



Celebrating Organization Theory

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ABSTRACT In this paper, we respond to recent critiques about the state of organization theory that have characterized it as being anachronistic, overly theoretical, or lacking the right kind of theory. We argue that organization theory is extremely vibrant and highlight several areas where there are flourishing and generative developments – institutional logics, categorization, networks, performance feedback, and strategy-as-practice. We also note the growing internationalization of organization theory as exemplified in the shifting demography of the Organization and Management Theory division at the Academy of Management as well as at the European Group on Organization Studies. As engaged organization theory supporters and scholars, we additionally argue for a more balanced appreciation of not only the weaknesses in the field, but also its strengths, and urge a re-engagement in more productive conversations about the important role of theory and theorizing.

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Recently, there has been growing disquiet about the state of organization theory (whether that be theory more generally or organizational theory). Hambrick (2007) characterized the management field as having an ‘idolization of theory’ that inhibits our ability to understand the world. As part of his ongoing critique of the field and call for problem-driven research, Davis (2010, p. 691) commented that ‘organizational research can sometimes appear like a living museum of the 1970s’. And in their introduction to the *Academy of Management Review* special issue on organization and management theory, Suddaby et al. (2011) echo Davis by criticizing the failings of contemporary organization theory and arguing that we need to generate new, preferably indigenous, management-centred theory.

While each of these recent commentaries make useful points about the nature of theorizing and the general state of the field that are worth considering, we also feel that the pessimism expressed about contemporary organization theory (OT) scholarship is

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overstated. Even though OT as a field certainly faces many challenges, we share the optimism of others (e.g., Corbett et al., 2014; March, 2007) that emphasize its vibrancy. For instance, in emphasizing the growing diversity of our field, Corbett et al. (2014, p. 15) claim that ‘there is a richness inherent in the study of management and organizations. Phenomena are abundant and theories and methodologies are likewise profuse’.

Indeed, we see contemporary theoretical conversations as quite generative and dynamic – with regard to both progressive knowledge accumulation and the spawning of new theoretical problematics and orientations. If organization theory is a museum, the collection is much larger than Davis (2010) acknowledges. New pieces are being added to the collection, and many of these are potential anchors for new rooms of the museum. In celebrating the dynamism of OT, we highlight exciting recent developments in five theoretical domains – institutional logics, categorization, networks, behavioural theory, and practice theories.

In brief, we note that new institutional theory has not been a static relic, stuck in the 1970s or even the 1980s; the recent proliferation of research in the top journals on the institutional logics perspective (Thornton et al., 2012) forges new theory about how organizations are variably interpenetrated with wider societal forces. Second, Hannan et al. (2007) reject some of the earlier assumptions of organizational ecology, embrace social constructionism, and have joined hands with a wider group of organizational theorists to develop a progressive research stream on categorization dynamics (see Hsu et al., 2010). Third, network research has revolutionized OT since the mid-1980s, providing an understanding of how the structure of networks and attributes of actors at all levels of analysis impact behaviour (e.g., Brass et al., 2014). Fourth, performance feedback theory (Greve, 2003a), spawned from behavioural theory (Cyert and March, 1963), examines with better precision some of the original theoretical questions in behavioural theory as well as raises new questions (see also Gavetti et al., 2012). Finally, practice theories across the social sciences and humanities have inspired the development of many new theoretical conversations including that of Strategy-as-Practice (SAP) which has its roots in working groups at the European Group on Organization Studies (EGOS) colloquia less than a decade ago, and has more recently coalesced into a formalized interest group at the Academy of Management (see, e.g., Golsorkhi et al., 2010; Jarzabkowski and Spee, 2009; Vaara and Whittington, 2012).

As current leaders of the Organization and Management Theory (OMT) division of the Academy of Management, the home for OT research and the third largest division in the Academy of Management, we will also report recent statistics about the growing internationalization (especially Europeanization) of OMT, as well as the innovativeness of our field in generating new kinds of questions and approaches. Since many of us in OMT are also intimately involved in EGOS, we note that there has also been a growing North American presence in European intellectual circles, creating two-way traffic for intellectual engagement and advance. It is also important to note that EGOS continues to be a vibrant focal point for the development of novel theoretical conversations, SAP being a case in point. There is much to celebrate and good reason to be optimistic about the state of our field and where it is going.

ORGANIZATION THEORY UNDER SIEGE?

The critiques of contemporary organization theory are varied, and include complaints about scholarship being overly theory-focused, anachronistic, too tied to disciplinary knowledge, and lacking in relevance. For instance, Hambrick (2007) accuses our field of having a ‘theory fetish’ – he elaborates, ‘like insecure adolescents who are deathly afraid of not looking the part, we don’t dare let up on our showy devotion to theory’ (p. 1347; see Miller, 2007 for a sympathetic critique). He goes on to argue that our obsession with theory is a profound malady, prohibiting us from developing valuable managerial knowledge. He suggests that our profession would be well served if we could publish important observations as is done in epidemiology.

