

## BUILDING THEORY ABOUT THEORY BUILDING: WHAT CONSTITUTES A THEORETICAL CONTRIBUTION?

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We distill existing literature on theoretical contribution into two dimensions, originality (incremental or revelatory) and utility (scientific or practical). We argue for a revision in the way scholars approach the utility dimension by calling for a view of theorizing that would enable theories with more “scope” (both scientific and practical utility). We also argue for an orientation toward “prescience” as a way of achieving scope and fulfilling our scholarly role of facilitating organizational and societal adaptiveness.

Theory is the currency of our scholarly realm, even if there are some misgivings about a possible overemphasis on theory building in organization and management studies (Hambrick, 2007). Every top-tier management journal requires a “theoretical contribution” before a manuscript will be considered for publication. This tenet is perhaps most strongly felt in this journal, the Academy of Management’s premier conceptual journal (and also, not inconsequentially, the most cited journal in organization studies [based on *Web of Science® Journal Citation Reports®* data for 2009]). Consistent with this concern, the *Academy of Management Review (AMR)* has published two special issues dedicated to theory building (1989, issue 4, and 1999, issue 4) and numerous “Editor’s Comments” dedicated to trying to articulate what constitutes either theory (e.g., Brief, 2003; Conlon, 2002) or a theoretical contribution (e.g., Kilduff, 2006; Whetten, 1990). These writings, however, despite their thoughtfulness, do not represent comprehensive treatments, especially of the latter issue, and do not seem to have hit the mark in a way that provides a satisfactory

resolution to the crucial question of what makes for a theoretical contribution. Thus, scholars are still trying to articulate what it means to make a theoretical contribution (Bartunek, Rynes, & Ireland, 2006; Kilduff, 2006; Rindova, 2008; Smith & Hitt, 2005).

A question that typically arises at this point is “What is theory?” Although there are many answers to this question, there is little agreement on a universal definition—to wit, “Lack of consensus on exactly what theory is may explain why it is so difficult to develop strong theory in the behavioral sciences” (Sutton & Staw, 1995: 372). For our purposes we use a simple, general definition: theory is a statement of concepts and their interrelationships that shows how and/or why a phenomenon occurs (cf. Gioia & Pitre, 1990). We believe, however, that a more productive question to ask, and for us to address, is “What is a theoretical contribution?” That is, what signifies a significant theoretical (as opposed to an empirical or a methodological) advancement in our understanding of a phenomenon?

Part of the difficulty in delineating the elusive concept of theoretical contribution is that organization and management studies is an eclectic field—and one with multiple stakeholders as well. Not only do we self-identify as “borrowers” from many other scientific disciplines (e.g., psychology, sociology, economics, etc.) but we also claim to speak to both academics and practitioners. This medley of foundations, voices, and au-

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diences often creates confusion when discussing contributions. What exactly is the consensual basis for claiming and assessing theoretical contribution? More pointedly, is there a consensual basis for declaring whether a theoretical contribution exists? One basis for arguing that our field has vague or inadequate standards for assessing theoretical contribution arises from comparing the list of *AMR*'s Best Articles (awarded one year after publication) with the list of each year's most cited *AMR* paper over the same years (based on *Web of Science*® *Social Sciences Citation Index*® and/or Google Scholar counts). In only four of the last eighteen years does the paper currently most cited also turn out to be the one chosen as the *AMR* Best Article for its year of publication (see Table 1), despite the "competitive advantage" in citations such articles have from their publicity for winning the award in the first place. This somewhat disconcerting statistic seems to imply some discrepancy in assessing a paper's value right after publication and its value in the future.

As scholars familiar with the practice of developing theoretical contributions, we believe the time is right for our field to turn a reflective lens on itself and try to establish more clearly not only what currently constitutes a theoretical contribution but also, and perhaps more important, what should constitute a theoretical contribution in the future. To help accomplish the first and set the stage for the second, we established two goals for this paper. First, we hope to contribute to the practice of making a contribution to theory as it currently stands. Toward this end, we provide a synthesis of the dimensions currently used to justify the existence of a theoretical contribution and provide some perspective on the usefulness of these dimensions. Our synthesis reveals two dimensions—*originality* and *utility*—that currently dominate considerations of theoretical contribution. We also note two subcategories underlying each of these main dimensions, which provide a more nuanced description of the current craft of contributing to theory.

Second, we hope to contribute to what we might call the theory of theoretical contribution—to build theory about theory building, if you will. Thus, we use our synthesis of the literature, as well as our reading of *AMR*'s Best Articles and most cited papers listed in Table 1, as

a point of departure for outlining the need for a renewed and reframed emphasis on practice-oriented utility as a focus for future theorizing. In addition, we call for and encourage organization scholars to adopt an orientation toward prescience in their theorizing. We define prescience as the process of discerning or anticipating what we need to know and, equally important, of influencing the intellectual framing and dialogue about what we need to know. An orientation toward prescience holds some promise for advancing our craft of theory development, as well as enhancing the receptivity of the audiences for our developing theories beyond the academy and, therefore, conferring a greater potential for influencing the organizations and societies we study.

We structure the rest of the article around these two issues. We begin with a synthesis of the theoretical contribution literature as it pertains to the field of management and organization studies, highlighting the current state of the art for making a contribution to theory in our top-tier management journals. Building on this synthesis, we then argue that a practice perspective on theory building would lead to our theories having greater scope and, furthermore, that an orientation toward prescience would enhance the value and impact of our theoretical contributions. Finally, we discuss implications of our proposals for the field.

### SYNTHESIZING CURRENT VIEWS ON "WHAT CONSTITUTES A THEORETICAL CONTRIBUTION"

Despite the relatively short time that organization and management studies has existed as a field, there is actually quite a collection of writings on the notion of theoretical contribution. We use the term *writings* because much of this literature is found in editorial statements published in *AMR* and *AMJ*. Although there are a number of other sources outside *AMR* that provide opinion and insight, because *AMR* is our field's premier theory journal, much of what follows stems from these writings about making a contribution to theory. In its inaugural (1976) issue, for instance, *AMR*'s "Suggestions for Contributors" provided a vision for the types of papers the Academy of Management's leadership saw as most appealing for the new journal:

**TABLE 1**  
**AMR Best Articles and Most Cited Papers: 1990–2008 (Citation Counts As of April 2010)**

Year	Props	Volume (Issue)	Author(s)	Google	SSCI	Rank
1990	Best Article	15(2)	Dollinger	79	16	23rd
	Most cited	15(1)	Reed & DeFillippi	1424	455	
1991	Best Article	16(1)	Oliver	2213	650	1st
	Most cited		Same			
1992	Best Article	17(4)	Treviño	113	70	19th
	Most cited	17(2)	Gist & Mitchell	1030	255	
1993	Best Article	18(4)	Pfeffer	604	305	4th
	Most cited (Google)	18(2)	Woodman, Sawyer, & Griffin	920	311	
	Most cited (Social Sciences Citation Index)	18(4)	Cordes & Dougherty	866	343	
1994	Best Article	19(1)	Kahn & Kram	49	20	23rd
	Most cited	19(1)	Ring & Van de Ven	2247	636	
1995	Best Article	20(3)	Van de Ven & Poole	1132	308	4th
	Most cited	20(3)	Mayer, Davis, & Schoorman	4014	1234	
1996	Best Article	21(1)	Chen	468	161	10th
	Most cited	21(1)	Lumpkin & Dess	1662	407	
1997	Best Article	22(2)	Gresov & Drazin	172	54	20th
	Most cited	22(4)	Mitchell, Agle, & Wood	2152	499	
1998	Best Article	23(2)	Nahapiet & Ghoshal	4106	1080	1st
	Most cited		Same			
1999	Best Article	24(1)	McGrath	462	151	8th
	Most cited	24(3)	Crossan, Lane, & White	1272	306	
2000	Best Article	25(4)	Lewis	317	86	7th
	Most cited	25(1)	Hogg & Terry	897	266	
2001	Best Article	26(4)	Mitchell & James	147	88	13th
	Most cited	26(1)	Priem & Butler	992	264	
2002	Best Article	27(1)	Adler & Kwon	1916	450	1st
	Most cited		Same			
2003	Best Article	28(2)	Benner & Tushman	555	180	1st
	Most cited		Same			
2004	Best Article	29(1)	Biggart & Delbridge	77	30	12th
	Most cited	29(4)	Phillips, Lawrence, & Hardy	291	96	
2005	Best Article	30(1)	Ferraro, Pfeffer, & Sutton	300	100	2nd
	Most cited	30(1)	Inkpen & Tsang	459	143	
2006	Best Article	31(2)	George, Chattopadhyay, Sitkin, & Barden	44	21	25th
	Most cited	31(2)	Johns	244	116	
2007	Best Article	32(1)	Dane & Pratt	103	36	7th
	Most cited	32(1)	Sirmon, Hitt, & Ireland	197	57	
2008	Best Article	33(4)	Birkinshaw, Hamel, & Mol	41	7	5th
	Most cited	33(2)	Matten & Moon	93	25	

