© Academy of Management Journal
2011, Vol. 54, No. 3, 432–435.

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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2011, Vol. 54, No. 4, 657–660.

FROM THE EDITORS

PUBLISHING IN *AMJ*—PART 2: RESEARCH DESIGN

Editor's Note:

This editorial continues 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 October with "Part 3: Setting the Hook."- J.A.C.*

Most scholars, as part of their doctoral education, take a research methodology course in which they learn the basics of good research design, including that design should be driven by the questions being asked and that threats to validity should be avoided. For this reason, there is little novelty in our discussion of research design. Rather, we focus on common design issues that lead to rejected manuscripts at *AMJ*. The practical problem confronting researchers as they design studies is that (a) there are no hard and fast rules to apply; matching research design to research questions is as much art as science; and (b) external factors sometimes constrain researchers' ability to carry out optimal designs (McGrath, 1981).

Access to organizations, the people in them, and rich data about them present a significant challenge for management scholars, but if such constraints become the central driver of design decisions, the outcome is a manuscript with many plausible alternative explanations for the results, which leads ultimately to rejection and the waste of considerable time, effort, and money. Choosing the appropriate design is critical to the success of a manuscript at *AMJ*, in part because the fundamental design of a study cannot be altered during the revision process. Decisions made during the research design process ultimately impact the degree of confidence readers can place in the conclusions drawn from a study, the degree to which the results provide a strong test of the researcher's arguments, and the degree to which alternative explanations can be discounted. In reviewing articles that have been rejected by *AMJ* during the past year, we identified three broad design problems that were common sources of rejection: (a) mismatch between research question and design, (b) measurement and operational issues (i.e., construct validity), and (c) inappropriate or incomplete model specification.

Matching Research Question and Design

Cross-sectional data. Use of cross-sectional data is a common cause of rejection at *AMJ*, of both micro and macro research. Rejection does not happen because such data are inherently flawed or because reviewers or editors are biased against such data. It happens because many (perhaps most) research questions in management implicitly—even if not framed as such—address issues of change. The problem with cross-sectional data is that they are mismatched with research questions that implicitly or explicitly deal with causality or change, strong tests of which require either measurement of some variable more than once, or manipulation of one variable that is subsequently linked to another. For example, research addressing such topics as the effects of changes in organizational leadership on a firm's investment patterns, the effects of CEO or TMT stock options on a firm's actions, or the effects of changes in industry structure on behavior implicitly addresses causality and change. Similarly, when researchers posit that managerial behavior affects employee motivation, that HR practices reduce turnover, or that gender stereotypes constrain the advancement of women managers, they are also implicitly testing change and thus cannot conduct adequate tests with cross-sectional data, regardless of whether that data was drawn from a pre-existing data base or collected via an employee survey. Researchers simply cannot develop strong causal attributions with cross-sectional data, nor can they establish change, regardless of which analytical tools they use. Instead, longitudinal, panel, or experimental data are needed to make inferences about change or to establish strong causal inferences. For example, Nyberg, Fulmer, Gerhart, and Carpenter (2010) created a panel set of data and used fixed-effects regression to model the degree to which CEO-shareholder financial alignment influences future shareholder returns. This data structure allowed the researchers to control for cross-firm heterogeneity and appropriately model how changes in alignment within firms influenced shareholder returns.

Our point is not to denigrate the potential usefulness of cross-sectional data. Rather, we point out the importance of carefully matching research design to research question, so that a study or set of studies

is capable of testing the question of interest. Researchers should ask themselves during the design stage whether their underlying question can actually be answered with their chosen design. If the question involves change or causal associations between variables (any mediation study implies causal associations), cross-sectional data are a poor choice.

Inappropriate samples and procedures. Much organizational research, including that published in *AMJ*, uses convenience samples, simulated business situations, or artificial tasks. From a design standpoint, the issue is whether the sample and procedures are appropriate for the research question. Asking students with limited work experience to participate in experimental research in which they make executive selection decisions may not be an appropriate way to test the effects of gender stereotypes on reactions to male and female managers. But asking these same students to participate in a scenario-based experiment in which they select the manager they would prefer to work for may present a good fit between sample and research question. Illustrating this notion of matching research question with sample is a study on the valuation of equity-based pay in which Devers, Wiseman, and Holmes (2007) used a sample of executive MBA students, nearly all of whom had experience with contingent pay. The same care used in choosing a sample needs to be taken in matching procedures to research question. If a study involves an unfolding scenario wherein a subject makes a series of decisions over time, responding to feedback about these decisions, researchers will be well served by collecting data over time, rather than having a series of decision and feedback points contained in a single 45 minute laboratory session.

Our point is not to suggest that certain samples (e.g., executives or students) or procedures are inherently better than others. Indeed, at *AMJ* we explicitly encourage experimental research because it is an excellent way to address questions of causality, and we recognize that important questions—especially those that deal with psychological process—can often be answered equally well with university students or organizational employees (see *AMJ*'s August 2008 *From the Editors* [vol. 51: 616–620]). What we ask of authors—whether their research occurs in the lab or the field—is that they match their sample and procedures to their research question and clearly make the case in their manuscript for why these sample or procedures are appropriate.

Measurement and Operationalization

Researchers often think of validity once they begin operationalizing constructs, but this may be too late. Prior to making operational decisions, an author developing a new construct must clearly articulate the definition and boundaries of the new construct, map its association with existing constructs, and avoid assumptions that scales with the same name reflect the same construct and that scales with different names reflect different constructs (i.e., jingle jangle fallacies [Block, 1995]). Failure to define the core construct often leads to inconsistency in a manuscript. For example, in writing a paper, authors may initially focus on one construct, such as organizational legitimacy, but later couch the discussion in terms of a different but related construct, such as reputation or status. In such cases, reviewers are left without a clear understanding of the intended construct or its theoretical meaning. Although developing theory is not a specific component of research design, readers and reviewers of a manuscript should be able to clearly understand the conceptual meaning of a construct and see evidence that it has been appropriately measured.

Inappropriate adaptation of existing measures. A key challenge for researchers who collect field data is getting organizations and managers to comply, and survey length is frequently a point of concern. An easy way to reduce survey length is to eliminate items. Problems arise, however, when researchers pick and choose items from existing scales (or rewrite them to better reflect their unique context) without providing supporting validity evidence. There are several ways to address this problem. First, if a manuscript includes new (or substantially altered measures), all the items should be included in the manuscript, typically in an appendix. This allows reviewers to examine the face validity of the new measures. Second, authors might include both measures (the original and the shortened versions) in a subsample or in an entirely different sample as a way of demonstrating high convergent validity between them. Even better would be including several other key variables in the nomological network, to demonstrate that the new or altered measure is related to other similar and dissimilar constructs.

Inappropriate application of existing measures. Another way to raise red flags with reviewers is to use existing measures to assess completely different constructs. We see this problem occurring particularly among users of large databases. For example, if prior studies have used an action such as change in format (e.g., by a restaurant) as a measure of strategic change, and a submitted paper uses this same action (change in format) as a measure of organizational search, we are left with little confidence that the authors have measured their intended construct. Given the cumulative and incremental nature of the research process, it is critical

that authors establish both the uniqueness of their new construct, how it relates to existing constructs, and the validity of their operationalization.

Common method variance. We see many rejected *AMJ* manuscripts in which data are not only cross-sectional, but are also assessed via a common method (e.g., a survey will have multiple predictor and criterion variables completed by a single individual). Common method variance presents a serious threat to interpretation of observed correlations, because such correlations may be the result of systematic error variance due to measurement methods, including rater effects, item effects, or context effects. Podsakoff, MacKenzie, Lee, and Podsakoff (2003) discussed common method variance in detail and also suggested ways to reduce its biasing effects (see also Conway & Lance, 2010).

Problems of measurement and operationalization of key variables in *AMJ* manuscripts have implications well beyond psychometrics. At a conceptual level, sloppy and imprecise definition and operationalization of key variables threaten the inferences that can be drawn from the research. If the nature and measurement of underlying constructs are not well established, a reader is left with little confidence that the authors have actually tested the model they propose, and reasonable reviewers can find multiple plausible interpretations for the results. As a practical matter, imprecise operational and conceptual definitions also make it difficult to quantitatively aggregate research findings across studies (i.e., to do meta-analysis).

Model Specification

One of the challenges of specifying a theoretical model is that it is practically not feasible to include every possible control variable and mediating process, because the relevant variables may not exist in the database being used, or because organizations constrain the length of surveys. Yet careful attention to the inclusion of key controls and mediating processes during the design stage can provide substantial payback during the review process.

Proper inclusion of control variables. The inclusion of appropriate controls allows researchers to draw more definitive conclusions from their studies. Research can err on the side of too few or too many controls. Control variables should meet three conditions for inclusion in a study (Becker, 2005; James, 1980). First, there is a strong expectation that the variable be correlated with the dependent variable owing to a clear theoretical tie or prior empirical research. Second, there is a strong expectation that the control variable be correlated with the hypothesized independent variable(s). Third, there is a logical reason that the control variable is not a more central variable in the study, either a hypothesized one or a mediator. If a variable meeting these three conditions is excluded from the study, the results may suffer from omitted variable bias. However, if control variables are included that don't meet these three tests, they may hamper the study by unnecessarily soaking up degrees of freedom or bias the findings related to the hypothesized variables (increasing either type I or type II error) (Becker, 2005). Thus, researchers should think carefully about the controls they include—being sure to include proper controls but excluding superfluous ones.

Operationalizing mediators. A unique characteristic of articles in *AMJ* is that they are expected to test, build, or extend theory, which often takes the form of explaining *why* a set of variables are related. But theory alone isn't enough; it is also important that mediating processes be tested empirically. The question of when mediators should be included in a model (and which mediators) needs to be addressed in the design stage. When an area of inquiry is new, the focus may be on establishing a causal link between two variables. But, once an association has been established, it becomes critical for researchers to clearly describe and measure the process by which variable A affects variable B. As an area of inquiry becomes more mature, multiple mediators may need to be included. For example, one strength of the transformational leadership literature is that many mediating processes have been studied (e.g., LMX [Kark, Shamir, & Chen, 2003; Pillai, Schriesheim, & Williams, 1999; Wang, Law, Hackett, Wang, & Chen, 2005]), but a weakness of this literature is that most of these mediators, even when they are conceptually related to each other, are studied in isolation. Typically, each is treated as if it is the unique process by which managerial actions influence employee attitudes and behavior, and other known mediators are not considered. Failing to assess known, and conceptually related mediators, makes it difficult for authors to convince reviewers that their contribution is a novel one.

Conclusion

Although research methodologies evolve over time, there has been little change in the fundamental principles of good research design: match your design to your question, match construct definition with operationalization, carefully specify your model, use measures with established construct validity or provide such evidence, choose samples and procedures that are appropriate to your unique

research question. The core problem with *AMJ* submissions rejected for design problems is not that they were well-designed studies that ran into problems during execution (though this undoubtedly happens); it is that the researchers made too many compromises at the design stage. Whether a researcher depends on existing databases, actively collects data in organizations, or conducts experimental research, compromises are a reality of the research process. The challenge is to not compromise too much (Kulka, 1981).