Hambrick’s (2007) comments have been among the most prominent amidst the current stream of disquiet, leading to the creation of a new journal, *Academy of Management Discoveries*, whose founding mission is to focus on publishing novel empirical findings. To make his point about our ‘theory obsession’ problem, Hambrick (2007) notes the early epidemiological research on cigarette smoking and ill-health, and suggests such important findings would be rejected by management journals because of the lack of theory. However, we advise caution: one of the great examples of statistical misidentification is the relationship between cigarette smoking and poor grades (Huff, 1954). Thus, even with multiple, well-executed studies (which is an important component of replication in many fields), as seen in epidemiology, we fear such a direction has hidden dangers. For instance, given the concern in related fields (strategic management, economics, and psychology) decrying a crisis of data mining, ‘p-hacking’, and the search for ‘asterisks’ (Bettis, 2012; DeLong and Lang, 1992; Goldfarb and King, 2013; Simonsohn et al., 2013), a focus on ‘facts’ may encourage searching vast quantities of data for statistical significance rather than producing good research. Alongside rigorous and replicable empirics, good theory should also be encouraged to help us assess empirical relationships. While an instrumental variable or natural experiment may allow us to differentiate between causation and correlation, it is still important to theorize about why particular relationships exist – especially if we are concerned about how our empirical findings may be used in policy.

Davis (2010, p. 706) agrees with Hambrick ‘that the theory benchmark prevents the publication of papers that would enrich our understanding of organizational life but do not hang on a particular paradigm, while encouraging authors to link whatever their regressions turn up to the theories currently in vogue’. He further notes, ‘given the diverse predictions of the many paradigms in organization theory, it is almost always possible to find a theoretical rationale for a result’ (Davis, 2010, p. 697). Of course, this cynical quip provides an overly-elastic conceptualization of organization theory that devalues the role of theory in research development while simultaneously indicting the ethics of organizational scholars as well as the capacity of reviewers and editors to assess knowledge contributions. While there is no doubt some truth to the complaints of Davis and Hambrick, the sweeping nature of their claims, as well as the flamboyant rhetoric they employ, distracts attention away from the value of their insights. One of Davis’ (2010) main critiques of organization and management theory is that we have made limited theoretical progress, or have been theoretically ‘stagnant’, for several decades due

to the fact that we remain focused on ‘a handful of paradigms from the late 1970s [that] still exercise a predominant influence’ (p. 693). While we believe that there is scant evidence for this claim, it is one we take seriously enough to challenge in this paper.

Of course, OT scholarship faces many challenges, including a ‘statistical fetishism’ (Davis, 2010), problems associated with the lack of experimental control in social science research (Cook and Campbell, 1979), the conservative nature of business schools where much of OT research takes place (March, 2007), and top journal-driven isomorphic pressures that threaten to direct scholars away from more unconventional research projects (Corbett et al., 2014). But is the state of organization theory so dire? We wonder if it is just sexier to be cynical and complain, rather than recognize and applaud positive developments.

THE DYNAMISM OF ORGANIZATION THEORY

Without denying some merits embedded in the aforementioned critiques, we alternatively see organization theory as a tremendously vibrant and generative field. For instance, in the OMT Division of the Academy of Management, there has been a growing diversity and internationalization of members. Following (and often leading) the Academy trends, OMT international membership is now over 50 per cent (up from 41 per cent in 2005). In addition, paper submissions to the OMT division from European members for the first time exceeded those from North America (43 vs. 41 per cent) 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 per cent, now accounting for 15 per cent of overall submissions. Of those advanced doctoral students that have participated in the OMT Doctoral Consortium in the last four years, 39 and 34 per cent were educated or are now employed outside of North America, respectively. Furthermore, the number of submissions has almost tripled over the past decade (from 239 in 2002 to 695 in 2012).

This growth in diversity is echoed by the experiences of the EGOS, which has experienced growing internationalization, including rapid growth in North American membership and participation, as well as increasing interest from researchers in Asia, South America, and elsewhere. For instance, the percentage of North American attendees at EGOS conferences has grown from around 5 per cent in 2001 to close to 15 per cent by 2012; and this percentage jumped to 23 per cent in 2013 when the EGOS colloquium was hosted by HEC Montréal – the first time in North America. This accords with internationalization data reported by Corbett et al. (2014), who note that the Strategic Management Society conferences have seen tremendous growth in numbers and countries represented, as has the *Journal of Management Studies*. In general, we believe that this increasing diversity of and interest in organization theory bodes well for the continued vibrancy of our scholarly field.

