



# PERSPECTIVE

## A Future for Organization Theory: Living in and Living with Changing Organizations

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We believe that the field of organization theory is adrift. In sailing jargon, we are “in irons”—stalled and making little headway toward understanding organizations and their place in our lives. We first attempt to diagnose our maladies and then, in this light, offer three broad research questions that just might reinvigorate our work: First, how can we understand today’s changing organizations? Second, how can we live in these organizations? And third, how can we best live with them? We close by calling attention to how our familiar approaches to building and testing theory might hamper any attempt to revitalize our field.

*Key words:* organization theory; twenty-first century organizations; theory-building agenda

We are adrift. The work of organization theorists has been called everything from solipsistic to dangerous, and prominent scholars are concerned. Augier et al. (2005) and Scott (2004) took stock of our field’s history and concluded that our research questions may be constrained by our perhaps too-comfortable academic affiliations with business schools and their economics-oriented, probusiness ethos. Hinings and Greenwood (2002, p. 413) spoke of our migration to business schools as a move away from our intellectual home within the university—among the social science faculty—to a professional school where senior managers’ perspectives dominate. Starbuck (2003a) worries that this affluent setting renders us “self-absorbed” (p. 441) and “self-indulgent” (p. 448) and as a result, inattentive to human welfare and world affairs. The charge is that our central theoretical questions have shifted along with our change in locale. Ironically, this shift to a purportedly probusiness setting has not left management theorists happy either. Before his untimely passing, Ghoshal (2005) appraised our scholarship and left us with some very pointed criticism. He argued that “bad management theories are, at present, destroying

good management practices” (p. 86). Strong words. There is a bad mood rising.

A debate now rages about just how “relevant” our work should be. While there are many subtleties in the various arguments, we observe several colleagues lining up to argue for closer ties to business practice, people like Bennis and O’Toole (2005), Ghoshal (2005), Starkey and Madan (2001), and Tranfield and Starkey (1998). Others like Grey (2001), Huff (2000), Kilduff and Keleman (2001), March (2003), Pfeffer and Fong (2004), and Weick (2001) argue for maintaining our academic distance. We do not want to enter this debate head on. Rather, we want to step back and consider how and why we come to find ourselves so self-critical and yes, most fundamentally, adrift. This self-criticism is a symptom of a larger problem. Our goals are to diagnose our maladies and then pose three broad research questions that just might reinvigorate our field.

### The “Irrelevance” of Organization Theory

We recognize that some of our colleagues may disagree with our view of the field. Some may insist that all is well, pointing to the assortment of theories developed

over a half-century and arguing that they have served us well and will continue to do so. How shall we respond to these colleagues? While it is impossible to conclusively prove such an assertion as theoretical “drift,” it is evident that our field is in a period of introspection. That said, we know that ideas that tire one scholar may exhilarate another. Beyond intuition, is there any evidence to suggest that our field is ripe for redirection? An analysis of the Organization and Management Theory (OMT) papers submitted to the 2005 meetings of the Academy of Management offers one sort of evidence.

Gerald Davis served as the 2005 Program Chair for the Academy’s OMT division. Four hundred twenty-nine papers and symposia were submitted for presentation at the meetings. Davis was stunned by what he called the “paucity of theory” among the submissions (Davis 2005). He asked each author to identify the theoretical tradition that served as a foundation for his or her work. Davis (2006) catalogued the responses and was amazed to discover how few of the papers were grounded in our major organization theories. The percentages of papers submitted in each of the established theoretical categories were as follows: institutional theory, 25.4%; network theory, 16.8%; population ecology theory, 6.7%; agency theory, 4.5%; resource dependence theory, 3.9%; transaction cost theory, 3.4%; contingency theory, 2.5%; and stakeholder theory, 2.5%. “None of the above” accounted for 56% of the papers submitted. More than half of our colleagues acknowledged that no established theory underpinned their work. Our foundational organization theories no longer capture the imagination of contemporary scholars. Indeed, we seem to be struggling to move beyond them in some way. If these fundamental theories no longer orient our work, what does?

Davis (2006) went on to analyze the authors’ self-selected keyword descriptors of their work. He counted 736 unique terms, including some familiar ones such as networks, learning, embeddedness, trust, knowledge management, and cognition. However, he found that 92% of the self-selected descriptors were invoked by only one or two authors. Thus, in addition to the papers’ disconnect from established theory, there was wide dispersion in the authors’ characterization of themes and problems that their work addressed. There is little apparent consensus in the field about the central themes of our research or our underlying theoretical paradigms. What brought us to this point?

### **Changing Times: Work, Organization, and Business Education**

Scott (2004) and Augier et al. (2005) began their historical appraisal of our research with a consideration of our post-World War II scholarship. Although Starbuck (2003b) traced our intellectual roots back to the pre-Christian age, he too noted that our contributions really started to accelerate in late 1950s, and that by 1960

“organization theory had arrived” (p. 174). It is important to understand why our research efforts flowered just then. Two interdependent stimuli prompted this explosive growth. Consistent with the critique we reviewed at the beginning of the paper, the easiest to observe is the fundamental change and subsequent growth in business schools. The opaque but more determinative factor was the change in the nature of work, unleashed by what Drucker (1993, pp. 19–47) called the third industrial revolution. He showed how we moved from the Industrial Revolution (1750 to 1880) to the Productivity Revolution (1880 to 1945) and then to the Management Revolution (1945 to the end of the twentieth century).

