

## Symposium on the Future of Organizational Sociology

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### Abstract

The 2014 American Sociological Association, Elizabeth Gorman organized a well-attended panel session entitled, “Does Organizational Sociology Have a Future?” This panel included Howard Aldrich, Elisabeth Clemens, Harland Prechel, Martin Ruef and Ezra Zuckerman Sivan. In response to the great audience interest, Professor Gorman worked with the editors of *Work in Progress*, the blog of the Organizations, Occupations and Work section of the ASA, to continue the discussion as a virtual panel. This article presents in a single document the short contributions of the 14 scholars involved in the virtual panel. The panel begins with short essays from the five original panelists, who recapitulate and in some cases extend their remarks from the ASA session. These are followed by additional contributions from Gerald F. Davis, Heather Haveman, Brayden King, Charles Perrow, W. Richard Scott, Mark Suchman, Patricia H. Thornton, Elizabeth Gorman and Matt Vidal. In line with the blog format, this article uses hyperlinks rather than a traditional referencing system.

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## Does organizational sociology have a future?

### Howard Aldrich

I think the future of organizational sociology depends on our doing a better job of things that we already know we should be doing, but aren't. So, I'm going to not recommend we do anything new, but instead that we do some things much better.

As Liz Gorman reminded me, we were asked to talk about organization sociology, not just organization theory. I didn't want to run afoul of Art Stinchcombe's jeremiad concerning the division between "theory" and "research" in sociology. In one of his many provocative essays, Art borrowed a sentiment from Groucho Marx, who famously said "any club that would have me as a member I wouldn't want to join"! In Art's case he said that he didn't want to be part of a discipline that allowed some people to call themselves "theorists" rather than just plain "sociologists." He argued that theory and research were inextricably intertwined, and I share that sentiment. It's why I think of *research and theory* when I think of organization sociology, rather than something separate and apart called "theory." Theory should be research driven, informed by research, and used to guide research.

I'm looking for a more cumulative organizational sociology, focused on systematically building findings and identifying their scope conditions.

We were asked to think about the future. But, how can organizational sociology even have a future when it has no past? By that I mean that it is firmly anchored in the present, given the way that scholars typically construct their explanation of organizational phenomena. The global, comparative, and historical approach we would expect in organizational sociology is just not evident in our publications in the 2<sup>nd</sup> decade of the 21<sup>st</sup> century. Almost all the explanations we get are highly contingent upon conditions within the period to which a scholar's "data" apply, built so that every explanation is essentially idiosyncratically ahistorical and parochial.

For example, a [recent study of mine](#) found that among about a decade's worth of papers published in *Administrative Science Quarterly*, between 2000 and 2007, nearly 90% were focused on a single country, and 90% of those were based on the United States. I joked with the editor of ASQ that he ought to call the journal "American Studies Quarterly"! Despite years of trying to encourage multinational comparative work, one of our premier organization studies journals is firmly anchored in North America.

These papers also weren't particularly historical, either. Strangely enough, in about one-third of the 128 papers, the authors reported no information in the methods section on when the data were actually collected! Trust me, I searched long and hard for this information and it was just not reported. What does it say about our discipline's sense of historical context when editors and reviewers allow authors to get away with reporting on data that apparently are floating timeless in the ether, not tied to any particular historical period? Imagine trying to replicate a study published in the ASQ a decade or so ago and not knowing in which century the data were collected!

To their credit, about two thirds of the papers reported on data collected from more than one point in time, although about half covered only 10 years or less. About one in 7 covered 30 years or more, which one might think is sufficient to uncover any effects of era or epoch, were one using an historically informed institutional approach.

As sociologists, we must acknowledge that organization theory lives and dies in large part because of what happens in the Academy of Management, not what happens in the American Sociological Association. The AOM has almost 20,000 members now, with a vibrant Organization and Management Theory section that every year makes a Distinguished Scholar Award. That event, held early on Monday morning at every AOM annual meeting, attracts a packed house and 2014 was no exception. Royston Greenwood's presentation on "institutional theory" rocked the house of several hundred people. Whatever its future, organization sociology has lots of enthusiastic followers in the AOM. Sociology gets lots of positive mention at AOM meetings.

Given my fondness for the AOM, it pains me to point out two rather odd features of its publications. First, ignoring Stinchcombe's stricture concerning the separation of theory from research, the AOM publishes a journal dedicated specifically to "theory." The *Academy of Management Review* tolerates no data within its pages. (I am currently working on an R&R for AOM and finding it exceedingly difficult to adhere to the data-free policy.) Second, the AOM has added a new journal every few years to its stable of publications. We now have the *AOM Annals*, the *AOM Perspectives*, and the newest journal, the *AOM Discoveries*. What is missing? There is no *AOM Replications*!

I use the word "replication" in two senses. First, "replication" in the sense in which it is used in the natural and health sciences to refer to attempts to replicate, insofar as possible, results from previous studies. Investigators look to see whether previous results are valid and can be reproduced by another team of scientists. Second, "replication" in the sense that a single investigator or team conducts replications within a line of investigation that builds a cumulative trajectory of work, based upon their own previous studies. I don't see a lot of evidence for either type of replication.

We don't repeat studies and few people do cumulative work, focusing on testing the boundaries of a theory or refining it. For example, [most people who have written about resource dependence over the past three decades only produced a paper or two on it](#), flitting into the domain, publishing a paper or two, and flitting out again. Almost no one fully committed to developing resource dependence theory as a coherent approach with consistently defined terms, basic principles, and replicated research findings.

The incentives just aren't there to encourage this kind of work. Editors want articles that are new and different, looking for work that breaks new ground or rejects existing findings, rather than extends and refines them. We have the "one and done" model and a fetishizing of the novel and different.

But, how can organization theory get any better if its raw materials are never more than semi-refined? Until we began doing things better, we will be perpetually stuck in the persistent present.

Liz challenged me to consider whether some of the problems I've described flow from the persistent loss of organizational sociologists from sociology departments to business schools, leaving only a few of us sociologists behind. Dick Scott and I have talked about this for decades and it is a concern. But I think these problems stem from changes in norms and practices with the academic profession, rather than from the "loss" of organization sociology to the "dark side."

Wait! What's that? Darth, you ARE my father?!

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## Possibilities for an interesting future for organizational sociology

**Elisabeth S. Clemens**

As teachers, we often hear that the future will be shaped by our students. If this is the case, then the signs are mixed and confusing. On one day, a good omen may appear, typically in the form of an enthusiastic undergraduate. At the University of Chicago, these students are often economics or public policy majors who have encountered a piece of organizational analysis and seized upon it as the key to understanding the complexities of the policy process, firm behavior, or the organization of markets. On less auspicious days, our most dedicated graduate students present a different vision of the future, one in which organizational researchers risk becoming overwhelmed by a meta-literature, focused on agendas, epistemologies, ontologies and reflections.

This tale of two students poses a challenge for organizational sociologists. How can we retain the capacity to inspire while demanding of ourselves the kind of rigor and clarity

that are represented by all those discussions of ontology, epistemology, and method? This challenge is not new. As a graduate student, I received the following job market wisdom circa 1990: “Go out on the market as an organizations person. Everyone knows they need one. Everyone thinks they are boring.” If I could pull off a performance as an interesting organizations person, I would do just fine.

These three tales remind us to revisit a key question for any scholar: What makes something interesting?

This question was a favorite of the late and greatly missed Mayer Zald, who enthusiastically promoted Murray Davis’ claim that [interest is produced by the combination of theoretically-motivated expectations with an unexpected claim or finding](#). Whether something is interesting depends upon its relationship to a background provided by theory, prior studies, and common sense: “Interesting theories are those which deny certain assumptions of their audience, while non-interesting theories are those which affirm certain assumptions of their audience” (1971: 309). When it comes to empirical findings, this threshold for interesting-ness is often relaxed: interesting explanations add some new factor to those that we already think are important or succeed in applying a familiar explanatory model to a novel case. At the level of either interesting theory or interesting findings, therefore, Davis’ argument poses a conundrum. Really interesting work undermines the strength of the expectations that make it interesting.

At a moment when sessions on “the future of organizational sociology” draw large crowds (implying, if Davis is right, that many think that future is uncertain), it is worth reflecting on how organizational sociologists in the past succeeded in “being interesting” and how we might confront the challenge going forward.

American organizational sociology found its stride in the post-war decades with compelling analyses of large corporations and public agencies. The central problems for this literature were closely connected to the recently-institutionalized canon of sociological theory: Adam Smith and Emile Durkheim on the division of labor, Karl Marx on the evolution of

capitalism, and, above all, Max Weber on bureaucracy. At a basic level, it was interesting to find that the models developed to explain the rise of European capitalist modernity were applicable to American cases. Still more interesting were the cases where those theoretical expectations were not met – or not met in the way that one might expect. Philip Selznick’s classic [TVA and the Grassroots](#) illuminated how formal bureaucracy was subverted by informal relations and cooptation in the effort to realize the goals of the Tennessee Valley Authority. This was truly interesting stuff at a moment when readers expected great things of professional public bureaucracies. Decades later, however, to a readership well-prepared to expect dysfunctional government, the findings may appear less surprising. As our structures of expectation become more fragmented or are undermined by historical change, being interesting becomes harder for everyone. One response to this situation has been a call for [“problem-driven” research](#) that, according to Jerry Davis and [Christopher Marquis](#), draws “on organizational mechanisms to make sense of more or less singular historical occurrences in institutional fields.” But what constitutes a historical occurrence that merits inquiry? If research is to be interesting, what constitutes an interesting problem? The authors of those classics of post-war organizational sociologists had an answer to this. Those “old institutionalists” knew – either second-hand or through their own experience – of the contrast between something they understood as “community” and the formal hierarchy of bureaucratic institutions. The receipt of a draft notice and the experience of induction into one of the most significant complex organizations were interesting both in their substantive (indeed life-and-death) qualities and as contrasts to the expectations derived from very different ways of life. For a slightly later generation, federal funding supported vibrant multi-disciplinary conversations centered on policy domains such as health and education, issues central to the practical organizational project of implementing new social programs.