These larger demographic trends also importantly signal theoretical dynamism. Theory development happens when new directions are explored and unanswered questions are illuminated. This growing internationalization can infuse the field with new perspectives, new viewpoints, and a focus on new problems (or at least a focus on

problems from a different perspective). Rather than needing new theory to stimulate the growth of the field, new people from new places can facilitate theory development. To see this, we can look at the intersection between theory and region in OMT division submissions. Despite representing 15 per cent of the overall submissions, Asian scholars submitted 32 per cent of the papers on governance and corporate strategy and 28 per cent of the papers on learning and adaptation to OMT in 2011. These existing theoretical domains benefit from the new perspectives. Applying, questioning, and extending our theories of corporate governance in China may in fact deepen our understanding of the theories themselves.

Davis (2010, p. 705) suggests that to expect organizational insights to remain generalizable over time is a 'vain hope', and perhaps the same can be true for theories across place (see also March, 2007). However, without theory, we collect empirical regularities (that may be time-specific and place-specific) without understanding the fundamental whys and hows that a good theory encourages us to understand. Isn't this what Pfeffer (1993) suggested we should be seeking when he called for paradigm development? We believe that the growth of global OT scholarship seems a step towards generalizable theory or at least understanding what portions of our 'old' theories continue to hold true in new places and new times. Without placing our findings in a theoretical frame, we collect a series of empirical observations without understanding how they may relate and build on each other.

OT scholarship has also exhibited a great deal of dynamism. This is partially determined by the position of organization theory in the field. Using the OMT division of the Academy as an example, for at least the past ten years OMT has served as the major bridge between the two major clusters in the Academy of Management (bridging the clusters anchored by strategy and organizational behaviour; Pearce, 2003). Thus, if we follow network theory, the position of organization theory within the larger management field makes it the most likely to generate and espouse new ideas. To that point, OMT scholars have helped to spawn new divisions and interest groups within the Academy: Managerial and Organizational Cognition (MOC), Critical Management Studies (CMS), and most recently Strategizing Activities and Practice.

Below, we address in more detail growth within the bounds of the organization theory field, focusing on exciting developments in five important and novel theoretical conversations that have emerged in recent years. Two of these theoretical conversations emerge out of the six theories highlighted by Davis (2010). In choosing theoretical areas to discuss in detail, we do not mean to discount other areas where vibrant research is happening. Among the OMT submissions in 2011, 16 different theoretical keywords were used by 74 per cent of the authors. The other 26 per cent of the submissions are studying topics like bureaucracy, diffusion, corporate social responsibility, identity, culture, control, knowledge, innovation, and status. These are not one-off categories – they are important areas of new theory development. Consistent with the demographic trends noted above, many of these research areas are being studied across the globe, and others are being driven by scholars outside the USA (e.g., social responsibility and bureaucracy submissions were 67 and 61 per cent, respectively, outside the USA in 2011). All of these trends provide indicators of vibrancy and variety in our scholarship.

THE INSTITUTIONAL LOGICS PERSPECTIVE

The prominence of institutional theory among OMT division members has been well documented (roughly a quarter of all submissions to the Academy of Management annual meetings in both 2005 and 2011). Yet this single theoretical category masks the diversity and evolution of research that underlies or is related to institutional theory. In fact, 19 per cent of all institutional submissions in 2011 were also about entrepreneurship, identity, or work (6–7 per cent each). In addition, one of the most exciting new institutional research domains focuses on the institutional logics perspective (Thornton et al., 2012). This domain was virtually non-existent as a cohesive theoretical conversation five years ago. In 2011, 18 per cent of all the institutional theory submissions to the OMT division also used institutional logics as a keyword (and an additional third of the institutional logics submissions did not identify as institutional theory). The numbers suggest progressive theoretical development.

Yet Davis (2010) singles out new institutional theory (NIT) as an example of all that is wrong with OMT, including the dominance of stale and constraining paradigms. His claim seems as hollow as the scant evidence he provides – essentially focusing on the limits of one paper, on the structuration of organizational fields, published over 30 years ago (DiMaggio and Powell, 1983). The limits of that paper notwithstanding, it is hard to make sweeping claims about an entire paradigm (NIT) that has underpinned hundreds of top journal publications, by focusing on a single paper 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 that focused on isomorphism, or even much of institutional research on diffusion in the 1980s and 1990s. In fact, the institutional logics perspective rejects 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). 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 catalysed an entirely new research stream that emanated but distinctively diverged from NIT.

This new stream began slowly, with a trickle of papers in the late 1990s (e.g., Haveman and Rao, 1997; Thornton and Ocasio, 1999; Townley, 1997), but has recently blossomed as publications in top journals have accumulated and theoretical conversations have concomitantly broadened (Lounsbury and Boxenbaum, 2013). Initial research focused attention on how historical shifts in logics opened up opportunities for the reconfiguration of organizational practices, but the scope of the literature expanded quickly to include a focus on the tensions associated with logic pluralism. For instance,

some research draws on the social movement literature to understand how actors usher new logics into fields (see Schneiberg and Lounsbury, 2008). Complementing this focus on collective action and logic struggle are studies that explore how competing logics relate to actors and their identities and practices (e.g., Jones et al., 2012; Lok, 2010; Lounsbury, 2007; Marquis and Lounsbury, 2007; Meyer and Hammerschmid, 2006; Pache and Santos, 2010; Reay and Hinings, 2009; Smets et al., 2012; Tracey et al., 2011).