American business schools were established a little more than 100 years ago. The University of Pennsylvania’s Wharton School awarded diplomas to its first five undergraduates in 1884, while Dartmouth College’s Tuck School of Business graduated its first four MBA students in 1901. These institutions, and the many that quickly followed, emerged in response to the profound economic changes unleashed by the Industrial and Productivity Revolutions. Their curricula mixed traditional academics with an attempt to bring the world of work into the classroom. The Tuck School of Business describes their early curriculum in this way:

The first-year courses were taught by Dartmouth instructors from fields such as law and political science, history, sociology, rhetoric and oratory, economics, and public speaking; the second-year courses drew heavily on outside business people, such as an export merchant, an attorney, an insurance company president, and an accountant. Edward Tuck was pleased as he wrote to President Tucker in February 1902, “I am glad that it will be the aim of the school to bring students in touch with practical businessmen.” (Munter 1990)

Ironically, a half-century later this vaunted connection to practical businessmen threatened to destroy business schools. Bennis and O’Toole (2005, p. 98) put it this way: “For the first half of the twentieth century, business schools were more akin to trade schools; most professors were good ole boys dispensing war stories, cracker barrel wisdom, and the occasional practical pointer.” Mid-century, the Ford and Carnegie Foundations challenged business schools to do better. Gordon and Howell (1959) captured the dissatisfaction and offered a new, scholarly, direction. As a consequence, professors began to distance themselves from day-to-day business life. Instead, they embraced the scientific method, brought formal analytical rigor to the study of business, and enhanced the status of business schools within the academic community. Our field, as we know it today, was born.

It is easy and maybe even correct to single out the limitations of “cracker barrel wisdom” as catalyzing an age

of serious scholarship about business. This diagnosis, however, is too self-referential and limited. It is self-referential because the diagnosis itself implies a comforting cure—invoking our own contemporary insight and wisdom. It is limited because it really does not come to grips with why the lessons these executives learned proved to be so vacuous. We argue that their insights were empty because their world of experience no longer resembled the world that faced their students. The world was changing dramatically and yet, as we entered the second half of the twentieth century, organizations were largely taken as given.

Scott (2004) reviewed the work on scientific management and the study of the informal social organization of the workplace in those early years. He concluded that “organizations were viewed, at best, as settings within which work was carried out, not as themselves distinct social systems, let alone collective actors” (p. 2). He then noted that “During the formative period in the 1950s and continuing into the 1980s, sociologists pursued a variety of topics, but their most distinctive and consistent focus was on the determinants of organization structure” (p. 4). This focus on the seemingly immutable firm reflected the firm’s competitive reality. The perceived organizational landscape back then was dominated by large firms, firms designed to exploit the scale-based production technologies unleashed by the Productivity Revolution. As the firms themselves were not particularly at risk, scholarly attention was focused internally, on the nature of control. Blau and Meyer (1956/1971) put it best when they described why scholars should study bureaucracy: “Rationalization in administration is a prerequisite for the full exploitation of technological knowledge in mass production, and thus for a high standard of living” (p. 7). It is hard to imagine that just 50 years ago, one could, in essence, assess competitive advantage by studying bureaucratic administration.

The empirical foundation for the received wisdom about organizational structure is built on firms faced with relatively simple competitive challenges and certainly not the kind of global factor and product market competition that we see today. Peter Blau, one of the most prolific and insightful scholars of the time, published a compendium and retrospective analysis of the work he had conducted over a decade, all with support of the National Science Foundation (Blau 1974). The research samples that he chose are revealing: personnel agencies of state and local governments, government finance departments, academic institutions, public employment security offices, and one capstone study that combined the insights from the prior studies and added new data from department stores and teaching hospitals (pp. 217–221, 324–325). Similarly, Jerold Hage and Michael Aiken (e.g., Hage and Aiken 1967, 1969) based their insights on data collected from 16 social welfare and health agencies in a large Midwestern city in 1964.

Apparently, few differences between public and private sector organizations were recognized at that time. To be fair, the Aston Group did try to develop a set of insights characterizing “all” organizations, drawing most of their conclusions from a study of 52 public and private organizations in the Birmingham, England area (cf. Pugh et al. 1968, p. 67; 1969, p. 116). The fact that the Aston Group felt so comfortable pooling firms from these different sectors to search for what Starbuck (1981, pp. 191–194) called “the Holy Grail” reveals just how tranquil the competitive landscape was then.

### “Frenzied Theorizing” in Historical Context

The late 1960s and 1970s ushered in what Scott (2004, p. 5) called a “frenzy of theorizing.” Most of our contemporary theories of organization were created in this period. Given that today’s senior faculty members were in graduate school during that time, and given that we generally train our students in our image, few scholars active today know a world without contingency theory, population ecology theory, resource dependence theory, transaction cost theory, and all the rest. Most of us do not stop to think about why these ideas appeared at that time, and more to the point, to consider why they no longer command our attention as they once did.

We are adrift because the theories that we have come to know so well either do not point us to questions that appear to be worth answering, or they do not serve us well when we bring them to bear on the questions that do grab our attention. With no new perspectives to reorient us, we muddle along. This is not to say that our theories have been inconsequential. On the contrary, once we situate our theories in their historical context, we can see immediately how they emerged to take stock of the key business changes triggered by the Management Revolution. We can also see how our research enterprise began to lose its immediacy once the changes wrought by the post–World War II revolution were consolidated, and when our 40-year research effort to understand them bore its fruit. We will first place the prolific theorizing of the last quarter-century in historical context and then move to consider what this awareness might mean for our research future.