A pragmatic approach to research design starts with the assumption that most single-study designs are flawed in some way (with respect to validity). The best approach, then, to a strong research design may not lie in eliminating threats to validity (though they can certainly be reduced during the design process), but rather in conducting a series of studies. Each study in a series will have its own flaws, but together the studies may allow for stronger inferences and more generalizable results than would any single study on its own. In our view, multiple study and multiple sample designs are vastly underutilized in the organizational sciences and in *AMJ* submissions. We encourage researchers to consider the use of multiple studies or samples, each addressing flaws in the other. This can be done by combining field studies with laboratory experiments (e.g., Grant & Berry, 2011), or by testing multiple industry data sets to assess the robustness of findings (e.g., Beck, Bruderl, & Woywode, 2008). As noted in *AMJ*'s "Information for Contributors," it is acceptable for multiple study manuscripts to exceed the 40-page guideline.

A large percentage of manuscripts submitted to *AMJ* that are either never sent out for review or that fare poorly in the review process (i.e., all three reviewers recommend rejection) have flawed designs, but manuscripts published in *AMJ* are not perfect. They sometimes have designs that cannot fully answer their underlying questions, sometimes use poorly validated measures, and sometimes have misspecified models. Addressing all possible threats to validity in each and every study would be impossibly complicated, and empirical research might never get conducted (Kulka, 1981). But honestly assessing threats to validity during the design stage of a research effort and taking steps to minimize them—either via improving a single study or conducting multiple studies—will substantially improve the potential for an ultimately positive outcome.

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© Academy of Management Journal
2011, Vol. 54, No. 5, 873–879.
http://dx.doi.org/10.5465/amj.2011.4000

FROM THE EDITORS

PUBLISHING IN *AMJ*—PART 3: SETTING THE HOOK

Editor's Note:

This editorial continues 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 December with "Part 4: Grounding Hypotheses." -J.A.C.*

Ten times.

If this were *AMJ Jeopardy*, the category would be "introductions," and "ten" would be the answer. What's the question?

You might be thinking of one, but this is actually the correct answer to two questions:

(1) On average, how many times do winners of the *AMJ* Best Article Award rewrite the introductions to their work?
(2) How many times did we rewrite this introduction? (Disclaimer: we're still not satisfied.)

We all know that articles are like dates: first impressions matter. Although it is typically the shortest section of an article, the introduction (i.e., the opening few pages, before the literature review) determines whether or not readers will continue reading. The introduction provides the interpretive frame that shapes how reviewers read a manuscript during the review process. If reviewers are intrigued by the research question, appreciate its importance, and understand how the study advances understanding of the topic, they are more likely to look for reasons to recommend revision. If reviewers are not excited after reading the introduction, they are more inclined to look for reasons to reject.

Despite the importance of introductions, surprisingly little explicit guidance exists on presenting the essentials of your study in a way that captures reader interest, identifies the "conversation" (Huff, 1999) you are joining, explains what your study contributes, and articulates how you will accomplish your goals. To identify tacit knowledge and make it more explicit, we surveyed 22 winners of the *AMJ Best Article Award* about how they develop their introductions. We also surveyed 20 of the most recent recipients of the *AMJ* Outstanding Reviewer Award. What we learned surprised us, and it may surprise you too.

But let's begin with the basics. Our aim is to discuss how to write an introduction that "sets the hook" and helps readers fully recognize and appreciate what your research has to offer and intrigues them enough to read further. We'll start with the product: What does an effective introduction entail? Then, we'll unpack the process. How do eminent scholars develop their introductions, and what are best practices and pitfalls?

The Product

In our view, an effective introduction answers three sets of questions:

(1) Who cares? What is the topic or research question, and why is it interesting and important in theory and practice?
(2) What do we know, what don't we know, and so what? What key theoretical perspectives and empirical findings have already informed the topic or question? What major, unaddressed puzzle, controversy, or paradox does this study address, and why does it need to be addressed?
(3) What will we learn? How does your study fundamentally change, challenge, or advance scholars' understanding?

Who cares? An effective introduction captures attention and interest, making readers curious to read on. The central objective is to highlight why the study's topic matters for both theory and practice, planting the study's roots firmly in "Pasteur's quadrant" (Stokes, 1997), where it can contribute to both basic and applied knowledge. The most effective introductions share the same features as ideas and teaching that "stick" (Heath & Heath, 2007): simplicity, unexpectedness, concreteness, credibility, emotionality, and story. To understand the strategies that successful authors employ, we examined the introductions of the 25 *AMJ* Best Article Award winners. We identified two archetypal hooks for opening an article: the quote and the trend.

One hook involves using a provocative quotation or vignette to engage the reader in the intriguing and practical nature of their topic. Ferrier, Smith, and Grimm (1999) examined erosion of market

leaders' market shares by industry challengers. They hooked readers with media headlines: "Kellogg's cutting prices . . . to check loss of market share" and "Amoco scrambles to remain king of the polyester hill." Some authors take the quote approach even further, opening with a captivating story (e.g., Plowman et al., 2007).

The other hook involves highlighting trends on Main Street or in the Ivory Tower. In the former case, authors describe recent changes in the workplace or broader social environment, noting that their causes or effects remain a mystery. Elsbach and Kramer (2003: 283) lamented that despite a "virtual cottage industry of management books and business school courses that extol the virtues of creativity and provide suggestions for eliciting higher levels of creativity," scholars had accumulated little insight into how expert decision makers judge creative potential in high-stakes situations. Margolis and Walsh (2003: 268) opened with a startling observation: "The world cries out for repair. While some people in the world are well off, many more live in misery. Ironically, the magnitude of the problem defies easy recognition." Alternatively, some authors describe trends in academic literature and identify limitations or contradictions. Barkema and Vermeulen (1998) identified two trends in research on foreign direct investment (FDI). One focused on ownership had received considerable attention; the other, focused on whether the FDI was started from scratch or occurred via acquisition, had received less attention. They further subdivided the second trend to identify their contribution.

What do we know, what don't we know, and so what? After setting the hook, effective introductions answer the second set of questions by identifying the conversation that the study is joining, where the conversation has not yet gone, and why it should go there (Huff, 1999). Locke and Golden-Biddle (1997) referred to this sequence as establishing and problematizing the intertextual field. *Establishing the field* involves entering two different conversations and bridging them (synthesized coherence), identifying an ongoing conversation and describing how it needs to move forward (progressive coherence), or presenting competing perspectives and explaining how you will resolve them (noncoherence). *Problematizing the field* involves convincing readers that knowledge about the topic needs to be developed further (incompleteness), is deficient because it fails to incorporate important perspectives (inadequacy), or is altogether inaccurate (incommensurability).

In our experience, authors frequently use ineffective approaches to problematize an extant literature. Some are too tentative, timidly poking at prior research with the incompleteness approach: they avoid making enemies but end up constructing their own contribution as incremental or obvious. Others are too aggressive, attacking prior research with the incommensurability approach: they pique interest, but their harsh condemnations of prior research often incur confrontations and backlash. The inadequacy approach strikes a reasonable middle ground, convincing readers that we truly need a fresh look without claiming that previous studies were a waste of time. For excellent examples, see Greenwood and Suddaby (2006) for problematizing based on theoretical importance, and Tsui, Pearce, Porter, and Tripoli (1997) for problematizing based on practical importance. For more exemplars, vivid illustrations, and incisive analyses of different problematizing approaches, see Locke and Golden-Biddle (1997).

What will we learn? The final ingredient of an effective introduction is a preview of your work's theoretical contribution. At its heart, this preview involves giving readers a clear sense of how you will deliver on your promise to change, challenge, or advance the conversation that you have entered. This is an important step that is often overlooked by scholars who were trained outside management and organization studies. As one *AMJ* "Outstanding Reviewer" explained,

> Just because a gap exists does not necessarily make the study interesting or worthwhile. Many authors write the introduction by stating that there is a gap but end there without clearly noting why filling this particular gap is important and interesting, or why this contributes to our enhanced understanding of the particular phenomenon. . . .

Hollenbeck (2008) noted that the two most effective ways to frame a contribution are through "consensus shifting" and "consensus creation." *Consensus shifting* occurs when authors identify widely held assumptions, proceed to challenge them, and describe the implications for ongoing research (e.g., Plowman et al., 2007). *Consensus creation* occurs when authors show a lack of consensus in the literature and either clarify the lines of debate or resolve the conflict (e.g., Sherer & Lee, 2002; Wall, Kemp, Jackson, & Clegg, 1986). Summarizing these ingredients, a Best Article Award winner noted:

> It should be a stand-alone "minisummary" of the paper: Clearly position the research question of the paper in the relevant literature or identify the importance of the phenomena being examined, articulate the research question succinctly, outline the main theoretical lens and empirical methodology including empirical context, and discuss in brief the contributions.

The Process

Since the inception of an annual award a quarter century ago, committees have selected 25 papers for the *AMJ* Best Article Award. We surveyed 22 authors of 16 winning papers about how they wrote their introductions. One of the distinctive features of these articles is that their authors hooked us in their introductions, and we wanted to know how they accomplished their aims. We inquired about timing and rewriting and asked what advice they would offer.

Timing. At what point in the drafting process did they write their introductions? Nine percent wrote it when they first developed the idea; 23 percent wrote it at the very beginning of the drafting process; 9 percent wrote it at the very end of the process; and 59 percent wrote it somewhere in the middle of the process, often times jotting notes when they first developed the idea and/or before data collection and analysis were finished. For example, one author described "starting with a very rough draft to get the flow of the paper going, but left the honing of the ideas and major editing of the introduction until after the rest of the paper was written."

How much time did they devote to the introduction, compared to the rest of the article? The average award winner estimated spending 24 percent of the total writing time on the introduction. This is striking, given that the introduction typically accounts for less than 10 percent of the total length of an article. Indeed, the modal award winner recommended that an introduction to an *AMJ* paper be approximately three double-spaced pages. More than a third of authors reported devoting 30 percent or more of their writing time to the introduction (maximum: 50%), and only two reported that it took less than 15 percent of their writing time.

Rewriting. Why does such a short section require so much time? As noted earlier, the average winner reported rewriting the introduction ten times. The minimum was three, and 45 percent reported rewriting it ten or more times. As one winner reflected, "I never count the number of revisions to a paper (especially the introduction). It would be too depressing." The vast majority (86%) reported rewriting the introduction more than any other part of the paper. We identified three different approaches taken: ruthless rewriting, iterative enactment, and following a map.

Ruthless rewriting involves multiple authors showing little pity and great trust as they better each other's work. Consider this illustration from two authors of an award-winning paper:

> Author 1: I wrote it. Author 2 ignored what I wrote and then wrote what s/he wanted. I then rewrote what Author 2 wrote. Author 2 then rewrote what I wrote. And on and on it went. . .
>
> Author 2: Author 1 is an excellent writer, but still has certain weaknesses. I think I have offsetting strengths and weaknesses. I tended to ruthlessly rewrite Author 1's prose and s/he did the same. Eventually we reached a point where we both agreed.

Iterative enactment follows the Weickian (1979) dictum, "How can I know what I think until I see what I say?" Winners rewrote their introductions multiple times until the question, gap or controversy, and contribution crystallized. This approach was especially common among authors of qualitative papers. Here are two illustrations:

> We wrote it last after we figured out the "gems" from the study. We rewrote it several times trying over and over to get to the essence of the gap that would showcase what the study revealed.
>
> It is often difficult to know what literatures to discuss in the introduction. One might think that a lack of similar prior studies would leave the author(s) with little to say, but ironically it tends to open the door to every literature or theory that could conceivably be applied to the setting. . . . I rewrote the introduction dozens of times and it changed dramatically from the initial submission to the published version.

