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TENT POLES, TRIBALISM, AND BOUNDARY SPANNING: THE RIGOR-RELEVANCE DEBATE IN MANAGEMENT RESEARCH

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A long-standing debate among management scholars concerns the rigor, or methodological soundness, of our research versus its relevance to managers. This issue has evolved into an "either/or" argument in which specific studies, researchers, journals, and even institutions or programs are quickly categorized into "silos." I argue that this debate is largely socially constructed by forces both internal and external to business schools. Further, it is perpetuated by tribes that form around rigor and relevance, sequestering themselves into closed loops of scholarship and dismissing the work of outsiders on the basis of their inclusion—or exclusion—of theory or of practical applications. Exclusionary behavior even occurs within the two tribes, especially in the rigor camp. Along with the prevention of productive cross-tribe dialogues, consequences of the conflict include the exclusion of boundary spanners. I offer here a normative model of managerial research with recommendations for bridging the artificial rigor-relevance divide through problem-oriented research grounded in theory.

As in most socially constructed debates, in the rigor-relevance dispute the definitions of the issues in question are controversial. Per Webster's dictionary, relevance is "relation to the matter at hand" or "practical and especially social applicability" (www.m-w.com/dictionary/). At first glance, this definition is not antithetical to that of "rigor": "strict precision . . . exactness." Indeed, I would assume that most management researchers prefer research to be both applicable and exact, relevant and rigorous. But the debate between seemingly separate camps of relevance and rigor suggests otherwise.

Hambrick, in a presidential address to the Academy of Management, alluded to one consequence of this gulf when he noted "our failure to present ourselves—our body of knowledge and our perspective—to the world of affairs" and to operating in an "incestuous, closed loop of scholarship" (1994: 15). With rhetoric such as this, it is no surprise that we management scholars would begin to doubt our work's meaning. Other AOM presidents have echoed this theme. For instance, Bartunek (2003) expressed her "dream" of reframing the "dichotomies" (between rigor and relevance) as "tensions and dualities" to be overcome through mutual appreciation for the disparate types of research that exist under our large tent. Other observers have also contributed to this debate. In a recent article, a business journalist wrote, "The thing that makes modern management theory so painful to read isn't usually the dearth of reliable empirical data. It's that maddening papal infallibility. Oh sure, there are a few pearls of insight, and one or two stories about hero-CEOs that can hook you like bad popcorn. But the rest is just inane" (Stewart, 2006).

At the core of this doubt is our field's artificial polarization of rigor and relevance. Let me extend Hambrick's argument: the rigor and relevance tribes close each other out of their research and their forums for sharing it, and they do this even at an intratribe level, with compartmentalized factions within each group. As my tone suggests, I firmly believe that the either/or debate is moot: our goal should be to seek rigor *and* relevance through boundary-spanning research focused squarely on phenomena of interest to managers. Below, I make the case that a gap between rigor and relevance is socially constructed by reviewing its historical trajectory and discussing its underlying dynamics. I discuss ways to foster boundary spanning, grounding my argument in the work of both the earliest architects of the social sciences and more recent observers of management research.

A BRIEF HISTORY OF THE DEBATE

What is especially surprising to me is how chronic the hand-wringing has been, with how lit-

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tle consequence. It is fascinating to note that the rigor-relevance debate dates back many decades.[1] The soul-searching in business schools initially centered on the question of what types of research were most appropriate for such programs to conduct. The question first arose in the 1950s and '60s, with multiple voices suggesting that management research was aspiring to be relevant at the expense of the rigor observed in other social sciences (e.g., Gordon & Howell, 1959; Pierson, 1959). A movement within business schools toward a more discipline-based, rigorous investigation replete with sophisticated data collection methods and quantitative analyses was a direct response to these criticisms. Amidst the cries for rigor, however, were the voices of those who cautioned that we should not overlook the balance between rigor and relevance. Simon, for instance, in a chapter called "The Business School: A Problem in Organizational Design," noted that management programs should "develop new knowledge that may be relevant to improving the operation of business" (1965: 335), and as such must be made attractive to scientists with access to the "real world as a generator of basic research problems and a source of data" (1965: 341). He argued that "knowledge and action in the social sciences is an intrinsic relation, such that the two cannot be easily separated" (1965: 466).