Going forward, organizational sociologists should be attuned to new substantive problems that challenge the expectations that are formed not only by reading the canon of organizational theory but by our immersion in a social world in massive flux. A number of years ago, Julia Adams, Ann Orloff and I edited [a book in response to a similar sense of disorientation that was deeply felt by historical sociologists](#). At the time, it seemed that the

vibrant debates of the late 1970s and 1980s had given way to projects that took classic debates over revolution or state-formation and then “added a theory” or explored another case. Many of these contributions were extraordinary in their own right, but the cumulative effect was to undermine those dramatic clashes between Marx and Weber that had animated the work of that earlier cohort of historical sociologists. Less than a decade after the results of that collective reflection were published, we find the subfield revitalized not by meta-discussions but, to use William Sewell’s phrase, through [an “eventful” revitalization](#). Multiplying wars, an apparent resurgence of “empire” as a form of political order, transformations and reconfigurations of political regimes – all this chaos reanimated the questions central to historical sociology.

So one route – and surely not the only one – to an interesting future for organizational sociology might be found in paying greater attention to the structures of relevance that make organizations interesting to other sociologists and to general audiences. One possible path to that destination requires engagement with a set of theorists broader than the familiar theory debates that have powered the subfield for so many years. A second path would be guided by a more eventful sense of the ways in which contemporary organizations confound the expectations not only of organizational sociologists but of broader audiences, questions that might be signaled by the claims of lawlessness on the part of professional police organizations, the ways in which organizational strategy has permeated political institutions, or the forms of networked organization that undergird the rapidly evolving security state and the transnational economy.

Questions generated in this way are likely to be big, complex and messy – and therefore risky in the context of the work world in which academics live. In a subfield increasingly focused on new ventures, perhaps it is time to rethink our trade-offs between classically Weberian calculability and entrepreneurial risk-taking.

## Revisiting theoretical presuppositions and the future of organizational theory

**Harland Prechel**

Organizational scholars have raised a number of questions with regard to the future of organizational theory. Is organizational sociology losing its audience? If so, is it because of the expansion of organizational studies in business schools and the increased interests in questions asked by other subfields in sociology? If organizational theory is losing its audience, is the shift due solely to changes external to the field or can it be explained by the origins and development of organizational theory itself?

Some organizational sociologists may be concerned by the expansion of graduate programs in business schools as undermining job opportunities for organizational sociologists. Although this is a relevant concern, it should not come as a surprise. Despite the interdisciplinary focus of a few graduate programs in organizational studies, business (and public administration) schools have questioned the relevance of the sociology of organizations for years. Many of these questions emerge from the inherently conflicting disciplinary agendas and the failure to confront and resolve these conflicting agendas.

Although organizational theory may be losing some of its audience, there are many reasons to be optimistic about the future of the field. Organizational theory has a long and rich history of high-quality empirical studies and useful concepts that explain a wide range of organizational phenomena. Despite these strengths, in my view, if organizational theory is to retain its audience, the primary task is to build on these analytic tools to construct organizational theories with greater explanatory power.

A central problem with organizational theory is the inherent tension between the sociological imperative and the managerial imperative and the development of theories that attempt to incorporate both imperatives. Although rarely discussed in current

organizational theorizing, these competing agendas are impediments to theory development.

On the one hand, in the classical tradition, the *sociological imperative* produces knowledge that explains the origin and nature of society where organizations are conceptualized as specific instances of a broader process of social organization. The sociological imperative lends itself toward research questions related to how different structures emerge and the implications of the historically specific organizational structures for society including how inequality emerges, who holds power, and how power is exercised. The answer to these questions begins with the understanding that organizational structures and behaviors vary over time and organizations are embedded in environments. To illustrate, the sociological imperative was central to Max Weber's observation that modern bureaucracies emerged from the historical transition to capitalist economies and democratic forms of government.

On the other hand, the *managerial imperative* produces knowledge that has implications for practical problems internal to organizations such as effectiveness, efficiency and productivity. Although these problems may have their origins in the environment, the primary focus is on how to solve them by modifying the internal strategy and structure of organizations. The managerial imperative also requires that managers give primacy to short-term outcomes. Although the managerial imperative may include the study of long-term outcomes, it gives primacy to the short-term because the failure to achieve short-term goals has important consequences for the survival of organizations and the careers of its managers.

The distinction between sociological and managerial imperatives cannot be equated with organizational research in sociology versus management departments; the sociological imperative informs much research conducted in management departments. Further, an argument can be made to pursue the sociological imperative and teaching it to prospective managers because they must understand organizations in order to effectively manage them and sociological theory provides the analytic tools to understand organizations.

The managerial imperative is only a problem for organizational theory when it directs researchers' attention away from the larger historical processes and social forces that effect organizational structures, processes, and outcomes. Unfortunately, the managerial and sociological imperatives have been intertwined since the early 20th century and the sociological imperative has often been secondary to the managerial imperative. To illustrate, the rational model in organizational theory was heavily influenced by Taylor, Fayol and other early advocates of the managerial imperative who focused on constructing organizations to increase efficiency. In contrast, the rational model gave limited attention to Max Weber's concern with the implication for social organization and society when people adhered to formally rational principles to increase efficiency. Similarly, drawing on Durkheim's theory, early human relations theory focused on informal characteristics of organizations and work groups within organizations. However, as the human relations perspective developed, it focused on how to manipulate the group in ways that increased motivation, efficiency, and productivity. Another illustration of the predominance of the managerial imperative is principle-agent analysis, which does not pay sufficient attention to the larger social processes and the organizational structures in which principles and agents are embedded.

In short, the separation of management programs from the sociology of organization can be understood as the manifestation of the conflicting agendas between the sociological and managerial imperatives. Rather than lament this development, from a social scientific perspective where theory development is primary, this separation is an opportunity for each field to assess their strengths, build on those strengths, and develop more integrated organizational and managerial theories with distinct disciplinary foci.

One implication of the conflicting agendas of the managerial and sociological imperatives is the absence of an integrated sociological theory of organizations. Instead, there are numerous competing perspectives that give theoretical primacy to a concept to explain organizational phenomena. To illustrate, population ecology gives primacy to fit with the environment, resource dependence gives primacy to resources, neo-institutionalism gives

primacy to culture, and transaction cost analysis gives primacy to efficiency. The narrow scope conditions of each perspective limit the questions that can be asked and answered.

More importantly, the separation of organizational phenomena into different perspective creates obstacles to developing a theory capable of explaining (1) [relational complexity between organizational entities](#) (e.g., divisions, subsidiaries, and partnerships) and the implication of these internal organizational arrangements for society, and (2) the interconnections among organizational characteristics such as fit with the environment, resource dependence, culture, and efficiency enhancing initiatives. What is needed therefore is a theory capable of identifying the most salient characteristics of organizations and employing them to explain historical variation in organizations and the meaning of those changes for society.

A sociological theory of organization should begin with specifying the conditions within which organizational behavior occurs and how behavior changes over time. Contemporary organizational theorists could benefit by reconsidering Weber's conception of ideal types, which identifies the most salient features of a phenomena. Among the prevailing perspectives, the evolutionary perspective has the most potential to develop a more inclusive and integrated organizational theory. Although it gives primacy to organizational change, strengths of the evolutionary perspective include incorporating concepts from other perspectives and examining multiple levels of analysis.

The narrow focus of the prevailing perspectives in organizational sociology can explain why so few organizational sociologists anticipated the 2007-2008 financial crisis even though most of the behaviors that caused the crisis occurred in organizations. As we now know, this was one of the most significant events in the last century. In contrast to the prevailing perspectives, building on the classical tradition (Marx, Weber, Durkheim, and Polanyi), organizational political economy examines how organizational and political-legal arrangements emerge and affect managers' behavior. My research in this area concluded that [the organizational and political-legal arrangements that emerged in the 1980s and 1990s permitted the emergence of capital structures similar to those that preceded the](#)

[Great Depression](#) and if another financial crisis occurs it will adversely affect a large proportion of the middle and working classes (Prechel 2000:265). In addition, prior to the financial crisis, journalists and public intellectuals exposed the [widespread use of complex financial instruments](#), and the [difficulties tracing financial transactions in the emergent corporate structures](#).

An organizational theory that conceptualizes organizations as part of society and examines historical variation and multiple levels of analysis will also direct researchers' attention toward organizational types as they emerge and spread. Several contemporary organizational types have important implications for society, but receive little attention from organizational sociologists. Although political sociologists have given some attention to political action committees (PACs) and 501(c)4s, organizational sociologists have given little attention to how these organizations are expanding their influence over domestic politics and society. Similarly, foreign sponsored nongovernment organizations (NGOs) are rapidly increasing in numbers and size in less developed countries where they fulfill government functions. The emergence and spread of these kinds of organizations have important implications for society because their managers yield a substantial amount of power over the distribution of resources controlled by them. Although health care organizations are rapidly expanding, they also receive little attention from organizational sociology.

One important characteristic of theory is that it provides a history and memory of knowledge that has been acquired. More inclusive theories provide a more comprehensive memory of acquired knowledge. This memory is important because it provides direction for future research and the bases to develop more complex theories with wider scope conditions and greater explanatory power. An integrated theory of organizational sociology specifies, for example, the conditions under which organizations remain stable, when change occurs, when organizations mobilize political to change their environment, when organizations change their structure, and what conditions are associated with high-risk managerial behavior. These topics are not new to organizational sociology. However,

incorporating them into a more inclusive theory would represent a significant shift in organizational theory and provide a basis to revitalize and strengthen the field.

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## Organizational sociology has a past ... Does it have a future?

### **Martin Ruef**

In my view, organizational sociology has a past – indeed, a LOT of past. But it is less clear whether it has a future.

Consider a simple word association test. When I say “organizational sociology”, what do you think of? You probably think of core theoretical paradigms such as organizational ecology, the new institutionalism, network theories of organizations, and the neo-Weberian view of bureaucracy. Perhaps you would be inclined to put evolutionary organizational theory into the mix. And, if you are feeling a bit more adventurous, you might add organizational ethnography, theories of organizational culture, and even the Carnegie School. These are the major frameworks that emerged (or were reborn) between the 1950s and the 1970s; and came of age in the 1980s and 1990s.

When I say “organizational sociology”, it is also likely that that you’ll think of the scholars who developed these perspectives. I’ve listed a number of them here, including many familiar names ([Ruef – Table](#)). I have also listed some of their most influential articles and books, as well as the staggering number of citations for them.

The past is about people, not just ideas. So what are these folks doing now? Unfortunately, we have had a number of leading organizational sociologists pass away over the last few years, including John Freeman in 2008 and Michael Cohen last year. And a number of our

leading luminaries have recently moved to emeritus or emerita status – although some, such as Dick Scott, remain as active as ever.

What about the rest? It is always dangerous to predict retirements, but I will play Social Security Administration and apply a very simple rule: let's identify everyone who is within one year of benefit eligibility (61+) as "near retirement", knowing that we are likely to witness many more productive years ahead. This status applies to pretty much everyone who is left on the list. Whenever I try to get in touch with Howard Aldrich, I find that he is often traveling somewhere to go *fly fishing*. Now the attraction of fly fishing remains a deep mystery to me. But I have also begun to wonder whether this activity is good for organizational sociology.