The Review publishes distinguished original manuscripts which (a) move theoretical conceptualization forward in the field of management, and/or (b) indicate new theoretical linkages that

have rich potential for theory and research in management, and (c) provide clear implications of theory for problem-solving in administrative and organizational situations.

This statement emphasizes notions of advancing knowledge and moving the field's thinking forward, providing new connections among previous concepts, and exploring the practical implications of these connections. Interestingly, some thirty-five years later, a review of writings on theoretical contribution in *AMR* paints a picture fairly consistent with this initial vision. Our synthesis of the literature indicated two key dimensions along which the notion of "value-added contribution" is typically defined, as captured succinctly by Kilduff: "Theory papers succeed if they offer important [read useful] and original ideas" (2006: 252). Our assessment of the current state of the art for publishing theory, perhaps especially in *AMR*, indicates that the idea of contribution rests largely on the ability to provide *original insight* into a phenomenon by advancing knowledge in a way that is deemed to have *utility* or usefulness for some purpose.

Given the historical progression of our field, the prominence of these two criteria (originality and utility) is perhaps not that surprising. Organization and management studies started out with a couple of distinctive attributes: reliance on anecdotal evidence from business practice (there was little "scientifically" codified knowledge) and eclecticism (we borrowed from everybody). Before we could establish ourselves as a distinctive, scientifically rigorous field, we had to deal with both of these "shortcomings." First, beginning in the 1960s and as a result of two reports critical of business education at the time (Gordon & Howell, 1959; Pierson, 1959), business schools (including management departments) began to emphasize more rigorous, quantitative orientations in an attempt to gain legitimacy within the larger academic community. By assimilating traditional academic standards and norms of knowledge generation and dissemination from the physical and social sciences, this transformation helped move our field away from the criticism that management and organizational knowledge was mainly a collection of anecdotes and case studies. Second, at about the time of the founding of *AMR* in 1976, management departments began to pride themselves on having become the academic repository of multidisciplinary knowledge oriented toward managerial practice—management study became a focal point for the intersection of psychology, sociology, anthropology, economics, political

science, and so on. The product of this intersection represented an attempt not only to assemble insightful thinking to develop a more systematic approach to knowledge about management (an attempt that some claim failed; see Pfeffer, 1993) but also to develop some distinctive domains of theorizing (e.g., decision making, leadership, institutional theory).

Since the 1970s, the field of organization and management studies has achieved some notable and laudable accomplishments. We have, for instance, become much more conceptually and empirically rigorous (such that the nature of management theorizing is much stronger, in scholarly terms, than it was only a generation ago), which has helped allay concerns that our knowledge base is too anecdotal. We have also begun to develop an institutional field identity that is no longer based mainly on the notion of eclecticism; that is, the top management journals are now seeing work that constitutes original organizational theorizing (not merely recycled and dressed up psychology or economics), which, in a recursive fashion, is now beginning to influence the very fields from which we once borrowed (Agarwal & Hoetker, 2007).

To better understand what the dimensions of originality and utility mean for those attempting to publish in the top management journals, it is necessary to explicate each notion in more depth. Interestingly, our synthesis of the literature indicates that both dimensions further divide into two subcategories (see Figure 1). Originality can be categorized as either (1) advancing understanding incrementally or (2)

**FIGURE 1**  
**Current Dimensions for Theoretical Contribution**

Originality	Revelatory	4	1
	Incremental	3	2
		Practically useful	Scientifically useful
		Utility	



advancing understanding in a way that provides some form of revelation, whereas the utility dimension parses into (1) practically useful and (2) scientifically useful. Our following discussion of these two main dimensions and their two subcategories is quite consistent with a common characteristic of many theoretical papers—the ubiquitous  $2 \times 2$  matrix.

### Originality

Perhaps more than any other criterion, originality stands out as the “dimension de rigueur” for *AMR* reviewers and editors. This is clearly seen in the first line of the current *AMR* mission statement—“The mission of the *Academy of Management Review (AMR)* is to publish new theoretical insights that advance our understanding of management and organizations”—as well as in editorial statements describing preferences for submissions: “The mission of a theory-development journal is to challenge and extend existing knowledge” (Whetten, 1989: 491); “We judge the value-added contribution of each and every article based on the potential contribution of the articulated new insights” (Smith, 1997: 8); and “[All *AMR* papers should] improve our understanding of management and organizations, whether by offering a critical redirection of existing views or by offering an entirely new point of view on phenomena” (Conlon, 2002: 489). Conlon’s distinction between extending current understanding and offering “entirely new points of view” is similar to Huff’s (1999) distinction between contributing to a current conversation and starting a new conversation. Accordingly, it provides the basis for discussing originality as representing either an incremental or a more revelatory or surprising advance in understanding—a subdivision that offers a more nuanced grasp of what reviewers and editors currently consider to be a theoretical contribution worthy of publication.

**Incremental insight.** The notion that theoretical contributions should progressively advance our understanding is a long-venerated one (Kaplan, 1964) and is well-represented in *AMR*’s (1976) initial statement of purpose (reread the original “Suggestions for Contributors” above). This orientation has been consistently and repeatedly invoked over the years—for example, “The ultimate value-added test of an article is that it has moved scholars in a field or advanced

our theoretical understanding” (Smith, 1997: 7). In perhaps one of the most recognized articles on theory, Van de Ven reiterates Lewin’s (1951: 486) assertion that “nothing is quite so practical as a good theory” in this way: “Good theory is practical precisely because it advances knowledge in a scientific discipline, guides research toward crucial questions, and enlightens the profession of management” (1989: 486). Based on the (then) groundbreaking work of Dubin (1978), this perspective is rooted in the belief that what makes one theory preferred over another is advancement toward “what is believed to be true” (1978: 13) or a state where “a group of people sharing an interest in some set of observations come to agree that one theoretical model best provides understanding or permits accurate predictions about the observational set” (1978: 13). In practice, this is often reflected in the idea that “theoretical insights come from demonstrating how the addition of a new variable significantly alters our understanding of the phenomena by reorganizing our causal maps” (Whetten, 1989: 493).