Finally, some authors followed a *clearer map*, answering the three questions that we outlined above in a relatively linear fashion. As one author explained:

> I had to think about what was new here and why anyone should care about reading the paper. I would make a list of reasons to convince myself. Next I would try to carefully identify the research gap being addressed and why someone else would find it as something really important. I also wrote a list of the positive features of the research, in terms of theoretical contributions and the uniqueness of the empirical setting. Next I would write and rewrite the introduction multiple times until I felt that the audience would truly believe that there was something novel here.

Best Practices: Insights from Outstanding Reviewers and Best Article Award Winners

We also surveyed 20 of the 35 members of *AMJ*'s Editorial Review Board who won Outstanding Reviewer Awards between 2008 and 2010, first asking them to name the best introductions they had ever read and explain what made them so memorable.

They nominated 30 empirical papers in the management and organizational studies field, of which 7 were *AMJ* "Best Articles." The articles on which the reviewers commented are listed in Table 1, with a few exceptions (when respondents identified multiple articles by the same author, we selected one; we dropped articles that were written by one of us or self-nominated).

TABLE 1
Exemplary Introductions Nominated by *AMJ* Outstanding Reviewers

Articles	What Made Them Memorable
Latham, Erez, & Locke (1988)	Consensus creation. Historically, at that moment there was complete lack of conceptual consensus on whether or not participation in goal setting led to the setting of higher or lower goals by workers. There were streams of empirical research that were coming to totally different conclusions. This paper showed how by bringing in scholars with different ideas and doing joint experiments, this could be resolved. It created almost complete consensus on the effects of participation on goal setting, and that consensus still persists today.
Schmidt, Hunter, & Pearlman (1981)	Consensus destruction. Virtually everyone believed in the theory of situational specificity of test validities. By the time they were done, almost no one did. Major theoretical shift in thinking with dramatic implications for the practice of testing in the real world. Ended the practice of "local validation" and replaced it with a practice of validity generalization . . . a huge paper for theoretical, practical and methodological reasons.
Staw, Bell, & Clausen (1986)	Consensus creation followed by consensus destruction. They start by highlighting an apparent lack of consensus on the source of job attitudes: are they caused by objective job characteristics or social information? Although these appear to be competing schools, they share the view that attitudes are caused by factors external to the individual. Thus, the authors perform consensus creation at a higher level. Then, they destroy it. The notion that attitudes are caused by external factors is incorrect. Much of the variance in attitudes is dispositional, reflecting individual differences that are stable over time—and across jobs that vary widely in terms of objective characteristics and social information. The introduction creates a new consensus, and then destroys it. Brilliant.
Barker (1993)	Told a story to ground you in the active situation.
Chatterjee & Hambrick (2007)	Memorable for its clean structure, clear direction, and teasing of the reader.
Elsbach & Kramer (2003)	They use active voice and do a great job tying the empirical context to a new theory. They also show clearly how a problem is further explained by their results and how the context can be extended to other situations.
Gersick (1989); Huselid (1995); Tsui et al. (1997)	Succinct and interesting. Identify contradictions in the literature, which is one way of identifying an important issue that needs to be resolved, and do a good job of resolving such contradiction.
Greenwood & Suddaby (2006); Madsen & Desai (2010); Sanders & Hambrick (2007)	They all addressed relevant and significant research questions (or unresolved tensions) in an interesting way. More specifically, each begins with effectively problematizing extant understanding/theory on the fundamental topic and then builds a resolution that helps to either shift a consensus or create a new consensus on the topic.
Gulati & Westphal (1999)	Masters of introductions: (1) clearly written and concise, (2) effectively identify a research gap by underscoring particular limitations of prior research, (3) convince the readers that the topic is important and relevant to study, (4) effectively explain how the study addresses the research gap, (5) clearly explain how the study will achieve its objectives—detailing the main conjectures and empirical setting, and (6) do not include references to irrelevant literature or use of ambiguous terms.
Hitt, Hoskisson, & Kim (1997); Khanna & Palepu (2000); Sanders & Tuschke (2007)	Clearly written; able to inform the readers about study motivation, research question(s), theoretical premise, and potential contributions; able to generate immense interest about the research.
Lounsbury & Glynn (2001); Rao, Monin, & Durand (2003)	(1) They are conversation starters. They are not the nth empirical study on a theory or phenomenon, but leave a certain amount of ambiguity that makes you want not only to read and understand what they are doing, but also be part of, and contribute to, this research stream. (2) They situate the work broadly, often in two (or more) literatures, but not so many to lead to confusion or dilution. (3) They think "big," connecting with often classical or canonical concerns. (4) They do not use jargon but attract, intrigue, and engage a broad readership.
Poppo & Zenger (2002)	Short but effective portrayal of the theoretical problem being solved.
Seibert, Kraimer, & Liden (2001)	First, the authors enumerate the contribution and purpose of their study very explicitly—what the two, three, or four contributions will be. Second, they briefly tell the reader where prior research has been and how their paper will contribute beyond what has already been done. It is critical to set up this contrast. Third, they write in a very accessible way . . . even someone with little expertise could understand.
van der Vegt & Bunderson (2005)	Clear identification of gaps; good explanation for why addressing the gaps would yield important contributions; elegant, well-justified theoretical development of hypotheses.
Whiteman & Cooper (2011)	Their intro was a narrative and captured my imagination. It was just good storytelling.

For further insights, we asked the Best Article Award winners for their advice on how to write a great introduction. A content analysis revealed three primary categories: focusing (45%), engaging the reader (32%), and problematizing the literature (23%). The advice the Outstanding Reviewers offered with respect to these strategies is consistent with our earlier discussions of the introduction "product." Some also noted that writing the introduction is an act of self-persuasion (Aronson, 1999). Through motivating readers to care about the work, authors themselves become more engaged. As one winner noted,

> Introductions are key: they set the tone for the rest of the manuscript, get the reader excited (or not), and help to shape expectations about what the paper will deliver. . . . I often go back to reread them if I get bogged down elsewhere. In that sense they are as much about motivating me as well as the reader.

Pitfalls: Common Mistakes in Introductions

The winners of the Outstanding Reviewer Award also commented on the most common mistakes that authors make in writing their introductions. The mistakes fell into three categories: failing to motivate the paper and problematize the literature (60%), lacking focus (45%), and overpromising (14%).

Failing to motivate and problematize. The most common pitfall involves providing insufficient justification for the importance of the topic and question, and for how the paper contributes new knowledge. One reviewer wrote that "most mistakes have to do with assuming that the motivation for the paper is obvious and failing to identify a clear research gap." Other reviewers wrote that authors often talk "only about filling a gap in the literature; not addressing the 'so what' question," and using "bad frames, like 'This has never been done before' and 'This fills in a gap.'" Two reviewers were especially clear. The first noted, "Some authors believe there is a 'first mover' advantage in our field (e.g., 'To our knowledge, ours is the first study to examine empirically the relationship between shoe size and job satisfaction')." The second emphatically stated, "Not all gaps need to be filled!" Another reviewer counseled avoiding the term "gap" entirely, as it is too self-limiting. Often, the strongest introductions focus on addressing questions, problems, puzzles, and paradoxes, not gap filling.

Lack of focus. This pitfall has several key symptoms. One symptom of an unfocused introduction is being too long and featuring extraneous details and asides rather than essential, interesting information about the paper's contributions. Reviewers often see authors trying "to cram too much of what the study covers in the intro at the expense of being compelling, intriguing, interesting" or writing introductions that are "long and rambling (needs to be short and snappy)." A second symptom is using "too many frameworks in positioning the paper," and a third is describing "what sections of the paper will be presented, in what order," instead of "defining the problem and laying out the contribution." One reviewer thoughtfully counsels: "Many authors do not clearly state the goals of their paper. I like to see them enumerated because it forces the authors to identify them. Oftentimes, authors make passing references to prior work, but they do not tell me enough about what prior research has found and how their study adds importantly to our understanding."

Overpromising. Some reviewers expressed their view that authors create "a mismatch between the introduction and the rest of the paper, typically setting too high expectations in the introduction and failing to meet them later on." This occurs when authors try very hard "to convince the reader of the contributions that they subsequently come off as implausible and ridiculously self-serving" or "to be so compelling and intriguing that they never really tell you what exactly the study does."

Conclusion

We only get one chance to make a first impression, and in academic publishing the introduction to your submission or your article is that chance. A good introduction hooks the reader by elucidating the topic's impact; what scholars now know, what we do not know, and why that matters; and how the research contributes to an ongoing research conversation or starts a new conversation. Effective introductions increase the likelihood readers will continue on to the remaining 90 percent of your article and fully appreciate what your research has to offer.

Good introductions also take considerable time and effort to write. We were pleasantly surprised to learn that our habits of rewriting our introductions at least ten times are not uncommon. Personally, we aim for three double-spaced pages and spend more time on the introductions than on any other parts of our manuscripts. We also generally draft them before we write the other parts of the manuscript. We have found that writing the introduction early provides a constructive outline for structuring the rest of a paper, motivating us, and making sure we stay on track as we develop our ideas. Of course, we go back and revise our introductions as our ideas and studies evolve. Our experience in writing this "From the Editors" suggests it is safe to say that

we are comfortable with the "ruthless rewriting" approach. If you can check your ego, trust your coauthors, and avoid falling in love with your own prose (at least until the tenth iteration), this approach can provide a meaningful learning experience, as well as a stronger product.

Mark Twain once said, "I would have written a short letter, but I didn't have the time." When writing an introduction, it's valuable to make the time. The effort will be rewarded.

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© Academy of Management Journal
2011, Vol. 54, No. 6, 1098–1102.
http://dx.doi.org/10.5465/amj.2011.4001

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PUBLISHING IN *AMJ*—PART 4: GROUNDING HYPOTHESES

Editor's Note:

This editorial continues 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 February with "Part 5: Crafting the Methods and Results Sections." - J.A.C.*

A theory section is a critical part of any paper but is particularly important for an *AMJ* submission. The primary purpose of a theory section is to ground hypotheses; this involves (1) positioning those hypotheses in relation to related research (2), developing a clear, logical argument explaining why the core variables or processes are related in the proposed fashion, and (3) creating a sense of coherence in the relationships among the variables and processes in the proposed model. All are important elements of the theoretical foundation for one's hypotheses. We discuss each separately and then address several potential pitfalls in explanatory logic.

Engaging Prior Research

A key element of creating a strong theory section involves entering into a constructive dialogue with other researchers who have examined the theory or theories that have guided research on a topic. *AMJ* reviewers look to the theory section to find a clear, theoretically driven narrative—not a literature review. Producing such a narrative effectively involves maintaining a delicate balance between engaging previous research and carefully developing one's own novel insights.

On the one hand, citing any remotely relevant paper runs a very real risk of what is sometimes called "argument by citation." When many of the sentences of a theory section start with citations (e.g., "Smith (2002) found . . ."), it is important to take a step back and verify that one is building a compelling argument based on explanatory logic. It is important to cite relevant prior works in building an argument, but the theory section should not be built around these prior works in such a way that the logical reasoning is pushed to the background. Reviewers are virtually certain to raise concerns about papers that have a couple of pages of literature review/discussion followed by a hypothesis that doesn't flow logically from the text immediately preceding it. Often the issue in this case is that the author became so engaged in telling the reader what others have done that the paper does not contain a strong case for the current hypothesis. Merely citing prior studies does not constitute a logical argument; instead, citations should be used to illustrate various elements of the logic of one's own argument (Sutton & Staw, 1995).