Looking back, the Management Revolution shaped the post–World War II period, but, naturally, it took some time for these changes to register in our scholarly world. The Gordon and Howell (1959) report, commissioned by the Ford Foundation, described the meager scientific foundation of U.S. business education, catalyzing a reorientation of our research focus. The stirrings of change became visible a few years later. When we consider work on individuals and groups (Katz and Kahn 1966), organizations (Thompson 1967), and managing individuals and organizations (Burns and Stalker 1961, Lawrence and Lorsch 1967), it is clear that scholars in the 1960s came to recognize that the firm’s external

environment was changing and deserved attention. The “open-systems” model of organization theory was born. Thompson (1967, p. 13) stated it well: “The open-system strategy shifts attention from goal-achievement to survival, and incorporates uncertainty by recognizing organizational interdependence with the environment... the central problem for complex organizations is one of coping with uncertainty.” Thompson was prescient. His words about survival prefigured the emergence of population ecology and its abiding focus on mortality and survival (Aldrich 1979, Hannan and Freeman 1977).

The theoretical floodgates opened once we looked outside the firm and recognized our changing world. This is not the place to offer a primer on organization theory; we will not go into any detail about the fundamental ideas or their subsequent nuanced amendments. Rather, we simply want to review their major foci to show how theorists aspired to come to terms with the dynamic post-World War II changes in the nature of work and organizations.

We have already noted that population ecology addressed the fundamental problem of firm survival in a time of increasing competition. Aware of these destabilizing and competitive forces, Pfeffer and Salancik (1978) situated their work on resource dependence at the firm level of analysis. Their opening words captured this awareness: “organizations are inescapably bound up with the conditions of their environment... [but] much of the literature on organizations still does not recognize the importance of context” (p. 1). Six years later, Freeman (1984) published his book on stakeholder theory. It can best be read as a kind of managerial companion to Pfeffer and Salancik’s (1978) resource dependence theory. Note how Freeman motivated his argument: “The business environment of the 1980s and beyond is complex, to say the least. If the corporation is to successfully meet the challenges posed by this environment, it must begin to adopt integrative strategic management processes which focus the attention of management externally as a matter of routine” (p. 249). Population ecology, resource dependence, and stakeholder management theories are emblematic of this hard turn in organization studies. A turn that focused our attention on the firm and its (rapidly changing) business environment.

We now recognize that this changing environment spurred intense competition all along the value chain. The development of global capital markets ushered in a period of investor capitalism (Useem 1996) and an abiding focus on shareholders’ interests. The formulation of agency theory (Jensen and Meckling 1976, Fama 1980) and its growing allure in our field (Eisenhardt 1989, Walsh and Seward 1990) mirrored the development and power of these capital markets.

New information and computer technologies combined with advances in transportation technology to

change the fundamental nature of the firm itself. Theories about networks emerged to help us portray the ties connecting these new firms, often disaggregated across the value chain, and the patterned relationships linking those who work in them (Baker 1994, Burt 1983, Powell 1990). Some struggled to understand why firms exist at all, buffeted as they are by market forces. Transaction cost theory helped shed light on that fundamental question (Williamson 1975, 2002); it also helped us to understand the explosive growth in strategic alliances (Hennart 1988).

The problem of uncertainty loomed large in our work. Firms groped for standards about appropriate behavior in this volatile world. Institutional theory emerged to help us understand how the social construction of standards guides and regulates organizational behavior (Meyer and Rowan 1977, DiMaggio and Powell 1983). Also, looking inside the firm, all manner of scholars began to focus on how work is accomplished under these conditions. March and Simon (1958) helped us understand decision making under uncertainty. Weick (1969) asked us to move away from our focus on the noun “organization” to consider the verb “to organize,” all the while recognizing that the work of organizations was constrained by exacting evolutionary pressures. Finally, Argyris and Schön (1978) helped us to understand organizational learning processes, learning that was essential to survival in this changing world.

It would be folly to try to encapsulate 50 years of organizational scholarship in just seven paragraphs. Again, our intent is not to formally review this work. We simply want to point out that the theoretical work that now seems to inexorably define our scholarly landscape is embedded in a broader story of the post-World War II economic development in the United States. This broader story helps explain its birth as well as its decline. We are adrift today because the theories that emerged to comprehend the Management Revolution are no longer as relevant to our contemporary economic situation.

### Late Twentieth Century Changes

Our theories have not assimilated the fundamental changes in organizations, organizational contexts, and the accompanying management practices that occurred during the late 20th century. Many of these contextual upheavals were chronicled by Bell (1976), Casey (1995), Davenport (2005), Drucker (1993), Greider (1997), Hirschhorn (1984), Piore and Sabel (1984), Reich (1991), and Zuboff (1988). These authors described how new information, communication, and automation technologies profoundly changed the nature of organizations and the nature of work itself. Five key implications of these changes were articulated by Bradley et al. (1999 p. 15):

(1) While land, labor, and capital were once the key factors of production in a system where profits resided

in manufacturing volume; *today*, knowledge is the key factor of production in value creation.

(2) While workers used to serve machines and capital (primarily with their muscles); *today* machines and capital serve workers (primarily with their intellect).

(3) While specialization, standardization, and mechanization provided the logic for industrial work; *today*, problem identifying, problem solving, and strategic brokering provide the logic for knowledge-based work.

(4) While workers were historically selected for their capacity for exertion, dexterity, and endurance; *today*, skills of perception, attentiveness, and decision making are valued.

(5) While formal education was largely seen as irrelevant to wealth accumulation, *today*, human capital investments in formal education are key to accumulating wealth.

In short, fundamental changes in the latter portion of the twentieth century upended practices inherited from the Industrial and Productivity Revolutions and created value chains that are heavily dependent on the creativity of individuals and their knowledge-based human capital. Our field has been slow to respond. Organizational theory has always embraced stability and incremental change, while deemphasizing processes of discontinuous change (Haveman et al. 2001, Meyer et al. 2005). Certainly we can still find plenty of examples of routine, unskilled work worldwide. That said, for many firms competitive advantage no longer resides in their ability to harness their employees' physical skills and endurance or in the efficient shop-floor control of scale-based production technology. Rather, it resides in knowledge obtained through their human capital investments (Leanna and Rousseau 2000, Tsui et al. 1997).