So, what happens to organizational sociology when Howard Aldrich goes fly fishing? Or, more generally, what kind of future can we expect in the field once the current wave of retirements and "near retirements" is done?

One possibility is that when Howard goes fly fishing, organizational sociology goes to hell. The underlying thesis is that there was something unique about the organizational scholars who received their Ph.D.'s in the 1960s and 1970s – a genuine cohort effect. It matters that many of them were trained in sociology departments, not business schools. And that many of them trained students outside of business schools. It matters that many of them made their careers at a relatively small number of institutions (places such as Stanford, Chicago, UNC, Cornell, and Michigan). And that they forged life-long commitments to developing perspectives with strong theoretical assumptions.

While it is plausible that organizational sociology (at least as we know it) would disappear with this cohort, I don't think that is going to happen. A second scenario is possibly worse – the field could be in limbo! If you go to the Academy of Management (AOM) conference these days, you know that citations to Meyer and Rowan or Burt are not going away. The "science" of business administration relies heavily on the disciplines (sociology among them) as a source of its legitimacy. But once the cohort of scholars who are so often cited

go into retirement, something curious may happen. Their work may be cited in perpetuity, often in a ritual fashion, without any serious effort to rejuvenate organizational sociology. The fate of the field will be that of an incremental, normal science.

Now, it is not clear that this state of limbo is a new development. Some would say we've been here since the 1980s. Try this as experiment: go to [Google ngram](#) and have it graph the prevalence of "organizational sociology" in the English-language corpus between 1950 and a year close to the present. You'll see that there are virtually no mentions of that term in the 1950s, followed by a modest increase beginning in early 1960's (perhaps spurred by books such as Blau and Scott's [Formal Organizations](#) or Burns and Stalker's [Management of Innovation](#)). Then you get to the mid-1970s and ... bam! Interest in organizational sociology takes off, particularly with the development of the core frameworks I mentioned earlier. By the mid-1980s, though, this wave of excitement has subsided and things have settled into the new normal. If we take the ngram results seriously, perhaps the field has been in limbo for some time.

Okay, let's try to end on a more positive note — we certainly don't want Howard to feel guilty about his fly fishing excursions! What if the field of organizational sociology was reborn with this wave of retirements? Let me offer some predictions (and hopes) for the rebirth of organizational sociology.

The first prediction is that rebirth won't come from the current leaders in the field (or their students). Speaking for myself, I have consumed too much of the Kool-Aid, having been raised on a steady diet of organizational ecology, new institutionalism, organizational learning theories, and the like. Folks like me can't help but think in terms of these well-worn paradigms. We are looking for a new cohort of scholars that will think in fundamentally different ways.

Second, the emergence of new sociological frameworks for studying organizations may benefit from some of the same structural factors that drove the earlier cohort. In his [Sociology of the Philosophies](#), Randy Collins talks about the law of small numbers. The

prediction is that we get vibrant intellectual fields when scholars are spatially concentrated in a few institutions; and when there is intense competition between a few frameworks. I can imagine geographic clusters of universities with a strong pedigree of organizational sociology (Harvard-MIT, UNC-Duke, Stanford-Berkeley) that would provide the structural basis for the next generation of paradigms.

Third, the progenitors of the new paradigms will need to ignore their elders. As much as we all adore the influential articles and books listed in the table above, the innovators among us need to stop citing them. This is not quite the same as claiming that ignorance is bliss – we don't want a new generation of organizational scholars that simply re-invent the wheel. But we don't want to be ham-strung by the past either.

Finally, I believe that the rebirth of organizational sociology should draw on cognate fields in the discipline, but must avoid being subsumed by them. Some of the most exciting developments in the study of organizations over the last fifteen years have occurred at the intersection with other fields – social movement theory, economic sociology, historical sociology, social psychology, to name a few. This cross-fertilization has been tremendously productive and should continue. But organizational sociology must also stake out a distinct intellectual identity in the process.

With those hopes in mind, I will return to Howard's favorite pastime and the following exhortation: organizational sociologists, let's go fish for new theories!

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## Why I'm bullish about organizational sociology

**Ezra Zuckerman Sivan**

As I noted in my remarks at the ASA this past August, I am optimistic about the future of organizational sociology. Why? In short, the reason is that over the past several years, I

have had the great privilege of serving on dissertation committees of students whose work demonstrates the continuing importance of organizational sociology and who have made significant progress in pushing the subfield forward. I mentioned three such scholars and their work. If I had time, I would have mentioned a few more. Of course, I could have followed a more quantitative route in trying to assess trends in the field. But I care much more about quality than quantity. That there are even a few young organizational sociologists doing such high-quality work is enough for me to be optimistic about the future.

The first such scholar I mentioned was [Catherine Turco](#), who graduated from Harvard sociology in 2011 and is now a colleague of mine at MIT, in the Sloan School of Management. It is hard to know where to start when discussing how Turco's various projects deepen our understanding of organizational phenomena. In my remarks, I directed our attention to Turco's 2012 article, "[Difficult Decoupling: Employee Resistance to the Commercialization of Personal Settings](#)." This paper is a rich and analytically powerful ethnography of a failed attempt to commercialize an area of personal life—maternal care. A key insight is the “market” is not the agent of commercialization; rather, for-profit *organizations* are. Moreover, successful commercialization is difficult because it requires that the organization enlist their employees in executing the organization's strategy of decoupling between their non-commercial discourse (which in this case is about caring for moms) and their commercial goals (selling moms expensive products). And this turns out to be quite difficult because employees may have their reasons (e.g., it coheres with their professional goals) for endorsing the discourse rather than the goals. What emerges is a deeper appreciation for (a) the role of organizations in a process (commercialization of personal and other non-commercial spheres) that has occupied much attention by economic and cultural sociologists; and (b) why organizational decoupling is not as easy to pull off as we thought.

The second scholar I mentioned is [Roman Galperin](#), who is a 2013 alum from [the Economic Sociology PhD Program](#) at MIT Sloan and is now on the faculty of the Carey School of Business, at Johns Hopkins. The paper I summarized is "[Organizational Powers: Capture of](#)

[Professional Jurisdiction in the Case of U.S. Retail Clinics.](#)” This paper is also centered on an interesting case, the emergence of clinics (typically at drug stores) where nurse practitioners provide primary care that is usually provided by physicians (or a nurse under the supervision of a physician). What’s strange about this case is that both professions—doctors and nurses—lose, while the organization wins. This kind of “jurisdictional settlement” does not fit into Andy Abbott’s [ecological theory of professions](#), and it thereby becomes an occasion for reworking the theory. What emerges is a clearer conceptualization of the notion of “jurisdiction” (involving not just rights but the resources for enforcing such rights) and a key role for organizations (as mobilizers of such resources) in determining the outcome of jurisdictional battles. I hope that Galperin’s paper will reinvigorate research on professional competition, which has remained largely dormant since Abbott’s 1988 masterpiece, the *System of Professions*.

The third scholar I mentioned was [Phech Colatat](#), who is a 2014 alum from [the Economic Sociology PhD Program](#) at MIT Sloan and is now at the Olin Business School at Washington University. Colatat’s main dissertation paper [“Imprinting Variation: The Diagnosis of Autism Spectrum Disorder at Two Specialty Clinics”](#) is a contribution to the puzzle of the rise of autism diagnosis in the United States. Colatat uses an array of qualitative and quantitative data from Kaiser Permanente Northern California to demonstrate that the marked upswing in autism diagnosis masks significant intraorganizational variation in diagnosis rates: in particular, some clinics at KPNC consistently diagnose autism at higher rates than others. And this is so even though the incentives and formal processes are the same and even when he controls for differences between patients. So why the variation? Colatat’s analysis of two clinics demonstrates that their persistent difference in diagnosis derives from organizational practices that were “imprinted” in the clinics via organizational routines by their founders, who differed markedly in how “conservative” or “liberal” they were in interpreting ambiguous behavioral markers. Besides demonstrating the relevance of key organizational processes to a phenomenon of major societal concern, Colatat’s research makes a key contribution to organizational sociology, by showing how processes associated with the “new institutionalism” (i.e., rapid diffusion of a new belief and associated practices; in this case, the idea that autism is common and the protocols for

diagnosing it) and the “old institutionalism” (i.e., that their specific histories give each organization [or in this case, an organization subunit] a distinctive character) can work together.

I hope that you will not rely on my quick summaries of the work of Turco, Galperin, and Colatat, and that rather I have inspired you to read these papers yourself. When you do so, I am confident you will see why I am so optimistic. If this is not the kind of work we want organizational sociology to be—i.e., based on deep knowledge of the phenomenon under study, demonstrating that organizations matter for outcomes that are of significant societal concern, and making significant theoretical progress in our understanding of organizational processes—I don’t know what is. And as I mentioned, there is other work by young scholars I could also include as providing reason for optimism about the future of organizational sociology (such as Rodrigo Canales on “[Managing Organizational Tensions Between Standardization and Flexibility in Microfinance](#)” ; Canales & Jason Greenberg on “[A Matter of Relational Style:](#)” and Ethan Mollick and Venkat Kuppuswamy on “[Formal Organizations and the Benefits of Crowdfunding](#)”).

Of course, if you define work as “sociology” only if the scholar works in a sociology department, you may disagree with me. But I think that is a self-defeating way to define things. Each of the three scholars I discussed identifies him or herself as a sociologist. And the kind of work they do—drawing on and extending past ideas in organizational sociology—is exactly what we want and need to push the field forward. Thus there is every reason to embrace the work I have reviewed and look forward to what comes next. I certainly am.

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## Organizational sociology after Nikefication

**Gerald F. Davis**

Organizational sociology may have reached its high water mark 25 years ago, when Chick Perrow penned "[A society of organizations.](#)" Perrow argued that organizations had absorbed society, which implied that organizational sociology was now the master key for making sense of society. He stated, "I argue that the appearance of large organizations in the United States makes organizations the key phenomenon of our time, and thus politics, social class, economics, technology, religion, the family, and even social psychology take on the character of dependent variables." Stratification happened through organizational practices of hiring and promotion. Work went on inside organizations, structured by organizational rules. Social movements increasingly constituted themselves as formal organizations. In a society of organizations, organizational sociology should be the sun around which the other subfields in sociology orbit. Instead, organizational scholars are scarce on the ground in most departments today, as if the Rapture had come and left behind only the demographers and criminologists.