Alternatively, it is important to avoid the other extreme, focusing exclusively on the argument and ignoring prior related conversation. Failing to cite several highly relevant papers will lead readers to question the value of the contribution, especially when they believe one or more of the neglected articles is closely related to what the current work addresses. Part of explaining how your work fits into the literature on a topic is to clearly articulate how the paper builds upon that literature, which requires explaining what has already been done and why what the paper proposes is a logical and important contribution that goes beyond prior work.

The key to covering prior work effectively is to look beyond just citing specific empirical results and focus instead on the underlying theoretical issues that are being addressed. Entering the conversation in previous research means engaging the underlying theoretical narrative that is the foundation for past empirical research—but not the empirical results themselves. Similarly, the contribution rests not solely on the results, but also on how they lead to new insights about organizational phenomena. Those insights will be meaningful to the extent that the ideas used to motivate them are clearly linked to the development of the underlying theoretical narrative informing the hypotheses.

One way to achieve the required balance between linking to prior work and developing clear reasoning is to start with the arguments themselves, as they serve as the organizing structure for ideas. An

exercise likely to help is first writing the "Theory and Hypotheses" section of a manuscript without a single citation to previous research. To be sure, the ideas of others are the foundation of this exercise. But crafting the explanatory logic in this pure form enables one to see whether it is clear, consistent, and persuasive on its own. Further, this exercise will require incorporating the ongoing theoretical narrative into one's own explanatory logic, and doing so will make the relationship of the proposed ideas to the larger conversation become evident. When this point is satisfactorily reached, one can go back and incorporate prior work, giving credit to those to whom it is due and explaining how the new work complements or challenges their work.

Among the challenges of starting a theory section by framing one's ideas relative to others' is losing the focus on making a clear argument—the most critical element in an effective theory section. By the time readers arrive at an effectively grounded hypothesis, the theory section should have led them to the point that (1) the hypothesis is not a surprise (i.e., the paper clearly led up to this specific prediction) and (2) the readers understand clearly *why* the constructs are associated. They might not completely agree, but they clearly understand the underlying relationship that is the focus of the hypothesis.

Building the Argument, or the Logic of Explanatory Logic

The sections of a manuscript that lead up to each hypothesis are among the more challenging to write, for good reasons. The objective in these sections is to persuade readers that the claims made in the hypotheses are plausible. Those readers (reviewers) were selected because of their subject matter expertise and, as reviewers, their role is to maintain an attitude of healthy skepticism regarding the claims (hypotheses) made in a paper and the logic that supports them.

Substantiating hypotheses. In simple form, a hypothesis is a claim that Y, a dependent variable, is systematically related to X, an independent variable. Logic forges the connection between the two and can be framed in several ways. The first is to link a hypothesis to a similar logical relationship that is a central tenet of an established theory or conceptual framework. For example, a hypothesis might depend on the idea that team members engage in cooperative behavior to enhance their standing. To substantiate this claim, an author might appeal to the group engagement model of Tyler and Blader (2000). As Sutton and Staw (1995) pointed out, *merely referencing* the group engagement model is not sufficient. The author must offer enough verbal explication for the reader that he/she understands why Y should be predicted by X *without having to read* Tyler and Blader (2000). The success of this approach depends primarily upon the correspondence between the claim(s) made in the paper and the established theory; if other elements in the logic are inconsistent with the group engagement model, then the premise will fail.

A related logical technique is to offer *empirical* evidence supporting claims similar to what the hypothesis states. Here, the implicit argument is that if it has been shown to occur in similar circumstances, then it should also apply in the present circumstances. Empirical evidence is persuasive, however, only when accompanied by a logical rationale.

A third approach is to focus on *how* the hypothesized relationship occurs by crafting a narrative that describes the role of intervening states and/or processes. For example, Seibert, Kraimer, and Liden (2001) developed a model integrating two perspectives on the career benefits of social capital. The roles of relevant theoretically relevant mediators (access to information, access to resources, and career sponsorship) were carefully explained, creating a compelling narrative of how social capital brings career benefits. When giving an account of how a hypothesized relationship "works," note the importance of operationalizing the primary intervening states and processes; without empirical tests, the role of mediators cannot be substantiated, and reviewers may see it as speculative.

A related consideration in framing hypotheses is context. Hypotheses may be intended to apply generally, or they may be limited to specific contexts, such as industries or national cultures. The boundary conditions need to be identified so that the relevance of the proposed relationships is explicit.

Utilizing multiple theories. The challenge of explaining the mechanisms underlying the hypotheses is particularly important when multiple theories are used. Different theories can be a source of novel insights into a variety of issues and may be from the same area (e.g., the resource-based view of the firm and transaction cost economics) or from different underlying disciplines (e.g., social psychology and economics). In either case, the challenge of combining insights from multiple theories is to explain clearly why addressing this research question requires using these theories and how exactly the theories will be joined in a way that creates a unique contribution to the research topic. The need for each additional theory should be clearly explained so as to avoid the impression that

theories are being combined ad hoc to justify disparate hypotheses.

There are several possible approaches to combining theories, each with potential advantages and disadvantages. Pitting one theory against another through competing hypotheses and letting the data decide the winner is a widely used approach that must be used with care, as it can leave the reader puzzled as to why one plausible theory should trump another equally plausible theory—especially given the likelihood that both theories enjoy considerable empirical support in the literature (Cooper & Richardson, 1986; Platt, 1964). An alternative approach is one that explains when and why one theory should take precedence over the other, and an especially effective way of doing that is to explain the conditions under which the predictions of each theory are likely to be most applicable and test these predictions empirically. Vanneste and Puranam's (2010) examination of when a learning effect will have more influence on contract design and distinguishing the learning effect from the effect of trust is an example of this approach.

In many other cases, authors are interested in combining theories to give a more complete account of an organizational phenomenon. Combining implies that the relationship is additive and leads to hypotheses that link different independent variables to dependent variable(s). The risk in this approach is the temptation to specify models combining independent variables simply because, in past research, each has been shown to affect the dependent variable. A conceptual framework that brings the two theoretical perspectives together and articulates their relevant differences is essential. Agarwal, Echambadi, Franco, and Sarkar (2004) made this type of theoretical combination effectively in their analysis of the creation and performance of spin-outs in the disk drive industry. Relatedly, a paper can explain how different theories are most applicable for related research questions that combine to address a particular phenomenon; for example, one theory may explain when a practice will gain traction but another may explain which firms will be the most likely to adopt that practice (e.g., Sherer & Lee, 2002).

A third approach is to seek more integration between two theories. This involves articulating how the two perspectives are complementary—that is, how the assumptions of one theory implicitly require those of the other to be fully realized, and vice versa. This kind of integration requires a thorough understanding of the logic underpinning each theory, and how the two are related has to be articulated before hypotheses are framed. The potential for making a significant contribution depends on whether the integration offers new questions and new insights to each theory and its respective literature. For example, Silverman (1999) integrated elements of transaction cost economics and the resource-based view of the firm in a study of corporate diversification.

We wish to emphasize that using multiple theories can be a very effective way to create strong theory. The challenges of explanatory coherence, however, are greater when the theories utilized are from different base disciplines. Although *AMJ* encourages multidisciplinary research, the majority of published management papers focus on a single core discipline (Agarwal & Hoetker, 2007). Work integrating ideas from different areas has significant potential to contribute to theory, but the actual integration of the ideas must be carefully done.

Coherence. One of the biggest problems in the development of an effective theory section is explaining why one has chosen a specific set of explanatory variables over others. Without a strong discussion of coherence, readers and reviewers will wonder what holds a theoretical narrative together (Dubin, 1976; Whetten, 1989). The key is to address the question of why these variables (and only those variables) were selected. An effective theory section must explain how these variables fit together in a way that creates a strong and coherent theoretical contribution and doesn't leave the reader wondering why other variables weren't included. The proposed hypotheses should be linked a way that creates an overall contribution to the topic. Graebner (2009) did a nice job of weaving together literature from trust and agency theory in a qualitative examination of acquisitions of entrepreneurial firms.

A strong conceptual framework does not require a figure with boxes and arrows to explain how the hypotheses fit together—although a figure can help readers visualize the framework. What matters is that a clear, overarching research question drives the hypotheses, and one explains clearly, by drawing on the underlying theoretical and empirical work on the research topic, how these explanatory variables come together.

What we have said above regarding entering the conversation with previous research leads to the conclusion that persuasive logic is best served by a combination of all three approaches: building on established theory, offering relevant empirical evidence, and explaining *how* variation in X leads to variation in Y. But explanatory logic serves as the foundation; without it, appeals to existing theory fail to ring true, and offering only empirical evidence leaves the reader wondering "why?" Further, building on established theory can lead to an ex-

planation of *how*, because mediators often flow out of theorizing.

Pitfalls

Having described the core elements of grounding hypotheses, we felt it would be useful to review some of the recurring pitfalls that reviewers identify when evaluating the hypothesis development in a submission. Common pitfalls in grounding hypotheses include lack of specificity, fragmented theorizing, and stating the obvious.

Lack of specificity. Lack of specificity occurs when one's explanatory logic draws from a theory that speaks to a much broader or more general domain. For example, trait activation theory (Tett & Guterman, 2000) offers an explanation of how the attitudes and behaviors associated with personality traits are "activated" in the context of an individual's social environment. It thus offers an important bridge to researchers who seek to explain attitudes and behaviors in organizations by means of personality traits. However, it is general in its application and, though perhaps necessary to explanation of why a particular ensemble of environmental factors will activate attitudinal and behavioral manifestations of a specific trait, it is not a sufficient explanation. Social exchange theory (Blau, 1964) offers another example; it can ground one's logic at a general level (e.g., favors beget reciprocation), but does not clearly ground more specific operationalizations of that relationship (e.g., civility predicts job performance). The particulars and specifics need to be explained—and this guidance applies to all instances in which the domain of the theory one draws on to buttress claims is broader or more general than that of the hypotheses themselves.

Fragmented theorizing. Fragmented theorizing is implied when authors have a model with multiple hypothesized relationships in which each link is supported by logic drawn from a different theory. This approach may be motivated by the mistaken belief that the more theories, the better. Unfortunately, the impression this can create in the minds of reviewers is that the authors are engaging in post hoc theorizing, casting about in the literature for a theory that seems to fit a given hypothesis or, worse still, one that matches the variables on which they have already gathered data. Our observation is not meant to suggest that authors should not use multiple theories to support their hypotheses. Rather, it suggests that support drawn from multiple theories needs to be integrated into a coherent and cohesive explanatory narrative. (See the section on coherence above.)

Stating the obvious. Though it seems counterintuitive, supporting one's hypotheses so thoroughly that they seem obvious and therefore uninteresting is not uncommon. If a hypothesis states the obvious or makes a claim that is common knowledge, then, although true, it also is likely to be trivial (Davis, 1971). When a reviewer says, "I can't imagine how or when the null hypothesis could ever be the case," she or he is making precisely this point.

