

Celebrating organization theory: The after-party

Gerald F. Davis
Ross School of Business
The University of Michigan
701 Tappan St., R6362
Ann Arbor, MI 48109-1234 USA
gfdavis@umich.edu
734 647-4737

Forthcoming, *Journal of Management Studies*.

I would like to thank Chris Marquis, Davide Ravasi, and Huggy Rao for comments on previous versions of this paper.

Abstract

Organization and management theory as a field faces criticisms from several scholars that it has an unhealthy obsession with ‘theory,’ while at the same time seeing very little cumulative theoretical progress. Some have even accused the field of being mired in the 1970s. Lounsbury and Beckman (2014) counter with an expansive review of several thriving domains of contemporary organizational research that demonstrate the theoretical vibrancy of the field. This article responds by seeking to define ‘theoretical progress’ in ways that extend beyond just the volume of articles produced. It finds that 1970s-era classics have seen a surge of citations since the turn of the 21st century, consistent with a view of limited progress. It concludes by outlining three areas of problem-driven research eminently worthy of attention from organizational researchers.

Keywords: big data; organization theory; philosophy of science; quasi-experiments science; theoretical progress

Organization and management theory has been a vibrant interdisciplinary endeavor for six decades, as societies have come to be increasingly enveloped by organizational processes. Organization theory promised to be the master key to unlock contemporary societies, and the study of organizations drew in scholars from psychology, sociology, political science, and professional disciplines such as management and strategy. In spite of this promise, some scholars, including me, have expressed skepticism about the direction that organization theory has taken, worrying that the field has not made the progress that it should.

In their article 'Celebrating organization theory' elsewhere in this issue, Mike Lounsbury and Christine Beckman take issue with this bleak assessment and make a vigorous case for the richness and diversity of recent work in the field. It is exciting to see a review of so many new developments in organization theory. I have written a dozen review chapters over the years, and I salute the craft that goes into these endeavors. Their argument is largely persuasive: after reading their essay, it is hard to doubt that outstanding scholarly work is being done in the world of organization and management theory.

In this article I summarize what Davis (2010) was trying to convey about theoretical progress in organization theory, distill Lounsbury and Beckman's implied alternative standards, and suggest a critique. If theoretical progress is assessed by the volume of articles published in a given year, then we are facing a tsunami of progress. On the other hand, if progress is judged by answering important questions about the world, then some skepticism is warranted. I conclude with three problem-driven research domains that could use more attention from organization theorists.

Is theory stagnating?

The article 'Do theories of organizations progress?' (Davis, 2010) was commissioned as part of an effort to assess whether theories in management (micro, macro, strategy, and human resource management) were making progress. A touchstone was Paul Meehl's classic 1978 article about the lack of theoretical

progress in psychology. Meehl (1978: 807) wrote that ‘in soft psychology theories rise and decline, come and go, more as a function of baffled boredom than anything else; and the enterprise shows a disturbing absence of the cumulative character that is so impressive in disciplines like astronomy, molecular biology, and genetics.’ My task was to determine whether this was true of organization theory.

In between the ‘cynical quips’ and ‘scanty evidence’ in my paper was an argument about cumulative theoretical development. I stated ‘The revolution in information and communication technologies over the past two decades has made comprehensive time-series data on millions of organizations widely available to scholars around the world. Yet it is not obvious that organization theory has become more precise, more general, or more accurate as a result’ (Davis, 2010: 690). Theory in astronomy improved after the development of the telescope (e.g., heliocentrism replaced geocentrism), and theory in biology improved after the microscope. As a field, organization theorists now have easy access to extensive information about public corporations, non-profits, hospitals, and government agencies that was costly and time-consuming to assemble previously. Scholars at many business schools can easily connect to the WRDS (Wharton Research Data Service) database and quickly download decades-long series on basic financial and performance data, social responsibility, corporate governance, and more. Orbis, the United National Development Program, and the World Bank provide extensive cross-national time series data on corporations and their institutional environments. Moreover, much of what organizations do internally is tracked through emails and other ‘passive’ records that could yield dynamic information about networks and workflows within companies. With all this information now readily available, we should be living in a golden age of new discovery and new theory enabled by more and better information. Yet we were still ritualistically citing a 30-year-old paper which claimed that organizations within fields all come to look the same over time (DiMaggio and Powell, 1983), without having any clear sense of what ‘field’ or ‘the same’ meant.