The key to connecting this notion of advancing understanding with the editor’s/reviewers’ crucial task of determining value-added contribution arises in how one defines “significantly” in the above statement—how much additional understanding must be provided to meet the significance criterion? Obviously, this is not an easy question to answer, which may help explain why the incremental perspective on insight seems to have fallen out of favor with reviewers and editors of many top journals over the past ten to fifteen years. In fact, reading more recent editorial statements on theory development from a spectrum of management journals conveys the sense that the advancing incremental understanding perspective has become rather too closely associated with the notion of minor, marginal, or even trivial improvements, where small advances in our thinking about a phenomenon provide the means to progress through “normal science” (Kuhn, 1962). Although incremental improvement is arguably a necessary aspect of organizational research, especially in service to the contextualization of theory (cf. Johns, 2001; Rousseau & Fried, 2001; Tsui, 2006), current thinking at *AMR* and other top theory and research outlets seems to have shifted to a focus on theoretical contributions

deemed to be more revelatory and nonobvious to organizational scholars.

**Revelatory insight.** An alternative to the difficult-to-answer question "How significant an advance in knowledge is needed to constitute a contribution?" rests in the idea that contribution arises when theory reveals what we otherwise had not seen, known, or conceived. In other words, new theory "allows us to see profoundly, imaginatively, unconventionally into phenomena we thought we understood. . . . theory is of no use unless it initially surprises—that is, changes perceptions" (Mintzberg, 2005: 361). Often cited in advocacy for this perspective is Davis's (1971) "That's Interesting!" article, in which he argues that research that is novel or counterintuitive or that questions assumptions underlying the prevailing theory (as well as those of the reader) will generally be seen as more interesting and, thus, more likely to make an impact on the reader (and hence make an impact on the field through increased citations): "The best way to make a name for oneself in an intellectual discipline is to be interesting—denying the assumed, while affirming the unanticipated" (1971: 343). The key distinction here from the advancing understanding incrementally perspective (rooted in Dubin's views on truth) is that "a theorist is considered great, not because his/her theories are [necessarily] true, but because they are interesting" (Davis, 1971: 309).

Beginning in the 1990s and rapidly ascending to prominence since, this idea that "all contributions to *AMR* should be 'novel, insightful, and carefully crafted'" (Brief, 2003: 7) has become a staple for editorial descriptions of desired *AMR* papers—"The challenge then for authors published in *AMR* will be to convince *AMR* readers 'to see nature in a different way,' even to violate *paradigm-induced expectations*" (Smith, 1997: 8; see also Gioia & Pitre, 1990, and Morgan & Smircich, 1980)—and is even boldly proclaimed on the current *AMR* website ("*AMR* publishes novel, insightful and carefully crafted conceptual articles that challenge conventional wisdom concerning all aspects of organizations and their role in society"). This refocusing on surprise has transcended *AMR* and is now touted as the basis for contribution at the other top organization and management journals, including *AMJ*, as seen in Rynes' articulation of reasons for paper rejection: "Reviewers are judging the results not

against prior literature, but rather against common sense or the likely reactions of the 'person in the street.' . . . 'Is this really surprising?'" (2002: 312) and Bergh's likening of theory contribution to the rareness criterion of competitive advantage: "[Is] a contribution . . . surprising and unexpected? Is the contribution more of a common sense derivation, or does it represent a novel and unique insight? Originality is a critical concern" (2003: 136).

Our synthesis of the existing literature thus points to insight based in original, especially revelatory, surprising, or even transformative thinking as a key factor affecting the attribution of a theoretical contribution at many of the eminent journals in organization study, and perhaps particularly at *AMR*. Therefore, our point in highlighting this shift is not that incremental advancements cannot provide a theoretical contribution, but simply that many editorial teams and reviewers at our top journals now prefer insights that reveal a new way of understanding as the basis for determining theoretical contributions, and thus as a preference for assessing publication potential. Importantly, however, the capacity for revelation is not the only criterion; also widely accepted is the sense that to be deemed a contribution, theory must be useful or somehow have utility in its application, either for other organizational researchers or for practicing managers.

## Utility

Although most contemporary editors see revelatory insight that discloses a new way of seeing as necessary for a value-added contribution, it is rarely sufficient; the insight must be seen as useful as well. That is, it must have the potential to either "improve the current research practice of informed scholars" (Whetten, 1990: 581) or improve the current managerial practice of organizational practitioners. Or, as Van de Ven argues:

A central mission of scholars and educators in professional schools of management . . . is to conduct research that contributes knowledge to a scientific discipline, on the one hand, and to apply that knowledge to the practice of management as a profession, on the other (1989: 486).

In general, *scientific utility* is perceived as an advance that improves conceptual rigor or the

specificity of an idea and/or enhances its potential to be operationalized and tested:

Theory can advance science by providing cohesion, efficiency, and structure to our research questions and design (Kerlinger, 1973; Van de Ven, 1989). In a very practical sense, good theory helps identify what factors should be studied and how and why they are related. A high quality theory also states the conditions and boundaries of relationships (Hitt & Smith, 2005: 2).

Alternatively, *practical utility* is seen as arising when theory can be directly applied to the problems practicing managers and other organizational practitioners face, or as Hambrick suggests, through "the observation of real-life phenomena, not from 'scholars struggling to find holes in the literature'" (2005: 124). Hambrick's larger point, however, is that such a practical problem focus is a good way to develop theory per se. Thus, theory directed at practical importance would focus on prescriptions for structuring and organizing around a phenomenon and less on how science can further delineate or understand the phenomenon.

This bifurcated view on the usefulness of theory arises directly from the trajectory of our field's history; traditionally pushed to produce insight for both the professional and academic realms, management scholars experience pressures to enact the norms of a bona fide scientific discipline (again, originating from the 1959 Pierson and Gordon and Howell reports) while also—at least ostensibly—speaking to and helping improve the organizations we study. We say "ostensibly" because our review of the literature reveals a very strong bias toward scientific usefulness as the driving factor in editorial thinking about theoretical contribution. Interestingly, however, these two "discrete" categories of utility are not necessarily mutually exclusive; there are examples of theoretical advancements that have the requisite capacity to improve both the practice of research and the practice of management, with a necessary "translation" from the scientific to the professional (as evident in the original mission of the journal *Academy of Management Executive*—"to provide a bridge or a link among theory, research and practice"—and the research-oriented subsections of various practitioner-oriented journals). Yet the reality is that, excepting some passionate calls for change during Academy of Management presidential addresses (e.g., Hambrick, 1994; Huff,

2000), practical utility's role in theoretical contribution seems to receive mainly lip service (e.g., several paragraphs on "practical implications" in the discussion section; see Bartunek & Rynes, 2010, for insight into the utility of these sections), especially in recent times. Most of the editorial statements advocating the practical utility of organization and management theories occur earlier in our field's history, with more recent editorial statements virtually ignoring this aspect of the utility dimension.

### Implications of the Originality and Utility Dimensions

The upshot of our synthesis of the literature, then, is the recognition that the current state of the art for developing conceptual papers that are deemed to provide a theoretical contribution rests in a scholar's ability to produce thinking that is original (and especially revelatory or surprising) in its insight and useful (preferably in a scientific manner) in its application. In our view, the originality and utility dimensions usually are treated as working together to produce varying levels of theoretical contribution. Modeling their interaction in a  $2 \times 2$  matrix (Figure 1) produces a basic insight about which papers ultimately succeed at the top management journals (and, by implication, at *AMR*). Papers that display both original, revelatory insight and scientific usefulness (Quadrant 1) clearly stand out as most likely to pass muster with editors and reviewers (assuming they also satisfy other desirable criteria<sup>1</sup>). Papers that only fit one of the dimensions well—scientifically useful but without adequate originality (Quadrant 2) or revelatory insight without adequate scientific usefulness (Quadrant 4)—present a challenge to both author and editor and usually must undergo significant revision, at minimum, if they are to be seen as making a significant theoretical contribution and, thus, become acceptable for publication. Finally, papers that score low on both dimensions (Quadrant 3) are likely to be desk-rejected or receive a firm rejection decision after the first round of reviews (and occasionally

<sup>1</sup> As Whetten outlines, "Is the paper well written? Does it flow logically? Are the central ideas easily accessed? Is it enjoyable to read? Is the paper long enough to cover the subject but short enough to be interesting?" (1989: 495).

what one of the authors calls an "RWS"—a "Rejection With Scorn").

