In Defense of Organization and Management Theory (OMT):

Challenges and Opportunities for Organization Studies [1]

Michael Lounsbury
University of Alberta

IOA Conversation Series. Copenhagen Business School

April 2013

It is easy to be pessimistic about the state of organization and management theory (OMT). There is certainly much to be concerned about, and there is no shortage of critics. For instance, Hambrick (2007) claims that we suffer from a "theory fetish" and suggests that our profession would be better served if we could just publish (atheoretical) "facts" as was done in epidemiology when connections between cigarette smoking and ill health were identified but could not be fully explained. Hambrick's comments have been taken so seriously that they have been used to mobilize support for the creation of a new journal, *Academy of Management Discoveries*, that will focus on publishing interesting "facts" about the "real" world that existing theories cannot explain (Andy Van de Ven has agreed to be the initial editor)—as if! However, as Hambrick acknowledges, "theory is essential, and the field of management will not advance without it" (p.1346). While Davis (2010: 691) does not necessarily believe that our theory fetish inhibits scholarship (nor do I), he does believe that OMT is theoretically stagnant, commenting that "organizational research can sometimes appear like a living museum of the 1970s (p. 691)." Perhaps exemplifying Hambrick's theory fetishism, Suddaby, Hardy and Huy (2011), in their introduction to the recent *Academy of Management Review* special issue on organization and management theory, echo Davis' critique and argue for the need to generate new theory.

While there are merits to each of these critiques, they do not put their fingers on the most pressing problems. In addition, they make little attempt to look at the other side of the theory coin, appreciating the vibrancy and dynamism of OMT. So I'd like to take on an apparently unpopular/or less glitzy effort to celebrate contemporary OMT and offer an alternative, rosier imagery of contemporary organization theory. I will also provide comment on what I see as our main challenges and opportunities.

The Vibrancy of OMT…We Don't Need No New Theory

In contradistinction to the images promulgated by the critiques eluded to above, from my view as Division Chair-Elect of the OMT division of the Academy of Management as well as Co-Editor of *Organization Studies*, I see tremendous growth and vibrancy (in theory as well as scholar

[1] Some of the ideas/text expressed here emanate from an invited *Journal of Management Studies* paper being developed with Christine Beckman.

demography). With regard to scholar demography, the OMT division and EGOS continue to grow their membership from both Europe and North America. While EGOS has seen tremendous growth in North American membership over the past decade, OMT has been experiencing the reverse—growing Europeanization. For example, paper submissions to the OMT division from our European members for the first time exceeded those from North America (43% v 41%) for the 2012 Academy of Management meetings in Boston. The OMT division has also witnessed growing interest from Asian scholars—submission growth between 2010 and 2012 grew 50%, now accounting for 15% of overall submissions. Furthermore, the number of submissions has almost tripled over the past decade (from 239 in 2002 to 695 in 2012). This increasing diversity of and interest in OMT signals the utility and relevance of our OMT globally, and bodes well for the continued vibrancy of our scholarship. From an *Organization Studies* standpoint, we have also seen growth in submissions outside of Europe as well as overall growth in submissions, and in my opinion, the quality of the papers published in the journal has been notched up over the past five years.

OMT scholarship itself has also exhibited a great deal of dynamism. According to OMT submission data, we have seen the creation of new theoretical domains as well as growth in interest in extant domains. For instance, we have recently witnessed the emergence and rapid growth of new theoretical domains—the institutional logics perspective and research on categorization dynamics. We have also seen the continued growth and vibrancy on theories of networks, learning, emergence, social movements and organizations, as well as the revival of the behavioral theory of the firm. In short, we have a tremendous amount of theory, including newly developing theory that provides great insight into social life. Let me briefly discuss the institutional logics perspective as a case in point. This domain which was virtually non-existent as a cohesive theoretical conversation five years ago has become one of the largest OMT topic areas (alongside corporate governance and strategy).

Davis' (2010) critique noted the dominance of 1970s-style new institutional theory (NIT) in OMT, and singled out NIT as an example of all that is wrong with OMT including the dominance of stale and constraining paradigms. However, his claim is as hollow as the scant evidence he provides—essentially focusing on the limits of one paper, on the structuration of organizational fields, published over twenty five years ago (DiMaggio & Powell, 1983). While there are merits to his critique of the DiMaggio & Powell paper, it is hard to make sweeping claims about an entire paradigm (NIT) that has underpinned hundreds of top journal publications over thirty years, by focusing on a single article no matter how important and ritually cited. More to the point, contemporary institutional research has little substantive connection to the ideas laid out in that paper focused on isomorphism, or even much of institutional research on diffusion in the 1980s and 90s. In fact, the institutional logics perspective questions many of the early ideas underpinning NIT and the study of isomorphism, providing a much richer and fluid theoretical apparatus that focalizes cultural heterogeneity and practice variation (Thornton et. al., 2012).