## A Future for Organization Theory

It is one thing to recognize that our theories and research questions are inextricably rooted in bygone features of economic production, and to argue that the returns from our scholarly pursuits have dwindled accordingly. It is quite another thing to gaze into the future and try to imagine what issues will orient our work in the years ahead. All we can do is reaffirm our broadest aspiration and then try our best to see the future. Pettigrew (2001, p. S68) reminded us of this fundamental aspiration: "The duty of an intellectual in society is to make a difference." Allen (1992, p. 261) sets the standard for us: "I admire scholars; I respect the effort and the talent that allows them to climb the high hills from which they see further than those of us who, in the struggle with more specific and immediate problems, sometimes are able to make out only the ground beneath our feet." But what hills shall we climb, in what direction should we look, and how can we make a difference?

First, we need to discern the currents that seem to propel the issues unfolding before us, and then we must

address them. For example, we know that the knowledge revolution and the accompanying globalization of factor and product markets fundamentally changed the nature of organizations. We know that as the environment's full impact on organizations became apparent, our focus on bureaucracy gave way to the study of population ecology, resource dependence, network theory, and the like. These theories gained currency as we tried to understand how organizations were responding to their changing external environments. However, what new currents are beginning to flow? How will they affect social and organizational life, and what new questions must organizational scholars address?

We expect that organization theory will evolve to grapple with three fundamental questions. (1) How can we understand today's organizations? One powerful current is *transnational emergence*. Novel social structures and systems are emerging with unparalleled speed and scope. The challenge for organization scholars is to develop new theories and methods for tracking and understanding the emergence of new organizational forms. We must view organizations from a wider angle—seeing them as embedded historically, institutionally, culturally, and politically. Our quest to understand new organizational forms will ask us to grapple with the dynamics of self-organization as well as the coevolution of interdependent organizational populations that make up organizational communities.

(2) How can we live in today's organizations? A second new current is *disaggregation*—the fragmentation of organizations, careers, and jobs. A central challenge for organizational scholars is to track and understand the impacts of this disaggregation on organization members and employees (who must now pack their own parachutes and manage their own careers), as well as managers (who must fashion new means to coordinate and control disaggregated, knowledge-based work systems that cut across borders and organizations).

(3) How can we live with today's organizations? A third current flows from the *growing influence and reach* of today's organizations. Organizations' impacts on humans' social and material lives and on our planet's ecosystem have mushroomed. The challenges for scholars are: First, to understand these new impacts and second, to choose certain outcomes worth pursuing and others to avoid. In so doing, we will need to reconceptualize organizations as social constructions, and reconceptualize organizing as a social technology to achieve desired outcomes, rather than as an immutable fact of economic and social life. We will elaborate on each of these three key questions.

## Understanding Twenty-First Century Organizations

We observed that the theoretical floodgates opened 40 years ago once we looked outside the firm and recognized

our changing world. Today, the full scope of contemporary organizational forms bears scrutiny (DiMaggio 2001). The floodgates may reopen once we discover just how varied these forms are. Competitive advantage no longer resides within the geographical confines of any country, particularly the United States. Today's organizational landscape is global. For many firms, competitive advantage lies in their ability to coordinate disaggregated and globally dispersed value chains (Powell 2001). The technological changes that allowed firms to disaggregate their value chains also allowed them to disperse their operations worldwide. A 2005 review of the top 10 companies in the *Fortune 500* revealed that, on average, these 10 companies operate in 93 countries (data available from the authors). Taking a broader perspective, data from the United Nations reveal that at the end of 2003, 61,000 transnational corporations (TNCs) operated 900,000 foreign affiliates worldwide (UNCTAD 2004). Given that there were "only" 39,000 transnational firms in 1993 (UNCTAD 1996), we have seen a 56% increase in their number in just 10 years. Furthermore, of the 100 largest "economies" in the world, only 47 are national states; the other 53 are multinational corporations (Gabel and Bruner 2003). These multinationals are not all U.S.-based. Gabel and Bruner (2003) point out that the United States houses only 38% of *Fortune's* Global 500 firms. Much of today's business activity is simultaneously legally consolidated and geographically dispersed (Bartlett and Ghoshal 2002).

The rise of transnationals illustrates the globalization story that is now well told (Parker 1996, Rodrik 1998). Drawing upon the American experience as an example, we will briefly document some key changes in these large firms' labor, capital, and product markets. First, consider the labor markets of U.S. firms. Slaughter (2004) presents 10 years of data documenting the evolution of domestic and international employment by U.S.-based transnational corporations. We learn that in 1991, these firms employed 24,837,100 people, and nearly 28% of them (6,878,600) were located outside of the United States. By 2001, U.S.-based transnationals' total employment had increased 34%, and their employees living outside of the United States had swelled to 9,775,600 (a 42% increase).

Although we all are aware that capital markets are now global, we may not realize just how global they have become. The Federal Reserve Bank of New York (2004, p. 15) provides a snapshot of this phenomenon. In December of 1978, the foreign holdings of U.S. long-term debt securities amounted to only \$51 billion. Twenty-six years later, that same figure had risen to \$3,514 billion. This 679% increase in foreign holdings vividly underscores the extent of today's global financial integration.