Ultimately, our synthesis of the theoretical contribution literature and its representation in our simple  $2 \times 2$  model lead to several observations about the current practice of making a contribution to theory. First, the dimension of original insight with the power to reveal a new way of seeing is likely to remain a perennial norm at all elite, theory-driven management journals, including *AMR*. We feel confident in making this less-than-bold prediction because, by its very nature, theorizing depends on new concepts to maintain idea vitality in the field. Incremental advances in theory simply cannot produce this energy and, although necessary in their own right, are unlikely ever to become the foundation on which a high-impact theory-building journal rests. Second, utility in the form of scientific usefulness also is likely to remain an important aspect of the norms for theoretical contribution for the foreseeable future, simply because scientifically useful ideas are critical to the larger project of establishing theory that is conceptually rigorous and internally consistent and, thus, the surest path to building and maintaining academic legitimacy. Additionally, scientifically useful ideas do a better job of contributing to the theoretical developments of other scholars who are also working to further legitimize the scholarly field of organization and management studies.

That said, after our attempt to synthesize the existing literature about attributes that constitute a contribution to theory, we were uneasy with simply accepting what the (mostly editorial) literature portrayed as a theoretical contribution. Part of this unease arose from our reading of both the *AMR* Best Articles and most cited papers listed in Table 1, which revealed that although the majority of these papers could be classified as being original in their ideas, those that provided revelatory insights did so not so much by introducing new concepts (as the typical editorial depiction would have it) but much more often by offering a novel approach to integrating prior thought and research into some model or framework that constituted a different way of understanding some phenomenon.<sup>2</sup> More

important, we also began to notice a subtle theme distinguishing the most cited papers from the Best Articles: although both sets of papers could be considered high in scientific utility, the most cited papers could more often also be characterized as higher in utility for practice. If articles receiving the most citations do a better job of bridging scientific and practical usefulness, this suggests the possibility of a unifying dimension that can account theoretically for both scientific and pragmatic value (i.e., a notion we would label *scope*). This also suggests the possibility of expanding our conceptualization of what constitutes a theoretical contribution if we can identify ways of enhancing our ability to achieve more *scope* in our theorizing.

### CONTRIBUTING TO THEORY ABOUT MAKING A THEORETICAL CONTRIBUTION

Adhering to the same standards for consideration we just reviewed, we believe there is room for a contribution in the process of building some new theory about the work of theory building itself—an unaddressed opportunity for our field to take the next step forward in maturity by extending our conceptions of what makes for a theoretical contribution. There are two considerations that suggest a strong need to move from where we are now to a more advanced plane of thought and wider influence. First, Katz and Kahn (1966) observed that organizations and institutions can (and often do) have multiple, disparate roles in society. Academia as an institution—and perhaps especially in the form of a professional academic institution like a school of management—has a dual and somewhat paradoxical role: we are the "keepers of the flame" in disseminating tried-and-true ideas and practices (usually via teaching but also via consulting), which Katz and Kahn term a *maintenance* role—a role that has the effect of structuring, stabilizing, and institutionalizing knowledge

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sion because we were trying to summarize and synthesize the content of prior writings by scholars discussing "What is a contribution?" Instead, after reading the Best Articles and most cited *AMR* papers, what we actually saw in practice was a dimension that is better categorized as incremental or revelatory, because relatively few of even these stellar papers could be labeled as transformative in the literal sense. On balance, most of these *AMR* articles are original in the way they *integrate*, rather than in the way they *create*, new concepts.

<sup>2</sup> In fact, we initially used the categories "incremental" and "transformative" to characterize the originality dimen-



and action; simultaneously, however, management schools are also charged with being in the vanguard in generating new knowledge (via cutting-edge theory and research) that serves to question received wisdom and accepted practice and, thus, to undermine "the way we do things around here." Katz and Kahn (1966) term this an *adaptive* role in society, and it serves as a substrate for transforming the social order. From our perspective, we are currently devoting much more energy to our maintenance role (via teaching,<sup>3</sup> if not practice-oriented research) and underplaying our putative adaptive role. Organization and management science has, therefore, come up short in fulfilling the charge of being on the leading edge of management thinking. Indeed, most of the new ideas in management that have been put into practice have come from the world of practice, rather than from academia (cf. Barley, Meyer, & Gash, 1988). Consequently, society has granted us respect, but not much influence (see Mintzberg, 2004, and Pfeffer, 1993). We believe our field has matured to a point that the time has come to begin rectifying that shortcoming.

As our retrospective on management and organizational theorizing suggests, the dimension of utility has historically been defined according to two tracks—scientific usefulness and practical usefulness—with a relative neglect of the latter. This neglect is somewhat understandable because practical utility is most often considered as addressing specific problems without necessarily tapping general principles, whereas good theory emphasizes generalities (with appropriate boundary conditions identified). On these grounds we clearly have assigned more importance to scientific utility, which, although understandable, is perhaps something of a misconstrual for a scholarly field that is so intimately associated with a professional practice domain (and schools of management are arguably more akin to schools of law or medicine than those of economics or sociology).

Second, the fact that rigorous scholarship is not often deemed by outsiders to be relevant scholarship that both ministers to and antici-

pates societal needs suggests that we have not devoted appropriate attention to the expectation that good scholarship should not only have practical relevance but *also* some degree of foresight in identifying important coming issues and problems that need to be conceptualized. One source of the problem is that management scholars have, in effect, created a closed industry engaged in producing knowledge intended mainly for other academic knowledge producers. This outcome is a consequence of overemphasizing scientific importance (in part because scientific importance tends to generate specialized language that only other cognoscenti can understand; see Bennis & O'Toole, 2005).

Simply put, we believe that theoretical contributions in management and organization studies have not done an adequate job of anticipating the important conceptual, as well as practical, needs of society's now most prominent members—business and social organizations. As Staw outlined the problem for OB research some twenty-five years ago: "The field may not have even served managerial interests well, since research has taken a short-term problem focus rather than having formulated new forms of organization that do not currently exist" (1984: 629)—an orientation that implies that we as a field are also guilty of employing short-term thinking, which Norman Lear has called "the social disease of our time" (cited in Bennis, 2003: 15) and which undermines our ability to contribute to organizational and societal adaptiveness. This is a nontrivial shortcoming in our approaches to making theoretical contributions that could have a much wider impact—a shortcoming we believe could be addressed by encouraging an orientation toward prescience in trying to anticipate, conceptualize, and influence significant future problem domains. We next consider these two related deficiencies in our theorizing as avenues for better fulfilling our adaptive role in society.

### Relevance to Practice As a Prominent Dimension of Theoretical Contribution

As implied above, the maturation of our discipline has seemingly moved us farther away from the very spheres of society we originally set out to influence. Consider the following anecdotes, each of which in its own way is quite telling about the current character of our theo-

<sup>3</sup> We acknowledge that many instructors work to include new knowledge into their courses, yet the practical fact is that the overwhelming content of most courses constitutes accepted theory and practice.

retical enterprise. The first story goes that in the early 1980s one of the officers of the Academy of Management concluded that as one consequence of the shift toward conceptual and empirical rigor then sweeping the field, the relevance of our theories to managerial practice was waning. As a step toward redressing this trend, he decided that our field needed to "connect" better with practicing executives. To that end, he hosted a number of high-level managers at the next annual AOM meeting. He shepherded the entourage around to various sessions, symposia, and social events, but they were not very forthcoming about their impressions of the proceedings. At the end of the day, the hosting professor, who could contain himself no longer, explicitly asked what they thought of the work of the academic management profession as manifested by the events of the day. Uncomfortable silence. One of the executives thought it over for a few pregnant seconds, however, and then said simply, "You people talk funny."

