© Academy of Management Journal
2006, Vol. 49, No. 1, 9–15.

# ACADEMY OF MANAGEMENT JOURNAL EDITORS' FORUM

# WHAT MAKES MANAGEMENT RESEARCH INTERESTING, AND WHY DOES IT MATTER?

**JEAN M. BARTUNEK**
**Boston College**

**SARA L. RYNES**
**University of Iowa**

**R. DUANE IRELAND**
**Texas A&M University**

> The first criterion by which people judge anything they encounter, even before deciding whether it is true or false, is whether it is interesting or boring.
>
> Murray Davis (1999: 245)

> Sometimes academics take very exciting, engaging, and important work and present it in such a way that it looks like a butterfly squashed between two pieces of glass.
>
> Blake Ashforth, quoted in Bartunek (2003: 203)

What makes research interesting? What can authors of scholarly work do to make it more interesting to other scholars and potential audiences? Just what is it about some scholarly work that holds the attention of those reading and studying it and subsequently influences them to act, often by completing additional scholarly work?

These questions are not new to social science scholarship (e.g., Black, 2000; Davis, 1971; Mitroff & Kilman, 1977). However, they took on considerable importance to *AMJ*'s current editorial team as the result of a survey of *AMJ* editorial board members conducted in the summer of 2004 and reported in *AMJ* in February 2005 (Rynes, 2005). This survey suggested that while board members viewed *AMJ* as unparalleled from a standpoint of publishing technically competent research that simultaneously contributes to theory, empirical knowledge, and practice, they also believed that it was both possible and desirable to raise the proportion of articles published in *AMJ* that are regarded as important, competently executed, and *really interesting*.

Table 1 summarizes board members' responses to a question about the most important thing for *AMJ* to do over the next few years. As shown in the table, the board members' most frequent suggestion was to "accept more innovative, less formulaic research." Although the board members did not always use the word "interesting" in their responses, the implication was clear.

As members of the editorial team, we realized that if we were truly going to move *AMJ* toward being a more interesting journal, a number of steps needed to be taken. One immediate step was to expand the *AMJ* mission statement to explicitly include publishing not only empirical research that tests or extends management theory, but also research that *develops* such theory. We also made it clearer that "the *Journal* seeks to publish work involving all empirical methods, including but not limited to qualitative, quantitative, field, laboratory and combination methods." This revised mission statement identifies more types of contributions than the older statement did, and it purposely includes theory development as well as theory testing.

A second step was to recruit some additional *AMJ* board members who were particularly well known for producing highly interesting research themselves. Our logic was that those producing interesting research are in a good position to mentor others (via the review process) on how to make their own work more interesting. Although we will not mention all such board members by name, two of the most prominent were Stephen Barley and Jane Dutton, whose thoughts about producing interesting work follow our introductory essay.

A third step was to conduct a second editorial

We are very grateful to Brad Almond, Ken Brown, Kevin Dooley, Ben Haimowitz, Amy Hillman, Chun-Yi Hung, Bradley Kirkman, Chet Miller, Michael O'Leary, Mauricio Reinert do Nascimento, Debra Shapiro, Paul Skilton, and Jordi Trullen for their assistance in developing this article.

**TABLE 1**
**Most Important Change to Make in *AMJ* Perceived by Board Members[a]**

| Change | Percent Mentioning |
|---|---|
| Accept more innovative, less formulaic research | 17% |
| Loosen the theory requirement | 10 |
| Keep a balanced, broad base of appeal and be open to all | 8 |
| Increase methodological rigor | 6 |
| Aim for higher impact; address more socially important issues | 5 |
| Reduce or eliminate research notes | 5 |

[a] $n = 83$.

board survey, this time to find out which specific management and organization science articles *AMJ* board members felt were most interesting, and why. Results of this survey will be presented shortly. But first, we discuss why we believe making our research more interesting matters and why it is important for this type of research to be published in *AMJ*.

## WHY DOES BEING INTERESTING MATTER?

At the outset, we wish to make it clear that being "interesting" is not all that matters to good research. The *importance* of the research question and the *validity* of a study's conclusions are, in our opinion, more central elements of high-quality research than is being regarded as interesting. When it comes to empirical research, being interesting cannot substitute for a lack of validity or importance—although readers are unlikely to see work they regard as unimportant as interesting. In this, we agree with Vermeulen, who wrote: "Just because something sounds intriguing and makes an interesting claim does not mean it should be said and published. Claims unsupported by thorough academic research, no matter how intriguing they may sound, to me are not relevant. Actually, I fear they could be dangerous" (2005: 979).