Many or most of the disappeared wound up in business schools. It's not hard to see why: the money is better, and the jobs were more plentiful. Yet it would be a mistake to imagine that b-schools are crammed full of organizational sociologists, at least in North America. While fancy schools like Stanford, Northwestern, and MIT are strong outposts for organization theory, most schools are not. Hiring is typically driven by teaching needs, and there is surprisingly little demand among MBA students for courses on organization design (much less institutional logics or categorization). Most organization theorists in business schools wind up teaching strategy and, if they want to get tenure, publishing work that can pass for strategy. All of this bodes ill for organization theory, wherever it is done. How did things go so wrong? The short answer is that the "society of organizations" described by Perrow has de-composed back into a primordial soup. The ever-expanding, enveloping organizations of the post-War era have reversed course; now, they want to be as "focused" and streamlined as possible, with as few assets and employees as is feasible. Every week brings news of corporations either splitting up into constituent elements (Hewlett-Packard, Symantec, GE, Time-Warner, Procter & Gamble, Abbott Labs, Sony), going private or bankrupt to radically restructure (Dell, GM), or evaporating entirely (Lehman, Countrywide, arguably Kodak). The number of public corporations in the US has

dropped by 55% since 1997, and those that remain are generally a lot smaller than they used to be: GM has as many employees today as it did in 1928. Organizations are increasingly hollow shells.

[I have argued elsewhere](#) that this shift is largely due to low-cost information and communication technologies that make it possible to rent (rather than buy) components of a supply chain and scale up or down as needed without bearing the cost of maintaining an ongoing organization. (Call it “Nikefication.”) It’s expensive to be Sony, with 150,000 employees. It’s cheap to be Vizio, with 200 employees and flat-screen TVs that are much less expensive and more popular than Sony’s. It’s expensive to be Blockbuster, with 83,000 employees and 9000 stores. It’s relatively cheap to be Netflix, with 2000 employees and server capacity rented from Amazon. (Amazon, in turn, rents temporary employees in bulk from staffing services. The firm plans to [temporarily expand by 80,000 workers](#) just for the holiday season this year.) The “society of organizations” increasingly resembles Disney’s Main Street USA, a series of facades with little substance behind them.

Perhaps the reason there are fewer organization-level questions of interest is an ontological one: organizations *qua* organizations have largely evaporated. Going forward, we are likely to see more pop-ups (like Vizio) and “platforms” (like Uber) and fewer countable, bounded, goal-oriented, hierarchical, employing organizations. The six characteristics of bureaucracy outlined by Weber in Chapter 11 of [Economy and Society](#) have little obvious application in this world. (As a reminder, they were: rule-governed jurisdictional areas; hierarchically-organized offices; official written files; specialized training for office-holders; office-holding as a full-time job; and management according to stable, general rules.) Enterprise resource planning software enables “flat” hierarchies by automating employee schedules and performance evaluations. Retail is en route to being [even more Taylorized](#) than manufacturing was, with human management replaced by algorithms. Those forced into the [TaskRabbit economy](#) (the “precariat”) have an even more marginal connection to regular employment than those in retail. The new book by Dan Clawson and Naomi Gerstel, [Unequal Time](#), highlights some of the difficulties imposed on the working class by unpredictable scheduling and uncertain employment.

Venerating the elders, as sociologists love to do, may be a path to irrelevance. Aiming to document the timeless truths of Weber's writings is unlikely to lead to a deep understanding of the contemporary organizational landscape and the new world of precarious employment. We may be weighed down by a devotion to the past. (Sociologists also don't help themselves by endless professional mitosis. 52 sections, srsly? When is Economic Sociology going to spawn Economic Astro-Sociology as a new section?) Where to next? When Dick Scott and his collaborator updated the classic text in organizational sociology in 2007, the new name was [Organizations and Organizing](#) to highlight the verb over the noun. And although network analysis provides some tools for thinking about organizing, theory has not caught up yet with our provisional world. Sociology is in a unique position to contribute to the study of organizing. Mustafa Emirbayer has called for a [relational sociology](#), and there have been various efforts at field theories. These point in the right direction. Perhaps abandoning the countless divisions within sociology among those who study organizations, collective action and social movements, stratification, economic sociology, and other related domains would be a step forward toward understanding how coordinated human action happens today.

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## Organizational sociology — just step outside and you'll see the future

**Heather Haveman**

In August the American Sociological Association held a panel, organized by Liz Gorman, on "The Future of Organizational Sociology." I want to follow Liz's instructions and react to the comments made by the panelists (Howard Aldrich, Lis Clemens, Martin Ruef, Harland Prechel, and Ezra Zuckerman). In doing so, I'll try to connect their comments and suggest how organizational sociology can have a vibrant future.

Is it really true that organizational sociology has no past? I'm not as pessimistic as [Howard Aldrich](#): I see many very good sociological studies of organizations that are historical, meaning that they are sensitive to both time and place. Let me give you just a few examples. First, my Berkeley colleague [Cristina Mora's](#) book *Making Hispanics* (2014 University of Chicago Press) reveals how social-movement organizations, the US Census Bureau, and Spanish-language media firms jointly created the ethno-racial category "Hispanic," despite the fact that Spanish-speaking immigrants to the US differ widely in terms of race and country of origin. This book powerfully demonstrates the effect of organizations in a particular time and place – the United States in the 1970s and 1980s.

Second, let me mention my work on the magazine industry in America from the mid-eighteenth to the mid-nineteenth century. In a series of published papers, all available on [my website](#), and a book entitled *Magazines and the Making of America* (which will be published in 2015 by Princeton University Press), I demonstrate how, over the first 120 years of their history, magazines connected people: this "old" new media literally mediated between people, facilitating frequent interactions between them even when they were far apart and would otherwise never meet face to face, thus creating many distinct communities whose members had common interests, values, principles, ideas, and identities. These included communities of faith (religion), purpose (social reform), and practice (commerce and specialized occupations). Communities of different types often intersected, which fostered the pluralistic integration that was central to American public culture in this era. In this way, magazines helped make an America that was distinct from European societies: magazines both pushed American society toward a common center and pulled it apart into many distinct subgroups.

Yet, as my examples of historically sensitive research reveal, Howard is right to point out the great extent to which the evidentiary basis for organizational sociology is US-based. But there are many studies of organizations in other countries – such as work on China by [Andy Walder](#), [Victor Nee](#), [Lisa Keister](#), and [Doug Guthrie](#), for example. The rise of social science in China will, I expect, continue to produce much more research on this huge, fascinating, constantly changing, and highly heterogeneous country.

Should sociologists be in business schools? Harland Prechel says no but I say *of course they should!* Why would you want to abandon the business school turf to economists and psychologists? Indeed, sociologists should be in all kinds of professional schools – law schools, public-policy schools, medical and public-health schools, education schools, and engineering schools. These professional schools have welcomed such diverse organizational sociologists as [Laurie Edelman](#), [Cal Morrill](#), [Marshall Ganz](#), [Steve Shortell](#), [Richard Arum](#), [Woody Powell](#), [Steve Barley](#), and [Pam Hinds](#).

For my part, being in a business school was immensely invigorating. I had to fight the dominant expectation about what was efficient – and I always love a good intellectual fight. The inherent tension between what Harland calls the sociological imperative and the business/policy/ applied imperative makes visible the assumptions inherent in sociological analysis of organizations and makes us confront the assumptions inherent in economic, psychological, and political science analysis. For example, take the question of what are and should be the main goals of business firms. Do they really seek to maximize shareholder value? What are the consequences and under what conditions is this dynamic manifested? When did firms start doing this and why? Moreover, the tension between imperatives forces us to ask questions that have practical, real-world implications – to have dependent variables that someone else cares about.

Finally, I think Lis Clemens, Martin Ruef, Ezra Zuckerman, and I agree on a future for organizational sociology: it must become more central and more interesting to sociology writ large, rather than just to members of the Organizations, Occupations and Work section. It can do so by explaining not organizations *per se*, but *organizations' impact* on outcomes that are central to people in other subfields – stratification, political sociology, cultural sociology, social psychology, gender, race and ethnicity, immigration, medical sociology, law and criminology, religion... I could go on and on, but you get my drift. Ezra highlighted several worthy examples of how studying the impact of organizations on outcomes that people outside of the OOW section care about. Let me add a couple more. My colleague Cristina Mora, whom I mentioned above, is studying social media to probe how

several different types of organizations – political parties, government agencies, immigrant voluntary associations, and even media firms – attempt to define, categorize, and promote identity narratives among Latin American immigrants to Spain. One of the ways she does this is by examining how political parties establish pan-ethnic identity associations – the way that they fund them, advise them, and even create identity narratives for them. Another way she does this is by examining how government agencies make decisions about which projects to fund. She finds that city managers like to fund projects that bring together multiple Latin American subgroups (such as projects by Colombians *and* Argentines). Facebook provides a great way of keeping track of these groups; for example, she can see how the Colombian group announces joint projects with other groups or how political parties develop Facebook pages to acquire the Latino vote.

Similarly, [Chris Bail](#) has studied how a wide array of political, civic, and religious organizations shaped the public understanding of Islam after the September 11 attacks. He showed that the types of claims about the nature of Islam – notably, their emotional tone – determined whether, how, and which members of this organizational ecology (fringe or mainstream) were able to garner attention for their particular political stands and ideologies. In follow-up work, he is analyzing Facebook posts by non-profit organizations to determine how certain messages “go viral.” More important, he is using the data he and his colleagues gather to make detailed suggestions about how non-profits can reach new audiences and maintain the ones they already have.

[Marissa King](#) and [Peter Bearman](#) have shown that organizational policies have tremendous effects on physicians’ prescribing practices. Specifically, those who attended medical schools with active conflict of interest policies are less likely to prescribe newly introduced, heavily marketed, and very expensive medications over older, less expensive but comparable alternatives. Most recently, Marissa and [Jennifer Jennings](#) showed that states’ educational policies, school schedules, and insurance company rules jointly affect students’ use of stimulants. Students were more likely to go off their stimulant medication during the summer months in states with strong educational accountability systems. The effect was quite large: students were 30% less likely to have a prescription filled for stimulant

medication during the school year than they are during the summer. Moreover, this trend was more pronounced among families with private insurance than those with public insurance.

Finally, several scholars, including [Toby Stuart and Waverly Ding](#), [Jesper Sørensen](#), and [Tiantian Yang](#), are working to understand how employing organizations facilitate or hinder employees' becoming entrepreneurs in the United States, Denmark, and Sweden. Following in the footsteps of [Rafe Stolzenberg](#), [Jim Baron](#), and [Bill Bielby](#), other sociologists (too numerous to list here) are studying how the structures and policies of employing organizations affect gender and racial/ethnic inequality – topics that are central to sociology.