A second anecdote involves one of the authors, who was discussing the criteria for Ph.D. student selection with his colleagues. There was little debate about the relevance of GMAT/GRE scores, grade point averages, experience with the research process, and so forth. Yet when he suggested that because it was a professional school organizational experience should be considered as well, another professor responded that practitioners were "not our audience" and that work experience had not been shown to correlate with Ph.D. program success and, therefore, should not be included on the list of desirable criteria.

A final example hints at the difficulty of reconciling academic and practitioner assessments of theoretical contribution and value. In 1985 Cummings and Frost edited a volume entitled *Publishing in the Organizational Sciences*, a compendium of writings, opinions, and advice about what it took to succeed when trying to publish in management journals. Early in that volume an academic writer touted the marvelous contribution offered by Gronn's (1983) article "Talk As Work." Curiously, later in the same volume a practitioner writer independently derided the same work as trivial and obvious to any practicing manager.

**The roots of our disconnect from practical utility.** From a layperson or practitioner perspec-

tive, we academics do indeed "talk funny." From our point of view, we do so for good reasons. Parsimonious theoretical language saves time and space and (we hope) enhances clarity for other scholars; theoretical notions tend to be specified in abstract terms that capture conceptual meaning. Yet we should recognize that our specialized language tends to distance us from the issues that generated the theories about the phenomena we are trying to describe and explain in the first place. Put differently, our distal language often seems to elide the relevance of our second-order theoretical constructs from the proximal parties whose experience we are trying to explicate.<sup>4</sup>

Worse, perhaps, the increasing distance between our theories and their practice referents is sometimes justified on the grounds that theory and practice are "different worlds" and need not have a close relationship (as implied by the Ph.D. admissions debate). The difference of opinion about the worth of a given academic article highlights the common view that the subjects of our theorizing do not seem to attribute much significance or value to our theorizing—a nontrivial issue if we hope to enact an influential stance toward our adaptive role in society. Shapiro, Kirkman, and Courtney (2007) argue that there are actually two sources for this "talking past each other" dynamic between scholars and practitioners, which they term the *lost before translation problem* (ideas that are essentially irrelevant to practice even before theories are formulated or studies are conducted) and the *lost in translation problem* (difficulty in explaining the relevance of theories or findings to practice).

Relevance to practice is actually a longstanding theme in our writings about theory and theoretical contribution (Bergh, 2003; Hambrick, 1994; Thomas & Tymon, 1982), especially in terms of empirical research (see the rigor versus relevance debate in Gulati, 2007; Palmer, Dick, & Freiburger, 2009; Polzer, Gulati, Khurana, & Tushman, 2009; and Tushman & O'Reilly, 2007, for examples). Yet evidence that we have paid serious attention to this de facto platitude in our theoretical efforts is woefully lacking. Our rela-

<sup>4</sup> Consider the last two sentences. To illustrate our point, we wrote the first in plain English and the second in academese.

tive inattention to a bona fide concern with the practical applicability of our theories has led to a troubling disconnect between management theory and practice (Ghoshal, 2005; Mintzberg, 2005; Pfeffer, 1993)—an observation that gives rise to a consequential question: How might we accomplish a reconciliation of academic and practitioner standards for judging contribution?

First, it is important to reiterate that our theories should be *problem driven*<sup>5</sup>—that is, in some fashion addressing a problem of direct, indirect, or long-linked relevance to practice, rather than narrowly addressing the (theoretical) “problem” of finding the next mediator or moderator variable or filling theoretical gaps simply because they exist. When we focus mainly on the latter, we end up advancing theory for theory’s sake, rather than theory for utility’s sake. Simply put, the focal problems in our chosen field of work (organization study) should relate more directly to the wider world’s work (organizational practice) by drawing more from the world of practice and the experience of real people, rather than from abstract derivations of hypothetical formulations. This, ideally, would lead to an integration of the scientific and practical utility dimensions and would produce a comprehensive utility dimension better aligned with the notion of scope.<sup>6</sup> As an example, one of the *AMR* Best Articles that also was that year’s most cited paper (and, not coincidentally, the most cited paper on Google Scholar of all those in our *AMR* sample) provides a good exemplar of theorizing that has scope. Nahapiet and Ghoshal (1998) provided theorizing having both scientific and practical utility that deals with a topic of interest for a broad audience and integrates existing views into a coherent and comprehensive theoretical model.

Second, and as already noted, we should embrace the fact that we are a profession (academia) studying another profession (management), so our orientation toward theoretical contribution should include an explicit appreciation for applicability. We want to be clear,

however, that we are not arguing that we should take our lead for new theorizing from practitioners’ specifications of mundane managerial problems—unless, of course, they are manifestations of wider or deeper issues. We should instead be aspiring to address significant problem domains that either require or will soon require theorizing. As Mintzberg (2005) points out, methodological (and even theoretical) rigor often interferes with both insight and relevance, perhaps especially when dealing with grand-level issues or problems. Bennis and O’Toole (2005) have accused us of delimiting the scope of our studies only to those variables we can easily measure, producing a kind of “methodolatry” that harbors the paradoxical possibility of blinding rather than illuminating things that really matter. The fact that Bennis and O’Toole’s article appeared in *Harvard Business Review*, whose readership is heavily oriented toward reflective practitioners and who on reading the article would further dismiss the relevance of the academic study of organizations, only deepens the hole out of which we must climb.

**Practical versus practice-oriented theoretical contribution.** In our view, one of the inhibitors of the relevance of our theorizing is that we have been treating the notion of pragmatic utility as practical utility. Based on conversations with colleagues who are reticent to formulate practical theory or conduct practical research, we suspect that there simply is not enough professional “gravitas” associated with theory that might have obvious practical application. Such ideas are not seen as “big enough” for grand-level theorizing. A small shift in orientation might make a big difference in the contribution of our theories to addressing important problems, however. Focusing on the weightier notion of *practice* (with its pedigree in the deep philosophical traditions of American pragmatism) would not only signal to scholars that we are working on significant issues but would provide a firm intellectual basis for theoretical formulations with pragmatic relevance.

The scholarly roots of a practice approach can be traced to James (1907, 1909) and Dewey (1938; Dewey & Bentley, 1949), who viewed theoretical knowledge not as an “object” to be possessed but as a dynamic phenomenon that manifests in the act of knowing something. The move from objectified, static knowledge to dynamic processes of knowing shifts the focus of theorizing

<sup>5</sup> Ideally, theories should also be *opportunity driven*—that is, anticipating opportunities for enlightened practice, in a fashion perhaps best exemplified by the positive organization studies (POS) movement (e.g., Cameron, Dutton, & Quinn, 2003).

<sup>6</sup> Thanks to both Don Hambrick and one of our reviewers for discussions relevant to the notion of scope.

to the activities that people engage in while dealing with problems. In this view the act of knowing influences what is known and how it is known (see also Morgan, 1983, and his discussion of "research as engagement"). Knowledge, therefore, inheres in the activities that individuals engage in to deal with their day-to-day interactions. This pragmatist philosophical orientation has had some influence on organizational scholarship. Pentland (1992), Cook and Brown (1999), Brown and Duguid (2001), Orlikowski (2002), and Nag, Corley, and Gioia (2007), among others, all have studied connections between the content knowledge of organization members (i.e., what they know) and their social practices (i.e., what they do). Cook and Brown (1999) treat practices as actions informed by meanings grounded in specific contexts. In this fashion, knowledge is viewed more as a recursive dialogue between practice (action) and meanings (cognition).