One way to remedy this problem is to flirt with the null hypothesis—that is, reflect on the plausibility of the opposite argument or the absence of a relationship. Then, frame the alternative hypotheses as alternatives to what can be seen as plausible, or even as received wisdom. This entails thoughtfully considering theoretical perspectives that would lend credence to the null. If it proves difficult to frame the null hypotheses as plausible, then your alternatives may in fact be obvious and trivial.

Conclusions

Hypotheses are the heart of a paper, and grounding hypotheses is one of the most important tasks in crafting effective theory. A strong theory section has to effectively engage prior literature, both theoretical and empirical, but must go beyond it to build a strong logical argument. A great deal of thought goes into every paper, and the theory section is key to explaining how one is going to add value to the research topic and why these specific hypotheses make sense individually and fit together to form a coherent conceptual framework.

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© Academy of Management Journal
2012, Vol. 55, No. 1, 8–12.
http://dx.doi.org/10.5465/amj.2012.4001

FROM THE EDITORS

PUBLISHING IN *AMJ*—PART 5: CRAFTING THE METHODS AND RESULTS

Editor's Note:

This editorial continues 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 April with "Part 6: Discussing the Implications." - J.A.C.*

Once the arduous, but exciting, work of selecting an intriguing and appropriate topic, designing and executing a sound data collection, crafting a compelling "hook," and developing a solid theory is finished, it is tempting to sit back, relax, and cruise through the Methods and Results. It seems straightforward, and perhaps a little mundane, to report to the readers (1) how and why the data were obtained; (2) how the data were analyzed and what was found. Indeed, it is unlikely that many readers of *AMJ* have waited with bated breath for an entertaining narrative in this installment of the Publishing in *AMJ* editorial series. If we fall short of being compelling, therefore, we hope to at least be informative.

As authors ourselves, we have, admittedly, succumbed to the temptation of relaxing our concentration when it is time to write these sections. We have heard colleagues say that they pass off these sections to junior members of their research teams to "get their feet wet" in manuscript crafting, as though these sections were of less importance than the opening, hypothesis development, and Discussion sections. Perhaps this is so. But as members of the current editorial team for the past two years, we have come face-to-face with the reality that the Methods and Results sections, if not the most critical, often play a major role in how reviewers evaluate a manuscript. Instead of providing a clear, detailed account of the data collection procedures and findings, these sections often leave reviewers perplexed and raise more questions than they answer about the research procedures and findings that the authors used. In contrast, an effective presentation can have a crucial impact on the extent to which authors can convince their audiences that their theoretical arguments (or parts of them) are supported. High-quality Methods and Results sections also send positive signals about the conscientiousness of the author(s). Knowing that they were careful and rigorous in their preparation of these sections may make a difference for reviewers debating whether to recommend a rejection or a revision request.

To better understand the common concerns raised by reviewers, we evaluated each of our decision letters for rejected manuscripts to this point in our term. We found several issues arose much more frequently in rejected manuscripts than they did in manuscripts for which revisions were requested. The results of our evaluation, if not surprising, revealed a remarkably consistent set of major concerns for both sections, which we summarize as "the three C's": completeness, clarity, and credibility.

THE METHODS

Completeness

In the review of our decision letters, perhaps the most common theme related to Methods sections was that the authors failed to provide a complete description of the ways they obtained the data, the operationalizations of the constructs that they used, and the types of analyses that they conducted. When authors have collected their data—a primary data collection—it is important for them to explain in detail not only what happened, but why they made certain decisions. A good example is found in Bommer, Dierdorff, and Rubin's (2007) study of group-level citizenship behaviors and job performance. We learn in their Methods how the participants were contacted (i.e., on site, by the study's first author), how the data were obtained (i.e., in an on-site training room, from groups of 20–30 employees), what kinds of encouragement for participation were used (i.e., letters from both the company president and the researchers), and who reported the information for different constructs in the model (i.e., employees, supervisors,

and managers of the supervisors). In addition, these authors reported other relevant pieces of information about their data collection. For example, they noted that employees and their supervisors were never scheduled to complete their questionnaires in the same room together. In addition, they reported a system of "checks and balances" to make sure supervisors reported performance for all of their direct reports. Providing these details, in addition to a full description of the characteristics of the analysis sample at the individual and team levels, allows reviewers to evaluate the strengths and weaknesses of a research design. Although it is reasonable to highlight the strengths of one's research, reporting sufficient details on the strengths and potential weaknesses of the data collection is preferred over an approach that conceals important details, because certain compromises or flaws can also yield advantages. Consider the example of data collected with a snowball sampling approach in two waves separated by a few months. A disadvantage of this approach would likely be that the sample matched over the two waves will be smaller than the sample resulting if the researchers only contact wave 1 participants to participate in wave 2. But, this approach also has certain advantages. In particular, large numbers of one-wave participants (i.e., those that participated either in the first wave or the second wave) can be used to address response bias and representativeness issues straightforwardly.

In many other cases, the data for a study were obtained from archival sources. Here a researcher may not have access to all the nitty-gritty details of the data collection procedures, but completeness in reporting is no less important. Most, if not all, archival data sets come with technical reports or usage manuals that provide a good deal of detail. Armed with these, the researcher can attempt to replicate the detail of the data collection procedures and measures that is found in primary data collections. For a good example, using the National Longitudinal Survey and Youth Cohort (NLSY79), see Lee, Gerhart, Weller, and Trevor (2008). For other archival data collections, authors construct the dataset themselves, perhaps by coding corporate filings, media accounts, or building variables from other sources. In these cases, a complete description of how they identified the sample, how many observations were lost for different reasons, how they conducted the coding, and what judgment calls were made are necessary.

Regardless of the type of data set a researcher has used, the goals in this section are the same. First, authors should disclose the hows, whats, and whys of the research procedures. Including an Appendix with a full list of measures (and items, where appropriate), for example, is often a nice touch. Second, completeness allows readers to evaluate the advantages and disadvantages of the approach taken, which on balance, creates a more positive impression of the study. Third, a primary goal of the Methods section should be to provide sufficient information that someone could replicate the study and get the same results, if they used exactly the same procedure and data. After reading the Methods section, readers should have confidence that they could replicate the primary data collection or compile the same archival database that the authors are reporting.

Clarity

Far too often, authors fail to clearly explain what they have done. Although there are many potential examples, a typical, very common, problem concerns descriptions of measures. Reviewers are often concerned with language such as "we adapted items" or "we used items from several sources." Indeed, not reporting *how* measures were adapted was the modal issue related to measurement in the evaluation of our decision letters. Ideally, authors can avoid these problems simply by using the full, validated measures of constructs when they are available. When this is not possible, it is imperative to provide a justification for the modifications and, ideally, to provide additional, empirical validation of the altered measures. If this information is not initially included, reviewers will invariably ask for it; providing the information up front improves the chances of a revision request.

Another very common clarity issue concerns the justification for variable coding. Coding decisions are made in nearly every quantitative study, but are perhaps most frequently seen in research involving archival data sets, experimental designs, and assignment of numerical codes based on qualitative responses. For example, Ferrier (2001) used structured content analysis to code news headlines for measures of competitive attacks. In an excellent example of clarity, Ferrier described in an organized fashion and with straightforward language how the research team made the coding decisions for each dimension and how these decisions resulted in operationalizations that matched the constitutive definitions of the competitive attack dimensions.

Credibility

Authors can do several uncomplicated things to enhance perceptions of credibility in their Methods

sections. First, it is important to address why a particular sample was chosen. Reviewers often question why a particular sample was used, especially when it is not immediately obvious why the phenomenon of interest is important in the setting used. For example, in Tangirala and Ramanujam's study of voice, personal control, and organizational identification, the authors opened the Methods by describing why they chose to sample front-line hospital nurses to test their hypotheses, noting (1) "they are well positioned to observe early signs of unsafe conditions in patient care and bring them to the attention of the hospital" and (2) "there is a growing recognition that the willingness of nurses to speak up about problems in care delivery is critical for improving patient safety and reducing avoidable medical errors (such as administration of the wrong drug), a leading cause of patient injury and death in the United States" (2008: 1,193). Second, it is always good practice to summarize the conceptual definition of a construct before describing the measure used for it. This not only makes it easier for readers—they don't have to flip back and forth in the paper to find the constitutive definitions—but when done well will lessen reader concerns about whether the theory a paper presents matches the tests that were conducted. Third, it is always important to explain why a particular operationalization was used. For example, organizational performance has numerous dimensions. Some may be relevant to the hypotheses at hand, and others are not. We have often seen authors "surprise" reviewers by introducing certain dimensions with no justification. In cases in which alternative measures are available, authors should report what other measures they considered and why they were not chosen. If alternative measures are available in the data set, it is often a good idea to report the findings obtained when those alternative measures were used. Fourth, it is crucial to justify model specification and data analysis approaches. We have often seen authors include control variables without sufficiently justifying why they should be controlled for. For some types of data, multiple possible methods for analysis exist. Authors need to justify why one method rather than the other(s) was used. Panel data, for example, can be analyzed using fixed-effect models or random-effect models. Multiple event history analysis methods can analyze survival data. Each method has its specific assumption(s). In some cases, additional analysis is warranted to make the choice (for example, doing a Hausman test to choose between fixed- and random-effect models for panel data).

THE RESULTS

Completeness

Effectively writing a Results section is not an easy task, especially when one's theoretical framework and/or research design is complex, making completeness all the more important. For starters, including a table for means, standard deviation, and correlations is a piece of "low-hanging fruit." The information in this table may not have directly tested hypotheses, yet it paints an overall picture of the data, which is critical for judging the credibility of findings. For example, high correlations between variables often raise concerns about multicollinearity. A large standard deviation relative to the mean of a variable can raise concerns about outliers. Indeed it is a good practice to check data ranges and outliers in the process of data analyses so as to avoid having significant findings mainly driven by a few outliers. Distributional properties of variables (such as means and minimum and maximum values) reported in a table are informative by themselves. For example, in a study on CEO succession, means of variables that measured different types of CEO successions can tell the distribution of new CEOs in the sample recruited from different sources. These distributional properties describe the phenomenon of CEO successions and have important practical implications.

In reporting results, it is important to specify the unit of analysis, sample size, and dependent variable used in each model. This is especially crucial when such information varies across models. Take Arthaud-Day, Certo, Dalton, and Dalton (2006) as an example. These authors examined executive and director turnover following corporate financial restatements. They had four dependent variables: CEO turnover, CFO turnover, outside director turnover, and auditing commitment member turnover. In models of CEO and CFO turnover, because they were able to identify the month of the turnover, they constructed the data using "CEO/CFO" as the unit of analysis and used a Cox model to examine the timing of the executive turnover. The sample size of the model on CEO turnover was 485, and the sample size of the model on CFO turnover was 407. In comparison, in examining turnover of outside directors and audit committee members, because Arthaud-Day and her colleagues were unable to determine the month in which outside directors and audit committee members left office, they constructed the data using director/auditing committee member-year as the unit of analysis and used logistic regression to examine the likelihood of their turnover. The sample size of the model on outside director turnover was 2,668, and the sample size for

auditing committee member turnover was 1,327. The take-away here is that careful descriptions such as those Arthaud-Day and colleagues provided help readers calibrate their interpretations of results and prevent reviewers from raising questions about clarification.