With that said, we believe that there are benefits to making research more interesting, so long as the core elements of high-quality research (e.g., importance, rigor, and validity) are present. One such benefit is that scholars who produce interesting research have more influence on others. In his classic study of sociological theories, Davis concluded that "a theorist is considered great, not because his/her theories are *true,* but because they are *interesting*. . . . The capacity to stimulate interest is a necessary characteristic of greatness" (1971: 309).

A second (and arguably more important) reason for being concerned about interesting work is that recent psychological research suggests that materials that are perceived as interesting produce a higher degree of learning. Sansone and Thoman (2005), Silvia (2005), and others have recently provided conceptual discussions showing that experiencing interest is an emotion, and as such it plays a central role in people's task performance. Interest fosters intrinsically motivated behavior on tasks and leads to greater persistence and long-term engagement with them. More specifically, with regard to reading and learning from written materials, research by Ainley, Hidi, and Berndorff (2002) suggested that the extent to which potential readers perceive an article's topic and title as interesting influences the probability that they will read it, as well as the degree of positive affect that they bring to the reading. Positive affect, in turn, increases the persistence readers bring to a task, which ultimately affects the degree of learning. Thus, scholarly articles that are more interesting to their readers are more likely to induce positive affect and are also more likely to be read, understood, and remembered.

Finally, producing more interesting research may be essential for attracting, motivating, and retaining talented and enthusiastic doctoral students. For example, a graduate student who read two of the articles nominated as "most interesting" in the recent board survey (reported below) said, "Both articles give me *hope*. These are among the articles that I consider as models for what I want to do later on. With many other articles, I sometimes have doubts as to whether this [i.e., academics] is what I want to do with my life." Vermeulen recently expressed a related thought: "I notice from reading the many applications to our Ph.D. program. . .that very few people aspire to become business academics with the intention to publish journal articles that will only be read by other academics (at best); rather, these applicants are much more inspired by the thought of gaining and developing truly relevant knowledge that might change the world of organizations" (2005: 980–981).

In sum, we believe that making our research more interesting would, in combination with asking important research questions and continuing to assure methodological rigor, increase the visibility and impact of management research, in part by motivating readers to be more engaged with the material. This is a particularly important aspiration for *AMJ*. As one of the journals sponsored by the Academy of Management, it is expected to have an impact on its readers, especially those who are

AOM members, in order to help accomplish the Academy's aim of enhancing the profession of management and contributing to the professional development of its members.

## WHAT MAKES THEORETICALLY BASED RESEARCH INTERESTING?

But what constitutes "interesting" research in empirical contexts? To date, this question has been addressed more thoroughly with respect to theoretical, as opposed to empirical, work.

In a well-known paper, "That's Interesting!", Murray Davis (1971) argued that what most makes scholarly work interesting is that it disconfirms some (but not all) of the assumptions held by its audience. More specifically, building on research regarding perception, Davis argued that what is interesting is something that engages readers' attention, something that stands out for readers "in contrast to the web of routinely taken-for-granted propositions which make up the structure of their everyday life" (1971: 311). Statements that stand out deny old truths that make up a reader's "assumption ground." But denying only *part* of the assumption ground is crucial. If a scholarly article denies all of a reader's assumption ground, the reader is likely to treat the article as absurd. In contrast, if an article is consistent with all of a reader's assumption ground, he or she is likely to see the argument as obvious, and thus not interesting.

Davis's article has been very influential. It has been cited more than 125 times, and its central argument has often been used without attribution to the original article. However, what it describes is not the sole determinant of what makes scholarly work interesting.

Because Davis was addressing theoretical work, his observations focused almost exclusively on the logic of a scholarly argument.[1] For example, he summarized general types of interesting contributions, such as showing that what appears to be an independent variable is truly a dependent variable (or vice versa), or that phenomena that appear to be heterogeneous are actually homogeneous (or vice versa), and so on. The basis of Davis's argument, however, is much broader: that in order to be interesting, scholarship must "stand out" in some way. When it comes to empirical work, there may be many features—in addition to the logic of a scholarly argument—that foster standing out.

