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## Where Are the Old Theories of Organization? Prospects for Retrospection in Organization Theory

In their *AMR* dialogue contribution, Cornelissen and Durand (2012) discuss issues arising from the 2011 *AMR* Special Topic Forum (STF) "Theory Development: Where Are the New Theories of Organization?" Notably, they ask whether the forms of theorizing produced in re-

sponse represent "more than novelty" and argue for the development of theory that is both interesting and "progressive with explanatory value" (Cornelissen & Durand, 2012: 152–153). We continue this discussion of theoretical novelty by adopting a different tack—making proposals for a philosophically and politically informed *retrospective* appreciation of theory development. We argue that such a strategy will help us to understand theoretical evolution while avoiding problems of theoretical neophilia (Rhodes & Pullen, 2010). Neophilia can focus attention on the immediate problems facing organizations and thereby detract attention from the wider relationship between organizations and society (Stern & Barley, 1996), and it can be ahistorical, or even antihistorical, in emphasis.

In the call for papers for the STF (see the April 2008 issue of *AMR*, pages 569–570), the guest editors' main motivation was the hypothetical assertion of a "growing disaffection" among management academics with the "existing set of theories that dominate the study of organizations." Developing this view, the guest editors proposed that the "mine of organization theory" may be "nearing exhaustion," with this implying a need to "challenge the field" and "renew the stock of theories we currently employ." In response to this scenario, potential contributors were encouraged to produce "*indigenous theories of organization* inspired by contemporary organizational conundrums" (our emphasis), the implication being that this is currently lacking in the field.

One can argue, however, that the STF has been disappointing in that, contrary to expectations, when the list of papers was finalized, the editors had to admit that "few of the manuscripts . . . offered new theories of organization" (Suddaby, Hardy, & Huy, 2011: 236). Instead, the pieces represented "a collective and self-reflective critique of the current state of theory in management research" (2011: 236). We argue that this failure to develop genuinely new theories or theorizing is not surprising, given that the STF was based on a questionable premise. Quoting Hinings (1988: 2), Suddaby et al. propose that organization theory has "developed as a *discipline* in its own right" (our emphasis) and, thus, operates "with its own problematics, theoretical structures and methods" (2011: 237). Hence, Suddaby et al. (2011) argue, once again, that organization theory requires "indigenous"

theory development in order, finally, to sever its "colonial roots" in social science.

We suggest that, on the contrary, this assumption of disciplinarity is erroneous, for organization theory is always empowered primarily by methods and perspectives from the wider social sciences. Predominantly, it is informed by theories and methods from anthropology, economics, psychology, and (especially) sociology. Thus, for many scholars, notions such as indigenous theorizing in organization theory are anathema to the everyday reality of research practice. Such notions are constructions of a spurious drive for native theory development—constructions inspired by a naive normal scientific quest for the carving out of new territories of organizational knowledge. This is the type of enterprise from which neologisms emerge and with which considerable care should be taken (e.g., see comments on institutional work by Green & Li, 2011; Kraatz, 2011; Willmott, 2011).

For us, therefore, when evaluating organizational theorizing, the task at hand should not be to extend the tentacles of neophilia—through promoting, for example, a "clear direction forward" (Suddaby et al., 2011: 236) based on forays into "new theorizing" (see also Daft & Lewin, 1993). It should not even be to bolster the recent trend for reconciliatory theorizing in social science—under, for example, agentic realism, neoinstitutionalism, or structuration theory; for, like others, we feel this project remains tentative. Rather, we would argue for establishing an analysis of the ontological, epistemological, and methodological domains under which organizational theorizing has been developed in the recent *past* and in relation to which significant developments can be understood.

For example, in contrast to the argument that organization theory is currently stuck in a "living museum of the 1970s" (Davis, 2010: 691; quoted in Suddaby et al., 2011: 236), we feel the previous three decades have witnessed profound and revolutionary changes in organizational thinking, notably under the influence of poststructuralism and, more broadly, postmodernism. These movements have promoted a "third order" of theory development, the meta-theoretical assumptions of which can be differentiated qualitatively from those underpinning the traditional first and second orders of social and organizational theorizing: structure and agency (Reed, 1997; see also Burrell & Morgan, 1979).