Clarity

The purpose of a Results section is to answer the research questions that have been posed and provide empirical evidence for the hypotheses (or note that evidence is lacking). We often see, however, that authors do not relate their findings to the study's hypotheses. We also see that authors report the results in the Results section, but discuss their linkage with hypotheses in the Discussion section or, conversely, begin to discuss the implications of the findings in the Results prematurely, rather than doing this in the Discussion. In these cases, the authors fail to describe what the results indicate with respect to the focal topic of the study in a clear manner. To avoid this problem, it helps to summarize each hypothesis before reporting the related results. Try this format: "Hypothesis X suggests that . . . We find that . . . in model . . . in Table . . . Thus, Hypothesis X is (or isn't) supported." Although this format may sound mechanical or even boring, it is a very effective way to clearly report results (see also Bem, 1987). We encourage and welcome authors to experiment with novel and *clear* ways to present results. We also suggest that authors report the results associated with their hypotheses in order, beginning with the first hypothesis and continuing sequentially to the last one, unless some compelling reasons suggest that a different order is better.

In many studies, the results do not support all the hypotheses. Yet results that are not statistically significant and those with signs opposite to prediction are just as important as those that are supported. However, as one editor noted, "If the results are contrary to expectations, I find authors will often try to 'sweep them under the rug.'" Of course, reviewers will catch this immediately. Needless to say, sometimes such results reflect inadequate theorizing (e.g., the hypotheses are wrong, or at least there are alternative arguments and predictions). Other times, however, unsupported results are great fodder for new, critical thinking in a Discussion section. The point is that all results—significant or not, supporting or opposite to hypotheses—need to be addressed directly and clearly.

It is also a good practice to reference variables across sections in the same order—for example, describe their measures in the Methods section, list them in tables, and discuss results in the Results section all in the same order. Such consistency improves the clarity of exposition and helps readers to both follow the manuscript and find information easily. It also provides authors with a checklist so that they will remember to include relevant information (e.g., a variable included in the models is not mentioned in the Methods section and/or in the correlation matrix).

Credibility

Although every part of a paper plays an important role in helping or hurting its credibility (e.g., adequate theorizing and rigorous research design), there are some things authors can do in their Results sections to enhance the perceived credibility of findings. First, it is crucial to demonstrate to readers why one's interpretations of results are correct. For example, a negative coefficient for an interaction term may suggest that the positive effect of the predictor became weaker, or disappeared, or even became negative as the value of the moderator increased. Plotting a significant interaction effect helps one visualize the finding and thus demonstrate whether the finding is consistent with the intended hypothesis. Aiken and West (1991) provided some "golden rules" on how to plot interaction effects in regressions. Beyond these, determining whether the simple slopes are statistically significant is often important in assessing whether one's results fully support hypotheses; techniques developed by Preacher, Curran, and Bauer (2006) are helpful in these calculations.

Second, if alternative measurements, methods, and/or model specifications could be used for a study, but authors only report results using one possible choice, readers may have the impression that the authors "cherry-picked" findings that were consistent with the hypotheses. Supplementary analyses and robustness checks can address these concerns. For example, Tsai and Ghoshal (1998) examined the value creation role of a business unit's position in intrafirm networks. Although they proposed the hypotheses at the individual business unit level, they generated several measures of business units' attributes from data at the dyadic level. These steps raised some concerns about level of analysis and the reliability of the results. To address these concerns, they also analyzed data at the dyadic level and obtained consistent results.

Third, even if a result is statistically significant, readers may still ask, So what? A statistically significant effect is not necessarily a practically important effect. Authors typically discuss the practi-

cal implications of a study in their Discussion; they can, however, conduct and report additional analyses in Results to demonstrate the practical relevance of findings. A good example is found in Barnett and King's (2008) study of spillover harm. These authors stated the following Hypothesis 1: "An error at one firm harms other firms in the same industry" (Barnett & King, 2008: 1,153). In addition to reporting the statistical significance of the predictor, the authors provided information to communicate the average scale of such spillovers. They reported that "following an accident that injured an average number of employees (3.5), a chemical firm with operations in the same industry as that in which an accident occurred could expect to lose 0.15 percent of its stock price" and that "after an accident that caused the death of an employee, the firm could expect to lose an additional 0.83 percent" (Barnett & King, 2008: 1,160). In other cases, authors may want to discuss the implications of small effect sizes, perhaps by noting how difficult it is to explain variance in a given dependent variable or, in the case, of an experiment, noting that a significant effect was found even though the manipulation of the independent variable was quite minimal (Prentice & Miller, 1992).

Conclusions

Crafting Methods and Results sections may not sound exciting or challenging. As a result, authors tend to pay less attention in writing them. Sometimes these sections are delegated to the junior members of research teams. However, in our experience as editors, we find that these sections often play a major, if not a critical, role in reviewers' evaluations of a manuscript. We urge authors to take greater care in crafting these sections. The three-C rule—completeness, clarity, and credibility—is one recipe to follow in that regard.

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© Academy of Management Journal
2012, Vol. 55, No. 2, 256–260.
http://dx.doi.org/10.5465/amj.2012.4002

FROM THE EDITORS

PUBLISHING IN *AMJ*–PART 6: DISCUSSING THE IMPLICATIONS

This editorial continues 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 conclude in June with "Part 7: Qualitative Distinctions." -J.A.C.*

Afterthought (*noun*):

1. a reflection after an act
2. something secondary or expedient
3. an action or thought not originally intended

By the time authors begin to craft a Discussion section, a long, sometimes arduous journey has been traveled. Study design and execution are normally well advanced, and the prospect of submission for publication consideration looms large. Thus, it is perhaps not surprising many authors view the Discussion as a perfunctory exercise—a final, obligatory hurdle to be overcome with dispatch so as not to delay a manuscript's transition to "under review" status. In approaching their Discussion as a technical formality (i.e., an afterthought in the mold of definitions 2 and 3) rather than as a forum in which to explore more deeply the significance of their work (definition 1), authors forego a number of valuable opportunities. Among them is the chance to strengthen their study's message, and in the process, convince readers of their manuscript's larger, underlying value. Another is the opportunity to embed their study more fully in the existing literature and thus engage like-minded scholars in a rich, robust theoretical conversation, perhaps even shape the future direction of that discourse.

These all-too-common lapses lead us to explore how authors might better approach the discussion of theoretical contributions. To be certain, Discussion sections encompass several dimensions, including practical implications, study limitations, and future research, each of distinct importance, and thus requisite components of any complete Discussion. That said, we restrict our attention to *theoretical implications*. In our experience as associate editors, we have found this aspect, which is both important and highly rewarding, often constitutes a major stumbling block. Thus, our aim is to outline some means of more plainly elucidating contributions to theory.

AN ENDING AND A BEGINNING

Our thoughts are shaped by the ideas of Whetten (1989) and Corley and Gioia (2011), who so very cogently answered the question, *what is a theoretical contribution*? We believe discussion of this important manuscript dimension can be enhanced through the use of a technique that treats the passage as a twofold, somewhat paradoxical entity—as both an ending and a new beginning, realized concurrently. It constitutes an ending in the sense that discussion of theoretical implications helps to bring closure to a study, illuminating its major inroads in a broad and reflective fashion. It also represents a new beginning in that it recasts contemporary theoretical understanding, bringing to light new and valuable ideas. In our experience, this approach has helped authors illuminate the two or three most critical theoretical insights afforded by their research investigation. We conclude with a summary of common pitfalls, or tendencies that compromise the effective summary of theoretical implications.

Theoretical Implications: An Ending

Why do scholars choose to undertake a particular study? In most instances, it is because they are captivated by a research question posing a novel and important challenge of broad consequence. The same is true of readers' interest. It is perhaps not surprising then that the most impactful studies are ones which explore larger questions of theoretical significance over issues of more incremental scope (Colquitt & Zapata-Phelan, 2007). Although the aim of resolving a grand puzzle plays a central role at the inception of any research study, its meaning, if not allure, is often lost on authors by the time they arrive at the Discussion. Intricacies of conceptual development, study design, and analysis often lead to losing sight of the broader theoretical challenge that started researchers on their path.

Yet an impactful Discussion section retains that focus. Better said, it makes a point of revisiting the study's original theoretical motivation, and it does so for a number of reasons: First, a return to the work's theoretical catalyst is a means of effecting orderly completion. Recap affords a basis on which to assess progress on the mission of resolving a theoretical puzzle. Of course, the original theoretical question need not be perfectly solved; the investigation may, for example, have uncovered some unanticipated issues or problematic assumptions. Nevertheless, revisiting theoretical motivation affords a valid reference point, one appreciated by authors and readers alike. In reaching a paper's Discussion section, most readers (as the paper's authors originally were) have been sustained by the tension inherent in the study's motivation. Revisiting ensures that authors deliver on their study's early promise—that is, they answer the underlying theoretical question(s)—and so fulfill their compact with readers.

Second, a return to the original theoretical motivation of a paper affords a means to cogently and succinctly address the *so what*? question. Among the more common reasons manuscripts are rejected at *AMJ* is their failure to offer a meaningful theoretical advance. Of course, the effort to do so begins months, if not years, before manuscript submission, with topic choice (see Colquitt and George, "From the Editors," *AMJ* 54: 432–435]) and its subsequent clear articulation in a manuscript's Introduction (see Grant and Pollock, "From the Editors," *AMJ* 54: 873–879). However, the Discussion section affords a venue in which to answer this question more robustly than before and to articulate in a richer fashion how the study changes, challenges or otherwise fundamentally refines understanding of extant theory (and/or its core concepts, principles, etc.). As experts in a given area, researchers often fail to appreciate that others may not share the same theoretical interests and/or see their underlying merit. Thus, an effective Discussion section not only reports the study's theoretical inroads, but also contextualizes them in a fashion that makes clear their larger utility for students of organization. Sherer and Lee (2002) offers an excellent demonstration. The authors both answer the theoretically grounded questions that gave rise to their research and frame those responses in a manner that casts light on some under-appreciated aspects of resource dependence and institutional perspectives—specifically, how their core processes conspire to drive innovation. Such elaboration shows how scholars and practitioners might better capitalize on these theories for purposes of understanding management and organization.

Finally, successful Discussion sections afford a synthesis of their studies' empirical findings. They examine results of hypothesis tests in an aggregate fashion, weaving them together to present a unified, theoretically grounded narrative of the studies' discoveries. Of course, some empirical findings may be unexpected, or even contrary to expectations. In that case, reconciliation is in order; so too is further examination of causal arguments to help readers, and indeed the field at large, to better understand the underlying phenomena. The end result, however, is always the same. Namely, integration not only fosters development of a single, coherent message—far more likely to resonate with readers than a mixed message—but also affords the chance to underscore the cohesive nature of a study's conceptual model, thus lending incremental credence to its design. Agarwal, Echambadi, Franco, and Sarkar (2004) demonstrate this skillfully. Their Discussion section synthesizes the results of individual hypothesis tests, integrating them in a manner that imparts a clear and parsimonious theoretical account of corporate spin-outs.

Theoretical Implications: A New Beginning

Perhaps the most straightforward implications are those derived from a logical interpretation of a study's findings. *What* do the results tell us about underlying theoretical constructs, principles, and their relationships? *When* do these patterns emerge, and in what context? *How* do they refine appreciation of the underlying theory? These are but a sampling of "first- order" theoretical implications that might be advanced. More interesting and valuable are insights that delve deeper into observed relationships to address the question *why*? In exploring this dimension, authors begin to examine more fully underlying mechanisms and processes—causal explanations that both enrich understanding of a given theory and allow readers to make greater sense of complex organizational phenomena (Whetten, 1989). Critical here is a bridge between a study's findings and the larger literature. It is only through a connection to broader understanding that the theoretical "value added" of a given study can be interpreted and, indeed, appreciated (see Rynes, "From the Editors," *AMJ* 45: 311–313 and Bergh, "From the Editors," *AMJ* 46: 135–136).

