. They assumed that the national institutions supporting developed economies needed to be replicated in the underdeveloped nations and that it was just a matter of time before such nations “caught up.” Nations were grouped into stages and it was assumed that developing nations were merely in an early, more primitive phase along a path to development. These linear, one size fits all models were subsequently discredited. Scholars realized that “development” reflects not merely the internal conditions of a nation but also where it fits into the global economy. Moreover, a nation’s historically determined past sets limits on where it can go next. Thus, current models recognize path dependence and global interdependence. In short, comparing organizations at one point in time does not necessarily reveal everything we should know about what makes them similar or different. We need to know the path along which each organization developed and where it fits into the larger context.

I have chosen three examples to illustrate the advantages of contextualizing organizational change in terms of what transpires over years and decades. First, to illustrate the power of taking age, period, and cohort into account, I have chosen an example from the health care field and changes in the hospice industry. Second, to illustrate the potential of focusing on diffusion processes within a single extended period, I have chosen examples from the field of corporate governance. Third, to illustrate the advantages of a multipronged project that investigates the genesis, shakeout period, and subsequent development of a population, I have chosen the radio broadcasting industry.

In studying the health care field after World War II, Scott and his collaborators (Scott, Ruef, Mendel, & Caronna, 2000) distinguished between three different historical periods. First, in the early era of professional dominance, physicians enjoyed high levels of prestige and a great deal of autonomy from external interference. Second, in the era of the 1960s, the federal government became heavily involved in health care. The national government began to play a major role in regulating health care and in controlling the allocation of funds to different kinds of health care facilities and equipment. Third, in the 1980s, a new era of market orientation and managerial control emerged, in which competition and market forces were seen by regulators and major corporations as the driving force behind how decisions ought to be made regarding health care.
Applied to the hospice industry between 1974 and 2006, Scott’s characterization helps us understand how and why that industry changed. A major period effect occurred in 1983 with the passage of the Medicare Hospice Benefit Act, as it affected all hospices. The act enabled hospices to receive reimbursement for patient care from the federal government. However, hospices that chose to become Medicare certified opened themselves up to government oversight, an unwelcome development for many of them. A major cohort effect occurred in the 1980s with the onset of the era of market orientation, as it affected hospices differentially by age. Traditional hospices were nonprofit, whereas new hospices were profit oriented. Between 1992 and 1999, the number of for-profit hospices increased by 300%, whereas the number of nonprofit hospices rose by only 43%. Going forward, then, there was a substantial change in the mix of organizational forms in the population in response to the changed conditions brought on by market forces.

In contrast with the hospice example, which drew upon age, period, and cohort principles, my next example focuses on organizational changes within a single period: the spread of two governance innovations within corporate elite networks in the decade of the 1980s. Davis and Greve (1997) examined the impact of corporate board interlocks on the diffusion of poison pills and golden parachutes between 1980 and the end of 1989. So-called “poison pills” were adopted by some corporations as a defense against hostile takeovers, whereas golden parachutes were contracts that compensated executives who lost their employment when their firm was taken over. History figured into their analysis in several ways.

First, Supreme Court and U.S. Justice Department decisions in the early 1980s changed the institutional regulations governing mergers, significantly lowering regulatory barriers to hostile takeovers. Second, the rate of diffusion of the two practices differed significantly, with golden parachutes increasing gradually from 1980 to 1989, whereas poison pills shot up dramatically in the middle of the 1980s. At the end of the decade, both of them had been adopted by about half the large corporations in the United States. Differences in the rate of diffusion between the two practices allowed Davis and Greve to assess the relative importance of network contagion versus direct contact in the spread of innovations across the corporate network.

A related example of historical contingency comes from Mizruchi, Stearns, and Marquis’s (2006) study of changes over a 22-year period in patterns of corporate borrowing. In the early 1970s, few large corporations had chief financial officers (CFO’s). Instead, they had financial officers who
acted more like bookkeepers than financial strategists. Bankers on their boards were expected to give advice on key financial matters, and Mizruchi et al. showed that ties to other corporations, through interlocking (shared) directors, influenced whether corporations decided to use debt financing. In the transformed economic environment of the 1980s, however, corporations began to internalize such decisions. Financial officers’ positions were elevated and given the title of CFO as that role became increasingly professionalized and the “corporation as a bundle of assets” view took hold. By the early 1990s, the earlier network effect of being influenced by other corporations’ borrowing behavior was gone. Thus, a claim that “interlocking directorates influence corporate behavior” could not be supported as a time-invariant empirical generalization. Instead, the network effect was contingent on a particular institutional and environmental configuration.