Over the next two decades, the pendulum swung toward research that was labeled as more rigorous; by the 1980s, some suggested that it had swung too far, potentially obscuring the relevance of management scholarship (Porter & McKibben, 1988). Beyer wrote that "increasing numbers of organizational scholars have begun to express concern that organizational/administrative science has had little effect on life in organizations" (1982: 588). Miner (1984) analyzed over 30 established organizational theories and concluded they were generally of little importance, validity, and usefulness. The debate simmered after that but didn't evaporate, and today it is hotter than ever. The rigorous research tribe and the applicable findings tribe each operates with little contact or synergy with the other, leading to what some scholars have called the "knowledge failure problem" (McKelvey, 2006). Two root causes for this disconnect between research and practice have been identified (Van de Ven & Johnson, 2006). The first is the "knowledge transfer problem," or the inability or unwillingness of academic researchers to translate their insights for practitioners. The second is the view that rigorous and relevant research represent distinct types of knowledge, each with its own ontology and epistemology, making them difficult to integrate.

More recently, the identity conflict regarding the kind of researchers we wish to be in business schools has been considerably intensified by the larger question of whether management programs add value in the first place. Critics question the relevance of what we teach business students as well as the meaning of the research performed at business schools. This debate has mounted to the point that business schools today are arguably in a crisis with an existential question at its heart: Who are we, really, and what are we here to do? (e.g., Khurana, 2007; Mintzberg, 1996; Mintzberg & Gosling, 2002; Pfeffer & Fong, 2002). For example, Leavitt argued that MBA curricula transform students into "critters with lopsided brains, icy hearts, and shrunken souls" (1989: 39), implying that academic rigor wrings the life, or relevance, from what students learn. Going even further, the late Sumantra Ghoshal argued that "many of the worst excesses of recent management practices have their roots in a set of ideas that have emerged from business school academics," and he cited the Enron and Tyco scandals as examples (2005: 75). Although Ghoshal did not explicitly blame the swing toward rigor for these ills, an assumption underlying his claim was that many of the theories taught in business schools come from narrowly specified social science domains removed from the world of practice.

Even high-level university administrators have joined the chorus of discontent. Richard West, a former dean of New York University's Graduate School of Business, described management research as "fuzzy, irrelevant, and pretentious" (Gaddis, 2000: 55), seemingly concerned with an absence of both rigor and relevance, and the former dean of the Yale School of Management, Jeff Garten, proclaimed: "The current model of business school education needs to change dramatically. . . . What business schools need to do is add some criteria for [faculty] promotion. One of them should be some real-world experience, in the same way that a doctor teaching at a medical school would have had to see patients" (Holstein, 2005: 13).

Garten's concern speaks to the rigor-relevance debate, implying that management research and education have swung fully to the rigor side. As Hambrick (1994) suggested, a theme that has bubbled up regularly over the last 20 years is that we management researchers have sequestered our-

[1] For a detailed account of the historical oscillations that business schools have gone through on the rigor to relevance spectrum, please see Khurana (2007: Chapter 7).

selves in our closed loop—or tribe—of rigor, gleefully discarding relevance. The normative conclusion that follows is that we should trade some degree of rigor for greater relevance. The long roots of the either/or debate can be seen here: To gain more of one, we must lose some of the other, in an ongoing zero-sum game.

Yet reality is much more complex, as either/or in this case is a false dichotomy that in most instances is socially constructed. Nevertheless, many business school academics have avidly pursued the debate, separating ourselves into two tribes on either side of a chasm of sometimes brutal identity warfare. We presume that someone writing for practitioners is, as if by definition, producing work that is not rigorous and, similarly, that someone writing primarily for other scholars is producing work of limited practical consequence. Not surprisingly, our identities form around these projections and expectations, so that we are either "serious scholars" or "management types," and these stereotypes are then extended to the journals in which we publish our research, with some labeled as "scholarly," others as "practitioner-oriented"—and nothing in between. Social construction of polar professional identities and social construction of polar rigor and relevance debates thus reinforce one another.

The debates have persisted for several decades without substantive action. But the voices of change have become more insistent, as Hambrick's (1994), Bartunek's (2003), and other leaders' clarion calls show. Perhaps, then, we are on the cusp of a social movement from words to action, from rhetoric to reality.