We can augment the transnational data to provide an even clearer picture of larger firms' global product markets. The growing volume of cross-border mergers, acquisitions, and strategic alliances attests to the expansion of global product market strategies. Sundaram and Black (1992) estimated that over \$1 trillion in cross-border mergers and acquisitions (M&As) activity took place during the 1980s. Taking stock of the 1990s, we learn that "the value of completed cross-border M&As rose from less than \$100 billion in 1987 to \$720 billion in 1999" (UNCTAD 2000, p. xix). The 1999 activity alone nearly reached the magnitude of the activity observed throughout the previous decade. Cross-border alliances show this same pattern of expansion. Between 1980 and 1989 we saw nearly 700 cross-border strategic alliances in the information technology sector, 280 in biotechnology, 140 in new materials, and over 80 alliances in the automotive sector (OTA 1993). Looking more broadly and more recently, the OECD reports substantial global alliance activity. We learn that a cumulative total of 46,269 cross-border strategic alliances formed between 1999 and 2000, with the annual number increasing from 2,532 in 1990 to 4,351 in 2000, a 72% increase over the decade (OECD 2001). Together, these statistics suggest that the process of globalization is not merely continuing, but accelerating.

The frenzy of work in organization theory that emerged to understand the Management Revolution often turned a blind eye to organizations and conditions beyond the institutional contexts of North America. This blindness is incapacitating in a globalizing world. Fifteen years ago, Boyacigiller and Adler (1991) described organization science as a "parochial dinosaur" (p. 262), arguing that the dominance of North American cultural values undermined claims of universal applicability. Some 10 years later, Hinings and Greenwood (2002, p. 417) again observed our heavy reliance on data from the United States and questioned if our "generalized" theories are not, in fact, highly contextualized in time and place. March (2004) recently concurred. He observed that organization studies is Balkanized into pockets of autonomous scholarship defined by cultural, national, linguistic, and geographic boundaries. Make no mistake; we certainly know a good deal about multinational and transnational corporations. Bartlett and Ghoshal's (2002) search of seven prominent organization and management journals turned up more than 350 articles on these firms. We know about such issues as mode of entry, entry timing, and the performance of these firms. This work is heavily influenced by industrial-organization economics, however. Our understanding of the social and political qualities of these corporations is limited.

As we struggle to comprehend these large global firms, we can forget that these corporations are but one type of firm populating the contemporary organizational

landscape. Our field's preoccupation with large corporations fosters "an aura of unreality among scholars, conveying an image of organizations as monolithic behemoths with massive power" (Aldrich and Ruef 2006, p. 7). Not all firms are so powerful. In fact, the vast majority of organizations are small and short lived, coming and going on a much shorter time scale than large firms. Their diminutive size and short lives, however, do not mark them as unimportant. For example, if we again narrow our focus to the United States, we find approximately 24.7 million business firms operating in 2004; 99.7% of them were small, employing from 10 to 500 people. Small as they are, these firms accounted for 45% of the total U.S. private payroll, and just over half of the 112.4 million workers who make up the non-farm private sector (U.S. Small Business Administration 2006). Small firms fuel economic growth, creating 60% to 80% of the net new jobs annually over the last decade. Interestingly, these small firms are more innovative than their larger counterparts, producing 13 to 14 times as many patents per employee. Moreover, the patents filed by these small businesses are twice as likely as those filed by large firms to be among the 1% of patents that subsequently garner the most citations (U.S. Small Business Administration 2006). Finally, U.S. firms go global before growing up. Small businesses constituted 97% of all U.S. exporters in fiscal year 2002, and they were responsible for 26% of the total value of goods exported in that year (U.S. Small Business Administration 2006). Statistics from the European Union and Japan reveal similar patterns (Aldrich and Ruef 2006, pp. 9–11), as do data from the People's Republic of China (Schoonhoven 2006).

Our empirical preference for data from large publicly traded organizations may distort the organization theory we build. The resulting studies sample on the dependent variables of financial success and survival. This is not to say that such corporations are unimportant. They are vitally important. Gabel and Bruner (2003) remind us that the planet's 1,000 largest corporations account for 80% of the world's industrial production. Nevertheless, we have seen that the smaller firms employ millions of people and serve as a steady source of innovation. We need to know much more about young and emerging organizations.

Today, we stand witness to the terraforming of the organizational landscape as wave upon wave of new sectors and industries materialize: biotechnology, mobile and wireless communication, alternative energy, security, nanotechnology, and more. To understand the organizations that emerge to populate these new spaces, we must grasp "the connection between the ongoing creative ferment in human societies and the particular realization of it in organizations" (Aldrich 1999, p. 1). A host of scholars have remarked on the dearth of organizational theory and research that addresses the emergence

of organizational collectives—new forms, populations, and communities (Aldrich and Ruef 2006, Chiles et al. 2004, Lubatkin et al. 1998, Schoonhoven and Romanelli 2001). Important as it is, tracking and understanding the emergence of new industries and organizations is just part of our challenge—at the same time, we need to comprehend waves of emerging social and political institutions. Indeed, commercial activity often coevolves with our developing social and political life (Peredo and Chrisman 2006, Selsky and Parker 2005).

The emergence of organizational forms, populations, and communities is "among the most fundamental, difficult, and under-addressed issues in the field" (Chiles et al. 2004, p. 501). Nevertheless, the foundations for this work are well established. We know that economic organizations are embedded both in the structures of social relations (Granovetter 1985, Uzzi 1997, Zhou et al. 2003) and in their variable institutional contexts (Baum and Oliver 1992). Some promising studies of emergence build upon these foundations. Novel organizational forms may emerge within a community of populations (Meyer et al. 1990, Ruef 2000) or, even more dramatically, within changing (Keister 1998, 2001) and contested (Spicer 2002) institutional contexts. Most recently, Li et al. (2005) found that institutional and locational embeddedness increased the growth of a community of industrial populations in China.