My third example of the advantages of historical analysis illustrates the advantages of investigating the entire life history of an industry. The radio broadcasting industry, with the benefit of hindsight, seems like an inevitable development, given the invention of wireless telegraphy in the late 19th century and subsequent innovations in the early 20th century. However, Kim and Lippmann (2008) showed that the development of the industry was substantially shaped by struggles between contending parties with very different conceptions of who should benefit from the new technologies. In the early days, wireless equipment manufacturers, comprising a group of inventors and entrepreneurs, tried to use patents and other devices to extend the old “point to point” telegraphy system, whereas the U.S. Navy defined the new technology as a national security issue and asserted their dominance over its uses. In contrast, amateur wireless operators framed the issue quite differently, taking advantage of the “point to many” broadcasting possibilities of the technology and arguing for an experimental and entertainment conception of its application.

Before 1920, amateur wireless operators also benefited from a more dynamic set of strategies, as their framing activities evolved over time, in response to changing conditions. In contrast, the inventors and entrepreneurs looked back in time and drew on the wired telegraphy industry as a model. Similarly, the Navy harkened back to an earlier age and tried to lock in the vision they had developed during World War I. Amateurs were continually reframing their conception of what “radio” could be and forging new alliances as field and environmental conditions evolved.

Although World War I gave the Navy a temporary advantage, developments after the war thwarted them as well as the wireless equipment manufacturers’ attempts to frame the situation to benefit their interests. For
a short time, between the war and the mid-1920s, innovation and entrepreneurship flourished across the radio spectrum as many diverse uses of radio broadcasting sprang up. Small businesses and commercial interests began using radio broadcasting to call attention to their offerings, and many noncommercial broadcasters, particularly religious broadcasters, also took advantage of the “one to many” opportunities offered by radio to reach a wider audience. Chaos ensued, as attempts by the federal government to limit the licensing of new stations were repeatedly rebuffed by the courts. Eventually, through a series of hearings held in Washington, DC, the federal government asserted centralized control, through the 1927 Federal Radio Act and the 1934 Federal Communications Act, which created the FCC.

In his analysis of the conflict and negotiation surrounding the various federal hearings and legislative acts, Lippmann (2005) coined the term “ideological capture” to describe the success commercial broadcasters had in framing the terms of the debate. At issue in the struggle over which stations should be licensed was the question of who represented “the public interest.” Commercial interests succeeded in their attempts to frame their own broadcasts as being in the public interest through their dominance of the testimony at hearings, their representation on the initial commission, and disorganization among the noncommercial interests.

Lippmann (2007) showed that the consolidation of commercial broadcasters’ interests, as enshrined in the legislation, had a devastating impact on the fate of noncommercial broadcasting. Whereas educational and religious broadcasters had made up a substantial portion of all stations throughout the early to mid-1920s, after the 1927 act the number of full-time commercial stations grew rapidly. In contrast, part-time commercial stations, educational, and religious stations gradually declined, both absolutely and proportionately. Failure rates for noncommercial stations increased, whereas those for full-time commercial stations decreased. Lippmann concluded that changes in the institutional environment of radio broadcasting shaped the way that market forces were allowed to play out in the industry. The 1927 Radio Act changed the institutional logic of radio broadcasting by bestowing legitimacy on a form supported by advertising, which only a few years before had been treated with hostility by many important individuals and groups (Lippmann, 2007). Full-time commercial broadcasters succeeded not because they had any direct competitive advantage but rather because competing uses of the radio spectrum were severely limited by federal legislation, which they played a major part in shaping.
Space in Comparative Research: Two Scales

I have already noted Abbott’s anguish at the neglect of location as a context in social science research. He argued that research that examines organizational phenomena in only a single community, region, or nation runs the risk of producing decontextualized empirical generalizations. Under such conditions, researchers may mistakenly construct an explanation “about the way things are” that merely reflects the local manifestation of a process that is actually highly sensitive to contextual differences. Or, investigators may claim originality for a structure or process that is, in fact, quite generic. In this section, I will give examples of using “space” at two scales: at the level of cross-national differences and at the level of differences across regions within nations.

I offer three cross-national examples from Canada, Japan, and Sweden to illustrate the gains from using “space” in organizational analysis across national contexts. First, Litrico (2007) analyzed the emergence and diffusion of a “new” organizational form in Quebec from 1984 to 2007: the “collective kitchen.” A collective kitchen is a group of people who meet regularly to prepare meals together. The participants create a number of meals that they take home to store for future consumption. Eventually, 1,400 of these new organizational forms were active in Quebec. As a collective action organization dependent on the continuing voluntary participation of its members, collective kitchens faced a classic problem of governance: avoiding shirking and eliciting effective efforts from participants. In short, such organizations need governance mechanisms that work.