Another important feature of Davis's argument is that it inextricably links article and audience: interesting work denies some assumptions of a particular audience. A piece of scholarship will be unlikely to be interesting to all audiences; indeed, scholarly work will probably be interesting only to those who share many, though not all, of its assumptions. In other words, scholars who wish to influence an audience must "read" that audience in much the same way that the audience reads a scholarly work (Davis, 1986).

The need to read the audience is a relevant point because the membership of the Academy of Management is expanding. For one thing, the number of members has grown considerably; there were 3,000 more Academy members in 2005 than there had been three or four years earlier. In addition, the proportion of members from outside the United States is growing; the membership of the Academy is now more than one-third international (i.e., not United States–based). In addition, several efforts have been made to increase practitioners' interest in the Academy. These shifts in membership have implications for the Academy's publications. At the very least, the audience for such publications has expanded, and so have the assumptions readers of the AOM journals bring with them about what is interesting and important research.

## WHAT MAKES *EMPIRICAL* RESEARCH INTERESTING? FOUR PERSPECTIVES

### *AMJ* Editorial Board Survey

For all the preceding reasons, the *AMJ* editorial team felt it was important to address the question of what makes empirical management research interesting. Accordingly, in the autumn of 2004, Jean Bartunek, the chair of the *AMJ* advisory committee, designed a Web-based questionnaire that was made available to all members of *AMJ*'s editorial board. Board members were invited to nominate up to three empirical articles related to management from any academic journal over the past 100 years that they regarded as particularly interesting and to describe why they saw them as interesting.

Sixty-seven *AMJ* board members nominated 160 different papers as exemplars of interesting research. Their nominations, and the rationales they gave for them, yield some indication of what it takes for journal articles to be seen as interesting at this point in time, at least by a group of people who review many manuscripts that are submitted to top-tier journals.

[1] More recently, Davis has extended his work to include the roles of phenomenology and rhetoric (1986) and linguistic structures such as aphorisms (1999) in producing interesting theories.

A wide variety of papers was nominated, and no paper was nominated more than five times. Table 2 shows all papers nominated two or more times. In addition, a wide variety of reasons was given for the nominations. We content-analyzed them to determine which reasons were most frequently stated. Reasons given with respect to at least 25 percent of the articles fell into the categories shown in Table 3.

Results of the survey support Davis's (1971) arguments regarding theory: empirical articles that challenge current assumptions are also particularly likely to be viewed as interesting. However, as Table 3 indicates, the board members' responses indicated that they applied a broad range of criteria for determining what was interesting. Additional criteria given by at least 25 percent of respondents included the quality of the article, how well it was written, the newness of its theory and findings, the importance of its practical implications, and the extent of its impact on subsequent research.

## A Brazilian Perspective

The *AMJ* board represents a select group of people in the Academy of Management—scholars who are already professionally successful and socialized according to Academy norms. Also, although the *AMJ* board is increasingly international, its members still come predominantly from North America. Do other scholarly associations share the same criteria for what makes an article interesting? If not, how do the criteria differ?

Thanks to the initiative of Maurício Reinert do Nascimento (who attended an early presentation of these survey results), 20 board members of *Revista de Administração de Empresas,* a scholarly journal published by ANPAD, the Brazilian Academy of Management, completed the same survey that