My point was not that theory in itself is bad or useless. Lounsbury and Beckman are surely correct that piling up a bunch of facts and findings without an organizing device (e.g., a theory) leads to incoherence. But what was puzzling was the lack of obvious theoretical progress. Our statistical methods had certainly gotten fancier, as a skim of any issue of ASQ will confirm, but I saw little sign that theory was cumulating. Had we used all this new data and statistical expertise to settle core questions of resource dependence and ecology, or resolve disputes between transaction costs and agency theory? Not obviously. If anything, it was becoming clear that basic concepts like organizational 'size,' 'age,' and 'performance' had limited construct validity. (Inter-correlations among sales, assets, employment, and market capitalization – typically taken as interchangeable measures of 'size' -- have declined substantially over the years, suggesting that 'size' is not a singular construct.) 'Power' and 'legitimacy' were even worse off. If we can't agree on common metrics for basic constructs, it is hard to see how our knowledge of organizations can cumulate.

I suggested several underlying reasons for our lack of progress. We lacked experimental control but often failed to recognize the inferential hazards this creates. (There are well-articulated standards for quasi-experiments and well-known hazards of ignoring them, but these receive little attention from organizational scholars.) Organizations are human-designed tools rather than objects occurring in nature, so there was little reason to expect law-like statements to hold up across situations, any more than a 'theory of diesel trucks' was likely to produce enduring scientific truths. And Hume's problem of induction (the observation that there is no logical reason to expect that regularities observed in the past will necessarily hold in the future) meant that statistical relationships discovered in one era were prone to disappearing in the next era. I closed with some alternative standards for judging progress. One clear sign of progress is that important questions get answered, which is the aim of problem-driven research (cf. Davis and Marquis, 2005).

In defense of organization theory

Lounsbury and Beckman responded that, contrary to my bleak account, organization and management theory is a vibrant, diverse, and growing community of scholars that is rich in new ideas and new theoretical directions. As a former representative-at-large and chair of the OMT division, I am delighted to see my friends, colleagues, and collaborators held out as exemplars. And as the co-author of a surprisingly expensive textbook on the topic, I am happy that my field is not a drought-stricken cornfield but a tropical rainforest. I have a stake in seeing progress here.

Lounsbury and Beckman describe five new domains of theory development that are not bounded by the six paradigms I originally highlighted (contingency theory, transaction cost economics, agency theory, resource dependence, population ecology, and new institutional theory). Where New Institutional Theory saw organizations coming to look more alike over time, the institutional logics perspective provides ‘a much richer and fluid theoretical apparatus that focalizes cultural heterogeneity and practice variation.’ People and organizations often face conflicting pressures, and therefore don’t always end up looking the same. Categorization research finds that belonging to more than one category can lead to devaluation. For example, a combination Pizza Hut and Taco Bell can confuse prospective consumers (cf. Das Racist, 2010), and Wall Streeters discovered in the early 1980s that diversified firms faced a conglomerate discount (LeBaron and Speidell, 1987). Network theory argues that who you know matters, who you’re seen with matters, and knowing people who don’t know each other can yield varied benefits (Granovetter, 1985; Podolny, 1993; Burt, 1992). Performance feedback theory drills down on central questions about aspiration levels and performance initially described in the behavioral theory of the firm (Cyert & March, 1963). Finally, practice theories bring the agency/structure dialectic to bear on how strategy is done. In each case, Lounsbury and Beckman review a large number of studies contributing to each of these domains and argue that these studies belie a diagnosis of stagnation.

What are the implicit standards of theoretical progress being applied here? Early on, they state that 'Theory development happens when new directions are explored and unanswered questions are illuminated.' Notably, unanswered questions are 'illuminated' rather than 'answered.' The value is in the newness of the questions and in the volume of conversation, not in discovering true things about the world: 'new areas of inquiry....vibrant and developing theoretical conversation....growing corpus of publications....vivid example of a new theoretical conversation....rich theoretical conversations....significant theoretical conversations....new and exciting theoretical conversations....'