While research on institutional logics began being published in the late 1990s, it did not emerge to become a mainstream paradigm until very recently as publications in all the top journals has begun to accumulate and grow, forming a stream of work focused on similar problems. As noted earlier, institutional logics research is now one of the largest submission categories at the OMT division of the Academy of Management, and a recent book (Thornton et. al., 2012) has been published that provides a cross-level theoretical model to progressively guide knowledge development. The divergence of institutional logics research from the NIT statement laid out by DiMaggio and Powell

(1983) was explicit in the rogue Friedland and Alford (1991) chapter in the so-called "Orange Book" edited by Powell and DiMaggio. In fact, it is written partially as a critique of NIT research on organizational fields. Entitled, "Bringing Society Back In: Symbols, Practices, and Institutional Contradictions", Friedland and Alford (1991) laid the groundwork for an institutional logics approach that offers a "nonfunctionalist conception of society as a potentially contradictory interinstitutional system" (p. 240). Contrary to NIT emphases on isomorphism, they highlight the centrality of competition, conflict and institutional contradiction. This paper has become very highly cited, and catalyzed an entirely new research stream that emanated but distinctively diverged from NIT.

While not only distinct from NIT, the institutional logics literature has grown rapidly and continues to have accelerated growth in interest and publications (Lounsbury & Boxenbaum, 2013; Thornton et. al., 2012). Institutional logics research is occurring at multiple levels, with most research explicitly focused on delineating cross-level mechanisms linking organizations and their environments. Published research includes that which is quantitative, qualitative, as well as mixed method. And there has been particular attention paid to the development of microfoundations (e.g., Thornton et. al., 2012: ch. 4) to bridge more macro structural institutional research to more situated process studies drawing on ethnographic methods, and to even embrace experimental methods. In short, the institutional logics perspective provides a prime example of the how OMT is far from stagnant, producing new areas of inquiry and paradigms that provide exciting new scholarly directions for the field. And given the vibrancy of OMT, as well as proliferation of new theory such as the institutional logics perspective, it is quite puzzling how some would be concerned with searching for new theory (e.g., Suddaby, Hardy, and Huy, 2011). As one of my wise mentors, Art Stinchcombe, once told me, a little theory goes a long way; and we seem to have plenty of it to go around.

Key Challenges and Opportunities: "Societal Relevance"

As I have indicated, I think the challenges we face as a field have little to do with theory fetishism, stagnation or impoverishment. I agree with Courpasson's (2012; see also our OS editorial statement—Courpasson et. al., 2008) remarks that our biggest challenges have to do with increasing careerist pressures which are detracting scholarly attention away from bigger questions about organizations, politics and society. This is reinforced by calls for "managerial relevance" as well as a growing "culture of productivity" characterized by an increasingly scholarly focus on the quantity of articles published in top journals, where importance of the research and even potential impact are diminished considerations. This has been driven by the growth of market logics in higher education reinforced by rankings, research evaluation "exercises", more intensive accreditation processes, and the like. As a result, much of our research, both qualitative and quantitative, seems quixotic, banal and formulaic. What gets published tends to have a technical beauty and quality (using cutting edge statistical methods or "systematic qualitative analysis a la Barley, Elsbach or Gioia), but often lacks imagination or deeper engagement with broader societal and political issues. We need to become more critical of each other and published work in all outlets, and reframe debates about "relevance" to emphasize "societal relevance". We need to move beyond firm-level issues of performance and sustainability, to focus on more systemic problems such as the growing creep of market logics that have got us into the mess we are in, not just in academia, but with climate change, financial crisis, austerity and the like.

As I argued with Ventresca (Lounsbury & Ventresca, 2002, 2003), since mid-century, the scope of research on organizations narrowed from a concern about the role of organizations in society with a big "S" (Michels, 1949; Selznick, 1949; Gouldner, 1954; Stinchcombe, 1965) to a set of perspectives such as resource dependence (Pfeffer & Salancik, 1978), transaction cost analysis (Williamson, 1985), and organizational demography (Hannan & Freeman, 1977) that focus on concrete exchange processes within and between organizations. While these developments enhanced our understanding of the variety of ways organizations are affected by transactions with other organizations in their environment, the study of organizations had become disconnected from the study of societal stratification, politics and social change that provided core problematics in earlier classic studies of organizations and society. Further, empirical research on organizations had become dominated by a conceptualization of inter-organizational relations as highly rationalistic and instrumental as opposed to ambiguous, culturally shaped, and politically inflected.

Institutional theoretic approaches (e.g., new institutionalism, institutional logics) alongside efforts to bring social movements into organizational analysis (and cognate efforts reflected in literatures on orders of worth, varieties of capitalism etc.) have provided a counterweight to these trends which reinforce instrumental approaches to organizations as well as scholarship. These are important efforts theoretically and empirically that we must embrace and reinforce, while encouraging ourselves and others to take on big problems of the day, and to move away from a focus on narrow instrumental issues to direct our gaze towards the societally relevant—the financial crisis, ecological destruction, poverty, religion, politics, the marketization of everything (on relevance, see also Hinings & Greenwood, 2002). No doubt at the center of these empirical issues is power, stratification, and the role of elites and expertise (Zald & Lounsbury, 2010). We should focus on the "command posts" of relevance, and speak truth to power. Theory is not a problem here—as I have indicated, many of our theories direct us towards these big societal issues, helping us to conceptualize and study such problems. It is not stagnant theory or the lack of new theory that fails us; it is the lack of scholarly imagination and pressures to produce insignificant study after insignificant managerially relevant study that we need to fight against. This requires astute efforts on the part of our collective to ensure we have pathways to top journals for significant work, and to convince administrators of our institutions that this is important research that has relevance—even in business schools.

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