Of course, a study's objective findings are not the exclusive source of valuable insight. Their juxtaposition relative with earlier results often affords rich and meaningful theoretical nuance. This is apparent, for example, in the case of competing evidence. An exploration of departures from earlier findings

can reveal unexpected boundary conditions, or perhaps even questionable assumptions. It can also shed light on previously overlooked gaps in theoretical understanding, such as unanticipated contingencies and/or critical omissions in definitions of focal constructs. Such is the case in Seibert, Kraimer, and Liden (2003); those authors explain how their theoretical work brings reconciliation to seemingly divergent perspectives, and correspondingly, nuanced understanding of the role social capital plays in career success. Although divergence from earlier findings is quick to captivate reader interest (Weick, 1989), findings consistent with prior research can also help to hone more subtle dimensions of understanding (Hollenbeck, 2008). Siebert et al., for example, discuss how controlling for previously identified predictors of career success strengthens the contribution made by their primary focus on network structure and social resources. Whatever the particular pattern (i.e., consistency or divergence), again, it is the exploration of findings relative to earlier, related work that often illuminates previously unappreciated theoretical insights.

Finally, we find that authors also effectively inform theoretical understanding by exploring the path that led to discovery of their study's findings. Few research investigations follow a linear trajectory. The final draft is often a portrayal of the most refined ideas (i.e., what worked), yet less successful efforts may prove equally informative. This is especially true if and when other theoretical perspectives were explored and found wanting. In fact, one of the tests of any study's theoretical inferences is the extent to which they hold up to the challenge of "alternative explanations." A post hoc reflection attending to the plausibility of other accounts lends incremental support to a study's conclusions and also potentially illuminates important differences among theoretical perspectives. This is demonstrated, for example, in Faems, Janssens, Madhok, and Van Looy's (2008) Discussion section, which not only examines the merits of alternative perspectives on the governance of alliances, but also illuminates key differentiating aspects of structural and relational perspectives.

The same is true of unsupported hypotheses. They often constitute a rich, yet commonly foregone, way to inform theoretical understanding. Our experience as associate editors suggests there is reluctance among many scholars to attend to (much less retain) unsupported hypotheses. Yet the failure to find rigorous support for key theoretical arguments is in itself informative and rather thought-provoking, and such findings are certainly helpful to continued theoretical development. Thus, in reflecting upon the discoveries that have accrued over the course of their study, authors are well served by attending not only to anticipated (i.e., supported) findings, but also to prominent and unanticipated insights (e.g., nonfindings).

COMMON PITFALLS

If the above sections outline some guidelines and suggestions, it is equally important to recognize some of the common errors authors make in articulating their studies' theoretical contribution. Our experience suggests three are highly prevalent: rehashing results, meandering, and overreaching.

Rehashing Results

The transition from the Results to the Discussion marks a change in a narrative's focus, from reviewing what emerged in the study to explaining why the findings are important and how they change the conversation that the research joins. A common mistake authors make is to devote too much discussion to summarizing and resummarizing the results of their hypothesis tests while devoting too little attention to explaining what the results mean. In some cases, authors restate the findings in the first few paragraphs of the Discussion section and then move on to other subsections (practical implications, limitations, future research directions, and so on) without addressing the study's theoretical implications whatsoever. As readers transition to a Discussion section, the study's findings are fresh in their minds. Consequently, what's needed at this point is not a rehashing of the results, but a thoughtful interpretation of why the findings are important and worthy of dissemination (in the form of a published article). It is appropriate to remind readers of the paper's key findings, but only as the departure point for explaining how the results bring resolution to the puzzle that motivated the research to begin with and set the stage for new and promising lines of inquiry.

Meandering

The second kind of mistake authors make in their Discussion sections, meandering, occurs when a narrative references numerous theoretical implications, some or all of which seem disconnected from each other, the paper's "hook" (see Grant and Pollock, "From the Editors," 54: 873–879), and/or the paper's theoretical development (see Sparrowe and Mayer, "From the Editors," *AMJ* 54: 1098–1102). Meandering implications subsections lack focus and come across as superficial. A paper's discus-

sion of theoretical implications should cohere around a small number of important issues that are covered in great depth. The implications themselves will likely reside at a higher level of abstraction than the data and parsimoniously explain the results of the hypothesis tests, both supportive and unsupportive. What can authors do to avoid crafting an implications subsection that meanders? Instead of identifying implications for each result, they might follow the better strategy of focusing on what the findings mean collectively. When it comes to beefing up theoretical implications, authors should resist the temptation to simply slip in an extra implication or two. Having completed a draft of the implications, they might find it is worthwhile to go back and ask whether the subsection is as focused as it could be. Do the implications close the loop on the specific problems that are introduced in the paper's opening? In other words, do they cohere with the research questions and theoretical inroads identified in the Introduction? Are there opportunities to reduce the number of implications that are addressed, while deepening the coverage of those that remain? Attending to these matters will make for a more focused and persuasive presentation of a paper's contributions to theory.

Overreaching

A third mistake authors make in their Discussion sections involves deriving sweeping conclusions that outstrip the data. In an effort to convince readers that their work has important and wide-ranging theoretical implications, authors may overreach. Admittedly, there may be some subjectivity associated with this judgment, as one person's overreach may be another's grand implication. Reviewers are likely to conclude that an author has gone too far when a narrative drifts into domains that seem disconnected from the empirics and/or went unmentioned in the paper's opening or theoretical development. When authors experience a strong temptation to weave new (i.e., previously unmentioned) theory into the Discussion, they should give some thought to how they might introduce those ideas earlier in the paper—perhaps using them to strengthen the paper's hook.

Overreaching is also more likely to occur when authors treat their papers' theoretical implications as an afterthought in the mold of definitions 2 or 3, rather than definition 1. Having crafted a paper's Introduction, Theory, and Methods sections, authors may set out to write the Discussion, only to realize that the paper's theoretical implications are somewhat pedestrian after all. The shortage of strong implications to which authors may legitimately lay claim gives rise to claims that cannot plausibly derive from the results. One way of avoiding this pitfall is to think about what the implications subsection will look like before writing a paper's Introduction and Theory sections. If it seems difficult, if not impossible, to outline an implications subsection that feels meaty and persuasive, it is likely that the project lacks the depth and scope that aligns with *AMJ*'s mission.

CONCLUSIONS

Ultimately, publishing refereed journal articles is a means to the end of making a contribution to a specific body of knowledge. The variation in mission statements across journals reflects differences in the kinds of contribution(s) journals value and aim to publish. At *AMJ*, theoretical advance is a primary emphasis, and it is in their Discussions that authors can make plain their accomplishments on this dimension. Our experience shows that the best Discussions (in addition to outlining their studies' limitations, practical implications, and suggestions for future research) provide a clear and compelling answer to the original research question, cast in a theoretical light. Of course, this necessitates a meaningful connection to the broader, relevant theoretical literatures and, in the interest of advancement, illumination of new and important insights uniquely generated by the immediate investigation. In short, a Discussion section affords a venue in which to elucidate how a study changes, challenges, or otherwise fundamentally advances, existing theoretical understanding. The quality of this section, and of a paper more generally, is greatly enhanced by avoiding three mistakes, best summarized as not doing enough (rehashing), doing too much (meandering), and going too far (overreaching). We hope that with this knowledge in hand, authors may more willingly embrace not only the opportunity, but also the rewards of contributing more cogently to ongoing theoretical conversations.

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© Academy of Management Journal
2012, Vol. 55, No. 3, 509–513.
http://dx.doi.org/10.5465/amj.2012.4003

FROM THE EDITORS

PUBLISHING IN *AMJ*—PART 7: WHAT'S DIFFERENT ABOUT QUALITATIVE RESEARCH?

This editorial concludes 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. -J.A.C.*

> I'm comfortable with my knowledge of qualitative work—and my ability to give some insight on a specific piece—but for whatever reason, this quantitative-to-qualitative comparison is hard for me to make. And I don't understand the reasons that is hard! -Panelist

Over the past six issues, our editorial team has presented a series on how to write effective *AMJ* submissions. Much of what this series has covered is relevant to both quantitative and qualitative papers. For example, the five criteria that Colquitt and George (June 2011 "From the Editors" [vol. 54: 432–435]) identify for choosing topics—significance, novelty, curiosity, scope, and actionability—apply equally well to qualitative work. However, there are also key differences. For example, qualitative work does not typically suffer from the measurement, operationalization and model specification problems identified by Bono and McNamara (August 2011 "From the Editors" [vol. 54: 657–660]). As our opening quote illustrates, these differences are not always easy to articulate or explain. In this final FTE for the "Publishing in *AMJ*" series, we provide our perspective on the key differences.

To do this, we focus our thoughts around this provocative question: If a colleague who has only ever published quantitative papers before asked you to identify the main differences between qualitative and quantitative papers (besides the type of data presented), how would you respond? We put this question to a panel of some of *AMJ*'s top qualitative authors and reviewers. We believe we hit a chord with this question, as we received 24 replies (from more than half of the people we contacted), a return that far exceeded our expectations. There was a range of responses from our colleagues; some felt the differences were stark, whereas others felt the differences were superficial.

Rather than merely reporting back what they said, we synthesized their views (and sprinkled in some of the more revealing quotes) while bringing to bear our own experiences from the more than 180 decisions we have cast in our tenure as associate editors responsible for qualitative manuscripts. Instead of providing a point-by-point comparison with what has been written previously in the series (a result that would be too long and too tedious), we offer a more holistic view of the unique attributes of a qualitative paper for *AMJ*. In this way, an author who reads this editorial will receive helpful guidance on the writing process without having to read the other six pieces but could also find direct comparisons if reading the current FTE in conjunction with the previous six pieces. We illustrate our points from the many qualitative *AMJ* Best Article Award Winners. We hope this editorial will prove insightful not only for those researchers who have attempted to publish qualitative research in *AMJ* in the past, but also for those who may wish to do so for the first time in the future.

BUILD THEORY INDUCTIVELY

Papers published in *AMJ* typically change, challenge, or fundamentally advance theory through insights on focal phenomena. Most qualitative papers advance theory by building it inductively, although qualitative data can be used for theory testing, or deduction, as well. This difference in purpose drives the most significant differences between qualitative and quantitative *AMJ* papers, which we discuss below.

A Short, Multipurpose Front End

> Qualitative researchers often have to build a case for their research question and motivate their work more strongly than quantitative researchers. . . . Thus in the front end of the manuscript, the writer has to work harder to establish the theoretical gap and make a compelling case for why this research question is important.

All *AMJ* articles need an engaging front end that motivates the research (see Grant and Pollock, October 2011 "From the Editors" [vol. 54: 873-879]).

The introduction and literature review provide key opportunities to grab and direct the reader's attention toward an understanding of theory that will carry throughout the paper. The front end of a quantitative article typically includes an introduction, literature review, and the development of new theory by way of hypotheses. The literature review, therefore, sets the background for the hypotheses. Because qualitative papers fulfill a different purpose, their front end is shorter, yet it serves more functions.