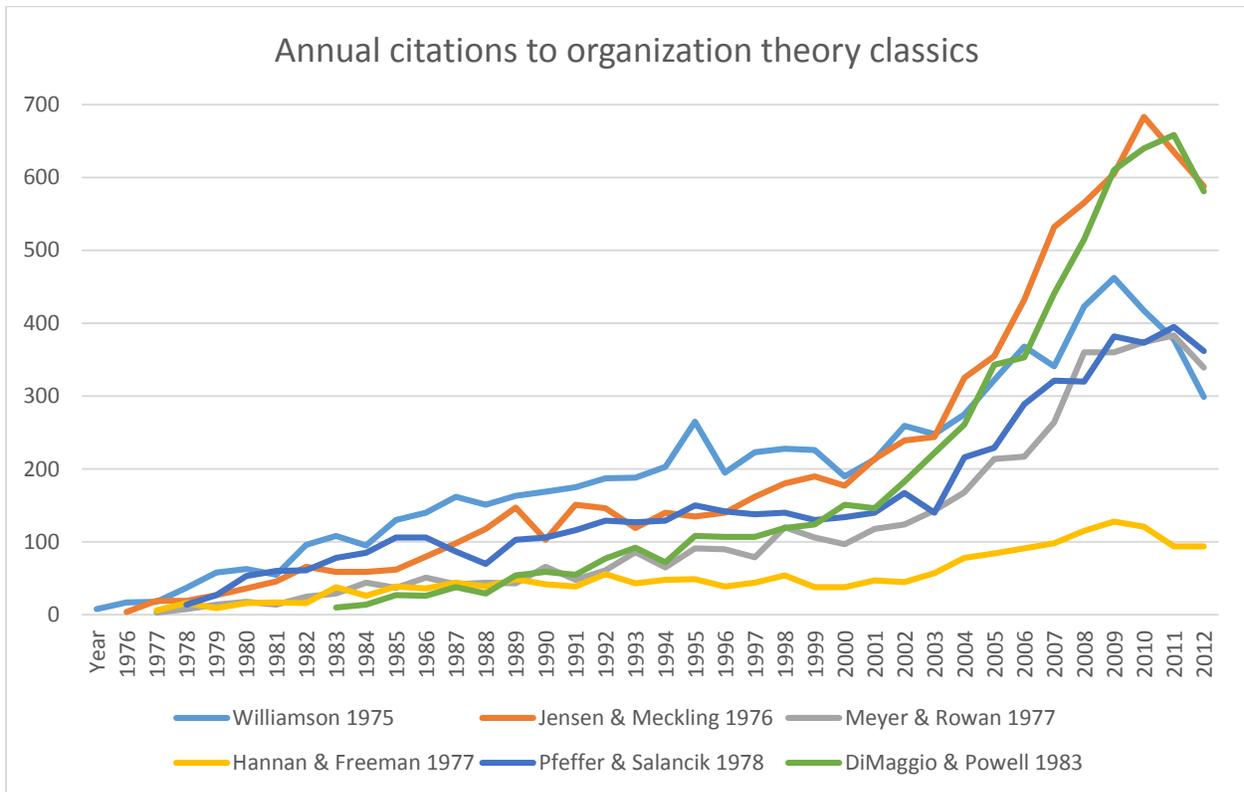
Are we genuinely talking about new and unexpected theoretical insights that help explain anomalous findings – a form of theoretical progress -- or are we simply re-labeling things to fit a new theoretical fad? When push comes to shove, are the arguments around 'logics' all that different from those around culture or identity?

More pertinently, is the existence of conversations about new topics a good measure of theoretical progress? For a true believer in performativity, perhaps yes. But consider an analogy. Many years ago, people who wore baseball caps pointed the brim forward. (Functionalists might claim that this was to keep the sun out of their eyes, but functionalism is now out of fashion.) At some point, a few bold innovators began wearing caps with the brim on the back, and within a few years many or most cap-wearers had adopted this new paradigm. Does this count as progress, or merely a shift in fashion?