My conclusion from this brief survey is that organizational sociology has a great future – but only if we don't become insular and ignore the many lessons we can teach our colleagues in other subfields.

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## Why we should stop worrying about organizational sociology

**Brayden King**

Following the panel on “The Future of Organizational Sociology” at the 2014 American Sociological Association meeting, there seems to be a worry that we're a subfield that is out of touch with mainstream sociology. But for a field in trouble, organizational sociology does very well in our discipline's most important journals. Looking at issues of the *American Journal of Sociology* and the *American Sociological Review* published in the last year (since June 2013) and based on my rough coding of abstracts, articles that are primarily about organizations, occupations, and work made up 29% of AJS articles and 32% of ASR articles. The articles cover a diversity of theoretical and empirical issues, from explaining [businesses' responses to economic recessions](#), the influence of [category-spanning on hiring](#)

[among freelance workers](#), to the effects of [corporate downsizing on management diversity](#). If I had included all articles that use organizational theories, in some way, to explain a phenomenon, the percentages would be much higher. Based on these numbers, it's hard to see how anyone could make the case that the future of organizational sociology is anything but bright.

So why is there such fear that organizational sociology's future is in danger? Clearly, the existence of high quality research on organizations and work is not the problem; rather, I think the fear reflects changes in the constitution of the subfield itself. Our concerns stem mainly from the lack of organizational scholars in sociology departments and an increasing association of organizational sociology with business schools. Some people worry that organizational sociology is being diluted as a category of sociological research due its increasing presence in business schools. If organizational sociology is no longer taught in sociology departments or practiced by people who have PhDs in sociology, the subfield is going to become disassociated from the rest of sociology and lose its relevance.

I have had this same worry myself and [wrote about it on orgtheory.net](#) several months ago. Organizational sociology is missing from many top departments. Some of the field's most prominent scholars are in business schools rather than sociology departments, and the PhD students they produce, although sociological in their training, have degrees from schools of management or business. At the same time, many of the field's most senior and influential theorists have recently retired or are nearing retirement. Due to these trends, organizational theory is disappearing from sociology graduate school curricula. Without any exposure to it in their graduate training, organizational sociology may seem uninteresting and irrelevant to future sociologists – “a weird alien species that occasionally shows up and reproduces in their territory.”

These structural changes have led to the concern that the content of organizational sociology itself is changing. The abstract for the American Sociological Association session on this issue stated: “Many sociologically-trained scholars have migrated to business schools and become absorbed by the large interdisciplinary field of organization studies,

which tends to have a managerial orientation. Little attention is directed to the broader impact of organizations on society.” One interpretation of this statement, whether intended or not, is that business schools corrupt organizational sociology. They force sociologists to translate their work to a management audience, and in the process, their work becomes more managerial. It is this corruption of ideas, not the structural distance between departments, that accounts for organizational sociology’s irrelevance to the rest of the discipline. Or so the argument goes.

But this is where you lose me. I don’t believe that the structural changes in organizational sociology have altered the content of the field. Being managerial, in my view, means that research is primarily done for the benefit of managers (e.g., to help them make better decisions or design better organizations). It’s certainly true that there is a lot of sociology that has managerial implications. That is, after all, why business schools started hiring sociologists in the first place – because sociological knowledge has useful, practical implications. In the MBA class I teach about power and influence in organizations, I draw extensively from sociology and social psychology, especially from research on gender, race, and inequality.

But just because sociology has practical implications doesn’t mean that organizational sociology has become more managerial as a result of moving into the business school world. The sociologists I know in business schools try to draw a very clear line between what they see as basic research and what they teach in the classroom. We’re purists too, and we believe in the integrity of social science. There is also a lot of work by sociologists in business schools that is simply trying to explain social phenomena and problems that happen to take place in organizational settings. For examples, see the work of Lauren Rivera on [cultural matching in elite labor markets](#) or research by Giacomo Negro and colleagues about how the expansion of lesbian/gay businesses made [communities more likely to pass ordinances banning discrimination](#). It’s hard to see how you could classify any of this work as managerial. In fact, this research is fundamentally concerned about the broader impact of organizations on society.

So why, then, are we worried about organizational sociology being taught to PhD students in business schools? The first reason is about identity. Sociology is a fragmented discipline. In the same sociology department, you'll have scholars who research marriage and family, labor unions and social movements, identity and emotions, and intergenerational mobility in China. The only thing sometimes that links all of these people is that they exist under one roof in a sociology department. Belonging to a sociology department becomes the identity marker of a sociologist. Once you have a subfield that is no longer housed primarily in sociology departments, we become concerned about whether it should really count as sociology. In that sense, organizational sociology is going through an identity crisis; although as I experience it, the crisis is occurring among those who don't belong to the subfield rather than among those of us who identify as such. Concerns about organizational sociology being too managerial are really post-hoc rationalizations for making boundary distinctions.

Another reason for concern is that it's not clear how organizational sociologists will reproduce themselves if they're no longer training PhD students in sociology programs. But this is only a problem if you think that having a PhD in sociology is a necessary requirement for contributing to the discipline. As sociologists we know that disciplines need closure, and certifying one's self as a member of the discipline by the type of PhD you hold helps to maintain that closure. But the reality is that there is a lot of amazing organizational sociology produced by scholars who have PhDs from management schools. Much of this work is published in journals like *ASR* and *AJS*, although it finds a place in specialty journals like *Administrative Science Quarterly* and *Organization Science* as well. Clearly, you don't have to have a PhD in sociology to be a major contributor to the field. If our primary goal is to produce sociological knowledge, then does it really matter if organizational sociology is taught in and practiced by scholars in business schools?

The structure of the field has changed and this has created an identity crisis among sociologists who study organizations. But this identity crisis isn't necessary as long as we all agree that our shared goals are the same – to produce sociological knowledge about organizations, their impact on society, and on the people who work in and around them.

The biggest challenge for the future of organizational sociology, as I see it, is to bridge the structural distance between the sociologists in business schools and those in sociology departments. If these two groups stop talking to each other, then a significant social chasm *will* develop that will eventually lead to a knowledge gap. I don't think that has happened yet, but if organizational sociologists stop conversing because they're not in the same departments or because their PhDs aren't the same, eventually they will stop contributing to a common body of knowledge. I think this is one reason Work in Progress and other forums like this are so important. We need places to seed those conversations and to create connections between sociologists who believe in contributing to sociological knowledge about organizations.

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## Organizational sociology that does not address climate change has little future

**Charles Perrow**

Does organizational sociology have a future?

The more important question is does mankind have a future in view of climate change. Sociology in general has been slow to deal with this problem, the major one facing mankind, and since organizations are responsible for most of the mounting emissions of greenhouse gases, organizational theorists should be leading the way. As I recall no one at the 2014 ASA panel on the future of organizational sociology mentioned climate change or the role that large polluting organizations play (even though Harland Prechel is doing great work on the topic).

Perhaps it is to be expected. Over the last 10 or so years papers at the annual American Sociological Association meetings that mention climate change (or global warming, as it used to be called before we got politically correct) were in rural sociology or the newly

emerging environmental sociology, and dealt the effects of warming on gender, race and poverty, and did not mention the big emitters. It was not until 2012 that we had a thematic session that dealt with organizations and warming. But we have a “society of organizations” and big polluters are among the biggest and the most powerful. Organizational sociology would have a great future if it turned from the themes of the panel and addressed the greatest threat to mankind.

The main reason organizational sociologists have not taken up this issue is structural. The top 10 sociology departments in the US set the agenda for the others. Their senior people matured one or two decades ago before climate change became a major issue. Their best students are trained in the standard topics of sociology, and some find jobs in another top 10 department, continuing the dominance of the traditional topics. Of course, new specialties arose even within organizational sociology such as quantitative methods used to study organizations, network analysis, and new variations on the cultural theme. But these were evolutionary for the field. To study the organizational aspects of climate change would be a sharp break.

That break was not likely to appear in the top tier of departments, secure in their dominance of acceptable topics. Instead it appeared in second and third tier departments and their many interdisciplinary ventures. Virtually all of the leading organizational sociologists concerned with climate change today are housed in second tier departments or “centers” and the like. They rarely gain admission to the top sociology journals, nor do the students they train gain admission to entry-level jobs in the top 10 departments. There is no decades-long tradition in this area of organizational sociology that can easily grow senior people in the top departments who can, in turn, shape the interests of the top students in those departments.

I am sure that all the senior people in the top departments pursuing their well-worn paths are aware of the problems of global warming and outraged that the US in particular is doing almost nothing about it. But I am not sure that they readily see big organizations as the major culprit. (In fact, most sociologists that are working on climate change do not

choose to study the organizational aspects.) This is a failure of organizational sociology to steadily research the impact of organizations upon society, especially as students of organizational sociology migrate to business schools. We do not drill deep enough into many social problems to disclose the organizational sources of perverse social energy. If we did, if we colonized other parts of our sociological terrain and dug deep to into them with an organizational perspective, we could have a brighter future.

I was propelled into the study of organizations and warming by reading an article by Constance Trevor-Tracy entitled "[Global Warming and Sociology.](#)" Added [evidence of sociological neglect](#) can even be found a year later in Nature. My own writings on organization and warming are mainly in two chapters: one at the request of Levor-Tracy, entitled "[Organizations and global warming,](#)" and second, with Simone Pulver, the forthcoming ASA Task Force publication (Oxford) (available on request).

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Organizational sociology has a past and, with luck, it can have a future

**W. Richard Scott**

I was happy that the Organizations, Occupations and Work Section sponsored a panel discussion at the American Sociological Association meeting this year (2014) on this topic—one that has long been of concern to me. I'm old enough to remember when organizational sociology was a major focus of our discipline and occupied a central place in the programs of leading sociology departments. In its modern guise, this field emerged slowly in the late 1950's, grew to prominence during 1970s, and was still strong well into the 1980s. We then witnessed the sad spectacle of the majority of graduates of these programs being snatched up by business schools, with others moving into various administrative or policy programs. These graduates continue to teach and study many aspects of organization, but their agenda has been curtailed by the context in which they

operate and is often inflected by the dominance of economics in these settings. In this brief essay, I propose to respond to and amplify the comments of the some of the panel members, in particular, Howard Aldrich and Lis Clemens.