TRIBAL WARFARE AND BOUNDARY SPANNERS

Causes of Tribalism

It is unfortunate how easily we management scholars label each others' research enterprises, often with little evidence. Let's start with rigor. I believe the confusion regarding it is tremendous. Sometimes scholars define rigor as use of a narrow disciplinary paradigm involving a set of theories, methodologies, and data analyses that they themselves would use. One established instance of this tension, at the intersection between economics and sociology, is a perceived gap between qualitative field-based research and elegant economic models (Hirsch, Michaels, & Friedman, 1987). More recently tribes have formed around specific theories and methods within each of these disciplines as well, and similar tribes appear in business schools. A related offshoot of this debate has been the schism between qualitative and quantitative research. With a dramatic shift in our field toward quantitative research in the last several decades, qualitative researchers increasingly find their work subjected to the rigor test by skeptics belonging to a different tribe that is inclined to a different set of methods.

Another dynamic within the rigor-relevance conflict concerns the dependent variables selected for study. Here again, researchers often fail to acknowledge our biases or differences in taste, framing our judgments of others' work as based on a criterion of rigor. On one side are those whose primary interest is explaining social order in markets and organizations, which translates into examining the antecedents of the behavior of individuals and of organizations—looking at why, how, and under what constraints these entities act. On the other side are those who probe the effects of behavior on the performance of individuals, organizations, and markets. Both research foci have import for theory and practice, but unfortunately the internecine wars persist here as well, with most researchers honing in on either behavior or performance and few considering both simultaneously. Instead of a productive dialogue and boundary-spanning efforts to link these two questions, researchers within the rigor tribe end up in paradigmatic wars that question the legitimacy of each set of questions.

I argue that the use of simple heuristics and stereotypes to define rigor has resulted in (1) a broad conflict between management researcher tribes labeled as rigorous and relevant and (2) subtribe paradigm clashes within these two groups, particularly in the rigor camp, based on researchers' identifying themselves with specific theoretical and methodological paradigms, which in turn discourages cross-disciplinary and cross-method research. At the roots of these identity clashes is an inherent duality in business school researchers: most of us are social scientists employed by professional schools. As scientists, we focus on theory building and rigorous empirical work; as business school teachers, we seek to advance managerial practice and share some conceptual products of this endeavor in the classroom. This bifurcation of identity is an outgrowth of the move in the 1960s to bring more rigor to management programs.

The entry of discipline-based researchers into management studies has been a welcome addition, forcing us to embrace theory building as a cumulative enterprise and to create what could be the foundation of a more integrated group effort.[2] It has also forced more genuine rigor in our methodologies and analytic tools. Unfortunately, the focus on disciplines has also strengthened core conflicts. Identities, by and large, are shaped around scholars' disciplines (and subgroups within them). People identify with narrow theory domains, along with specific methods and data analyses. Those become the hallmarks of rigor for their users, and those who use them best or most often are celebrated. Gathering within the theoretical, methodological, and analytical fence posts that mark their tribal boundaries, tribe members ignore those outside, no matter how connected their work might be. In this dynamic, each academic is forced into membership in a tribe and/or subtribe to survive in the field of management.

Arbiters of rigor and "good taste" have also arisen in our field and have had a strong influence on which researchers are celebrated through publications, citations, awards, and other recognition. This covertly tribal behavior may be masked as impartiality. It can be exemplified by those in ostensibly influential roles or by an invisible politburo that builds and just as quickly destroys reputations. The weapon of choice of these arbiters? The granting of a badge of honor or shame (i.e, "rigorous" or "nonrigorous") to a given body of work, regardless of its impact on research or practice.

Effects of Tribalism

What is most unfortunate about "rigor" and "relevance" is that they stimulate a debate completely antithetical to a productive dialogue on how management researchers can embark on a more truly synergistic research enterprise, one in which we might perform work that is rigorous and relevant at once. People are too busy dismissing highly relevant works as without rigor, when in reality some of them may merely be outside their paradigms of choice, use a method outside their comfort zones, or simply come from the wrong tribe. Similarly, those interested in managerial practice studiously ignore academic research that may have direct import for practice merely because it was not published in a preferred outlet or might not have made its practical implications sufficiently explicit. We begin to view these two sets of endeavors and the individuals involved in them as the proverbial oil and water—no matter how close you bring them, they will never truly merge. Thus, failing a productive dialogue, we haven't given the notion of integration, no matter how idealistic, a chance.