Lounsbury and Beckman assert that there is 'scant evidence' for my claim that a handful of paradigms from the late 1970s still exercise a predominant influence. One sign of progress would be that we have discarded these worn-out theories in favor of the new approaches they review, and that authors no longer acknowledge an intellectual debt to these venerable paradigms. Figure 1 below shows that quite the opposite has happened: with the notable exception of population ecology, the foundational articles of all of the major paradigms in organization theory show a substantial upward spike in annual citations

in the Web of Knowledge beginning around 2001. Among theories not arising within economics, New Institutional Theory by far the most prominent.

Figure 1: Annual citations of organization theory classics, 1975-2013. (Source: Thomson Reuters Web of Knowledge)



Theory for what?

If the goal of theory is to produce conversations and articles, then as G.W. Bush would say: mission accomplished. The Academy of Management now has nearly 20,000 members from around the world and it grows more every year, with roughly half of its members attending the annual meeting and engaging in conversations at hundreds of panels and presentations. Between 2001 and 2012, the number of ‘management’ journals indexed by the Web of Science nearly tripled, from 61 to 174. These 174 journals published 33,647 articles between 2010 and 2013. If we extended outward to newer print journals, open access journals, and online-only journals not included in the Web of Science, we would

find a multiple of this number. Simply put, there are thousands of management researchers out there publishing tens of thousands of papers. If the volume of papers published is evidence of the efficacy of management theories, then we are in a golden age, and theory is flourishing like never before.

A skeptic might wonder how many of those 33,000 papers are read by other scholars. A realist will conclude that no more than one in a thousand will be read carefully by a practicing manager or policymaker. But if conversation (actual or virtual) is what we want from theory, then we have plenty.

Richard Lewontin, a well-regarded evolutionary biologist, proposed different criteria for evaluating theory. In a review of a book applying Darwinian ideas to cultural change, Lewontin (2005) puzzled over the rationale offered for the wholesale import of biological theory to a quite different domain.

Evolutionary theory was valued by social scientists not because it gave an accurate account of human culture but because it was clear, simple, and generated further work.

‘That a theoretical formulation is desirable because it makes it easier and more efficient to write more articles and books giving simple explanations for phenomena that are complex and diverse seems a strange justification for work that claims to be scientific. It confuses ‘understanding’ in the weak sense of making coherent and comprehensible statements about the real world with ‘understanding’ that means making correct statements about nature... We would be much more likely to reach a correct theory of cultural change if the attempt to understand the history of human institutions on the cheap, by making analogies with organic evolution, were abandoned. What we need instead is the much more difficult effort to construct a theory of historical causation that flows directly from the phenomena to be explained.’

Actually *answering* questions about the world correctly should be at least as valuable as raising interesting new questions that prompt a lot of articles.

If we have abandoned NIT in favor of institutional logics, as Lounsbury and Beckman suggest (and contrary to what citation data imply), is it because we have finally locked down NIT's core empirical claim, that organizations are becoming more similar over time? Or have we abandoned NIT because it has been empirically falsified? Or are we just bored with it, like a forward-facing baseball cap?

Questions worth answering

Organization and management theory has an impressive set of mechanisms for making sense of social processes. We know a lot about diffusion, logics, status, networks, sources of power, organizational birth and death processes, and more. A well-trained organization theorist is like a highly skilled carpenter, able to take the materials at hand and shape them into a thoughtful and accurate explanation with a comprehensive toolkit. For example, a recent paper by Chin, Hambrick, and Trevino (2013) examined the effects of the political ideology of CEOs (as measured by their political contributions prior to taking the CEO job) on a company's propensity to engage in corporate social responsibility. Was CEO ideology really a *cause* of CSR, or were both an *effect* of the company's prior situation? (It was a cause; moreover, liberal CEOs enhance CSR regardless of the company's performance, whereas conservative CEOs' devotion to CSR depends on the company's recent financial performance.) Does CEO ideology influence other outcomes? (It does; appointment of liberal CEOs is followed by increased corporate campaign contributions to Democrats.) This article uncovered new empirical regularities, making novel use of data (political contributions by executives) that would have been hard to access previously. After reading this paper, we know something new about the world – we have not just learned a new way to talk about things.