There is also renewed attention to how institutional dynamics affect and play out inside organizations. Analysing organizational downsizing in Spain, Greenwood et al. (2010) developed the term 'institutional complexity' to refer to situations where organizations are faced with pressures stemming from multiple institutional logics. Greenwood et al. (2011) theoretically developed the notion of institutional complexity to direct research towards an understanding of how the structural dimensions of fields (fragmentation, formal structuring/rationalization, and centralization) as well as characteristics of organizations (field position, structure, ownership/governance, and identity) shape how organizations respond to competing institutional logics. Hybrid organizations that combine multiple logics are particularly opportune focal points for the study of institutional complexity (e.g., Battilana and Dorado, 2010).

Thus, 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 is quantitative, qualitative, as well as mixed method. And there has been particular attention paid to the development of micro-foundations (e.g., Thornton et al., 2012, ch. 4) to bridge more macro structural institutional research to more situated process studies. In short, the institutional logics perspective provides a prime example of how OT is far from stagnant, producing new areas of inquiry and paradigms that provide exciting new scholarly directions for the field.

CATEGORIZATION

Another relatively new theoretical domain in OT that has grown in significance and interest focuses on categorization processes. Recent interest in categorization was sparked by the work of Zuckerman (1999), who documented how categories provide clear and legitimate boundaries for identity and role conformance, and spanning categories can lead to the inattention of key audiences and concomitant penalties – in his case, firm stock price devaluation. Since that paper, there has been a growing community of scholarship detailing the sources and consequences of categories, and this scholarship brings together and reanimates ecological and institutional theories (Hsu et al., 2010). While this new domain has roots in these 'older' theories, the OMT submission data suggest that 11 per cent of all submissions in 2011 were self-identified as 'identity and categorization' (compared with one self-identified categorization paper and 6 per cent identity submissions in 2005).

Categorization combines similar entities (e.g., organizations or products) into distinct clusters, rendering them understandable, and creating shared understandings about how groups of entities differ across categories (Zerubavel, 1997). Since categories simplify reality and make cognition and behaviour more efficient, they can play a powerful role

in organizing the attention of varied audiences, as well as enabling and constraining behaviour (Hsu and Hannan, 2005). For instance, organizations that do not fit categories easily (e.g., innovative or novel firms) may have difficulty being recognized and 'counted' (Kennedy, 2008). Thus, category schemes are pervasive and importantly shape the identities and interests of actors, providing an important foundation for role conformity in both market and non-market settings (Zuckerman, 1999).

Even though the study of categorization processes is not new (e.g., Rosch, 1983; Wittgenstein, 1953), growing organization theoretic interest in categorization, like the study of institutional logics, is exemplified by a growing corpus of publications in top journals as well as high submission rates to OMT for the annual Academy of Management conferences (this area has ranged from the third to fifth most popular OMT submission topic in the last three years). Research demonstrates the categorical imperative across different contexts, and has begun to identify scope conditions. For example, Zuckerman et al. (2003) showed how typecasting in Hollywood films is a double-edged sword. While actors initially benefit from being typecast in a single genre, this benefit dissipates over time as mature actors gain from the ability to move across genres. It has also been demonstrated that penalties associated with category spanning may be mitigated when a category system is emergent or in flux (Ruef and Patterson, 2009), when categories become blended (e.g., Hsu et al., 2012; Rao et al., 2005; Wry and Lounsbury, 2013), or when market makers seek the diversity and novelty associated with category spanning (Pontikes, 2012). Vergne (2012) has also shown that actors in a stigmatized category can avoid stigma related penalties by spanning to non-stigmatized categories. And Wry et al. (forthcoming) show how category spanning can actually be rewarded.

The research agenda goes far beyond research on the categorical imperative, as exemplified by the diverse contributions included in the 2010 Research in the Sociology of Organizations volume on *Categories in Markets: Origins and Evolution*, edited by Hsu, Negro, and Koçak, and recent statements that direct the field towards the study of how categories emerge and change (e.g., Kennedy, 2005; Khaire and Wadhvani, 2010; Navis and Glynn, 2010; Phillips, 2013; Wry et al., 2011). Indeed, Kennedy and Fiss (2013) lay out an explicit agenda for research on category emergence. This type of theoretical development may not be the Darwinian selection between theories that Davis (2010) hopes for, but perhaps Lamarckian analogies and a blending of theories is more generative for organizational theory.