It is only a small leap from these works, which focus on people at work, to treating ourselves (as theorists) in a similar fashion as we examine our theorizing practices and our assumptions about what constitutes a contribution. In this light it becomes apparent that we should conduct a more intimate dialogue between practice (actions of practitioners) and meanings (theoretical contributions that both derive from and inform practice). A practice view of theoretical knowledge also connotes a significant shift because focusing on theoretical knowledge as somehow independent of its pragmatics overlooks the processes through which knowledge use, value, and utility emerge "from the ongoing and situated actions of organizational members as they engage the world" (Orlikowski, 2002: 249).

The most important insight from a practice orientation concerning the assessment of theoretical contribution is that theoretical knowledge does not exist as a set of theory-building rules independent of actual practice; rather, it becomes inextricably intertwined with the manifestations of the theoretical knowledge in practice (and vice versa). The two key notions arising from adopting a practice view of knowledge, then, are (1) that knowledge should be treated as process and (2) that the production of knowledge should be treated as a recursive dialogue between theorists and reflective practitioners. The upshot of this orientation is that merely holding

theoretical knowledge contributes little if that knowledge is not exhibited in organizational practice and does not affect practices other than our own theorizing practices. In addition, however, widely influential theorizing should also hold the potential for trying to identify domains that will soon be in need of theorizing (e.g., virtual societies, sustainability, accelerating globalization, new models of management not based on the preeminence of economic factors).

### **An Orientation Toward Prescience to Increase the Utility of Our Theoretical Contributions**

On what level might scholars most influence practice? As noted, because our theoretical work is aimed at more general formulations, it is often removed from direct application to particular problems. For that reason our potential for influence in practical domains is often limited. If we want to have more influence on society's conversations and better fulfill our adaptive role in society, we also should direct our energies and capabilities at focusing on the future of managing and organizing. Such an observation suggests that we should try to theorize about incipient organizational, managerial, and societal issues and problems—that is, we should work to develop what we term *theoretical prescience*. Prescience is most often defined in terms of foreknowledge, foresight, or forecasting of events. For the purposes of better executing our adaptive role, however, prescience involves *anticipating* and *influencing* the type of managerial knowledge needed to deal with coming societal and organizational concerns. More specifically, theoretical prescience can be defined as *the process of discerning what we need to know and influencing the intellectual framing of what we need to know to enlighten both academic and reflective practitioner domains*. In our view, prescient scholarship not only fulfills the usual role of supplying conceptual knowledge used almost exclusively in scholar-to-scholar communiqués but also anticipates the conversations both scholars and societal leaders should be having and influences the framing of those conversations in conceptual terms.

**On theoretical prescience.** Abraham Lincoln is often credited with the observation that "the best way to predict the future is to create it." Computer scientist Alan Kay famously articulated a small adaptation of this pithy statement



in posing that “the best way to predict the future is to invent it.” We would suggest a modest variation on this theme by noting that perhaps the best way to predict the future is to influence the conversation about what it could or should be. This orientation includes but goes beyond the notion of detecting “weak signals” (Shoemaker & Day, 2009) and discerning where trends are likely to lead—or, as Bennis has put it, “It’s not as important to know where the puck is now as to know where it will be” (2003: 194; citing hockey star Wayne Gretzky). Prescience, in our view, involves not only sensitivity toward developing trends but acting to influence those trends via prospective sensemaking (Gioia, Corley, & Fabbri, 2002; Weick, 1979, 1995) and sensegiving (Gioia & Chittipeddi, 1991; Maitlis & Lawrence, 2007)—in other words, giving meaning to ambiguous informational cues and articulating viable interpretations and actions to cope with coming organizational and environmental demands.

In terms of broadening the scope of our theoretical contributions, then, we do not view prescience as a matter of “predicting the future” or gazing at a crystal ball. Prescience in our terms is a matter of anticipating and, more important, influencing the definition of significant organizational problem domains to illuminate an important area for consideration—that is, drawing attention to areas we need to understand from a theoretical point of view that have relevance for significant organizational and societal issues and problems—as opposed to trying to predict the next big theory from a purely intellectual viewpoint and, thus, increasing the likelihood of creating the next managerial fad (see Abrahamson, 1991). An orientation toward prescience accentuates the notion that we in academia *should be* leading-edge thinkers—we *should be* become more oriented toward advancing not only the field’s relevance to future scholarship but also the field’s relevance to reflective practice concerning problems that matter (Pfeffer, 1993; Schön, 1983).

For instance, it is apparent that sustainability is an important issue to theorize about for the foreseeable future. Sustainability is currently viewed as “atheoretical,” so it becomes apparent that prescient theoretical work should be devoted to issues of sustainability and other concepts in its nomological net (e.g., implications of global climate change for organizing and organization [Gjeltén, 2009] and employee

and leadership issues arising from the economic shifts accompanying green organizing and green firms). Or consider the emergence of do-it-yourself (DIY) manufacturing (Anderson, 2010), a rising phenomenon involving virtual teams that develop open source plans for objects such as circuit boards, custom tools, furniture, and even cars, which are then outsourced to a small batch manufacturer who produces a prototype and offers to set up production for a set price (if it is not intended to be a one-off project). These small, extremely flexible, and surprisingly profitable companies represent a new form of organizing better suited for dealing with an era where transaction costs become almost nonexistent because of the potential for distributed innovation in our digitally interconnected society (Lakhani & Panetta, 2007).

Other current examples of nonobvious organizing (e.g., Linux, Wikipedia, Grameen Bank) suggest that our theorizing has done an inadequate job of anticipating and accounting for these kinds of organizations, a problem likely to be magnified in the future if we do not encourage some form of prescience in our theorizing. The implications for organizational theories, from the micro (e.g., psychological contracts, compensation/motivation, leadership) to the macro (e.g., organizing, institutional change) and strategic (competitive dynamics, agency theory), could be significant. Similarly, one could imagine prescient management theory touching on issues ranging from demographic shifts in society (affecting our OB and HR domains) to social changes arising from technological advances (e.g., value of privacy, virtual societies, artificial intelligence) to strategic/institutional changes arising from trends in local and national governance.

Because the current view of what constitutes a contribution is oriented toward making judgments mainly on the basis of conceptual originality and utility—where utility is rather narrowly defined as useful for further (scientific) concept development—we believe our scholarly community needs to try to develop an orientation toward prescience to make informed inferences about what will be important to know and to conceptualize. To broaden the scope of our influence in making theoretical contributions, management scholars need to more credibly enact our adaptive role by generating theory that anticipates problem domains that will inform

future thought and action.<sup>7</sup> Incorporating a future orientation can help keep our theories not only vibrant but also relevant in a constantly changing landscape of organizational realities. Additionally, we believe that over time, as prescience becomes more prominent in our theoretical formulations, the distinctions between scientifically useful and practically useful will begin to blur.

**Attributes of prescient scholarship.** Although we recognize that not all organizational/managerial scholars will (or should) try to adopt or practice prescient theorizing, we nonetheless think it is important for the field to encourage those interested in attempting to build prescient theory. Toward this end, we identify two key attributes that would be hallmarks of prescient scholarship.

First and foremost, prescient theorizing would direct attention toward future problem domains. Prescient scholars would first attend to cues in the present in a way that suggests that some consequential issues or problems might need to be addressed by management scholars in the future. This orientation does not involve some sort of academic clairvoyance but, rather, a focus on what we might term *projective futurism*. As an example of such an orientation, climatology data indicate that global temperatures are within historical fluctuations, so we cannot objectively assert that the globe is warming. Yet because of rising levels of various greenhouse gases and so forth we have sound *theoretical* bases for arguing and predicting that climate change is a threat and will soon be demonstrably out of the envelope of normal fluctuations and, therefore, that prudent action is necessary now. Those actions are likely to have implications for organizing differently, and we should try to be anticipating possible theoretical implications. The same could be said for advances in artificial intelligence and the impact they will have on workforce decisions and strategic planning in organizations of the future, or changes in privacy law following in the wake of popular social networking technologies. Likewise, pay-

ing attention to popular press and news sources that report on cutting-edge business can provide early insight into new phenomena (like the DIY manufacturing example) that serve as a harbinger of emerging trends. Similar logic applies to our existing theories of organizing and managing; a prescient theorist would ask, "What is it about current models that are insufficient to account for likely future trends such that we need different or embellished theories to account for reasonable projections?"