I was surprised (shocked!) to hear [Howard opine that organizational sociology could have no future because it had no past!](#) For evidence, he pointed to the fact that most of the contemporary research on organizations published in the leading sociology and management journals deals with contemporary organizations, mostly located in the U.S. I think this observation is empirically biased because it excludes the large and rich array of studies published as books and monographs. (Sociology is fortunate enough to be a two-literature field: we still write and value books as well as articles!) But, whether or not the evidence is confirmatory, it is irrelevant to the issue of concern. A review of what contemporary sociologists are currently doing tells us nothing about the past history of our area of scholarship!

As most of us know, and as I have been emphasizing through six editions of [my textbook on organizations](#) (chaps. 2-5), organization sociology emerged in the latter stages of the social and intellectual upheaval that accompanied the industrial revolution and modernization movements of the late 19<sup>th</sup> and early 20<sup>th</sup> centuries. Marx, Weber, Durkheim, Michels, and Simmel, among others, all pointed to the increasing rationalization, technicization, and differentiation that characterized these changes, as more sectors of society witnessed the rapid increase in the number, size and influence of formalized, special-purpose collectivities. After this promising start, the impetus for development in the field shifted to engineers, such as F.W. Taylor, and industrial psychologists, such as Elton Mayo. Taylor emphasized the impact of technology and advocated the rationalization of work activities; Mayo emphasized the importance of social and political processes. As I have pointed out previously, it was on this rock—a “cleft” rock pairing the technical and the social, the rational and the natural—that the modern field of organization theory was formed. Our history matters because, through path-dependent processes, this tension has remained with us up to the present moment.

During the late 1950s and into the 1960s, sociologists reclaimed the field with a concerted effort stimulated by Merton and including the seminal work of Bendix, Blau, Gouldner, Lipset, and Selznick. Their efforts consolidated the renewed field by stressing middle-range theory and solid empirical research. A number of texts, particularly those by Blau and Scott, Etzioni, Hall, and Perrow, appeared in the early 1960s and provided conceptual frames and agendas for the reemerging field. On this strong foundation, later scholars erected a diverse and productive collection of theoretical paradigms: including contingency theory, resource dependence, population ecology, and institutional theory. There is a history, and it is complex, dynamic, and, in many ways cumulative. (Howard, as an evolutionary theorist, would have to acknowledge this truth!) Lis Clemens correctly pointed out the effects of developments in the broader social context on our work. In the 1940s and 1950s, organizations had become much more salient to a wide range of people—our nation state had grown explosively and its reach into every arena of social life was increasingly apparent; women had entered the work-force in large numbers for the first time; and many men who served in the armed forces experienced for the first time the muscular power of discipline and regimentation. Under these conditions, it is no wonder that scholars and their students would be attracted to a field that could shed light on their present circumstances.

And what of today's scholars and students? It seems obvious to me that organizations today are no less salient, although I believe our discipline has been slow to recognize, document, and interpret important new developments in the current era. The "industrial bureaucracies" are still here: in the older industries, in most parts of the government, and in our school systems. However, they have been joined by a new wave of industries, associated with new modes of organization. Indeed, as [Jerry Davis and I have emphasized \(chaps. 10-13\)](#), it is time to broaden our attention from the subject of relatively stable organizations to include the more flexible new modes of organizing: network forms, transitory teams, value chains. Many in our field are beginning to track these fundamental changes but, in my opinion, there remains insufficient attention to these innovative forms—both in our empirical work and in our theoretical development. We are in need of a new collection of middle-level theories to help us sort out and understand changes in the

ways in which new modes of organizing are developing. We are fortunate that the increasing penetration and cross-learning occurring [between social movement and organization/institutional theorists](#) in recent years provides a stimulating flow of new ideas and mechanisms to enliven and inform this work.

Let me conclude with one additional observation. I want to comment on deficits in the institutional context within which new organization scholars are trained. As an organizational scholar and an institutionalist, I take seriously the importance of the contexts within which we work and learn.

In what is arguably the most productive period of organization sociology or, more broadly, organization studies—the 1970s-80s—scholars involved in this work were embedded in interdisciplinary contexts. At that time there existed a sizable number of locations in the U.S. in which some sort of interdisciplinary center provided a node for scholars from psychology, economics, sociology, political science, as well as from business, educational, engineering, health care, and political administration schools, to come together and learn from one another. The case I know best and, I believe, one of strongest of these centers was located [at Stanford](#), but they also operated, during this period, at Carnegie Mellon, Illinois. Northwestern, Michigan, Pennsylvania, Minnesota, North Carolina, and Texas. Because these programs forced scholars studying public agencies to explain their ideas to others studying for-profit firms, as well as to others studying schools or engineering construction teams, for example, the discourse that emerged had to take place at the more abstract level of “organizations and organizing”—the ways in which activities are structured and the mechanisms by which they operate. Organization theory was the beneficiary of this “more elevated” level of discourse.

Today, sadly, such interaction goes on in fewer places—certainly in ICOS (Interdisciplinary Center for Organization Studies) at U. Michigan; also at COR (Center for Organization Research), UC Irving; at SCANCOR (Scandinavian Consortium on Organizations) at Stanford; increasingly at Harvard-MIT; within the school of business, U. of Alberta; and the IOA Department of Copenhagen School of Business—but not in most university settings. In

this age of Skype, tele-conferencing, and other forms of virtual/actual communities, can't we find ways to reinvent these productive forums?

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## Does organizational sociology have a future? Leadership matters in shaping the turn of events

**Patricia H. Thornton**

A response to the question of what is the future of organizational sociology first depends on understanding how the institutional and organization environment has changed.

In commenting on changes I make some assumptions, i.e., [an open systems perspective](#) and [competition for attention](#) occurs in [an ecology of institutional space](#).

Below is a partial natural history of observations, not necessarily in an event sequencing order or from systematic research. I don't know much about blogging and assume that the purpose is to be provocative to raise questions that generate discussion.

1. In the mid-1990s the name of the "Organizations Section" ASA was changed to "Organizations, Occupations, and Work." Think about this from the perspective that occupations and organizations are alternative (competing) means to organize. The question I have is did this change shift scholarly interests to focus on what I call variable studies (i.e., [gender is a variable](#)) and categorical theories of personal identity related to stratification and distributive justice? Note the recent job search announcement on the OOW list serve for a position in migration and inequality.
2. In the mid-1990s the "Economic Sociology Section" of ASA was born. This created a home for international and network scholars, but do these scholars self-identify as organizations scholars? As Howard Aldrich reflected on articles published in *Administrative Science Quarterly*, organizational sociology is predominately American, not international.

3. From the mid-1990s the development of entrepreneurship as a field of study has skyrocketed. Innovation and entrepreneurship are golden words on university campuses as witnessed by the rise of business plan challenges and entrepreneurship concentrations and certificate programs. I'm not talking about business schools only, but colleges of liberal arts and arts and sciences—where sociology departments are located. However, organizational sociology speaks more to established organizations than to starting profit-making organizations—not fully embracing the largess and push of the entrepreneurship movement. This is a missed opportunity as entrepreneurship directly addresses issues of interest such as poverty, wealth, and income distribution in society. Check out for example the recent Forbes special edition that defines the network of entrepreneurs that have grown corporations and the billions they are giving back to society, e.g., Gates, Buffett, Zuckerberg and many others.
4. The organization and management theory division of the Academy of Management has grown significantly. I agree with Howard Aldrich in that you are more likely to site organizational sociology alive and well at the Academy than at the American Sociological Association. Has anyone thought that maybe many of the organizational sociologists are in business schools because the study of organizations is more appreciated there?
5. The rapid and prolific development of network analysis as a field of study has reshaped many areas of study. Network studies are one of the largest categories of submissions to the Organization and Management Theory Division of the Academy of Management. Note networks are often not referred to as organizations and networks are assumed to have advantages over hierarchies (organizations) – see the seminal contributions by [Granovetter](#), [Powell](#), and [Saxenian](#).
6. The [increasing salience of individual identity and the retreat of corporate sources of identity](#) can be interpreted that it is becoming less of a world of a certain kind of organizations.
7. There has been come-back and growing methods development and legitimacy of qualitative research methods and published qualitative research. I benchmark it to Kathleen Eisenhardt on [building theories from case studies](#) and the influence of

Steve Barley's seminal work. While a necessary correction to the era of mindless regressions, [qualitative research has been critiqued](#) for not systematically accumulating and generalizing. Qualitative research is typically not focused on testing theory; yet note that theory testing was a driving force in the development of organizational sociology and organization theory. Take for example the five strands identified [by Scott](#): [institutional](#), [resource dependence](#), [population ecology](#), [transaction cost](#) and [contingency](#). Note that these five strands placed organizational sociology in the position of embracing and reaching out to interdisciplinary scholarship. Knowledge of how theories and research areas grow suggests that separating organizational sociology from organization theory will stunt growth of organizational sociology.

8. Harvard economist Michael Porter's [five forces model](#) spearheaded the development of strategy departments in business schools as often distinct from the organizations and management areas. Both the rise of teaching strategy and as previously noted entrepreneurship in business schools caused concern over declining teaching of org theory to MBA students, i.e., org theory lacked applications and MBAs want to solve problems. Jerry Davis led a teaching round tables effort through OMT to engage organizations experts to demonstrate how to teach an org theory version of strategy and entrepreneurship. Is there a counterpart in Sociology departments to keep organizational sociology alive and well?
9. Some cultural sociologists used to study organizations, but then the French theorists caught on in American sociology. The question is where are the organizations in their views? Does anyone recall the standing room only Academy of Management symposium on what the French conventionalists can teach organization theorists.
10. Institutional theory is developing to focus on explaining agency and institutional heterogeneity by no longer siting phenomena through structuralism and the lenses of the [organization and organizational-field levels of analysis](#), but instead through [the lens of societal level institutions](#). Yet, institutional theory has always been considered under the umbrella of organizational sociology.

11. The world is in a time of transformation and organizational sociology may be behind in explaining the transformation. Products are now created without organizations and entrepreneurship is funded by the crowd.

In sum the consequences of these observations are the development of other perspectives that compete for scholars' attention. Please do not misinterpret my comments. I am not saying any of these observations are necessarily systemically true, good, or bad.

There is one last argument, but important argument. A sequence of events or changes does not by itself determine the health of a field of study. The formation of fields requires leadership. Dick Scott with his multiple edition best-seller text, visiting scholar exchanges, and conference venues was a leader in collecting organizational sociology as an interdisciplinary field. One bright spot is Michael Lounsbury's editorship of *Research in the Sociology of Organizations*. There are others coming forth as [Ezra Zuckerman cogently points out](#). But, single publications alone will not create this—it takes the gift of active leadership.