More generally, we argue that there is not a paucity of stimulating theorizing available to analysts of organization but, rather, a paucity in some elite (frequently North American) management journals. The STF itself, for example, describes how in "OMT" (organization and management theory) the "popularity" of scientific management, institutional, and complexity/chaos theories far exceeds that of discourse, postmodern, or actor-network theories (Oswick, Fleming, & Hanlon, 2011). We note, further, how more overtly critical or political approaches relevant to organizational analysis, such as the new feminisms, postcolonialism, or the autonomism implicit in Oswick et al.'s (2011: 334) own call for "new and radical homegrown OMT theories," fail to even register on their list of theory contributions.

Many of these latter theories, of course, owe their origins to writing in Europe, where their intellectual profile and popularity are arguably much greater (Üsdiken & Pasadeos, 1995). This reflects an environment where organizational research is "commonly associated with macro-oriented, critical, voluntarist and/or processual approaches, with eclecticism, and with receptiveness to alternative paradigmatic perspectives" (Meyer & Boxenbaum, 2010: 738): in other words, receptiveness to theories concerned primarily with *philosophical* and *political* issues of organization and management, rather than to *pragmatist* traditions that combine with the institutional pressures of the U.S. tenure system to lead to the production of increasingly fine distinctions within an established line of research (e.g., the embedded agency paradox within institutional theory).

Rather than make recurrent calls for "new and exciting theories," our view is that leading journals should take an analytically more retrospective and politically broader view when accounting for theory development. This would provide a greater degree of contextual credibility for reflections on the status of organizational theorizing and also recognize, rather than ignore, the health of a range of epistemic communities. Such an approach would see a reevaluation of foundational (Adler, 2009) thinking for assessing the development of theory orders. Instead of pursuing theoretical novelty, the basis for such analysis would be an overview of recent research *paradigms* and, thus, comparison of their fundamental metatheoretical characteristics.

This would represent the type of traditional sociological exercise that, on both sides of the Atlantic, appears to have been thrown all too quickly on "the bonfire of the dualisms" (Law, 1999: 3). In adopting what may appear to be an essentialist (or even "heretical"; see Calás & Smircich, 2003) stance, we would be asking not so much "Where are the new theories of organization?" as "Where are the old?"—and what has happened to them and why?

It can be argued, for example, that the celebrated attrition of ontological/epistemological antonyms (Cunliffe, 2011) and the associated decline in paradigm thinking (Tsoukas & Chia, 2011) are factors that have worked to the detriment of sociological explanation. In contrast, we argue that organizational knowledge *can* and *should* be explained metatheoretically. We would overturn the decline in paradigmatic analysis through promoting a thesis for conceptualizing, philosophically and politically, the evolution of major theory orders. As "disciplinary matrices" comprising a "constellation of commitments," we feel that paradigm communities continue to reproduce assumptions of what represents appropriate professional behavior, with everyday practice reflecting the influence of accepted "exemplars" of research (after Kuhn, 1970; see also Knorr Cetina, 1991, 2010).

Our intention, therefore, is to develop the type of analysis traditionally reserved for sociologically theorizing structure and agency to account for the evolution of the *missing paradigm* in contemporary organizational analysis—post-structuralism. This latter order would be explained, initially, in terms of the philosophical assumptions that underpin its exemplary contributions to OMT. Such theorizing would explain, *inter alia*, the relativism, relationism, and reflexivity that characterize the paradigm's ontological, epistemological, and methodological preferences. Above all, it would allow these metatheoretical assumptions to be compared, directly, with those of other major theory orders.

In line with earlier forms of such theorizing (notably Burrell & Morgan, 1979), this approach would be extended to explore the political assumptions underpinning the research domains that make up a poststructural paradigm. Domains such as actor-network theory, archeogenology, autonomism, deconstructionism, postcolonialism, and poststructural feminism, for example, would be unpacked in terms of their

political preferences (i.e., in relation to traditional dualisms, such as conflict-consensus, critical-normative, or radical-regulatory). Again, this would facilitate a third-order paradigm being classified within—rather than outside of, or after—previous frameworks articulated for explaining the politics of organizational analysis.