It is not easy to climb the high hills and discern our changing organizational landscape. Consider DiMaggio's (2001) quest. Not so long ago, he engendered a terrific debate about just how prevalent, and indeed how paradigm-breaking, network organizations really are. We simply do not know if we are witnessing a broad shift to networked forms of organizing or just converging on a set of categories and scripts to make sense of some variations on an established Weberian theme. We may have become too entranced with these seemingly novel forms of organizing and, as such, oversampled on an alluring dependent variable. The consequence is that we could overestimate the magnitude of the change. DiMaggio (2001) reminds us that we would be wise to collect unassailable empirical evidence that will reveal whether or not we are witnessing such a fundamental change. Difficult as it may be to collect these data, our work will not end when we better understand this changing organizational landscape. We also need to keep a very clear eye on the effects these contemporary organizations generate. Questions of how to live in and live with these organizations loom large.

### **Living in Twenty-First Century Organizations**

A host of recent factors conspire to leave employees and managers anxious. It is exhausting to live with the upheavals that Bradley et al. (1999) chronicled (changes in the nature of work, capital markets, product-market competition, organizational forms, and changes in the

regulatory environment). Indeed, Weick (2001, p. S73) observed that the “demands in the twenty-first century may be starting to approach the limits of human beings.” It is not easy to meet the demands of knowledge work. Continuous learning is exciting, but navigating the exploration-exploitation frontier exacts a physical and mental toll on individuals. The collision of exacting product-market competition, globally competitive labor markets, and the need for continuous learning can leave a firm’s members vulnerable and exposed (Baumol et al. 2003). Consider the automobile industry. In the wake of billion-dollar losses in domestic markets, Ford Motor Company announced the closing of 14 manufacturing plants in 2006, coupled with a 25% reduction in their North American workforce (Maynard 2006). Stuningly, some observers predict that GM’s bankruptcy is inevitable (Loomis 2006). We barely understand what these kinds of changes mean for individuals as they make their way in this new world (Feldman 2000, Hirsch 1987).

The employment dislocations and organizational upheavals that attend corporate outsourcing decisions are the focus of growing journalistic attention (Engardio and Einhorn 2005, Engardio 2006), outrage (Dobbs 2004, Dorgan 2006, Uchitelle 2006), and scholarly investigation (Baumol et al. 2003). We are only beginning to learn how knowledge workers will meet the challenge of working in a global labor market. Barley and Kunda’s (2004) ethnography on itinerant information technology workers is noteworthy for bringing the issue to the fore. Their study describes how individual professionals adapt to the unraveling of permanent employment in the wake of the collapse of sheltered bureaucracies, the blurring of firm boundaries, and the growing importance of occupational forms of organizing. Ashford et al. (2007) provide an comprehensive overview of recent developments in nonstandard work practices.

We have not yet sorted out the implications of managing the mix of knowledge work and global factor markets. Organization and control is very problematic in twenty-first century business organizations. It is axiomatic that markets are controlled by a pricing mechanism. However, pricing does not function smoothly as a control mechanism for postindustrial work. Ouchi (1979) pointed out years ago that remuneration-based outcome and behavioral controls will not easily accommodate knowledge work, work defined by elusive means-ends connections and problematic outcome assessments. He pointed out that clan-control systems, systems held together by accepted normative standards, could work well in such circumstances. Barker (1993) would certainly agree. On good days, clan control bonds workers to a purpose greater than their own self-interest (e.g., a firm’s noble mission). Such employees can be managed with a very light

administrative touch, but on bad days, clan-control systems are pilloried as hegemonic tools of domination (Willmott 1993). Employees are left alienated and conclude that compensation is neither reliably connected to their work nor equitable, given the downward wage pressure of global labor markets. Firms have yet to discover how best to manage these tensions.

Now that organization theorists are coming to understand what modern organizations look like and how they compete in this new world, we believe scholars will increasingly turn their attention to consider how best to live in them—and yes, how best to manage them. Management theory or perhaps a more neutral term, organizing theory, will likely attract a growing share of scholarly attention in the coming years. Learning how to *live in* contemporary organizations is only one pressing challenge, however we need to learn how to *live with* them too.

### Living with Twenty-First Century Organizations

It is not at all clear how we are to live with these fluid and increasingly powerful organizations. The reach of organizations has never been greater. We have argued that as the Management Revolution unfolded, organizations were knocked from their relatively quiescent perch and forced to compete in global markets of all kinds. Vulnerable to competition, some would even say hypercompetition (Ilinitich et al. 1996), survival was by no means guaranteed. As we have seen, organization theorists raced to understand these changes and what they meant for the firm. Along the way, concerns for human welfare and its relationship to business were subordinated to an abiding focus on the economic concerns of organizational efficiency, effectiveness, and performance (Hinings and Greenwood 2002). Walsh et al. (2003) analyzed every empirical article published by the Academy of Management between 1958 and 2000. They showed us that the field’s focus on human welfare peaked around 1980, a time when interest in firm performance escalated and indeed, a time when economics commanded increased attention within the field. This kind of shift was not at all unreasonable. After all, 1980 marked the beginning the century’s fourth merger and acquisition wave (Golbe and White 1988), which triggered waves of “downsizing,” “rightsizing,” and “reengineering.” Firms, and those who worked in them, were vulnerable to globalization-induced shakeouts. Jensen (1993) catalogued the firm-level changes wrought by advances in technology and the worldwide integration of markets. However, whither our focus on human welfare? Organization theorists continue to investigate these concerns, but largely within the confines of an economic logic. This constraining focus is about to be relaxed.