The study of category transformation and change is also a key focal point for research. Lounsbury and Rao (2004) emphasized the politics of categorization and showed how categories were reconstituted as the variance of entities within a category grew. Schneiberg and Berk (2010) argue that categories serve as cognitive infrastructures for learning, discovery, and innovation in markets, and show how collective action can lead to category reformation. Mohr and Guerra-Pearson (2010) emphasize how category change can be triggered by jurisdictional conflicts over practice. Kennedy et al. (2011) introduce the notion of 'category currency', and suggest that category change goes hand-in-hand with the efforts of actors to be valued by helping to reshape categories in ways that make them more appropriately recognized (see also Alexy and George, 2013).

Overall, the growing diversity (both topically and demographically) and theoretical innovativeness of categorization research is one of the most exciting contemporary

developments in OT (see also Durand and Paoella, 2013; Glynn and Navis, 2013; Kennedy and Fiss, 2013; Vergne and Wry, 2014). While old theoretical paradigms such as the new institutionalism and population ecology may provide resources for understanding categorization dynamics, neither paradigm as constituted in the 1970s and 1980s is up for the task. More pointedly, categorization research provides a vivid example of a new theoretical conversation that stands in stark contrast to the imagery of 'stagnation' painted by Davis (2010) or puzzling complaints about the lack of new theory (Suddaby et al., 2011).

NETWORKS

Networks research is active in OT and in management writ large (17 per cent of OMT submissions in both 2005 and 2011), and networks are both a methodological tool and a theoretical lens that operate at multiple levels of analysis (Borgatti and Foster, 2003; Moliterno and Mahony, 2011). Research on networks has explored the particular influence of *individual* ties, as well as the origins and consequences of *overall* network structures. Research examining how particular network structures emerge has been considered the 'theory of networks' (Borgatti and Halgin, 2011, p. 1168). For example, the network of interlocking directorates (board of director ties) is predicted by the geography and social cohesion of a region (Kono et al., 1998). Another strand of network theory seeks to understand the consequences of networks. For example, we can predict the spread of corporate practices such as acquisitions by the structure of the interlocking directorate network (Haunschild, 1993).

The origins of network theory date back many decades (see Beckman, 2010; Borgatti and Foster, 2003 for reviews and terminology). Beginning with Granovetter (1973) and Burt (1992), scholars have developed a 'flow' or pipes model, where networks act as a conduit for information to flow between actors (Borgatti and Halgin, 2011, p. 1172). What information and how fast it flows or diffuses through network ties depend on the structure or position of those ties in the networks. For example, structural holes and cohesive ties can be both a source of and constraint on innovation (Ahuja, 2000; Burt, 2004). These effects appear to be contingent on the orientation of actors in particular structural positions (Obstfeld, 2005), the interaction with the geography of ties (Whittington et al., 2009), the uncertainty and thus information available in the market (Podolny, 2001), and the knowledge embedded in those network ties (Carnabuci and Operti, 2013; Wang et al., forthcoming). Another subset of this work examines small-world systems where a combination of interconnectivity across immediate ties and connections spanning clusters of ties provide both heterogeneous and shared information. These small-worlds have been associated with creativity and innovation (Schilling and Phelps, 2007; Uzzi and Spiro, 2005).

The 'flow' network model also includes diffusion research, whereby research has examined how practices spread through networks (Davis, 1991; Haunschild, 1993). Recent research has examined which ties are more likely to be influential, such as when resisters of contentious practices adopt them (Briscoe and Safford, 2008), as well as the nature of the practice. For example, Briscoe and Murphy (2012) find that opacity rather than transparency aids the diffusion of the controversial practice of curtailing retiree

health benefits. At a macro-level, Powell et al. (1996) suggest that learning flows within a network of collaborating biotechnology firms.

In addition to understanding the consequences of particular network structures, network theory also focuses on the origins of networks. For example, Zaheer and Soda (2009) find prior status and centrality predict the formation of structural holes. Such research responds to Salancik's (1995) call for a better 'theory of networks', whereby we understand how network ties emerge. There is more work to be done here, and a better understanding of network antecedents needs to incorporate multilevel theorizing (Moliterno and Mahony, 2011). That said, we concur with Borgatti and Halgin (2011) who argue that a 'theory of networks' is only one flavour of network theory.

Other network theory focuses on networks as the 'prisms' that provide status and legitimacy that help actors obtain important outcomes (Podolny and Page, 1998) or as the 'bonding' that helps aid coordination (Borgatti and Halgin, 2011). The high-status position of network ties signals the underlying quality of an actor (Podolny, 1993), and multiple benefits accrue to firms from affiliations with high status others (e.g., Burton et al., 2002; Stuart et al., 1999). Other network theories emphasize how networks facilitate coordination and cooperation, and how economic behaviour is embedded in a social context (Granovetter, 1985). Although it is widely accepted that embeddedness facilitates economic exchange (Uzzi, 1996, 1997), embeddedness may also result in biased decisions rather than better coordination that benefits both parties (Sorenson and Waguespack, 2006). At a more macro-level, our theory of network forms of organization (Podolny and Page, 1998; Powell, 1990) also explains coordination across organizations, and this understanding is extended by recent research on why networks fail (Schrank and Whitford, 2011).

