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FROM THE EDITORS

PUBLISHING IN *AMJ*—PART 1: TOPIC CHOICE

Editor's Note:

This editorial kicks off a seven-part series, "Publishing in AMJ," in which the editors give suggestions and advice for improving the quality of submissions to the Journal. *The series offers "bumper-to-bumper" coverage, with installments ranging from topic choice to crafting a Discussion section. The series will continue in August with "Part 2: Research Design." -J. A. C.*

At the moment of this writing, there are 64 submissions in the hands of *AMJ* reviewers, who have been asked to critically evaluate the merits of those submissions relative to the mission and goals of the *Journal.* Although those reviewers will read their assigned manuscripts carefully and thoughtfully, their recommendations to the action editor will depend, in part, on a choice made years earlier: the topic of the study. The seeds for many rejections are planted at the inception of a project, in the form of topics that—no matter how well executed—will not sufficiently appeal to *AMJ*'s reviewers and readers. Likewise, many manuscripts ultimately earn revise-and-resubmits as a result of topic choices that gave them clear momentum, right out of the gate. What is the anatomy of a topic that, in our opinion, creates that sort of momentum at *AMJ*? Our editorial will focus on five distinct criteria of effective topics: significance, novelty, curiosity, scope, and actionability.

Significance: Taking on "Grand Challenges"

A starting point to consider when selecting a topic is whether the study confronts or contributes to a grand challenge. The term "grand challenge" is credited to a mathematician, David Hilbert, whose list of important unsolved problems has encouraged innovation in mathematics research since the turn of the 20th century. Grand challenges have been applied to diverse fields in the natural sciences, engineering, and medicine. Examples of grand challenges used by the United States National Academy of Engineering include engineering better medicines and making solar energy economical. The grandest of these challenges are reflected in the United Nations Millennium Development Goals to eradicate global poverty, disease, and hunger. The fundamental principles underlying a grand challenge are the pursuit of bold ideas and the adoption of less conventional approaches to tackling large, unresolved problems.

Of course, few *AMJ* submissions will deal with topics as globally significant as reducing poverty or combating hunger. What *AMJ* submissions can do is deal with large, unresolved problems in a particular literature or area of inquiry and tackle those problems in a bold and unconventional way that leaps beyond existing explanations. Often that leap will engender new paradigms or open new pastures for scholarly discourse. For example, Ferlie, Fitzgerald, Wood, and Hawkins (2005) took on a grand challenge in asking why evidence-based innovations failed to spread in the health care industry. Innovation diffusion is an issue of vital importance in a number of literatures, and the focus on health care innovations lent additional weight to the topic. Ferlie et al. (2005) then confronted the topic in a bold and unconventional way by going beyond linear models of diffusion and arguing that factors that could seemingly aid diffusion—such as professionalization—could instead create "nonspread."

This conceptualization of grand challenges provides a crucible for melding discussions of theoretical usefulness and the broader perspective that individual and societal benefit can accrue from economic and entrepreneurial activity (Brief & Dukerich, 1991; Ghoshal, Bartlett, & Moran, 1999; Schumpeter, 1942; Sen, 1999). Understandably, every topic choice cannot introduce a new paradigm; the cumulativeness of scholarship and the progress of social sciences require us to build on prior work. Moreover, the "grandness" of unresolved problems will vary from literature to literature over time. Nonetheless, posing each topic within a grand challenge framework provides voice to a study's raison d'être; it allows the author to articulate how the study solves a piece of a larger puzzle, and in so doing, moves the field forward with rigor and relevance (Gulati, 2007).

Novelty: Changing the Conversation

Like many other top journals, *AMJ* also emphasizes novelty in topic choice. Given that scientific work can be viewed as a conversation among scholars (Huff, 1998), one simple way to check the novelty of a topic is to consider whether a study ad-

dressing it would change the conversation that is already taking place in a given literature. Does the study merely add to the momentum created by existing voices, or does it cause heads to turn as the conversation darts in an entirely new direction? Sometimes that new direction is created by adding new vocabulary to the conversation, in the form of new ideas or constructs, and sometimes that new direction results simply from new insights not articulated by prior voices.