Prescient scholars would, therefore, develop an orientation toward *prospecction*—they would assume the role of making informed projections about coming issues, act as if those issues have manifested, and then infer what theoretical domains need attention or invention. The military, for instance, has noted that traditional orientations and actions have involved informed *retrospection*, which has enabled it to better fight the last war. The military's new orientation is to ask, "In what ways can we prospectively imagine how to better fight *the next war*?" The traditional retrospective sensemaking orientation (wherein we ask ourselves what consequences our actions have had, leading to a better understanding of the past) inevitably produces refinements in current ways of thinking. Organization studies have been mostly retrospectively oriented, which has had the consequence of fulfilling mainly our "maintenance" role in society. We would benefit from adopting a notion of *prospective sensemaking* (Gioia et al., 2002), even if it is usually a variation on the theme of *retrospective sensemaking* (Weick, 1979)—that is, casting ourselves figuratively into the future and acting as if events have already occurred and then making "retrospective" sense of those imagined events.

Second, and equally important, prescient theorizing involves a focus on *sensegiving*. It is not enough simply to refine our prospective sensemaking abilities. It is also important to articulate the sense made in a way that affects the character of both academic and reflective practitioner discussions, as well as the spectrum of possible actions that ensue from exercising influence over the framing and tenor of the discussions—that is, we need to become more adept at *sensegiving* (i.e., Gioia & Chittipeddi, 1991; Maitlis & Lawrence, 2007). It is in this domain that we can better fulfill our adaptive role in society. Doing so implies more attention not only

<sup>7</sup> We are not advocating that all or even most theoretical efforts should aim for prescience, or that prescience should become a key dimension for manuscript evaluation. We are suggesting, however, that an orientation toward prescience should become a not uncommon hallmark associated with some of our best theorizing.

to new ideas but ideas that have a more proximal relationship to practice than has been our tradition. This means finding ways to communicate our sense made (i.e., our theoretical contributions) such that those most likely to find our work pragmatically useful (i.e., thoughtful practitioners) understand it and are motivated to apply it.

Consider an endeavor that is parallel to the task of trying to gauge contribution—trying to assess “creativity.” Creativity in business is different from creativity in the arts. Creativity in the arts is usually defined mainly in terms of novelty (Amabile, 1996). In business, however, creativity has two key dimensions: novelty and pragmatic value (Ford & Gioia, 1995). We would argue that we should be encouraging both of these dimensions in trying to formulate contributions to theory that have wider scope; in other words, we should be offering not just original or revelatory insights but new insights that are valuable for advancing ideas with a praxis dimension—melding two of Aristotle’s three kinds of knowledge, *theoria* (theory) and *praxis* (practice)—in a way that practice informs theory, and vice versa. As we have noted above, historical writings in organization studies have acknowledged these orthogonal dimensions implicitly (i.e., in the form of the originality and utility criteria), but the *de facto* outcome has been notably more attention to the originality criterion. We believe an orientation toward prescience would enhance our potential for grander scope in making theoretical contributions.

## DISCUSSION

Precisely what constitutes a theoretical contribution in organization and management studies is a vexing question that cannot be answered definitively, although it does seem to have a conventional answer—for example, “The notion of contribution—like many other abstract concepts, such as quality or truth—is somewhat subjective and can only be assessed in the context of each unique manuscript” (Rynes, 2002: 311). Our initial approach to addressing this question was to review the existing literature and provide a synthesis of current views. That synthesis suggests that theoretical contribution presently has two germane dimensions, originality (classified as either incremental or revelatory) and utility (scientific and/or pragmatic

usefulness), with a strong preference in the scholarly community for works that are revelatory/surprising and carry mainly scientific value. We have summarized the current idiom for declaring a theoretical contribution in Figure 1, which provides a framework for scholars to assess whether an idea will be seen as making a theoretical contribution.

If we take a more critical and expansive view of our scholarly potential and ask whether it is possible to make the grand notion of “theory” more pertinent to solving significant managerial and societal problems, we might ask the more pointed question, “What would constitute a theoretical contribution beyond that engendered by the currently accepted criteria—that is, a contribution to theory that is relevant not only for scholar-to-scholar consumption but one with greater scope and potential to influence current and future organizational practice?” We agree with Whetten’s assertion that “the mission of a theory-development journal is to challenge and extend existing knowledge” (1989: 491). Yet we would encourage a conceptualization of an even more robust theory, one that has greater reach and would enable organization studies to realize its potential as both thought leader and practice leader in the wider society. We take seriously Katz and Kahn’s (1966) observation that organizations and institutions can be viewed as fulfilling various societal roles, and we hold to the ideal that academia is charged with the responsibility of fulfilling both maintenance and adaptive roles in society. Our assessment of the current criteria for judging theoretical contribution, however, has led us to conclude that we have not enacted our adaptive role as well as we could. For that reason our approach to making a contribution to theory in a paper about theoretical contribution is to suggest a renewed and recursive consideration of the practice element for theory development, in part by suggesting that authors adopt an orientation toward prescience as an avenue not only to improve our relevance to practice but also to enhance our influence in society.

Huff (1999) memorably characterized research and writing as a series of multidimensional conversations. To get the most out of each of these conversations, she counseled that we scholars consider four guiding questions. Our concern with importance for practice, foresight, and sensegiving has implications for each of these

questions: (1) "Which conversations should I participate in?" (those that will make the greatest difference for science *and* practice); (2) "Who are the important 'conversants'?" (scholars *and* practitioners); (3) "What are scholars talking about now?" (answer this question mainly for the purpose of discerning a point of departure for tomorrow's question); and (4) "What are the most interesting things I can add to the conversation?" (those that direct conversation toward what needs to be known for the present *and* the future).

If we attend to the overarching message conveyed by the relevance of practice and pre-science to our theorizing, it encourages us as theorists to raise our own awareness of the limitations of our historical ways of thinking and suggests viable grounds for reconsidering how we might think more broadly about our most important activity—generating new theory with scope (i.e., theory that contains both scientific and pragmatic usefulness). Thinking in such terms also encourages us to be a bit more intellectually adventurous. As Mintzberg has noted, "If there is no generalizing beyond the data, no theory. No theory, no insight. And if no insight, why do research?" (2005: 361). We would add that if theory does not have the potential for foresight and for changing the conversation, why do theory building? Our charge, then, is to become more expansive in our theorizing, to work to infuse our theorizing with significance for practice—present and future—and to "give sense" to wider communities within society about the relevance of our theoretical work.

### On Practice

If we embrace the idea that we are scholars charged with the responsibility of generating and disseminating *useful* knowledge, then the notion of utility simply must extend beyond the implicitly accepted idea that "useful for theory development" is the sole criterion of consequence and begin to account for relevance to practice in a much more substantive manner. Kilduff notes that "the route to good theory leads not through gaps in the literature but through an engagement with problems in the world" (2006: 252). This sort of observation means that we must begin to "choose our theories according to how useful they are, not how true they are" (Mintzberg, 2005: 356). Our current tradition has

tended to turn us into a closed community conducting a dialogue with ourselves (Miller, Greenwood, & Prakash, 2009), which implies that we have unwittingly focused some of the best minds in the world on a sphere too small for their capacity for greater contribution. We cannot be a closed community if our intent is to influence current and future organization studies and practice (which should be our intent, given our adaptive role in society).