Such an approach would thus offer a *relational* representation of theory development in OMT. Contrary to those metaphoric proposals advocating paradigms as static and discrete entities engaged in hostile combat (Jackson & Carter, 1991), our analysis of a triumvirate of theory orders would define them as rational, adaptable, and referential phenomena—akin to communities in “essential tension” (Kuhn, 1977) with one another. Paradigm communities are constantly seeking a balance between “exploitation and exploration” (Knudsen, 2003: 280)—at once working within a framework and trying to transcend it. Whereas a paradigm may be variously opposed to the research philosophies, practices, or politics of a second, it may also be relatively *disposed* to elements of a third, for the intellectual forces can be both divergent and convergent.

For example, the methodologies of one paradigm may directly influence the development of another, as when (poststructural) actor-network theory is influenced by (agentic) ethnomethodology (Law, 1994). On the other hand, (poststructural) autonomism may arguably share more ideological ground with (structural) labor process theory than it does with the “new conservatism” (McNall & Johnson, 1975) of (agentic) phenomenology and symbolic interactionism. In short, we can identify *ideal-type* metatheories for paradigms, but we do not have to define the latter as intellectually static, procedurally uniform, or ideologically preserved. Within sociological paradigms there is always the potential to engage in significant “internal debate” (Deetz, 1996) and for researchers to perform “boundary work” (Willmott, 1993) at the interface with other paradigms.

Above all, rather than speculate or hypothesize on the potential for “new” or “novel” contributions in OMT, such analysis would enable the historical development of paradigms to be traced in a comparative and evolutionary manner. This approach could document how theoretical domains experience a range of fortunes over time: from materializing to make innova-

tive contributions, through operating subsequently in a relatively “steady state,” and ultimately to appearing in decline, decay, or liquidation. Additionally, we may describe how the decline of one theoretical domain and rise of another can be linked in terms of the research agenda being pursued, for the process of evolution may suggest not necessarily the demise of one theory and dawn of another but more a rebranding or relabeling.

In sum, we propose the cultivation of a retrospective approach to evaluating the development of organization and management theories in recent decades. The starting point would be a thesis to explain the philosophical principles on which major OMT paradigms are predicated. By defining the main theory orders of the field, such an analysis is directed against those who would argue that the theoretical structure of OMT has now “turned into ‘paradigm soup’” (Buchanan & Bryman, 2009: 4). In turn, this would form the basis of a broad theoretical investigation outlining the composition and evolution of constituent research domains, one that accounts for their various political preferences.

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## A Heart-Mind-Opportunity Nexus: Distinguishing Social Entrepreneurship for Entrepreneurs

I congratulate Miller, Grimes, McMullen, and Vogus on their *compassionate* theoretical piece on social entrepreneurship. They certainly “noticed, felt, and responded to the pain” (Clark, 1997) of a lack of theory in that important subfield, where “enthusiasm has outpaced conceptual development” (Miller et al., 2012: 35). Given that the purpose of their article was “to hone in on the role of compassion in encouraging” social entrepreneurship (2012: 11), they only focused on one very specific part of the “whole elephant.” The purpose of my commentary is to complement their work by suggesting alternative approaches to building social entrepreneurship theory that will be effective in advancing that subfield as well as the larger entrepreneurship domain. I formulate the alternatives by highlighting concerns arising from the choices made in their article, including their choices of explanatory variable, of the type of explanatory variable, and of the level modeled.

While the explanatory variable of compassion is certainly worthy of research in management, it is a questionable choice in a study of social entrepreneurship, for several reasons: it is a poorly distinguished concept because it is only a “borderline emotion” (Lazarus, 1991: 827), it overlaps heavily with more basic concepts like empathy, and it can be fleeting. It can involve many negative consequences that Miller et al.