Novel topics can often result from knowledge recombination, with something "new" being created by building a bridge between two literatures or disciplines. Fields that draw from within themselves for extensions of ideas tend to become more insular over time, reducing the likelihood that novel solutions will emerge (George, Kotha, & Zheng, 2008). The organizational theory and strategy literatures often refer to "knowledge recombination" as a way to generate new ideas. The premise is that organizations generate new and creative solutions by exploring new technological domains for inspiration and recombining the ideas that emerge with knowledge already resident in the organizations (e.g., March, 1991; Rosenkopf & Nerkar, 2001). In extensions of this argument, Ahuja and Lampert (2001) found that organizations must overcome three pathologies of learning to create novel breakthroughs: the tendency to favor the familiar over the unfamiliar, the tendency to prefer the mature to the nascent, and the tendency to prefer solutions that are near to existing approaches, rather than completely new.

These three pathologies—dubbed "the familiarity trap," "the maturity trap," and "the nearness trap"—become worthy considerations when choosing a topic for *AMJ*. Picking a topic that is too familiar may result in a study that is perceived, at best, as a marginal extension of an existing conversation. Picking a topic that is too mature raises concerns about a contribution that is viewed as too redundant. Similarly, topic choices that represent spaces adjacent to existing literatures could be seen as too overlapping and as departing radically enough from existing perspectives on the core phenomenon. Agarwal, Echambadi, Franco, and Sarkar's (2004) study of "spin-outs" represents a topic that avoids the familiarity, maturity, and nearness traps. Spin-outs are entrepreneurial ventures started by former employees of a firm that go on to compete in the same space as that firm using knowledge gained from its history. Agarwal et al.'s (2004) study changed the conversation in the entrepreneurship and capabilities literatures by focusing attention on a new and underresearched phenomenon.

Curiosity: Catching and Holding Attention

Although a novel topic may draw a reader in, it takes something more to catch and hold their attention. The best topics for *AMJ* spark and maintain curiosity. In this context, curiosity can be seen as an approach-oriented motivational state that is associated with deeper, more persistent, and more immersive processing of information (Kashdan & Silvia, 2009). Davis's (1971) "index of the interesting" is one useful way to describe how to arouse a reader's curiosity. According to Davis (, topics are interesting when their propositions counter a reader's taken-for-granted assumptions. For example, a study focused on showing a seemingly good phenomenon to be bad would arouse curiosity because it challenges the reader's initial expectations.

Another way to think about arousing and maintaining curiosity is to use mystery as a metaphor. Alvesson and Kärreman (2007) argued that interesting research topics flow out of "breakdowns": surprising findings in one's own data or the extant literature that cannot be explained by methodological issues or existing explanations. Breakdowns provide an opportunity for scholars to use their imagination, and they signal the potential existence of a mystery: "When asking more questions, hanging around . . . and walking to the library and reading more books fails to be sufficient, a mystery is at hand" (Alvesson & Kärreman, 2007: 1272). Interesting topic choices then arise out of a desire to solve or reformulate the mystery. Such topics are believed to arouse more interest than the more typical "gap-spotting" approach to generating research questions (Alvesson & Sandberg, 2011).

Indeed, we can carry the mystery metaphor one step further by considering why mystery novels are so absorbing and engaging. Consider Agatha Christie's *And Then There Were None*, wherein ten guests find themselves trapped on an island mansion before being murdered, one-by-one, in accordance with the "Ten Little Soldiers" nursery rhyme. The story is a page-turner for one simple reason: the reader does not know the ending. Unfortunately, the ending of many *AMJ* submissions is clear and obvious from the title on, even without the "spoilers" provided in the typical academic abstract, because only one conclusion seems plausible. Consider this title: "The Effects of Leader Displays of Happiness on Team Performance." A reviewer could guess the contents of the ending—or, at least, the contents of the Results section—because of the intuitive nature of the topic. A study by Van Kleef, Homan, Beersma, van Knippenberg, van Knippenerg, and Damen (2009) aroused significantly more curiosity. Motivated by inconsistent

findings about the effects of positive and negative leader displays of emotion, the authors examined whether team performance would be facilitated by leaders displaying happiness or by leaders displaying anger. They also examined whether those effects could be explained by follower emotions ("searing sentiments") or by follower inferences about performance ("cold calculations"). Which leader display is more effective, and which mechanism explains the results? If you cannot guess the ending, then the authors made an effective topic choice.