Walsh et al. (2003) discovered that 1980 was an inflection point in our field’s history (p. 873). Before 1980,



almost no published articles referenced work in economics. Since then, nearly one in five papers published in *Academy of Management Journal* relies on economic reasoning. Scott (2004, p. 16) raised the concern that organizational research is growing more applied (more problem driven than theory driven) because business school scholars labor under “the long shadow of economics.” Although economics may be helpful in understanding the organizational changes afoot, it is less helpful in understanding how society has been affected by these changes. Scholars are just beginning to come to terms with how organizations affect the society that created them. It is time to walk out from under that long shadow.

Perrow (1991) observed that we have become “a society of organizations.” He argued that their reach and power is so great that one might reasonably conclude that “politics, social class, economics, technology, religion, the family, and even social psychology take on the character of dependent variables” (p. 725). That may be so, but business people understand this observation as a paradox. In the aggregate, firms seem to possess the sort of omnipotence that Perrow sees, but take a look inside any single firm and you will find performance anxiety and vulnerability.

Notwithstanding the recent concern about “relevance,” Walsh et al. (2003) concluded that organization scholars have, in fact, been quite attentive to managers and their concerns. Scholars’ concern for performance at various levels of analysis increased steadily over time. Tellingly, Staw (1984) observed that many researchers feel obligated to tether their interests to concerns about firm performance, if only to find an audience for their ideas. Even concerns about human welfare need to be encapsulated in the logic of wealth creation. Margolis and Walsh (2001) told us that 1972 marked the beginning of an ongoing quest to discover an empirical link between a corporation’s social investments (CSP) and its financial performance (CFP). Why is this “corporate social performance–corporate financial performance” question so important? It is important because it neatly illustrates how the shadow of economics influences our approach to something as basic as human welfare. Unless it is linked to wealth creation, CSP has little standing within organization theory.

Advances in information and communication technology brought the firm to the world (i.e., the globalization story), but they also brought the world to the firm. Firms’ power and reach mark them as ready targets for appeal. Critics have long demanded that firms address the ills they cause (e.g., Nader 1972). The new story, however, is that some in society now ask firms to solve problems not of their own making. A case in point: In his speech to the U.S. Chamber of Commerce, UN Secretary-General Kofi Annan (2001) implored the American business community to fight the scourge of HIV/AIDS in the developing world. While there is certainly cause to address

the AIDS epidemic if your own workforce is in jeopardy (Rosen et al. 2003), it may be more of a stretch to ask businesses not so directly affected to become involved (Friedman 1970). A positive CSP-CFP link, however, would legitimize an investment in combating HIV/AIDS (and all manner of other social interventions) as a business interest, and not simply a humanitarian impulse.

Griffin and Mahon’s (1997) review of the CSP-CFP literature is noteworthy not so much for the reach of their analysis, but for the candid expression of their values. They concluded their paper by noting that the empirical results provide “hope for those of us who believe in some positive relationship between corporate financial and social performance” (p. 25). Other scholars share this hope, but few are as plainspoken. Margolis and Walsh (2003) counted nearly 130 investigations of the CSP-CFP relationship, noting that more recent work tries to cement the relationship by explicating the mediating mechanisms that link social investments to enhanced firm performance. The search for CSP-CFP mediating mechanisms is underway within almost every business discipline. Gourville and Rangan (2004), for example, focused on how “cause marketing” efforts can leverage CSP to build brand awareness in the minds of consumers; Greening and Turban (2000) considered how social investments might enhance a firm’s ability to recruit employees; and in the field of accounting, Schneitz and Epstein (2005) considered how the reputation acquired by making such investments serves as financial insurance in times of social crisis. More broadly, colleagues argue that social investments will serve as a stimulus for innovation (Kanter 1999), build a firm’s competitive advantage (Porter and Kramer 2002), and in the best of all worlds, reduce global poverty while enriching the firm’s shareholders (Hart 2005, Prahalad 2005). Once again, scholars’ interest in firms’ social performance is refracted through a wealth-creation lens. A humanitarian logic on its own will not garner much attention.

While some hope for a robust CSP-CFP link and look forward to the day when we can unleash the corporation’s capabilities to directly serve humankind, others are more cautious. Russo and Fouts (1997, p. 551), for example, pointed out that expecting markets to promote social welfare ignores a fundamental bias: “since the relative influence of consumers is not democratic but is based on what they spend, marketplace outcomes will not always reflect social equity considerations.” Even the humanitarian logic itself needs to be appraised. Some are concerned about how social investments might affect employees, consumers, and indeed, society as a whole (Korten 2001). We mentioned the dark side of clan control earlier (Willmott 1993). Jacoby’s (1997) masterful study of the evolution of welfare capitalism asks us to rethink many of today’s efforts to build

high-commitment work systems. Frank (1997) looks at how companies seek to understand and then manipulate their customers' identity needs in the pursuit of a sale. Klein (2000) worries about how this manufactured consumerism affects us all. And to this day, Levitt's (1958) early critique of the corporate social responsibility movement may be the most trenchant of all. His language is as colorful as it is indicting, "...all these well-intentioned but insidious contrivances are greasing the rails for our collective descent into a social order that would be as repugnant to the corporations themselves as to their critics" (p. 44). The presence of manifest human need and manifest corporate power does not necessarily argue for conjoining the two in corporate-sponsored human welfare programs. We clearly need to better understand the corporation's possible contributions to human misery, its possible roles in alleviating it, and both the intended and unintended consequences of corporate social investments.