Another consequence of the field's inter- and intratribal conflicts is that boundary spanners get shut out. This is especially disturbing because much of innovation, especially in maturing intellectual landscapes, takes place at the intersections among fields and subfields. The pursuit of narrowly defined endeavors for highly circumscribed audiences has some merit (e.g., it allows a researcher to develop and focus on a few core strengths and establish a strong intellectual identity among a well-defined peer group), but also a serious downside: we often find ourselves saying essentially the same things in slightly different ways, rather than generating original ideas—which often result from connecting disparate theories and/or concepts.

In their normative statement of how things could be if researchers pursued "engaged scholarship," Van de Ven and Johnson (2006) advised "intellectual arbitrage" and discovering the symbiotic benefits of conducting research that is both rigorous and relevant. Wilson (1998) argued that the social sciences lack "consilience" (cross-disciplinary interlocking of causal explanations) and that this deficit has prevented social sciences from making the kinds of advances that medicine, built on the strong foundations of molecular and cell biology, has. He makes a good point.

Historical Exemplars of Rigor and Relevance in the Social Sciences

Perhaps the greatest irony of these multidimensional tribal wars is that the theorists and schools of thought providing the foundations of modern management research represent strong bonds between rigor and relevance, between theory and application. Let's take the rigor side of the equation. In the social sciences of sociology, psychology, and economics, the key architects of some of the most influential theories were actively involved in shaping the societies in which they lived. For example, as a reserve officer, German sociologist Max Weber ran nine military hospitals, tried to found a political party, and advised the German delegation to the Versailles peace conference, all of which had a profound impact on his writings, which, in turn shaped public policy. Emile Durkheim, a renowned French sociologist, advised France's Ministry of Education and became the secretary of the Committee for the Publication of Studies and Documents on the War, all of which shaped his writing but also

[2] I myself graduated from a joint Ph.D program between a business school and the school of arts and sciences that was created with the intent of bridging this divide.

allowed his writing to shape contemporary practice. Karl Marx's involvement in both theory and practice, and its far-reaching consequences, is well documented.

Psychologists are also well represented among theorist-practitioners. Kurt Lewin, considered one of the founders of social psychology, worked with the State of Connecticut to develop workshops to combat racial and religious prejudices, laying the foundation for today's sensitivity training. Subsequently, Lewin worked extensively with former occupants of displaced persons camps. Thus, his research both informed and was informed by practice. Father of psychoanalysis Sigmund Freud developed his revolutionary theories largely through his own work with patients, which included psychotherapy and medication.

Among our economist forefathers the story is the same: Adam Smith served as Scotland's commissioner of customs; John Maynard Keynes advised British governmental bodies on credit and currencies and served as a delegate to the Versailles peace conference after World War I; Austria's Joseph Schumpeter was also actively engaged in the world of practice. Each of these scholars greatly influenced mainstream public policy in the domains of economic and industrial policy.

So it would be safe to suggest that the management scholars who take a rigor-or-relevance approach have lost touch with their roots. In fact, as the examples above demonstrate, at the heart of the rigor-relevance debate is the issue of identity: whether a given researcher identifies him or herself as a rigorous scholar or a practitioner of applied science (for a similar example from scientists working in the commercial sector, see Nag, Corley, and Gioia [2007]). Once self-defined, management researchers tend to close the loop by writing for and listening to only their own tribes. Yet the social scientists highlighted above seemed to have no trouble crossing this divide to see themselves as both. Another important scholar of an earlier generation, Roethlisberger, was particularly clear from the outset that his greater purpose as a scholar was to communicate his ideas to managers. In his intellectual autobiography (Roethlisberger, 1977), he described the inherent connection between knowledge and action in the social sciences and saw no need for a disconnect between the two. As such, he and others—including the scholars noted above—produced vast bodies of work with both rigor and relevance. An unfortunate by-product of our tribal thinking, then, is the demise of symbiotic research intended to cross the boundaries we have created for ourselves.