**TABLE 2**
**Articles Nominated Two or More Times in the *AMJ* Board Survey on Interesting Research**

| Number of Mentions | Article |
|---|---|
| 5 | J. E. Dutton & J. Dukerich. 1991. "Keeping an Eye on the Mirror: Image and Identity in Organizational Adaptation." *Academy of Management Journal*, 34: 517–554. |
| 4 | S. R. Barley. 1986. "Technology as an Occasion for Structuring: Evidence from Observations of CT Scanners and the Social Order of Radiology Departments." *Administrative Science Quarterly*, 31: 78–108. |
| 3 | S. Barley, G. Meyer, & D. Gash. 1988. "Cultures of Culture: Academics, Practitioners, and the Pragmatics of Normative Control." *Administrative Science Quarterly,* 33: 24–60. |
| 3 | K. M. Eisenhardt. 1989. "Making Fast Strategic Decisions in High-Velocity Environments." *Academy of Management Journal,* 32: 543–577. |
| 3 | M. Huselid. 1995. "The Impact of Human Resource Management Practices on Turnover, Productivity, and Corporate Financial Performance." *Academy of Management Journal,* 38: 635–673. |
| 3 | R. I. Sutton & A. Rafaeli. 1988. "Untangling the Relationship between Displayed Emotions and Organizational Sales: The Case of Convenience Stores." *Academy of Management Journal,* 31: 461–487. |
| 3 | B. Uzzi. 1997. "Social Structure and Competition in Inter-Firm Networks: The Paradox of Embeddedness." *Administrative Science Quarterly,* 42: 35–67. |
| 2 | K. D. Elsbach. 1994. "Managing Organizational Legitimacy in the California Cattle Industry: The Construction and Effectiveness of Verbal Accounts." *Administrative Science Quarterly,* 39: 57–88. |
| 2 | K. D. Elsbach & R. M. Kramer. 2003 "Assessing Creativity in Hollywood Pitch Meetings: Evidence for a Dual-Process Model of Creativity Judgments." *Academy of Management Journal,* 46: 283–301. |
| 2 | W. J. Ferrier & C. M. Grimm. 1999. "The Role of Competitive Action in Market Share Erosion and Industry Dethronement: A Study of Industry Leaders and Challengers." *Academy of Management Journal,* 42: 372–388. |
| 2 | H. Ibarra. 1998. "Provisional Selves: Experimenting with Image and Identity in Professional Adaptation." *Administrative Science Quarterly,* 43: 764–789. |
| 2 | A. Kluger & A. DeNisi. 1996. "Effects of Feedback Intervention on Performance: A Historical Review, A Meta-Analysis, and a Preliminary Feedback Intervention Theory." *Psychological Bulletin,* 119: 254–284 |
| 2 | A. Meyer. 1982. "Adapting to Environmental Jolts." *Administrative Science Quarterly,* 27: 515–536. |
| 2 | M. G. Pratt. 2000. "The Good, the Bad, and the Ambivalent: Managing Identification among Amway Distributors." *Administrative Science Quarterly,* 45: 456–493. |
| 2 | P. Sherer & K. Lee. 2002. "Institutional Change in Large Law Firms: A Resource Dependency and Institutional Perspective." *Academy of Management Journal,* 45: 102–119. |
| 2 | B. Staw, N. Bell, & J. A. Clausen. 1986. "The Dispositional Approach to Job Attitudes: A Lifetime Longitudinal Test." *Administrative Science Quarterly,* 31: 56–78. |
| 2 | K. E. Weick. 1993. "The Collapse of Sensemaking in Organizations: The Mann Gulch Disaster." *Administrative Science Quarterly,* 38: 628–652. |

**TABLE 3**
***AMJ* Board Members' Reasons for Rating an Article as "Most Interesting"**

| Percent Giving Reason | Reason | General Domain of Reason | Illustrative Quotations |
|---|---|---|---|
| 57% | Counterintuitive | Challenges established theory; is counterintuitive; goes against folk wisdom or consultant wisdom, etc; creates an "aha" moment. | "This paper flew in the face of conventional wisdom and demonstrated that 'conventional wisdom' was based on a very biased view of what had been done." |
| 57 | Quality | Includes at least one of the following: well-crafted theory; good technical or methods job; good fit of data and theory; sophisticated methodology; great sample; makes the complex look simple and elegant. | "The data are qualitative, collected by lengthy interviews and participant observation. Cluster analysis is used to identify different types. Extensive significance testing and rigorous quantitative validation procedures provide a template for any researcher employing this method. This is followed by the presentation of qualitative data in support of each type, providing further insight." |
| 48 | Good writing | Is well framed; builds momentum; provides good examples; is clear and engaging; has rich descriptions. | "The story is compelling, and it is written with such clarity and such drama that it is a good read all on its own." |
| 46 | New theory/finding | Creates new theory; synthesizes previous theories; integrates multiple perspectives; comes up with an important finding. | "The study provides insightful theory development to account specifically for nationality-based diversity effects which are then verified through three different studies." |
| 31 | Practical implications | Generates usable knowledge in the "real world"; addresses a subject that is very relevant to the real world. | "The article also seemed to have important implications for managers of any organization at or near the top." |
| 28 | Impact | Stimulates new empirical or theoretical work; has been cited or quoted a lot; has opened avenues for research in a new areas. | "This classic paper obviously made a huge impact on the field, stimulating hundreds of empirical papers and further theoretical development." |

members of the *AMJ* board had completed. They listed 44 articles, none more than once. Table 4 shows the two reasons the *RAE* board members most frequently gave for their choices of interesting articles—impact and quality.