We are a professional field (Adler, 2006; Pfeffer & Fong, 2002). Rather than downplaying the importance of pragmatic contribution, we should celebrate it by formally bringing that dimension more prominently into our judgment structures and processes. We need to focus more on contributions of a grander scope by including utility for practice in our assessments of theory. To date, our most influential (and most highly cited) theories (e.g., macrolevel theories, such as new institutional theory, resource dependence theory, contingency theory, agency theory, and transaction cost theory; meso theories, such as social capital and organizational identity and organizational learning; and microlevel theories, such as equity theory, procedural justice theory, goal theory, and prospect theory) have been formulated for scholars but nonetheless have major pragmatic implications that expand their scope (see Adler & Kwon, 2002; Benner & Tushman, 2003; Nahapiet & Ghoshal, 1998; Oliver, 1991; Reed & DeFillippi, 1990; and Ring & Van de Ven, 1994, for AMR articles we believe exemplify this notion of scope). How do we get such scope? By thinking in a more encompassing fashion about what we ought to be doing as theorists. One avenue for doing so is to pursue a practice view of knowledge generation (i.e., theory development) in the venerable philosophical tradition of American pragmatism.

Our arguments in this paper resonate with those in a recent spate of articles focused on the issue of balancing methodological rigor and practical relevance in empirical management research (see the "Editor's Forum" in volume 50, issue 4, of the *Academy of Management Journal* and the "Editor's Choice" section in volume 18, issue 4, of the *Journal of Management Inquiry*). All address the general question of how business schools can better "pursue fundamental understanding of phenomena with the goal of tackling major real-world problems" (Tushman & O'Reilly, 2007: 769). Unfortunately, an implicit



assumption in much of this debate appears to be that scholarly journals that favor theoretical development eschew pragmatic relevance—a belief seemingly based in the peculiar assumption that theoretical contribution and pragmatic relevance are somehow incompatible (see Tushman & O'Reilly, 2007, for an exception). They are not; theoretical contribution and pragmatic relevance clearly can and sometimes do work hand in hand. There is nothing about the nature of either theory or practice that prevents them from being served simultaneously (cf. Gulati, 2007). Ultimately, we believe the dimensions of originality and utility/scope can come together to produce theories that make a difference for science *and* practice. The question is simply whether we as an academic discipline can change our own practices to better account for the issues and problems central to the organizations we study. We believe that achieving scope in our theorizing should be an institutional goal for our field.

### On Prescience

Those from outside academia reading our theoretical work might reasonably conclude that we have a rather impoverished view of what constitutes a contribution if we emphasize mainly clever insights and intellectual usefulness. As Polzer et al. note, "The failure of business school research to either anticipate or deeply understand some of the most fundamental challenges of our times threatens the legitimacy of our enterprise" (2009: 280). In a turbulent era of organizational ambiguity and complexity, a key task is to try to be prescient about what is important to theorize about—and such prescience will undoubtedly lead us to focus on problem domains with significant import for future practice. Relatedly, Kilduff, citing Lakatos (1970), notes that one of the "important criteria for evaluating theory is the extent to which it runs ahead of existing empirical research in terms of alerting us to research opportunities hitherto unanticipated" (2006: 252–253). To this observation we would add that our theorizing should not just be running ahead of empirical research but, rather, should be anticipating coming conceptual domains in need of theory and research.

Hambrick, in his critique of our field's overemphasis on theory, agrees with Helfat (2007) in

proposing that we begin by generating practical facts "that can inform us as to what we need a theory for" (2007: 1349). Of course, we theorists are not necessarily attuned to the complex dynamics of the organizational world, and we could use some help in picking up the weak signals (Shoemaker & Day, 2009) that suggest areas ripe for theoretical development. Penn State's Smeal College of Business publishes a periodic "Early Indications" electronic newsletter written by a practitioner-academic who keeps his antennae tuned to the business environment. Similarly, the School of Information Science and Technology's "Institute for Global Prescience" is run by a "Professor of Practice" (similar to the thirty-person foresight staff at Mercedes-Benz's Society and Technology Research Group). There also are organizations that are attuned to future trends, such as the Institute for the Future ([ifff.org](http://ifff.org)), the McKinsey Global Institute ([mckinsey.com/mgi](http://mckinsey.com/mgi)), and Deloitte's Center for the Edge (Deloitte, 2009). Each of these early-warning systems signals issue and problem domains in need of conceptual development and raises the possibility that even the way we structure ourselves to generate and disseminate theory may need to change.

Our view of prescience, however, encourages scholars to become not only early sensemakers but also early sensegivers—that is, not only to see the coming wave but to attempt to shape the conceptual conversation by influencing the premises on which the conversation is predicated (Simon, 1959, 1991). Framing matters for creating influence, but we as a field have drawn the boundaries of our intended influence so narrowly that we have unintentionally abdicated our societal leadership responsibilities. A focus on foresight also represents a way of helping to achieve societal influence without the polemics implied in Pfeffer's (1993) *AMR* article (a Best Article), which essentially suggests that we should dispense with multiparadigm approaches to organization study so that we can achieve the kind of paradigm consensus associated with other fields that ostensibly have been more successful at garnering resources. An orientation toward prescience implies that regardless of theoretical approach, so long as that approach is attuned to identifying or anticipating theoretically and pragmatically relevant future problem domains, the desired societal influence is more likely to follow. An orientation toward or

at least a sensitivity to prescience as an attribute of theoretical contribution does not merely constitute an addition to a "theory of theoretical contribution"; it also changes the way we think about our traditional notions of contribution itself. Admittedly, this orientation makes the practice of theory generation more challenging because it forces us out of our intellectual comfort zone, but then maturation (even as a field) is never an easy undertaking.

### Implications for Our Practice

Miller (2007) refers to the "straightjacket" many top-tier journals put on authors that restricts the notion of contribution to "topics that fit neatly within today's popular theories and allow the development and tweaking of those theories" (Miller et al., 2009: 278). DiMaggio (1995) notes the extent to which the "cultural resonance" of theories affects their receptivity (i.e., the degree to which a given theory aligns with the cultural beliefs of the time and of the scholarly audience for the theory). As gatekeepers for the most desired and valued outcome in our academic pursuits (published articles), journal editors will obviously have decisive influence on the likelihood any change will be implemented. If the editorial teams of our top-tier journals continue to reward only those papers demonstrating theoretical contributions that are scientifically but not pragmatically useful, little will change in the way authors practice the development of theory. We have experienced this in our own attempts to focus our theory development on the pragmatic aspects of a phenomenon, as evidenced by the following reviewer comment concerning one of the authors' recent manuscripts that attempted a melding of academic and practitioner voices in the paper: "Your narrative at times sounded like a practitioner rendition." Although our author's reply attempted to explain (and perhaps educate) the reviewer on the value of a pragmatic aspect to theory building, it remains to be seen whether this reviewer will be convinced of the wisdom of trying to bring convergence to academic and practitioner views.

If, however, journal editors become more open to contributions that demonstrate originality and scope (scientific + pragmatic usefulness) and encourage these dimensions in guidelines to authors and reviewers, then reviewers will begin to shift how they assess theoretical contribution. Authors will then be rewarded for de-

veloping more pragmatically useful (and perhaps even prescient) theoretical contributions to match the shifting expectations of journals. If we do not soon change our scholarly traditions in ways that enhance theoretical relevance to practice and our sensegiving potential to the wider audiences, then we will continue to underperform our adaptive role in society and condemn ourselves to increasing irrelevance and diminishing influence in describing, explaining, understanding, and improving organizations and their management.

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