From Litrico’s description, it is clear that the collective kitchens in Quebec “invented” all the mechanisms of governance that previous forms of collective action at the small group and small organization level had already discovered: leadership by charismatic figures and a strong ideological bond between participants, based on the norms and values of “self-help” and “empowerment.” The Quebec women responsible for collective kitchens made common cause with a similar movement in Peru, which also sprang up in the 1980s. (In 1988, approximately 1,500 collective kitchens existed in the Lima region of Peru.) For my purposes, what is important is that the basic organizing principle of a “collective kitchen” is the same as that of a “rotating credit association” (Light, 1972), which has been discovered periodically in various regions of the world. Taking a comparative view helps us see that the “new” organizational form was not unique to Quebec. Instead, when we recognize that its organizing principles are similar to those of other similar forms, we are led to ask “under what conditions does this collectivist form emerge and become successful?”
Second, a comparison between the United States and Japan illustrates the principle that “law constructs wealth patterns.” The concept that stakeholders “own” a corporation was created by corporate law in the United States in the 19th century, with the law privileging stockholders as the owners of a corporation. However, one could simply call them “investors” instead, recognizing that others also have valid claims on a corporation, such as employees, citizens living nearby, and so forth. In contrast, in Japan, corporations have been run for the benefit of employees, not stockholders. Indeed, stockholders have historically been forced to accept lower rates of return on their investment because corporate managers have pursued a lifetime employment policy that retained people who “should” have been purged.

A comparative organizations project needs to recognize that corporate law varies over time and across nations. More importantly, law varies over time within a nation and thus what is “legal” in one era might not be so in another era. From a cross-national comparative point of view, we need to take account of the different legal regimes that constitute organizations in different forms across nations. Some of the differences in organizational forms across nations are caused by differences in law, rather than technology, managerial competence, and so forth.

Third, research on startups in the United States has found that about half are started by teams, and about half of the teams are spousal pairs (Ruef, Aldrich, & Carter, 2003). In contrast, a study of high-technology services firms in Sweden found a much higher proportion of husbands and wives as co-owners (Hellerstedt,Wiklund, & Aldrich, 2007). As many as 60% of the established firms were owned by couples. On first glance, one might ask whether the explanation for the higher proportion of family-owned firms is that Swedes are more “family oriented.” However, the most likely explanation is that it is a response by Swedes to Swedish tax law regarding small firms. If the income of a firm goes over a certain threshold, the firm is taxed at a higher level. One way to keep a firm’s income down is to pay out the profits as salary to two people, rather than just one. With a husband and wife on the payroll, income can be brought below the threshold. Thus, the difference between the United States and Sweden is not differential familism, but rather different tax structures.

As a reader pointed out, identifying differences between nations is only the starting point for a comparative analysis of organizational structures and processes. Differences at the national level, in aggregate, may conceal substantial heterogeneity at the population and organizational level. For example, Schneiberg, King, and Smith (2008) examined social movement
processes and outcomes (the institutionalization of alternative, noncorporate forms) across three industries. Similarly, national differences could be disaggregated to the regional or community level, a point to which I now turn.

A second spatial dimension involves differences across communities or regions in the physical location of organizations (Freeman & Audia, 2006). One important issue in this regard for organizational ecology concerns the geographic scale at which population density should be measured. Evolutionary theory proposes that we focus on the level at which variation and selection forces are strongest. Some organizations compete at local levels, some at regional levels, and others at a national or international level. For instance, Ranger-Moore, Banaszak-Holl, and Hannan (1991) found that increases in density had a stronger inhibiting effect on foundings of banks, which were studied at a city level, than on foundings of insurance companies, which were studied at a national level. Similarly, in his study of bank branches and headquarters in Tokyo between 1894 and 1936, Greve (2002) found that founding rates depended on local neighborhood density.

Note that we can link cross-national differences with the issue of geographical scale. For example, Hannan, Carroll, Dundon, and Torres (1995) argued that the geographic scale at which competition operated was often more local than that at which legitimation processes transpired. The cultural templates defining an organizational form tend to flow more freely across spatial boundaries than the material resources invoked in competitive interaction. Using a data set of automobile manufacturers in five European countries between 1886 and 1981, Hannan et al. (1995) found some support for this multilevel density-dependent model, with competition typically occurring within country boundaries and legitimation occurring across the European context as a whole.

In choosing to focus on geographic scale, a question arises as to whether we should view spatial segregation in terms of discrete and discontinuous boundaries or continuous distances between different regions. A discrete boundaries approach seems most useful when political or cultural boundaries generate meaningful constraints or advantages for organizational populations. For instance, Saxenian (1994) argued that California’s Silicon Valley and Boston’s Route 128 have distinctive identities that cannot be reduced to the physical or human geography of these regions. In comparing these two regions, she argued that the high startup rate in Silicon Valley was due to its culture of cooperation, in contrast with the culture of competition in the Boston area that suppressed startup efforts. In a similar study, Molotch, Freudenberg, and Paulsen (2000) compared Santa Barbara and Ventura,
California. They found dramatically different populations of voluntary and business organizations, despite the fact that both cities have an almost identical climate, topography, and early history in cattle ranching and citrus agriculture.