As this table indicates, there is some overlap between the most frequent reasons given by *AMJ* and *RAE* members. Both groups care about quality. However, for *AMJ* board members, presenting something counterintuitive and/or new theoretically was more important, while for *RAE* members, impact (including practical impact) was most important. The variance in these results points to the likelihood that readers in different parts of the world have diverse criteria for scholarly interest.

## A Media Perspective

For more than a decade, the Academy has employed a public relations firm, Hurley and Haimowitz, to publicize the work of its members. Ben Haimowitz, who works with the Academy, seeks to convince news outlets that their readers would be interested in reading the work published in the Academy's journals. We interviewed him to

**TABLE 4**
***RAE* Board Members' Reasons for Rating an Article as Interesting[a]**

| Percent Giving Reason | Reason | General Domain of Reason |
|---|---|---|
| 39% | Impact | Stimulates new empirical or theoretical work; it has been quoted a lot; it has opened avenues for research on new areas. |
| 32 | Quality | Includes at least one of the following: well-crafted theory; good technical or methods job; good fit between data and theory; sophisticated methodology; great sample; makes the complex look simple and elegant. |

[a] *RAE* is the Brazilian journal *Revista de Administração de Empresas.*

find out what the reporters with whom he works are most likely to see as interesting.

Haimowitz's view is that some studies can be quite interesting to scholars, but not to other people. He says, "There is a fundamental difference between scholarship and what the rest of the world is interested in." But he also listed several factors that are crucial to having academic research cited in broader outlets.

According to Haimowitz, the most important factor is timeliness, since reporters are trying to deal with issues that are in the forefront of the public's attention today. This factor presents quite a challenge, as there can be long time lags between a researcher's recognizing a topic as "interesting" and the research results appearing in print.[2] Publicists such as Haimowitz often have to be creative in "pitching" the timeliness of academic results. For example, he persuaded several reporters to conduct interviews with N. Anand and Mary Watson the week before the Grammy Awards to talk about their February 2004 *AMJ* article, "Tournament Rituals in the Evolution of Fields: The Case of the Grammy Awards." Although "rituals in the evolution of fields" are not of great interest to most news readers, according to Haimowitz, the article also contained "a lot of interesting observations about the Grammy Awards" that could be highlighted because of their timeliness.

Haimowitz also indicated that reporters want *findings,* especially in the form of numbers: "Pitching *Academy of Management Review* articles doesn't work, because they don't have findings. Findings equate with news." Thus, at least some of the articles that scholars find very interesting (indeed, many of those on our list of 160 interesting articles) would not be an easy "sell" to the media, because they focus predominantly on processes rather than outcomes. In addition, *names* are also important to the media. Thus, the fact that most academic articles report data from unnamed companies or anonymous case studies limits reporters' interest. Still, with a bit of creativity, Haimowitz can sometimes find a way to pitch generic findings in a specific context. For example, he generated media interest in Yan Zhang and Nandini Rajagopalan's August 2004 *AMJ* study, "When the Known Devil Is Better Than an Unknown God: An Empirical Study of the Antecedents and Consequences of Relay CEO Successions," by pitching its relevance to the Michael Eisner power struggle at Disney Corporation. Similarly, he publicized Jiatao Li and Donald Hambrick's October 2005 *AMJ* article, "Factional Groups: A New Vantage on Demographic Faultlines, Conflict, and Disintegration in Work Teams," by linking it to demographic and educational differences in the top management teams involved in the ill-fated merger between AOL and Time Warner.

Finally, Haimowitz indicated that some topics are *always* of interest to the media. Two such topics are sex and stock options—sex, because it is a "taboo" topic that has not been studied much in a management context, and stock options because they may concern "big money" and the fates of large organizations.

So what is Haimowitz's advice for scholars who want to make their work more interesting? Should they start studying sex and dropping names? Should they read the newspapers to find hot topics? Haimowitz's answer is an emphatic no. He says, "If I were to make one suggestion, it would be to conduct studies that go the extra mile, that don't just stop at filling in the little scholarly brick, but take it further, to larger questions that push the issue. That's likely to be an interesting study. . . . If something is really interesting, unless it's pretty abstruse I can usually do something with it."