Although this large body of research is not without problems, theoretical developments are encouraging. We know that the informal and formal networks work together to influence action (Gould, 1991), and recent work is examining how multiple types of networks interact in ways that exert power over organizational decisions (Beckman et al., forthcoming), facilitate the maintenance of interorganizational ties (Rogan, 2014), and influence the formation of other types of ties (Shipilov and Li, 2012). Although an incomplete overview of this research, we have highlighted some of the important and interesting theoretical ideas in this research space. To suggest this work is not theoretical, or not relevant to our understanding of the world, seems implausible.

PERFORMANCE FEEDBACK THEORY

Next we turn to one of our oldest 'indigenous' organizational theories (Corbett et al., 2014, p. 12), the behavioural theory of the firm (Cyert and March, 1963; March and Simon, 1958; Simon, 1947). Although behavioural theory contains a broad set of assumptions and ideas, it focuses attention on the processes by which organizational decisions are made. Work emerging from these foundations accounts for approximately 20 per cent of OMT division submissions in 2011. From these roots come organizational learning and evolutionary economics (Argote and Greve, 2007), as well as work on cognition, politics, attention, routines, adaptation, and performance feedback (for a

review, see Gavetti et al., 2012). Here we focus on performance feedback theory (Greve, 2003a).

Performance feedback theory suggests that firms make assessments of their performance in relative terms (as a function of aspirations) rather than in terms of absolute outcomes, and feedback about whether performance falls short of these aspirations is an antecedent of organizational change (Greve, 2003a). One of the core insights – that when performance falls short of aspirations, decision makers engage in ‘problemistic search’ to try and improve performance – emerges directly from the Carnegie School (e.g., Cyert and March, 1963, p. 169). But performance feedback theory itself did not develop until the 1990s.

Studies have commonly identified two distinct sources of aspirations: the firm’s own prior performance (historical aspirations), and performance of a meaningful referent group (social aspirations). While Cyert and March (1963) posit a very general model to understand the choice of aspirations and the level of performance to which firms aspire, scholars have utilized a number of different conceptual and empirical strategies (Washburn and Bromiley, 2012). In part, this is because performance relative to one aspiration may have different salience to decision makers as a function of how the organization is performing relative to other aspirations (Moliterno et al., forthcoming; Washburn and Bromiley, 2012); further, multiple aspiration points, taken together, may offer consistent, inconsistent, or ambiguous performance feedback (Audia and Brion, 2007; Baum et al., 2005; Gaba and Joseph, 2013). Finally, firms do not always focus their aspirations upward: they also focus on a ‘survival level’ which is the performance level in which the firm’s existence is threatened (Audia and Greve, 2006; Chen and Miller, 2007; March and Shapira, 1987, 1992; Miller and Chen, 2004) or a ‘reference group threshold’ whereby firms seek to maintain membership in their reference group (Moliterno et al., forthcoming).

This work all advances our understanding of how firms choose aspiration points. Although some scholars have conceptualized the social aspiration level as the average performance of others in a firm’s industry (Baum and Dahlin, 2007; Bromiley, 1991; Greve, 1998; Miller and Chen, 2004), more recent research has begun to consider ‘striving’ comparisons where attention is focused on those of higher status or performance (Boyle and Shapira, 2012; DiPrete et al., 2010; Kim and Tsai, 2012; Labianca et al., 2009; Moliterno and Beckman, 2013). These studies question the face validity of firms striving for ‘average performance’.

Other developments have focused on the choice of social reference groups. Here, scholars have drawn on important work in managerial cognition and categorization (Porac et al., 1989; Reger and Huff, 1993) and on strategic groups (Fiegenbaum and Thomas, 1990, 1995; Panagiotou, 2007; Schimmer and Brauer, 2012) to suggest reference groups are smaller than the whole industry and important referents for organizational decision-making. This general idea, that reference groups are smaller than the industry, has been embraced by scholars and offers important specificity to Cyert and March’s original model (Baum et al., 2005; Ketchen and Palmer, 1999; Labianca et al., 2009; Massini et al., 2005). Finally, there is work beginning to examine aspirations that arise from comparisons within the firm (Kacperczyk et al., 2013; Mezas et al., 2002). This work ties performance feedback theory with work on politics, internal

coalitions, and garbage can decision processes – another theoretical development emerging from behavioural theory (Gaba and Joseph, 2013; Gavetti et al., 2007; Lomi and Harrison, 2012; Vissa et al., 2010).

Finally, performance feedback theory has also examined different types of search. For example, Baum and Dahlin (2007) find that performance near aspirations fosters local search and performance farther away stimulates distant search. As another example, when firms exceed their aspirations, scholars have found that slack search is a mechanism that motivates innovation (Greve, 2003b). This work bridges to other work emerging from behavioural theory, work on organizational learning, and the organizational search for new knowledge (Katila and Ahuja, 2002; Rosenkopf and Almeida, 2003).