The front end of a qualitative manuscript must not only hook the reader, expose a significant gap in a current theoretical conversation that warrants the development or extension of theory, and situate research questions in that conversation, but also provide a framework for the textual data that follow and a springboard for the new emergent theory. If the literature review reveals too much, then readers feel that theory did not emerge from the data; if the literature review reveals too little, the project will seem too broad in scope to be manageable. Thus, much is riding on these first sections of a qualitative paper.

Plowman, Baker, Beck, Kulkarni, Solansky, and Travis's (2007) article on radical change, for example, grabbed readers' attention by describing changes at "Mission Church" and built the theoretical platform from extant theory on radical organizational change and from complexity theory. Even the framing of their research question was able to simultaneously describe their project, create intrigue, and expose the theoretical gap: "In this research, we attempt to understand how and why an initial small change, whose ultimate consequences were unintended, escalated and led to radical organizational change" (Plowman et al., 2007: 516).

A Long, Robust Back End

Quantitative work often builds theory in the front end by developing hypotheses that are then tested. Since new theory is discussed in the front end, the back end of a quantitative paper focuses primarily on the implications of the empirical results. Qualitative works, on the other hand, reserve the biggest punch for the back end. A strong Discussion section should not only summarize the findings and ultimately delineate the theoretical and practical implications that are also demanded of quantitative papers (see Geletkanycz and Tepper, April 2012 "From the Editors" [vol. 55: 256–260]), but also integrate data and theory in a way that explicitly conveys the connections between the analyzed data, the emergent theory, and the literatures at which the contribution is aimed. This often results in a complex and dynamic discussion, especially given the high interdependence of the anchoring theory, data analysis, and theoretical contribution.

Plowman et al. succinctly summarized their findings in a single sentence in the back end of their article: "Mission Church's experience of decline and renewal supports the notion that change can be viewed as continuous/evolutionary . . . but also provides empirical evidence that continuous change, whose pace is much slower than that of episodic change, can become radical" (2007: 537; embedded citations removed for clarity). To efficiently manage the theoretical extension, Plowman et al. listed their propositions in a table, juxtaposed against the theory of change and complexity theory, which allowed them more room to discuss the implications. This emphasized the uniqueness and importance of their work.

Comprehensive, Personal, and Transparent Methods

> There is not as clear an agreement among qualitative researchers as to what constitutes acceptable methodology and analysis. . . . The signature of qualitative research is its solid grounding in the phenomenon; however each researcher's journey in uncovering the phenomenon is unique and nonlinear.

Qualitative researchers have considerable latitude in their methods, including the way in which they conduct interviews or ethnographies and the techniques they use to analyze data. Unlike quantitative studies, qualitative research cannot simply reference well-known data sets and statistical tests. It is critical, then, that qualitative researchers offer detailed accounts of their data sources and analysis. Communicating the journey (from initiating their project to submitting their manuscript) gives meaning to the accounts of the data and emergent theory as well as signaling the quality of the research exercise, the credibility of the researcher, and, ultimately, the trustworthiness of the data and the emergent theorizing. As such, the researcher often features prominently, in first person and reflexively, in the description of the methods.

Describing that journey is a hallmark of many of the award-winning qualitative articles published in *AMJ*. Dutton and Dukerich's (1991) study of homelessness at the New York and New Jersey Port Authority is often hailed as an exemplar of qualitative research. Their description of their methods is detailed and personal and clearly reflects their nonlinear journey:

> Our initial research objective was to explore differences in how groups in the organization interpreted

and responded to the issue. The objective was consistent with research on organizational culture and the creation of meaning in organizations. . . . However, the data generated by informants indicated a surprisingly consistent pattern of issue interpretations . . . [that] emphasizes the dominant logic, collective beliefs, and consensual elements in how the homeless issue was interpreted over time. (Dutton & Dukerich, 1991: 552; embedded citations removed for clarity)

Creative Data Displays

Qualitative and quantitative scholars are similar. We all try to edit the messiness out of our research presentation. Yet, on the margin, qualitative research comes a bit closer to representing the messiness. And, that is the strength of what we do.

Unlike quantitative data, qualitative data cannot be easily synthesized or reduced into tables, so qualitative researchers must think creatively about showing their data. Some researchers account the data chronologically, others seek patterns across observations and prefer data displays based on first- and second-order codes. Most importantly, data must be shown, not merely described, so the reader can connect the raw data with the analyzed data, and the analyzed data with the emergent theorizing. The data must transport the reader into the context to provide a personal experience of the focal phenomenon and support for the emergent theory. The challenge is to show enough richness and depth of the data while respecting *AMJ*'s page limits. The data deluge forces qualitative researchers to confront the limitless possibilities and show discipline by discarding irrelevant data.

Gersick (1989), for example, investigated transitions in work groups asked to complete a creative task over an hour. She video-recorded teams' efforts and a wall clock that showed the elapsed time. Her article illustrates the transitions with an asterisk in a figure that showed every team's efforts over the hour. The pattern of asterisks in the diagram vividly illustrates the transitions and pacing that contributed to successful outcomes.

TELL THE STORY

I think all academic writing has to tell a compelling story, and this is doubly true of qualitative research.

Over half of our colleagues used the word "story" in their responses to us and emphatically expressed the belief that a compelling story is critical to good qualitative work (see also Golden-Biddle & Locke, 2006). There is no question that quantitative researchers also try to build stories in their manuscripts, but story is the very essence of qualitative research. Quantitative articles generally follow a well-defined structure: introduction, literature review, hypotheses, methods, results, and discussion. Accounts of the data are spliced between accounts of theory; data and theory appear almost episodic. Qualitative researchers, on the other hand, attempt to create narratives through these accounts. The theory narrative comprises current and emergent theory; the data narrative describes the collection, the analysis (the methods), and the actual data (the results or findings).

Two Narratives Jointly Contributing to an Overarching Story

Whereas quantitative researchers typically look at a handful of "trees" and try to draw the implications for the forest, in qualitative research, we are trying to see the forest through the trees.

Through the two data and theory narratives, qualitative articles tell a compelling story. They create tension through a provocative question, build plot through a data narrative, and provide an interesting and even provocative explanation and conclusion through a theory narrative. Moreover, the data and theory narratives are tightly interwoven—so interwoven that it is sometimes difficult to isolate either narrative (unlike in quantitative works, in which the data and theory are clearly marked). The data are needed to give the theory context, and the theory is needed to give the data meaning. Qualitative articles, thus, use current theory as the backdrop for interpreting the data, the data to provide the context and describe the phenomenon in-depth, and the emergent theory to expose the phenomenon in new light.

For example, Elsbach and Kramer (2003) created their story by asking how experts assessed the creative potential of others. They grounded their theory narrative in social judgment theory, which, they argued, has focused on laypeople, not professionals, and been developed in the lab, not in the field. They wove the data narrative through the theory narrative by providing a rich account of screenwriters pitching ideas to Hollywood studio executives and producers, sprinkling this account with quotes and rich descriptions of incidents. They concluded their theory narrative by showing that assessors judge targets' creative potential not only on the basis of the targets' attributes, but also on the basis of their relationship with the targets. The two narratives interlocked to tell a compelling story. Like a good novel, good qualitative work seduces readers and motivates them to continue reading.

A Unique and Inspiring Story

> When I read qualitative research, I want to be wowed. I want to have the experience of a "shazzam!"—a spark of inspired recognition or deep insight that comes from an author providing me with an idea or a way of seeing that I had not previously entertained.

A good story is engaging and pushes frontiers. Qualitative research does so through both its data and theory narratives. The data narrative situates data in a unique context, narrates skillfully, and reveals something new and powerful about management and organizations. The theory narrative connects to a prior conversation and reveals something new that changes the way in which readers see other phenomena. The theory narrative must offer a significant contribution, involving both revelatory and scientifically useful insight (Corley & Gioia, 2010), but the revelatory dimension is particularly important in qualitative research.

Each of the articles that earned an *AMJ* Best Article Award offers something truly unique. For example, Dutton and Dukerich (1991) offered insights into homelessness in New York—insights drawn from data that revealed the important interaction of image and identity. Greenwood and Suddaby (2006) showed the processes by which institutional entrepreneurs mobilize change in heavily institutionalized environments—insights to theory that were revealed by a deep dive into the evolution of the multidisciplinary practices of the Big Five accounting firms.

EMBRACE THE PROCESS, NOT THE PLAN

> Quantitative research is about careful preparation and faithful execution of the plan laid out in the beginning; qualitative research is about exploring ideas.

The tools, techniques, and processes of qualitative researchers vary considerably, not just at the beginning, but throughout the research endeavor, including the writing process. At the beginning of the process, qualitative researchers often do not know where they will land. Quantitative researchers often follow detailed plans because data collection is so focused on testing a priori theorizing. Qualitative researchers often do not even know the theory they will anchor their insights on prior to collecting the data. Where they land may be very different from where they started. This iterative process poses immense challenges to qualitative researchers.

Concurrent Writing and Research

> I think the main difference is that the ideas and findings get reconceptualized with each writing.

Tight interweaving of the theory and data stories in a work of qualitative research breaks down the boundary between "researching" and "writing," so that the two occur simultaneously. For instance, qualitative researchers find that their data analysis is closely tied to the writing process. Often the emergent theory narrative is revealed when the back end of a paper is written, which forces changes to how theory is narrated at the front end and how data are narrated. Once the data are rewritten, additional theoretical insights may emerge. Theoretical discovery, therefore, often occurs when writing. Such an iterative process defies the detailed planning that is often characteristic of a good quantitative study.

Submission: Just Another Beginning

> Much of the discovery occurs as one writes in that as one writes, one identifies remaining gaps, inconsistencies and questions requiring further exploration. So in that sense . . . writing in qualitative research is a highly iterative process.

As many of our panelists explained, this highly iterative process is often sustained through the review process for a submitted paper. Reviewers often become cocreators (but should not become anonymous coauthors) because the true scope of an inductive study's theoretical implications cannot be fully understood until reviewers have provided feedback on the socially constructed meaning of the data. In this way, qualitative researchers can be thought of as like sculptors: they use an array of tools to work and rework their materials to form their composition. Critics and reviewers expose new ways of seeing the composition, which sometimes forces a significant reworking. As our panel noted, often a qualitative researcher cannot finalize the front end of a paper until the back end has been finalized; both will continue to be revised during the review process all the way until the final draft is accepted.

FINAL THOUGHTS

In reflecting on our journey in preparing for and writing this editorial, we saw as many similarities as we saw differences between good quantitative papers and good qualitative papers at *AMJ*. Writing a strong scholarly article is a challenging yet rewarding undertaking, regardless of the type of data one reports. In that sense, our aim here was not to make qualitative papers seem more difficult to write, or to push quantitative and qualitative research apart. In fact, qualitative manuscripts have benefited from the strong traditions of quantitative research, and they have

much to offer for the composition of quantitative manuscripts.

Our ultimate goal was to help researchers publish their qualitative data in *AMJ* and understand some of the unique attributes of writing qualitative papers that typically are learned from experience. Because the hallmark of qualitative work is its ability to expose theoretical boundaries and push theoretical insights, we all will benefit from better qualitative research gracing the pages of our most-read journals. Hopefully the insights and knowledge provided in this editorial will encourage more scholars to publish strong qualitative research in *AMJ*.

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