A major challenge for organization theorists is that we need good taste in problems. Research questions derived internally from theory are not always worth answering, particularly when we are confronting pressing questions in the real world that OMT researchers have unique capacities to answer.

In the United States today, we face epochal challenges in social organization. Income and wealth inequality are at the highest levels in a century. Precarious financial institutions nearly collapsed the global economy, and now they have grown more concentrated than ever before. The number of public corporations has dropped by more than half in the past 15 years. Major employers go bankrupt (General Motors, Chrysler) or disappear entirely (Circuit City, Borders, Eastman Kodak, Blockbuster), to be replaced by pop-up businesses with the size and lifespan of a fruit fly.

It seems that business has taken seriously Karl Weick's advice to replace 'organization' with 'organizing,' creating rampant economic insecurity. As large employers have disappeared, pathways to economic mobility have become inscrutable. A tiny handful of teen entrepreneurs create clever app startups that allow them to retire before they reach drinking age, while their peers work multiple unpaid internships and part-time jobs to pay off their mountainous student loans, or postpone the inevitable with three fruitless years in law school. Their parents find that the free-agent employment system that replaced the corporate career ladder has left them with an empty pension savings account and bleak prospects for retirement.

Major institutions of the twentieth century economy are collapsing around us, and we have no clear idea or plan for what will replace them. Is this the best time to be studying Hollywood films, fancy restaurants, and game shows, or introducing a new lexicon to talk about things we already knew?

What I have argued previously is that problem-driven research is likely to be more fruitful than theory-driven research (Davis and Marquis, 2005). If judged by the volume of articles produced, then theory-driven research has been a big success. If judged by the volume of questions definitively answered, however, I am less sanguine. After thirty years, we still don't know if organizations are becoming more similar over time.ⁱ I don't think I am going out on a limb in saying that we won't be resolving the agency/structure dialectic any time soon either.

An alternative to organizing research around theories and the questions they raise is to organize it around problems. That is, rather than starting from ‘I’m interested in institutional logics and how status and networks are influenced by conflicting logics’ (or whatever), one would start with phenomena in the world that are worth explaining, e.g., ‘What accounts for increasing income inequality around the world, how do countries vary in their trajectories of inequality, and how might this be linked to organizational practices?’ A series of research studies on this topic would not be data-driven exercises that fail to cumulate, as Lounsbury and Beckman fear; rather, scholars examining this topic would most likely make real progress, as part of a research community focused on a common overarching question.

There is, clearly, no shortage of problems that face society, and to whose solution organizational scholars might contribute. Here are three pressing questions that are appropriate topics for OMT researchers. Answering them should both create conversations and yield cumulative insights, whether or not they advance theory.

Where do jobs come from today? First, around the world, including in Europe and North America, unemployment and under-employment are very high. Joblessness seems to be structural rather than cyclical, and there is little sign that governments have figured out policies to address it effectively, other than vaguely alluding to entrepreneurship. What organizational structures might generate greater growth in employment? How do ownership, organizational form, management practice, and job design interact to produce jobs? What public policies and institutions support employment-generating organizations?

To my knowledge, there is almost no research among organization and management theorists on the creation of employment (although there were a handful of studies about downsizing, its obverse, in the 1990s). Yet it is a topic clearly suited to the conceptual and methodological tools of OMT. Transaction cost economics proposes that stable attachments between workers and firms (i.e., ‘jobs’) should be

found where workers invest in firm-specific human assets. We can imagine technological, educational, and legal factors that might promote or inhibit investment in firm-specific (vs. general) human assets. We might expect that the structural power of labor would influence job stability, and that resource dependence would suggest factors that increase employment-promoting labor power (as well as actions that firms might take to limit it). Institutional theory would note potential sources of policies and practices favoring employment growth and decline, such as the spread of professional human resource offices, or broader ecologies of organization types that promote stable employment (e.g., mutuals and co-ops). Agency theory points to the importance of ownership structure in corporate decision making; for instance, family-owned businesses might be more prone to maintaining long-term employment than listed companies because their decision makers are more willing to trade off the warm glow of being a good employer for more pecuniary considerations. Ecologists might examine the vital rates of organizations employing more or fewer people as a source of aggregate employment growth and decline. There are any number of mechanisms that OMT scholars could draw on to unpack the factors that add up to (un)employment in the broader economy, and these might help provide the basis for employment-promoting policy.