In summary, performance feedback theory has led us to examine how firms set aspirations, and their selection of social referents, and to better understand problem search. Theoretical progress is not always linear and empirical results not always easy to interpret, yet by engaging with the theoretical predictions we can better examine and test the actual mechanisms underlying behaviour. Performance feedback theory is a vibrant and new theoretical domain that is very much in development.

PRACTICE THEORIES

One last example of the theoretical dynamism we celebrate involves the rise of practice theories. Over the past four decades, practice theories (e.g., Bourdieu, 1977; Giddens, 1984; Lave and Wenger, 1991; Ortner, 2006) have increasingly influenced OT research (e.g., Feldman and Orlikowski, 2011; Nicolini, 2013; Schatzki et al., 2001). While there are different versions of practice theory, practice theories highlight the co-constitutive aspect of structure and action; that is, social structures emerge from the situated action they also condition. Situated action shapes and is shaped by shared understandings about forms of socially meaningful activity that are relatively coherent and established. For many social theorists, practice is a key concept that links broader cultural belief systems and social structures to individual and organizational action, often in a dialectical way. Actors are conceptualized as knowledgeable and practical. Schatzki et al. (2001, p. 3) comment that ‘practice approaches promulgate a distinct social ontology: the social is a field of embodied, materially interwoven practices centrally organized around shared practical understandings’.

Practice theories embodied in notions such as ‘culture as toolkit’ (Swidler, 1986) revitalized the study of culture as more dynamic and heterogeneous and as practices that can be utilized as a strategic resource (e.g., Friedland and Mohr, 2004; Weber and Dacin, 2011). In addition to redirecting institutional research (Lawrence and Suddaby, 2006; Thornton et al., 2012), practice theories are embedded in a new dynamic approach to the study of routines (Feldman and Pentland, 2003), novel approaches to sociomateriality (see, e.g., Leonardi and Barley, 2010; Orlikowski, 2007; Orlikowski and Scott, 2008), and the rise of the Strategy-as-Practice research field (see Vaara and Whittington, 2012, for a review). We focus our discussion on developments related to Strategy-as-Practice scholarship which is a prolific example of a research community spawned in Europe, incubated at EGOS colloquia, and that has spread to North America and taken root in a recently created Academy of Management interest group.

SAP research was initially motivated as a response to conventional approaches to strategy research that was dominated by the microeconomics tradition and tended to downplay the role of actors and human agency (e.g., Jarzabkowski, 2004, 2005). SAP researchers sought to address this gap by focusing on 'strategizing' as the 'doing of strategy', to emphasize how strategic practices are embedded in and influenced by their social contexts (Whittington, 2006). SAP research broadly focuses on studying 'practitioners (those people who do the work of strategy); practices (the social, symbolic and material tools through which strategy work is done); and praxis (the flow of activity in which strategy is accomplished)' (Jarzabkowski and Spee, 2009, p. 70).

As Vaara and Whittington (2012) nicely overview, there are by now a wide variety of notable empirical and conceptual SAP related publications. For instance, drawing upon discourse analysis, Vaara et al. (2004) showed how different discursive practices can be used to legitimate or de-legitimate strategies. Jarzabkowski and Seidl (2008) showed how strategies can be stabilized or de-stabilized through the use of strategy meeting practices such as bracketing of issues, turn-taking, voting, and stage-managing. Mantere and Vaara (2008) document how mystifying, disciplining, and technologizing discourses inhibit participation in strategy-making, while self-actualization, dialogization, and concretizing discourses enable such participation. Kaplan (2011) drew on ethnographic methods to highlight how PowerPoint presentations are a kind of discursive practice, and an important component in the 'epistemic machinery' of strategy.

In contrast to mainstream strategy research that tends to focus on large-scale statistical analyses, much of this work draws on qualitative methods including ethnography, discourse analysis, participant observation, and interviews. The emphasis is on exploring strategy-making practices and processes in-depth to highlight the reflexivity of actors as they navigate complex organizational processes as well as wider cultural influences. Actors do not merely employ strategic practices, but as skilful, knowledgeable agents, they creatively adapt such practices to the situation at hand. For example, the cognitive 'framing' contests engaged in by managers alter the power structure within organizations and politically inflect strategy-making processes (Kaplan, 2008). In addition, mindfulness is at the core of performativity-based approaches to practice and institutions (e.g., Lounsbury and Crumley, 2007; Orlikowski, 2000). As Vaara and Whittington (2012, p. 14) note, 'SAP studies have therefore extended mainstream strategies research by bringing to light practices that have largely passed unnoticed, and discovering in them effects that previously were hardly imagined'. In addition, the roles and identities of those partaking in strategy are being problematized, focusing on the processes by which actors get to participate and how.