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## Why before how: “Distinctive and indispensable” beats “sophisticated but superfluous”

### **Mark Suchman**

Organizational sociology, I would argue, has become increasingly sophisticated over the years; but it has done so in ways that make it less interesting to non-organizational sociologists and, hence, less able to survive outside the hothouse microclimate of a self-styled organizational studies program. From my particular vantage point in a [sociology department on a campus without a business school](#), the problem is this: Most of my students – graduate as well as undergraduate – arrive in sociology with interests that they do not see as organizational: Globalization and development, health and medicine, environment ([yes indeed Chick!](#)), social movements, inequality, urban structure, science and technology, occasionally law. But only rarely “organizations.” I (like most readers of

this blog, I suspect) see all these topics as profoundly and thoroughly organizational. But how do I make that case to the next generation of sociologists?

Of course, I can share my excitement for the ins and outs of organizational ecology and institutional theory, resource dependence and network embeddedness, even institutional economics and competitive strategy. But these conversations all too often end on an awkward note: “Wow,” says the student, “You really seem to like this stuff. So what does org theory tell us about [insert a “non-organizational” topic from the list above]?” I pause and gather my thoughts: “It tells us that organizational practices are institutionally constructed and constrained; that outcomes are shaped by cultural and political forces; that interorganizational exchanges are embedded in social networks; that organizational boundaries are porous; that workplaces are rife with interpersonal dynamics and informal structures and biases; that...” “Wait,” says the student, sounding as though I’ve just plucked away the football that he/she was about to kick, “so why should I study organizations, then, instead of studying institutions or culture or politics or networks or small-group processes?”

“Why should I study organizations?” is not, I suspect, a question that gets debated much in business schools or in interdisciplinary org-studies training programs. Those venues exist solely for that purpose, and by the time people arrive, they have long since resolved the “why organizations?” question to their own and their colleagues’ satisfaction. But in sociology departments, this question deeply matters, and if we can’t answer it, sociology and organizational studies will inevitably drift apart, to both sides’ detriment.

To stay healthy and relevant, I’d posit, a subfield needs to meet three challenges: First, it needs to build an ever more *sophisticated* understanding of its chosen subject matter (e.g., organizations). Second, it needs to capture *distinctive* aspects of its subject matter that are not simply reducible to the subjects of other adjacent subfields. And third, it needs to identify ways in which understanding its chosen subject matter is *indispensable* for fully understanding the adjacent subfields themselves. The first challenge is about internal elaboration; the latter two are about boundary management.

Transplanted into a microclimate where it is a field unto itself, organizational studies risks fixating on the intra-field challenge of sophistication, and losing sight of the equally crucial inter-field challenges of distinctiveness and indispensability. Many of the developments that we most cherish in organizational sociology's century-long march toward sophistication – the discovery of informal organization, the opening to the environment, the acknowledgement of conflict, the turn toward culture – would seem to imply that organizations are not as distinctive as one might naively expect, and that the study of organizations is therefore not as indispensable for understanding teams, industries, politics and beliefs as we, in our more grandiose moments, might claim.

Within a b-school, this may seem like a fair trade: An emphasis on the continuity between organizations and the rest of social life allows us to assert the importance of sociology and the myopia of managerialism. But within sociology itself, we are left with little to offer except the assertion that organizations simply *must* be of interest, because we and our b-school friends find them interesting. The problem is not that the shift toward business schools has made organizational sociology more managerial (I see relatively little evidence of this), but that it has made organizational sociology more self-absorbed.

Administrative science can't save us from this narcissistic tar pit: We cannot escape our self-absorption simply by going back to the days of seeing organizations as closed, rational systems – as things to be managed, not things to be reckoned with. This might “solve” the challenge of distinctiveness, but only by sacrificing our hard-won sophistication and our potential indispensability.

“Society of Organizations” rhetoric fares little better: We cannot merely assert that organizations are powerful and prevalent – crucial “independent variables” in society – and leave it at that. Crassly done, this, too, reflects a closed-system mindset, an unsophisticated assumption that organizations are indispensable simply because they are the “cause without cause” in the modern order. The choir may believe this, but it won't convince the rest of the populace, who worship at the altar of other forces – class, gender, globalization, culture, politics – with at least as much claim to causal priority as organizations.

Even our beloved open-system theories come up short: Open-systems thinking is a useful corrective, reminding us that organizations are socially embedded, and that the organizational processes that may appear as independent variables in any given social dynamic are themselves the dependent variables of other social dynamics further upstream. Unfortunately, if organizations are nothing more than conduits for external forces, then their independent causal significance is likely to be minor: Understand the external forces, and you will understand the external outcome, no org-soc prelim exam needed!

Rather, to balance sophistication, distinctiveness and relevance, we need to cultivate a stronger sense of organizations as neither independent nor dependent variables, but as [mediating and moderating](#) variables. As mediators, organizations matter when they respond not only to exogenous, non-organizational forces, but also to distinctive dynamics and rigidities within the organizational system itself. And as moderators, organizations matter when exogenous, non-organizational forces themselves operate distinctively within an organizational system versus outside it. Mediators and moderators are sociologically interesting precisely because they have independent – distinctive and indispensable – causal significance. They are embedded within the process under study, without being fully reducible to prior features of that process.

Over the years, organizational sociology has offered several such accounts of organizational mediation and moderation: [Merton](#) argued that bureaucratic work changes workers' personalities; the [Aston Group](#) argued that size, formalization, centralization, administrative load and the like channel one another into a limited number of viable configurations; [strategic contingencies theory](#) argued that organizations exalt uncertainty-reduction as a basis of power, in ways that other social forms may not; [organizational ecology](#) argued that industrial change often reflects endogenous organizational population-dynamics rather than exogenous variations in environmental carrying capacity; [institutional theory](#) argued that isomorphism does not arise primarily from the (exogenous) demands of efficiency, but from structuration dynamics within fields of mutually-aware and mutually-constitutive organizational players.

None of these theories have gone away. But in our rush to ever-more-macro levels of analysis, perhaps we have become a bit distracted from the distinctively organizational mechanisms that made such accounts seem sophisticated and indispensable to our non-organizational colleagues in the first place.

And, as [Jerry's](#) and [Dick's](#) posts suggest, perhaps we have also become a bit distracted from the crucial task of updating our toolkit of “distinctively organizational mechanisms” to address the new generation of organizational structures and practices now emerging on the scene. Work still shapes personality, even if the nature of the work has changed; size, formalization, centralization, etc., still interrelate, even if information technology has changed the nature of the interrelation. But at the same time, many crucial “organizational” processes may have broken out of the bureaucratic shell, moving to field-level decision-making bodies, network-level interaction-circuits, or profession-level cultures and procedures. Industry standard-setting may be taking the place of organizational rulemaking; referral networks may be taking the place of job ladders; branding may be taking the place of culture-building; and entrepreneurship may be taking the place of employment. This is hardly a harbinger of the death of organizational sociology, however. It is simply a new set of problematics, challenging us to articulate new models of mediation and moderation that can reestablish organizational sociology's claims to distinctiveness and indispensability.

As long as important social dynamics play out differently within and around organizational systems than outside organizational systems, organizational scholars will have a place at the sociological table. The only question is whether we will make the intellectual effort to fight for that place, or whether instead we will retreat into the easy comfort of our own company, where we can engage in the undistracted pursuit of sophistication, treating distinctiveness as irrelevant and indispensability as self-evident. In a b-school, that latter course may have a certain appeal; but in a sociology department, it may not have much of a future.

## The end of “organizational sociology” as we know it?

**Elizabeth Gorman**

I organized the panel on “The Future of Organizational Sociology” at the 2014 American Sociological Association annual meeting, which inspired [the present virtual panel](#). The motivation for the original session arose when, in quick succession, I had to update the syllabus for my graduate course on organizations and design a comprehensive examination reading list in the field. Both tasks force the instructor to take stock of recent developments in a field and try to make sense of them for students. Contemplating the work published over roughly the last two decades, I found myself puzzled about what to include. On the one hand, there were active research conversations that seemed to be taking place almost entirely among management faculty and in management journals—and thus arguably outside the disciplinary boundaries of sociology—such as the one on “[institutional logics](#).” On the other hand, there was no shortage of sociological research involving organizations in some way, but most of it seemed better classified under (and was often clearly intended to speak to) another subfield of the discipline such as sociology of work, economic sociology, or social movements.

Work that could be uniquely identified as “organizational sociology” seemed to have largely disappeared.

What happened? Historically, organizational sociology operated at a relatively high level of abstraction. The goal was to understand and explain the structures and practices of complex organizations of all kinds, across multiple spheres of social life—not only business organizations, but also government agencies, schools, hospitals, nonprofits, even voluntary organizations. To be sure, in practice the empirical focus was on businesses and, to a lesser extent, public agencies. Still, there was an underlying assumption that it was possible and

worthwhile to identify general concepts, principles, and processes that applied to all types of organizations. As [Dick Scott has pointed out](#), there were always dual intellectual and practical aims, but they dovetailed in supporting the study of “what is” and “what works” across organizations in general. Today, it seems there are few sociologists (and even fewer graduate students) who are interested in developing or extending abstract concepts and theories about why organizations in general exhibit certain structures and practices, or which ones work best from the organization’s point of view. The broad pattern is the same in both sociology departments and business schools, although the institutional details differ.

In sociology departments, work appears to be moving either toward greater specialization, or toward higher or lower levels of analysis. The discipline is becoming increasingly specialized by sphere of social life, such as education, health, work, politics, religion, urban poverty, and so on, rather than by overarching processes or theoretical frameworks. Researchers in each sphere tend to be interested in organizations that are relevant to that sphere—such as schools *or* hospitals *or* urban community organizations—not in organizations in general. (Unfortunately, the study of *business* organizations has been all but entirely ceded to management scholars based in business schools.) For such researchers—as several of the original panelists pointed out—the abstract frameworks of traditional organizational sociology serve as *tools* to aid in understanding their more specific instances. They utilize organizational tools together with other abstract theoretical ideas drawn from various subfields of sociology.

Interest has also moved down to the individual level or up to the level of markets and entire economies. At the individual level, thriving streams of research examine workplace inequality, the continuing contest between employers and workers over the nature of work, the growing phenomena of professional work, interactive service work, and care work, and the problem of combining work and family. At the level of markets and economies, interest has focused on the cultural, political, and historical influences on market development and evolution. These two bodies of work are usually classified under

“sociology of work” and “economic sociology,” respectively. Neither represents a continuation of traditional organizational sociology.