In contrast with this discrete perspective on space, economic geographers often view spatial segregation in terms of continuous, physical distances (Sorenson & Audia, 2000). Following classic location theory, geographers argue that physical distances impose real limitations on the ability of organizations to secure inputs from their logistical networks and to reach users or supporters. For instance, the spatial markets of acute-care hospitals are often represented as catchment areas, based on the distances that patients can feasibly travel to get to a hospital (Ruef, 1997). In health care, discrete political or cultural jurisdictions mean little because patients will travel across such boundaries to receive health care. However, in primary and secondary education, such jurisdictions mean a great deal because students do not typically travel more than 5 miles to school.

Regardless of the scale at which space is considered, it is an important contextual feature affecting organizational structures and processes. My argument is not that every research project should be cross-national or cross-regional, but rather that investigators must make decisions, a priori, as to whether their project makes sense if conducted within only one spatial unit. For many projects, where investigators have deep knowledge of other contexts, it might be sufficient if authors simply point out what is being taken for granted in framing the problem at the level of a single unit.

CONCLUSION

Comparing organizations not only shows us how organizations are alike but also how they are different from one another. How much can we learn from such comparisons? As it happens, what we learn depends very much on which organizations we choose to compare and what context we put them in. If “context” has a powerful influence on an organization’s structure and all the organizations we pick for comparison are from the same spatial and historical context, we will see mostly the impact of the context. Or rather, we will not see it, because we will simply think that “all organizations are pretty much alike.” Conversely, if we sample from “contexts” that are quite different, then we increase the likelihood of finding organizations that differ, as well. Again, this is under the assumption that “context” plays a major role.
But of course, until we compare organizations from differing “contexts,” we will have no basis for drawing conclusions about contextual differences. Thus, paradoxically, if context really does not matter, we will have to sample from a wide range of context to figure that out! Although I have taken a strong position that most organizational processes vary across time and geographically, there may be some time and geographically invariant processes (e.g., density dependence appears to be one such process). However, unless we conduct comparative analyses, we cannot know that these processes are, in fact, temporally and geographically invariant. At the very least, we should request more replication studies.

To find out what is missing from the organizations in our samples, we will need to ensure that we sample from as wide a range of as possible, across time and space. How do we know how broadly to cast our sampling net, if we have never done it before? I would suggest letting evolutionary theory be our guide. It shows us that ecological pressures are a powerful differentiating force, and that whereas populations may comprise fairly homogeneous entities, the community level shows great diversity across populations.

Some may object that the skills required to find contrasting cases are daunting and perhaps beyond the reach of many scholars. I would argue that scholars who are willing to invest time in learning about the context, the history, and the full story behind their units of observation will always come out ahead. There are many skills that take time to learn, such as recognizing that after framing one’s problem, it is best to try “backing out” one level. This requires being explicit about units of analysis, the main line of generalization, and so forth. Of course, I think everyone would benefit from learning a little more evolutionary theory, and not only for the benefits it confers with regard to understanding time and space!

In a related objection, some readers of this paper have pointed out the daunting challenges facing scholars seeking the data needed for explicit consideration of time and space in their research. Junior scholars may object that the time invested to collect comparative and dynamic data does not yield an adequate return, given that it is much easier to ignore time and space. I have heard this objection for many years, and my response has been twofold. First, senior scholars need to lead the way in showing what can be done and in using the freedom afforded by their tenured status to take more chances in creating research designs. Second, junior scholars should not attempt such work on their own, but rather should follow the model increasingly used across the sciences of creating collaborative research teams. Spreading the work of collaborators with diverse skills can make such projects feasible.
Finally, in a world of proliferating journals, online discussion forums, and conferences, organization studies researchers can be forgiven if they sometimes feel overwhelmed by information overload. How can scholars make their work stand out in all of this clutter? If we apply the “is this interesting” test to what we read, much of this work comes up short. It simply does not challenge what we thought we already knew. In contrast, bringing time and space explicitly into our research designs and analytic modeling opens a world of possibilities. I have implied that much of what we think we know about organizations is, in fact, heavily constrained to particular places and within particular times. In the new world I envision, iconoclastic organization studies scholars would make it their job to offer counterfactuals to received wisdom, challenging researchers not to get lost in space, out of time.

ACKNOWLEDGMENTS

I thank Susan Cohen, Frank Dobbin, Teppo Felin, Joseph Galaskiewicz, Phillip Kim, Brayden King, Steve Lippmann, Russell Nichols, Steve Vaisey, Dave Whetten, and several anonymous reviewers for their help in sharpening the arguments in this chapter. Nekia Pridgen provided excellent research assistance in the preparation of this chapter.

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