One reason why employment growth may have received little attention is that available data were limited. In the US, whereas financial data are disclosed in great detail on a regular basis, employment data are surprisingly minimal: public companies reveal their total employment once per year in their annual report. Firms historically broke this out by US-based employment and global employment, but even this convention has broken down in recent years, making it difficult to track the geographic growth and decline of employment in any detail. On the other hand, Census data at the establishment level have been made increasingly accessible, which should allow analysts a more fine-grained view of where, exactly, jobs come from.

Can supply chains be accountable? Second, the widespread Nikefication of the economy, through which corporations have outsourced large parts of their production and distribution, has created recurring dilemmas around corporate responsibility. The building collapse in Dhaka, Bangladesh that killed more than 1100 garment workers in 2013 revealed how extensively Western brands rely on dangerous practices for the production of their low-priced goods. The Dodd-Frank Wall Street Reform Act of 2010 included a provision requiring electronics companies to disclose if their products contain minerals such as tantalum that originated in the Democratic Republic of the Congo, where mining revenues can support armed conflict. Corporations seeking to comply typically had little idea where materials so far back in the supply chain originated, and the exercise of tracing where the goods carrying their brands originate is proving enlightening. How can corporations enforce accountability throughout their supply chains? How can consumers and governments ensure corporate accountability? What institutions could raise the bar on justice and human rights for enterprises that are dispersed across organizational and national boundaries?

This question is of great theoretical and practical importance. To an astonishing extent, the goods and services we buy are not produced by the organization whose name is on the label, as the vertically-integrated mega-corporations of the 20th century have been replaced by supply chains coordinated by a central node that owns the brand name. For instance, where Apple manufactured its original Macintosh computer in Fremont, California in the 1980s, almost all of its current line of products is assembled in China by Taiwanese contractors such as Foxconn. The same is true across many industries, include clothing, pharmaceuticals, pet food, and various government services, as the NSA leaks vividly illustrated.

Theoretically, this raises ontological questions about what we are studying when we study ‘organizations.’ Many organizations today are more analogous to a Web page than they are to an organism with goals, boundaries, and ongoing identity. Practically, dispersed supply chains raise both

moral and safety issues. The moral concerns are straightforward: we often unwittingly enable abhorrent practices through our purchases because supply chains are opaque. The safety concerns are now gaining more attention; for example, a recent news article on the pharmaceutical industry noted that counterfeiting and poor sanitary practices were common in the generic pharmaceutical industry, which generally operates beyond the purview of North American and European regulators, and stated that 'The crucial ingredients for nearly all antibiotics, steroids and many other lifesaving drugs are now made exclusively in China' (<http://www.nytimes.com/2014/02/15/world/asia/medicines-made-in-india-set-off-safety-worries.html>).

As with job growth, supply chains can be studied using the tools of OMT, although new tools will have to be developed for a 'web page ontology.' Network analysis provides one set of tools and concepts for thinking about supply chains. It is also possible to imagine interventions that would enhance the well-being of employees and consumers of supply chains, e.g., creating mechanisms of transparency using IT or encouraging 'race to the top' competitions among countries of origin (Davis, 2013).

Can new technologies liberate us? Third, steep declines in the cost of production equipment and advances in design tools suggest that the sort of vast re-structuring that occurred in the music and publishing industries will soon happen to manufacturing as well. Knowing what we know now, can we design organizations and institutions that will allow this technology to be implemented in a democratic and empowering way? What lessons can we learn from, e.g., open source software, or non-corporate organizational forms such as co-ops?