Perrow (1991) argued that organization theorists need to see the organization as an independent variable and society as a dependent variable in their work. We agree. Indeed, there are signs of movement toward that kind of aspiration. In the last 10 years, Hinings and Greenwood (2002), Margolis and Walsh (2003), Perrow (2000), and Stern and Barley (1996) have all called for similar investigations. Starbuck (2003a, p. 449) went further and bluntly asserted that "organization theory can and should contribute more to human welfare." The path to this new world of contribution is not yet clear. Some obvious paths may not be as helpful as we think. For example, there may be less to our stakeholder theories than we might imagine (Walsh 2005) and more to the new "win-win," bottom-of-the-pyramid investments than meets the eye (Walsh et al. 2005). And yet, we are beginning to see all manner of experiments that harness the corporation's capabilities to ameliorate social maladies (Mair and Marti 2006). Understanding how to hold organizations accountable to society as they slip the bounds of the nation-state is tough enough (Vernon 1998); holding them accountable as they penetrate and shape our social lives may be the most prodigious challenge of all.

### Relevance, Values, and Methods

The new theory-building agenda sketched here will stretch our imaginations and test our tools of inference. Organizational scholars must be ready to confront two significant challenges when they begin to answer the kinds of questions we raise here. First, reconnecting organization theory to the critical problems facing the world will challenge us to surface, question, and change some of our core values as scholars, educators, and citizens. Second, studying how organizations compound and alleviate global problems may challenge us to update some of our favored research methods and even abandon others.

Our intrinsic conception of the organization is re-framed when our field's central construct is treated as an independent variable. Many of us have regarded organizations as natural entities—predestined products of economic exchange or social context. Indeed, many who teach in business schools regard the corporation as an almost sacred institution on the order of the judiciary, university, or church. However, research models that place organizational variables on the right-hand side of the equation recognize that an organization is a social technology constructed by human agents, a technology that can be harnessed to achieve social objectives. In recent years, organization theorists have outsourced questions like these to legal scholars, political scientists, and economic historians. We must reclaim these questions if our field is to address the future's critical organizational and societal problems. While multiple global challenges have existed for years, formal opportunities to reclaim these questions are just now appearing. For example, management scholars were recently challenged to consider "Business as an Agent of World Benefit" at a 2006 forum of the same name organized by Case Western Reserve University, the Academy of Management, and the United Nations Global Compact (see <http://bawbglobalforum.org>).

It was unnerving for some of us to see Griffin and Mahon (1997) unabashedly root for the outcomes they valued in a body of empirical research. Even those of us who have retreated from the unadulterated positivist credo cling to vestiges of pride in our imagined agnosticism and detachment from our subject matter. However, conducting research that takes organizing as the independent variable will ask us to spell out the outcomes we value. When we choose our dependent variables, will we opt for shareholder wealth, income equality, or HIV-infection rates? As deconstructions of some of the field's classic work have shown, we probably were never as agnostic as we thought (Calás and Smircich 1991, Kilduff 1993). In the coming years, we all may need to be as transparent as Jennifer Griffin and John Mahon.

We will come face-to-face with fundamental questions of freedom and dignity when we undertake research that examines the control mechanisms that forge connections between individuals and their employing organizations. In writing about how corporate culture is used as a control mechanism, Van Maanen and Kunda (1989, p. 92) audaciously labeled the process as "social molestation." We all will need to articulate our values as we investigate postindustrial control systems, indeed, when we investigate all aspects of organizations and organizing (Alvesson and Deetz 1996, Calás and Smircich 1996). We will also need to balance Levitt's (1958) concerns with the aspirations of other scholars who advocate corporate social responsibility (see Kotler and Lee 2005 for just the latest example). One thing seems certain: If we reconnect organization theory to the some of the world's

more urgent problems, no one will need to deconstruct our work in 10 or 20 years to see what we think about such matters.

If our field is to engage the world's crucial problems, organization researchers must retool to collect data on processes that unfold right before our eyes. To do this, we must rethink our methodological assumptions and revamp our methodological techniques. Discontinuities are endemic to today's richly connected organizational fields. In their telling account of several studies of nonlinear organizational change, Meyer et al. (2005, p. 471) concluded that "rigid research designs become a liability when studying social systems that are far from equilibrium" and urged their colleagues to employ research designs that are "temporary rather than permanent, correctable rather than correct, and discoverable rather than known." Becoming so nimble may be difficult for a field whose methods are typically ahistorical (Isaac and Griffin 1989). Indeed, Pettigrew (1987, p. 655) once indicted our field as "ahistorical, aprocessual, and acontextual." More often than not, our theories are culture bound, biased toward stability, and contextually restricted. As a result, our work is relevant to a dwindling fraction of the world's organizations and organizational contexts.

Pursuing this agenda may force us to revise some of our cherished research assumptions. We may find ourselves working closer to practice (Schultz and Hatch 2005), generating insight from small-sample research (March et al. 1991), and doing more action research (Lewin 1946, Susman and Evered 1978). We may even need to check our reflex to assail those who celebrate best practices (Strang and Macy 2001) and acknowledge that there may be a time and a place to sample on our dependent variables (Dion 1998). This issue often arises when we self-consciously commingle our values with our research ambitions (Walsh et al. 2005). In short, we need to prepare ourselves to move outside our methodological comfort zones if we are to investigate the kinds of questions we raise here.

## Conclusion

The field of organization theory is adrift. The press for "relevance" is but one symptom of a field that has lost its bearings. Executives were ejected from the MBA classroom in the 1960s because their lessons learned from a lifetime of experience were no longer relevant in a time of rapid change. Scholarship was the answer to this problem. Fifty years later, we face another crisis. The scholarship that was once so relevant is now irrelevant (or worse). The answer is not to jettison scholarship and bring the executives back into the classroom. Rather, the answer is to change our research foci. The theories we developed to comprehend the Management Revolution no longer have the traction they once did. That is

because we did our job. We understand how firms survived the Management Revolution. Our challenge now is to learn how to live in and live with these new and emerging organizations. Organization and management theory is uniquely positioned to ask and answer questions that speak to the defining characteristics of our new century. The future is ours. We need to seize it.

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