BRIDGING RIGOR AND RELEVANCE: A NORMATIVE MODEL OF MANAGERIAL RESEARCH

A Big Tent View

I hope I have made the case that the rigor-relevance debate is misguided and ultimately damaging to our field, with excessive energy going to tribal conflicts rather than collaborations. The tyranny of either-or is wringing a great deal of value from our work. The more challenging question, then, is whether we can emerge from under our small, private umbrellas to erect tall and thick poles to prop up a big tent shielding us from the charges of poor rigor, low relevance, and consilience deficits. This will mean existing in a new domain, one that replaces "either-or" with "and." If such a tent is not feasible, especially in the short-term, can we at least learn to sit simultaneously under multiple umbrellas and occasionally invite strangers into our ovals of shade? Vermeulen (2005) referred to this process as synthesizing rigor and relevance at a higher level, rather than trying to strike a balance between them.[3]

The solution, as I suggest below, is to accept that rigor and relevance are not opposites after all. Here I follow scholars who have envisioned a positive-sum world where intellectual world-views are integrated (e.g., Tushman & O'Reilly, 2007; Van de Ven & Johnson, 2006). Seeing this world rests on recognizing the existence of a clear and open space for research that is *both* rigorous and relevant. Yet synergizing rigor and relevance is not the only thing we must do to elevate our research. Many other camps have been built around narrow paradigmatic and methodological preferences, and moving beyond these, too, will serve us well.

If we can view rigor and relevance as outcomes to be simultaneously maximized, we can more actively pursue true synergy. Let me offer several normative suggestions for research creating such academic and practice-focused synergy. These recommendations, which synthesize others' work with some of my own ideas,[4] are aimed at bridging the divide between rigor and relevance and, in the

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[3] Some scholars have debated the possibility of symbiosis between rigor and relevance explicitly. For a review, see Van de Ven and Johnson (2006) and the response by McKelvey (2006). Here I presume that combining these pursuits will generate synergy. I hope that this discussion makes clear the justification for my assumption.

[4] For excellent statements regarding how to bridge the rigor-relevance divide, please see Lawrence's (1992) discussion of the challenges of problem-oriented research,

aggregate, are a multistep process for performing relevant and rigorous work. (I present discrete steps, but it is important to note that one doesn't have to follow them in this sequence.) Seeking synergy through the set of steps outlined below forces the reshaping of theories and also at the same time generates a deeper understanding of important observables. This process should be followed regardless of the publishing outlets for which one aims. Note too that synergistic work need not be disseminated through only one publication; one vehicle for peers and another for practitioners is feasible.

Steps in an Integrative Process

1. Rely on managerial sensibility to shape research questions. The starting point for all research must be subjecting hunches to the crucible of managerial insight. Ghoshal, in many personal communications to colleagues and friends, referred to managerial problems having "sizzle" and thus warranting study. Similarly, discussing the construction of compelling academic business cases, Siggelkow (2007) suggested centering on "talking pigs"—phenomena of great interest—even if they are associated with small, "nonrepresentative" samples (i.e., one talking pig, a single-case sample, is of great interest). It is worth noting that this reliance on managerial sensibility doesn't always necessitate managers telling researchers what is most important, but rather, researchers' discovering through interactions with managers what is of importance to them. Sometimes managers are not fully aware of or are not able to clearly articulate important issues. Discovery is interactive.

Lawrence (1992) discussed such interaction in the context of "problem-oriented" research, or work that focuses on real-world managerial challenges, writing, "Our subjects can tell us what needs to be studied—where our theories and knowledge are inadequate" (1992: 140). He also stated that problem-oriented research findings are more likely to be used, as they are likely to be in language that practitioners understand, and there is always a market for readily applied, real-world-driven insights. Some have taken this idea a step further by engaging practitioners as partners in the research process itself (e.g., Amabile et al., 2001; Van de Ven & Johnson, 2006). Using real-world phenomena to drive our research may also help management researchers break out of our narrow tribal affiliations. By probing more deeply into the problems and other issues that managers care about, we can naturally align our interests with more practice-relevant research, without sacrificing rigor.

2. Test theory in the classroom. Most business school students and business executives we teach in our burgeoning executive education programs are past, current, and/or future managers, so there is no better group on which to test the relevance and potential value of theoretical concepts. Thus, we should strive to bring knowledge, including that derived from the most rigorous, analytical research, into the classroom. As Chris Bartlett suggested at a recent London Business School conference commemorating Ghoshal's work, we have a great deal to learn from the reactions of those subjected to our lectures and cases. The classroom, then, is the perfect petri dish for innovation and experimentation.

3. Build theory. Regardless of the variables under study (e.g., behaviors or outcomes), or one's method and data of choice (qualitative or quantitative), researchers should try to ground ideas in existing theory or clearly articulate new theories—not only to gain legitimacy among our peers, but also to make clearer contributions to the body of management knowledge (Colquitt & Zapata-Phelan, 2007). Theory building is a cumulative enterprise and as such can only happen if we are explicit about both our theories and their impacts on managerial practice.