The disappearance of organizational sociology from sociology departments was brought home to me when I set out to organize the original ASA panel, because I wanted the majority of panelists to come from sociology departments. The task of finding them proved to be quite challenging. Indeed, although I was ultimately able to assemble a distinguished panel, several panelists were careful to say that they did not primarily define themselves as organizational sociologists.

In business schools, [as Jerry Davis points out](#) in his own contribution to this virtual panel, sociologists constitute a rather small component of the larger interdisciplinary field of management or organization studies (a community primarily affiliated with the Academy of Management, [as Howard Aldrich notes](#) in his blog post.) Business-school based sociologists, too, have moved away from the traditional topics and themes of organizational sociology. Because they remain largely focused on business organizations, they have avoided horizontal specialization, but their work shows the same vertical pattern of movement up or down in level of analysis. An example of upward movement is [Heather Haveman's work](#) on the historical and political role of magazines in America (references in her own post to this panel). Business-school-based sociologists have also moved down to the individual level to examine aspects of workplace inequality and the experience of work (see work referenced by [Ezra Zuckerman](#) and [Brayden King](#) in their blog panel posts). It is interesting that business-school-based scholars point to these streams of research as evidence that organizational sociology is thriving. I would argue that most of it is not, in fact, “organizational” sociology. Viewed from the standpoint of a business school, I suppose, virtually any sociological research appears to be “organizational” sociology, because business schools are, by definition, concerned with (business) organizations. If the authors were employed in sociology departments, however, their research questions would seem to fall under sociology of work or economic sociology. They would also need to engage more closely with those literatures, whereas currently their work is evolving largely in isolation from them.

Thus, my sense is that organizational sociology as we have known it is fading away. This is not necessarily a bad thing. It may mean that sociology departments stop offering graduate courses and comprehensive examinations in “organizations,” but it doesn’t mean that sociologists will cease to study organizations. Specific types of organizations will be studied in different subfields of the discipline, such as sociology of education and medical sociology. Individual-level workplace inequality and experience will be considered within the sociology of work and occupations, and organizations as actors within markets and economies will be studied under headings such as economic sociology or global and transnational sociology.

I am not sure where I would pigeonhole it, but I hope that sociologists will also reclaim the subject matter of business organizations—but with a new orientation and a new set of questions. Responding to recent calls to do more “[public sociology](#),” sociologists could explore the consequences of business organizations’ structures and actions for societal outcomes that are important to all of us, such as inequality, poverty, health and safety, politics, and the environment (including climate change, as Charles Perrow argues in his post to this panel). Over the years, a smattering of work has paid attention to the broader impacts of business organizations on society: Perrow’s warnings about “[normal accidents](#)” and the “[society of organizations](#),” Baker and Faulkner’s work on [organizational conspiracies](#), Prechel’s research on [corporate financial malfeasance](#), McKenzie’s article showing that [organizational processes contributed to the production and sale of toxic mortgage-backed securities](#) prior to the recent recession, Davis’ work on the effects of corporate forms [on employment](#) and [income inequality](#). Much more could be done along these and similar lines, and this is the kind of research that sociologists excel at doing.

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No political economy in the future of organizational sociology?

**Matt Vidal**

In this closing essay of a virtual panel on [The Future of Organizational Sociology](#) I want to suggest a direction that was only briefly hinted at in two of the preceding 13 essays: More engagement with political economy. Harland Prechel argued for a need to focus on [how political-legal institutions shape managerial behaviour](#) and Jerry Davis discussed [increasingly precarious employment for the working class](#). The broader subfield is also largely silent on issues of political economy, with a very few notable exceptions including [Neil Fligstein](#) and [Jerry](#) on financialization, [Mark Mizruchi](#) on the corporate elite and [Harland](#) on big business and the state.

In my view there is much to be gained from engaging traditional organizational theory with political economy focused on structures and dynamics of profit seeking, capital accumulation and class relations. A turn to political economy can help to grasp the deeper structures and historical dynamics underlying the mid-range phenomena that are typically the focus of organizational theory.

On the organizations side, my focus here is on what is most commonly known as neo-institutionalism, but is more accurately termed sociological institutionalism (which differs fundamentally from neo-institutionalism in economics). In the two foundational articles of the subfield, John Meyer and Brian Rowan argued that [legitimacy can be more important than efficiency for organizations in particular environments](#), while Paul DiMaggio and Woody Powell emphasized how [institutional pressures generate similarity in the organizational population](#).

As Woody Powell noted in his 1991 edited volume on *The New Institutionalism in Organizational Analysis* (chapter 8), up until that time [the subfield had focused almost exclusively on the public and nonprofit sectors, ceding “too much terrain to market processes.”](#) In the 1990s there was increasing emphasis on the operation of institutional pressures for legitimacy in the for-profit sector.

In the same volume, Roger Friedland and Robert Alford (chapter 10) introduced [the concept of competing institutional logics](#). This approach took off in the 2000s, with logics

being defined as cultural beliefs, schemas and templates that structure cognition and practice. Theory was now focusing on how market logics interact and compete with other logics, such as professional, bureaucratic and family logics.

Institutional logics theory made a substantial theoretical refinement on the legitimacy *versus* efficiency frame. As Mike Lounsbury argued, [logics theory examines how technical concerns with organizational efficiency become embedded in broader institutional beliefs](#). This is analytically powerful and logics theory has exploded, but it has signally failed to consider issues of class analysis or the dynamics of profit seeking and capital accumulation. Logics theory thus has much to say about markets but little to say about structural dynamics of capitalism. Below I suggest some useful points of engagement.

Political economy is a broad tent covering a diverse range of approaches focusing on the political construction of economic institutions, including economic sociology. Yet, as I have argued in a chapter with Jamie Peck, although economic sociology has begun to expand from a core focus on networks and embeddedness to embrace comparative political economy and a Polanyian emphasis on the political construction of markets, [these approaches too have been resistant to engage forms of political economy emphasizing structures and dynamics of profit seeking, capital accumulation and class relations](#). Mark Mizruchi has made a similar argument with reference to [political economy and network analysis](#).

Of course, these issues are given the most sustained theoretical and empirical analysis within the Marxist tradition, which is, indeed, extremely vibrant: there are currently 19 Marxist academic journals. I have a forthcoming article with Paul Adler and Rick Delbridge in *Organization Studies* that proposes a number of points of potentially fruitful engagement between Marxist political economy and mainstream organization studies. Let me briefly highlight a few examples of such engagement with reference to the 2007-8 global crisis.

Jo-Ellen Pozner and colleagues argue in *Markets on Trial* (chapter 6) that within the mortgage industry [there was a shift from a Main Street logic in which mortgages were seen](#)

as [“consumer loans geared at supporting the institution of homeownership,”](#) to a [Wall Street logic](#) in which mortgages were seen as investment instruments. But they leave open the question of why this logic was embraced by the industry in the late 1980s. From a longer-term political economy perspective, as political economist Eric Helleiner has shown, [following the Great Depression laissez-faire ideology and the banking fraction of the capitalist class became discredited](#), opening up space for a coalition of industrialists, labor leaders and state officials to establish the hegemony of Keynesianism, including strict capital controls that existed through the early 1970s.

Although bankers and others continued to advocate financial liberalization, it was only after a decade of stagnation in the 1970s that it began to achieve widespread acceptance. Neoliberalism took many institutional forms, including logics such as shareholder value, the efficient market hypothesis and the Wall St approach to mortgages. Following capital’s assault on labor in the 1970s and ‘80s to recover profits out wages, there was a dramatic rise in debt-based consumption by households to maintain living standards as real wages stagnated, thus fueling the housing boom. Indeed, Fed Chair Alan Greenspan explicitly saw the equity and housing markets as means to maintain flagging demand in the economy, resulting in what political economist Robert Brenner has called [asset-price Keynesianism](#).

Finally, Mauro Guillén and Sandra Suárez analyze the crisis (*Markets in Trial* chapter 8), in particular the asset securitization market, using Chick Perrow’s theory of [normal accidents](#). The first condition for normal accidents is increased complexity, which Guillén and Suárez argue occurred in the 1990s with [the rise of giant, diversified, decentralized, globalized financial institutions](#), thus increasing both opacity and interdependence in the economy. The second condition is tight coupling (or lack of buffers), which occurred (among other ways) through the growth of lean global supply chains, thus increasing fragility in the economy. This analysis is deeply complementary with Karl Marx’s analysis of [the tendency toward the concentration and centralization of capital](#).

For Marx, these processes were an expression of the contradiction between the forces and relations of production: the productive forces tend to increase the organizational

capabilities of society while the production relations often have destructive effects due to class struggle. *On the one hand*, the centralization of capital (vertical integration, mergers and acquisitions) led to dramatic increases in organizational efficiency in the early 20<sup>th</sup> century, as documented by Alfred Chandler in [the rise of the multidivisional corporation](#).

(Note that decentralized production networks are consistent with Marx's argument regarding increasing organizational capability: they are coordinated ways of handling information and resource flows. But the tendency toward centralization cannot be easily discounted even in the age of the networked firm: *The New York Times* announced on Tuesday that [we are currently experiencing "one of the biggest booms ever in mergers and acquisitions](#) ... on pace for a year of deal-making rivaling those during the dot-com bubble and the private equity upsurge just before the financial crisis.")

*On the other hand*, the search for endless growth and profits by capitalists has generated many negative outcomes. Marxist research has robustly demonstrated that the US corporate profit rate began a steady decline from 27% in 1951 to a low of just 9% in 1982. In a recent paper I have shown that [profit rates were recovered over the 1980s and '90s by reducing labor's share of the national income](#), leading to stagnation and asset-price Keynesianism. As Marxist sociologists John Bellamy Foster and Fred Magdoff have shown, [stagnation in the real economy took a form predicted by Marx over 100 years ago](#): a combination of overcapacity across the economy combined with a surplus of investment capital looking for outlets, driven by a tendency toward overproduction and the concentration of capital.

In the case of the latest crisis, centralization in the financial sector, along with the concentration of massive amounts of financial capital in the hands of a small number individuals and organizations, facilitated a number of negative outcomes: giant banks had extraordinary influence on policy (hence deregulation) and were able to restructure the entire industry according to the Wall St logic, leading to predatory lending, including the widespread use of no-income, no-asset (NINA) loans for subprime borrowers (isomorphism and herding behavior are central to the analysis of Pozner and colleagues).

Gorman *et al.*

Organizational sociology and Marxist political economy are highly complementary. More engagement between the two promises an even more vibrant future for both.