How technology and organization structure mutually influence each other is a venerable question of social theory, traceable to Adam Smith and Karl Marx. New technologies enable and encourage new kinds of organization. For instance, the 20th century Fordist corporation was particularly suited to mass production technologies, although the implementation of these technologies varied across countries

according to the size of the market, the nature of property rights, and institutional factors regulating product, labor, and capital markets. Yet some technologies, such as the Internet, can be the organizational equivalent of a mass extinction, creating a context for new varieties of organizing to arise.

Here again, OMT offers tools to help understand and perhaps shape how this coming change will happen. The music industry was blindsided by Napster and the digital revolution, and the publishing industry is still experiencing the shakeout precipitated by the Web. The analogous revolution in manufacturing is still at a very early stage, and prior history suggests that its direction is not foreordained. Organizational scholars have the ability to seek out, compile, and publicize experiments in new organizational forms. Rather than waiting 20 years to give a post-mortem and explain why things went wrong, we might intervene prospectively to enhance the prospects for democratic alternatives to emerge. Consider here the example of Richard Stallman, who pioneered the free software movement in the early days of the PC and arguably changed the course of industry development.

These are decidedly problem-driven topic areas. The results of a sustained set of studies aimed at these questions may not generate new theory, or yield deep insights into the interpenetration of societal institutions through conflicting logics. I for one am willing to take that chance.

References

- Burt, R. S. (1992). *Structural Holes: The Social Structure of Competition*. Cambridge, MA: Harvard University Press.
- Chin, M.K., Hambrick, D.C., and Trevino, L.K. (2013). 'Political ideologies of CEOs: Illustrative evidence of the influence of executive values on corporate social responsibility.; *Administrative Science Quarterly*, **58**, 197-232.
- Cyert, R.M. and March, J.G. (1963). *A Behavioral Theory of the Firm*. Englewood Cliffs, NJ: Prentice-Hall.
- Das Racist. (2010). 'Combination Pizza Hut & Taco Bell.' In *Shut Up Dude*. Accessed at <http://mixtapemonkey.com/mixtape?m=270> (accessed on 6 May 2014).
- Davis, G.F. (2010). 'Do theories of organizations progress?' *Organizational Research Methods*, **13**, 690-709.
- Davis, G.F. (2013). 'Can global supply chains be accountable?' *YaleGlobal Online* <http://yaleglobal.yale.edu/content/can-global-supply-chains-be-accountable> (accessed on 6 May 2014).
- Davis, G.F. and Marquis, C. (2005). 'Prospects for organization theory in the early 21st century: Institutional fields and mechanisms.' *Organization Science*, **16**, 332-43.
- DiMaggio, P.J. and Powell, W.W. (1983). 'The iron cage revisited: Institutional isomorphism and collective rationality in organizational fields.' *American Sociological Review*, **48**, 147-60.
- Granovetter, M. (1985). 'Economic action and social structure: The problem of embeddedness.' *American Journal of Sociology*, **91**, 481-510.
- LeBaron, D. and Speidell, L.S. (1987). 'Why are the parts worth more than the sum? 'Chop shop,' a corporate valuation model.' In Brown, L.E., and Rosengren, S.S. (eds.), *The Merger Boom*. Boston: Federal Reserve Bank of Boston.
- Lewontin, R. (2005). 'The wars over evolution.' *New York Review of Books*, October 20.
- Meehl, P.E. (1978). 'Theoretical risks and tabular asterisks: Sir Karl, Sir Ronald, and the slow progress of soft psychology.' *Journal of Consulting and Clinical Psychology*, **46**, 806-34.
- Podolny, J.M. (1993). 'A status-based model of market competition.' *American Journal of Sociology*, **98**, 829-72.

Notes

ⁱ Lounsbury and Beckman state that one should not make sweeping claims about an entire paradigm based on a single article. But my claim is a perfect case for Popperian falsification: 'After almost three decades during which NIT has come to be the dominant theory of organizations, and DiMaggio and Powell (1983) has become the most widely-cited paper in the field of sociology, I would be hard pressed to point to a single study that has ever tested any of its field-level hypotheses as they are stated in the theory.' One single 'black swan' is sufficient to prove my claim wrong.