4. Appreciate—and synthesize—the dialectic between theory and phenomenon. In our focus on domain building within our tribes and subtribes, we have neglected the important dialectic between theory and the phenomena or practices to which it relates. Ideally we can stretch our understandings of theory and practice simultaneously by recognizing the dialectic between them: theory forces one to examine phenomena from a novel or more integrated standpoint, whereas observable behavior or outcomes allow one to define, refine, or discard theory. Thus, we can use theory to understand more subtle or counterintuitive aspects of the behavior we observe and use phenomena to illuminate the boundaries of our theory. In this way, rigor abets relevance, and vice versa.

5. Become "bilingual interpreters" for and active collaborators with practitioners. Along with a heightened managerial sensibility comes an appreciation for both distilling new insights from managers and sharing research findings with them in a comprehensible manner. Overcoming this knowledge transfer problem requires researchers to oper-

Amabile et al.'s (2001) discussion of the fruitful aspects of academic-practitioner collaboration and, most recently, Van de Ven and Johnson's (2006) account of "engaged scholarship."

ate as translators (e.g., Mohrman, Gibson, & Mohrman, 2001). But bilingual interpretation leaves the consumers of our work (i.e., practitioners) in a largely passive role. Thus, an extension of our role can be to co-create knowledge with the managers whose behaviors we study (Amabile et al., 2001; Van de Ven & Johnson, 2006). In the London Business School conference mentioned above, Yves Doz gave an inspiring example of a research partnership with an executive that blossomed after each developed a deep appreciation of the others' approach and context. If we are not willing to go so far, then at the very least we should engage in more productive dialogues with practitioners. Note that we wouldn't be the only ones to benefit from closer collaboration. In fact, Weick argued that the relevance gap decried by academics and managers alike is "as much a product of practitioners wedded to gurus and fads as it is of academics wedded to abstractions and fundamentals" (2001: S71). Weick went on to suggest that greater attention to management theory and concepts would help practitioners transcend performance barriers, and he explicitly recommended deeper practitioner-researcher collaboration to this end.

DO WE NEED A STRUCTURAL SOLUTION?

Simon suggested that business schools, and indeed all professional programs, have the "common problem of bridging the gap between the social system that produces scientific knowledge . . . and the social system where professional practice takes place" (1967: 16). He concluded that despite efforts to integrate researchers focused on theory with those oriented toward practice, "left to themselves, the oil and water will separate again" (1967: 16). This statement might be interpreted to imply that we need to reflect these mutually exclusive domains within management programs with a specific division of labor, or multiple tracks. Garten made a similar recommendation upon his retirement from the Yale School of Management (Holstein, 2005).

I do believe that pursuing separate tracks is a fine idea. In fact, it has the potential to provide greater opportunities to pursue synergies between rigor and relevance. But I also believe that we in the field of management should at the same time continue to look for a middle ground and seek out room for reconciliation between the rigor and the relevance tribes and subtribes. Bridging this gap through the research process I and others have recommended is a major challenge, but it would open the door to a truly collective enterprise, one having both rigor and relevance and, ultimately, greater impact on both cumulative research and management practices. We should therefore aim not only to restructure our programs and processes, but also to reconfigure our identities as more fluid and inclusive, allowing us to be comfortable conducting research for application and real-world impact but also to increase academic understanding. This reconfiguration means freeing ourselves from the tribalism of either/or, to integrate rigor with relevance. If we succeed, we may be able to raise the tent poles of collaboration, rather than chopping down even their possibility.

Perhaps a safe starting point would be to promote a constructive dialogue between tribes. But this would mean rising above the politics of identity and recognizing the benefits of collaboration. As Lawrence said, "Let's get over our identity crisis" (1992: 142). On a practical level, we might create more mechanisms for promoting the work of the few ambidextrous scholars who are able to address and be heard by both tribes. Nurturing such boundary spanners will only help to bridge the rigor-relevance divide, so that even the notion of boundaries will eventually become a thing of the past. In the 1930s, with the dark clouds of war on the horizon in Europe, Winston Churchill warned that "the era of procrastination" was giving way to "a period of consequences." Maybe the time has finally come for us business school academics to actually do something about this long-simmering debate and operate more as a collective than as warring tribes.

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