Scope: Casting a Wider Net

Even the best topic ideas can be undermined if the resulting study is too small. Our discussion defines scope as the degree to which the landscape involved in a topic is adequately sampled, in terms of relevant constructs, mechanisms, and perspectives. Studies cannot tackle grand challenges if they are not ambitious in scope, and casting a narrow net limits the investigation of relevant mysteries or gaps in the literature. Submissions may have inadequate scope because authors are under the mistaken impression that *AMJ* still publishes "research notes." It does not, and in fact rarely publishes any article that is significantly shorter than the 40 pages (in Microsoft Word) given as a guideline in our "Information for Contributors." Anecdotally, we suspect that other submissions struggle with scope because authors slice their data too thin—trying to get multiple good papers out of a data set rather than one great one.

The best topics set out to fully and comprehensively sample the landscape in a given domain and may even include constructs and mechanisms derived by using multiple lenses. Seibert, Kraimer, and Liden's (2001) examination of social capital and career success provides a good example of effective scope in topic choice. Discussions of social capital have pointed to three theoretical perspectives that can explain why the size and composition of an employee's social network can impact his or her salary, promotability, and career satisfaction. Seibert et al. (2001) could have chosen to focus on the first of those perspectives, or the second, or the third. Instead, they focused on all three perspectives, operationalizing mediators for each of them. Of course, it is possible for a submission to get too big. Those issues can be addressed in a revision, however, as reviewers can suggest dropping variables to bring more focus to a topic.

Actionability: Insights for Practice

Finally, a topic should be actionable: it should offer insights for managerial or organizational practice. One way to approach the actionability criterion is to consider variability in practices that our existing vocabulary of constructs cannot explain—that is, places where our scholarly language or words fail us. For example, the innovation literature typically paints innovation as the result of capital-intensive research and development efforts. How, then, can we explain emergent innovations that have low capital intensity, severely restricted research and development spending, yet still create value? Products such as a $20 artificial knee and low-cost medical equipment remain "white spaces" in both a competitive and academic sense. The academic study of such topics therefore has an inherent actionability.

McGahan (2007) states five major ways that management studies can be actionable: (1) offering counterintuitive insights, (2) highlighting the effect of new and important practices, (3) showing inconsistencies in, and consequences of, practices, (4) suggesting a specific theory to explain an interesting and current situation, and (5) identifying an iconic phenomenon that opens new areas of inquiry and practice. All five of these pathways are present when topics represent grand challenges and when their pursuit is ambitious in scope and offers novel and unconventional changes to existing conversations. Vermeulen (2007) offers a complementary perspective, noting that research has relevance when it can generate insights that practitioners find useful for understanding their own organizational realities, especially if it concerns variables that are within the control of managers.

Conclusion

In sum, an effective topic is one that allows researchers to tackle a grand challenge in a literature, pursue a novel direction that arouses and maintains curiosity, build a study with ambitious scope, and uncover actionable insights. The 64 submissions that are currently in the hands of *AMJ*'s reviewers will fare better if their topics have that anatomy, as opposed to being more modest, incremental, intuitive, narrow, or irrelevant in nature. Given that topic choice is one of the least revisable aspects of any submission, we would urge any future submitter to ask frank and critical colleagues for feedback on their topic choices—especially if those colleagues are familiar with *AMJ*. Doing so can help those topics achieve a momentum that

will be helpful down the road, once the manuscript is in the hands of reviewers.

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