In summary, seeded in Europe and transplanted to North America and elsewhere, Strategy-as-Practice scholarship is a very exciting development that highlights how broader theoretic movements can help motivate the creation of new organization and management theory. As we alluded to earlier, practice theories have also motivated a number of other theoretical shifts in OMT, and as our intellectual field becomes even more globalized, it will be interesting to see how ideas from Asia, South America, Africa, and elsewhere may work their way into OMT to inspire novel insight as well as theoretical development.

CONCLUSION

When we re-examine the arguments of prominent scholars such as Hambrick (2007) and Davis (2010) in light of our assessment of the field, we are left somewhat puzzled. Are scholars developing and extending theoretical ideas? Most certainly. Do we lack ‘insight’ and contributions ‘to our knowledge and understanding’ as Davis and Hambrick suggest? We don’t think so. Within the broad category of institutional theory, we see significant theoretical conversations around institutional work and identity. Institutional logics and categorization may even be creating new theoretical rooms in our OT museum. Importantly, categorization brings together the research traditions of ecology and institutional theory. Rather than one theory ‘winning’ and driving out the other, categorization scholarship draws on insights from both to develop new understandings. These multiple-lens explanations are increasingly important (Okhuysen and Bonardi, 2011). In network theory and performance feedback theory, our ability to develop theory has been stimulated by new methodological techniques. Strategy-as-Practice and the practice turn more generally has spurred new approaches to the study of cultural processes and spawned new and exciting theoretical conversations.

Perhaps we need to reconsider what it means to ‘contribute to theory’. Davis (2010) focuses on the limitations of theorizing, concentrating on six ‘vintage’ theories, and championing the development of mechanisms-based theorizing (Davis and Marquis, 2005). Hambrick (2007) suggests we lift the requirement to make a ‘contribution to theory’ in at least some domains, but he also suggests these non-theoretical papers should ‘cry out for future research and theorizing’ (p. 1350). In the ‘Information for Contributors’ for the *Academy of Management Discoveries*, they highlight ‘evidence’ and ‘phenomena’ but also note the role of theory to understand underlying ‘mechanisms’ and the role of ‘discovery’ to highlight the boundary conditions or to reject the basic assumptions of a theory. Underlying the rhetoric, we see recognition that theory is indeed important and central to developing knowledge. Yes, theoretical development looks different in different life stages of a theory and there are many variants of theorizing – but we need theory. We have seen graduate students and junior scholars latch onto this ‘anti-theory’ fad and offer anaemic papers without mechanisms or deep understandings of the phenomenon they seek to understand. We believe we need a broad view of theoretical contribution, not a rejection of it. Indeed, regardless of the cleverness of the instrument or natural experiment, empirical work without theory does not feel like progress.

Theory is critical to understanding. We are in favour of understanding mechanisms and processes – we see these as important areas of inquiry. We need ‘ambidextrous’ scholars who both extend existing theory and discover new theoretical insights (Corbett et al., 2014, p. 5). Yet if we report ‘facts’ without engaging a theoretical conversation, how will we learn from each other? We advise doctoral students to find an existing conversation to which they want to contribute because it allows us to learn from and build from each other, but that doesn’t mean that we only seek incremental insights and ignore new theoretical directions – it means only that we document our movements from one set of ideas to another. Finally, we don’t see the call to contribute to theory as something that discourages work in areas of practical interest, like how the personal attributes of CEOs shape firm behaviour (like narcissistic CEOs who take outsized risks;

Chatterjee and Hambrick, 2007, 2011). To hark back to an earlier debate, we need to re-engage in a conversation about what ‘constructing theory’, ‘developing theory’, and ‘theorizing’ looks like (Stinchcombe, 1968; Sutton and Staw, 1995; Weick, 1995) because it seems to us that theorizing is what we see in organizational theory and we should continue to strive for theoretical development as the way to build knowledge and provide insight.

We end with a call for knowledge building. It is clear that our understanding of the emergence and evolution of theory is hampered by our cognitive models of the field and how we categorize papers. Davis’ (2010) assessment of organizational theory in ‘theoretical stalemate’ was supported by his analysis of outdated keywords. Certainly editors and reviewers need to have a broader view of what contributes to theory than what was on the reading list in their own doctoral seminars. To that end, we suggest that as a field we continue to have a conversation about the theories that are developing, and the exciting new ideas that can redirect extant theoretical conversations. We can use some of the technological advances of our time to aid in those efforts. Rather than keywords being updated every half a dozen years by Academy leaders (like us) with their own biases and blind spots, we might consider a regular crowdsourcing effort to allow our keywords to reflect the state of the field. This might resemble more of an EGOS-like track model that is constantly changing on an annual basis as new constellations of scholars convene around different themes. If we find a way to track, and indeed celebrate, the emergence of new theories and theoretical conversations, we will not be trapped by our old mindsets and categories, and the dynamic and flourishing nature of organizational theory can be foregrounded.

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