### The Views of Three Interesting Researchers

As the data from our various surveys indicate, no one single factor makes an empirical research project interesting. Still, a few articles (and authors) stand out as being mentioned more often as interesting. In the two pieces that follow, three of these authors—Steve Barley, Jane Dutton, and Janet Dukerich—talk about the factors that they believe have facilitated their ability to produce interesting research. We chose Barley to comment because his work was cited the most frequently in the survey: eight times. Table 2 shows two of his cited articles; a third article, "The Alignment of Technology and Structure through Roles and Networks," which appeared in *Administrative Science Quarterly* in 1990, was also mentioned. We chose Dutton and Dukerich to comment because their June 1991 *AMJ* paper, concerning how the Port Authority of New York and New Jersey responded to homelessness, was the most frequently cited individual article.

These authors and articles received many compliments. For example, respondents noted of Barley's work:

- It was like a light bulb went off for me.
- He brings his case studies to life.
- The article both added to and challenged organizational debate at the time.

[2] To reduce this time lag, the *Academy of Management Journal* makes in-press papers available to Academy members on the *AMJ* Web site.

- This article was well-written, investigated an intriguing question, and offered valuable insights on a question that many had asked but few had attempted to answer.

Similarly, respondents said of the Dutton and Dukerich paper:

- It really made the abstract idea of "identity" come to life.
- The moral dilemmas posed between the desire to be perceived "professionally" and the desire to be humane—and how these internal conflicts came to change the organization's policies—were very palpable.
- One of the finest pieces of grounded research I've ever read. A wonderful blend of story-telling and theory development. Just fascinating.

Although our commentary has focused on the *characteristics* of interesting research, the commentaries by Barley, Dutton, and Dukerich (which follow) shed some insight on the *processes involved* in creating interesting research. We thank Steve, Jane, and Janet for sharing their insights and hope they may prove useful to readers/scholars who would like to produce more interesting research in the future.

## REFERENCES

Ainley, M., Hidi, S., & Berndorff, D. 2002. Interest, learning, and the psychological processes that mediate their relationship. ***Journal of Educational Psychology,*** 94: 545–561.

Anand, N., & Watson, M. R. 2004. Tournament rituals in the evolution of fields: The case of the Grammy Awards. ***Academy of Management Journal,*** 47: 59–80.

Barley, S. R. 1990. The alignment of technology and structure through roles and networks. ***Administrative Science Quarterly***, 35: 61–103.

Bartunek, J. M. 2003. 2002 presidential address–A dream for the Academy. ***Academy of Management Review,*** 28: 198–203.

Black, D. 2000. Dreams of pure sociology. ***Sociological Theory,*** 18: 343–367.

Davis, M. S. 1971. That's interesting! Towards a phenomenology of sociology and a sociology of phenomenology. ***Philosophy of the Social Sciences,*** 1: 309–344.

Davis, M. S. 1986. That's classic—The phenomenology and rhetoric of successful social theories. ***Philosophy of the Social Sciences,*** 16: 285–301.

Davis, M. S. 1999. Aphorisms and clichés: The generation and dissipation of conceptual charisma. In J. Hagan & K. Cook (Eds.), ***Annual review of sociology,*** vol. 25: 245–269. Palo Alto, CA: Annual Reviews.

Li, J., & Hambrick, D. C. Factional groups: A new vantage on demographic faultlines, conflict, and disintegration in work teams. ***Academy of Management Journal,*** 48: 794–813.

Mitroff, I., & Kilmann, R. H. 1977. Systemic knowledge—Toward an integrated theory of science. ***Theory and Society,*** 4: 103–129.

Rynes, S. L. 2005. From the editors—Taking stock and looking ahead. ***Academy of Management Journal,*** 48: 9–15.

Sansone, C., & Thoman, D. B. 2005. Interest as the missing motivator in self-regulation. ***European Psychologist,*** 10: 175–186.

Silvia, P. J. 2005. What is interesting? Exploring the appraisal structure of interest. ***Emotion,*** 5: 89–102.

Vermeulen, F. 2005. On rigor and relevance: Fostering dialectic progress in management research. ***Academy of Management Journal,*** 48: 978–982.

Zhang, Y., & Rajagopalan, N. When the known devil is better than an unknown god: An empirical study of the antecedents and consequences of relay CEO successions. ***Academy of Management Journal,*** 47: 483–500.