

FROM THE EDITORS

Thinking Strategically about Contribution

When I began submitting manuscripts for publication consideration in journals such as the *Academy of Management Journal*, I believed that what mattered most to reviewers and editors was a good, solid study. Although I was submitting theory-testing manuscripts, I paid little attention to the thought that my studies should have implications for theory. I suspected that what really made the difference in determining whether a manuscript would be accepted or rejected was the strength and importance of the study it reported. My thoughts about the theory section were that it provided the basis for the predictions and that it was malleable: it could be molded and shaped to fit the contributions and strengths of a strong research method. Looking back, I guess my mantra was, do good studies and then build the theory from the study's unique features.

This “build a better methodological mousetrap” strategy did not fare well at the journals. At first, the rejection letters surprised me, and I interpreted them as showing that the reviewers and editors were somehow missing the methodological strengths in my studies. For some unknown reason, they didn't realize how strong my research methodologies were. Thinking that the reviewers and editors were simply not “getting it,” I reemphasized the methodological nuances of my studies and submitted my manuscripts elsewhere, thinking that the next set of reviewers would finally grasp and appreciate the methodological foundations of the new results. Well, to put it nicely, the only one that wasn't getting it was I, as these resubmissions usually met with the same fate as their early counterparts.

Over the years, I have learned that theory and contribution do matter, and so does rigorous research. But the reason I am recounting and disclosing my painful history is that many current *AMJ* submissions contain the same mistakes that I used to make. And today's reviewers seem just as adept at locating and pointing out the errors of this “magic in the method” approach. Fortunately, there are some ways to leave this failing strategy behind.

This column offers my ideas about how authors can avoid some of the mistakes that I used to make.

In general terms, I borrow from the strategic management literature to frame how researchers might want to think about the publishing process. We know a lot about strategy and strategic management, and strategic concepts of competition and performance can help us when we try to publish our work. I hope that my thoughts will help authors improve the publication prospects of their submissions.

In my case, my submission outcomes improved when I changed the way I thought about the publishing environment. I had been working under the assumption that the journals have unlimited space for worthy articles and that all articles that met the most rigorous methodological standards would be published. Unfortunately, this was and is not true. Journal space is scarce, and many manuscripts compete for it. It was when I realized this and changed to a strategic approach that my fortunes changed.

The insight that the publication process as a strategic one led me to begin thinking about manuscripts as “firms” competing for limited resources (journal pages). Like a firm's performance, a manuscript's performance can be influenced by its degree of fit with its environment—in this case, a journal. Different journals publish different types of contributions.

This reasoning rests on two critical assumptions: (1) the firm must match its environment (the manuscript must match its journal) and (2) the closer the link, the higher the performance (the higher the likelihood of publication).

From my experience, manuscripts that focus on methodological features without an attendant theoretical contribution do not fare well at our journal (they are not aligned with our environment). Such approaches can take many different forms. For example, authors may argue that the sample of their study is better, or the measures are stronger, or perhaps that their design adds a new variable to an existing equation. But usually there is no or little explanation as to why the methodological advance is important. The question that arises, of course, is why should the stakeholders of *AMJ* care about the new methodological feature? What is its value-added? For a journal such as *AMJ*, the feature must represent some valuable contribution to theory development.

This logic, then, supports my first piece of ad-

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vice: Authors, if you want your manuscripts to appear in *AMJ* or a similar journal, then you need to structure your manuscripts to highlight and explain their empirical and theoretical contributions. Or, more generally, you need to match your manuscripts with their most pertinent environment. I recommend that authors craft their manuscripts to align with their targeted journal's specific needs. In the case of *AMJ*, this means offering empirical and theoretical contributions. Both of these contributions are necessary for publication in *AMJ*.

This reasoning seems straightforward. But what is not at all easy is trying to decide whether the contributions, once identified and explained, are actually interesting or important enough to warrant publication. It may be easy to articulate a contribution, but how does one judge how compelling it is? In other words, how does one judge how well a manuscript matches its environment?

At first, this question seems highly subjective. However, when one thinks strategically about it, contribution may not be that tacit after all. For instance, there are frameworks that remove some of the guesswork in considering contribution (see Sara Rynes's discussion of contribution in the April 2002 issue of *AMJ*). But there is still more that needs to be understood about how the content of a contribution influences its performance in the publishing environment.

Borrowing from a popular perspective in strategy, I would like to suggest that authors evaluate the contributions of their manuscripts relative to three tests. Applying these tests can help authors (and reviewers) determine whether the type of contribution a particular journal tends to publish is present and if the contribution will meet readers' expectations. If an author believes that her or his manuscript's contribution passes all of these tests, then I further recommend that the contribution be presented and defended in such a way that the tests are met.

The first test is whether the contribution is valuable. By valuable, I mean adding an insight that is important and relevant for other researchers and/or practitioners. For instance, does the contribution revise or extend theory development? Do managers learn something new from it? Is the contribution going to be of any use? Will it change the development of explanations or how researchers think about a subject? Why? This test is best applied in practice, though often it is more implicit than explicit. Nonetheless, I want to encourage authors to argue how the contributions of their manuscripts are valuable. Adding a variable to an already-exam-

ined subject is not of obvious value, but explaining why that addition revises or extends knowledge may be. Going the extra step to explain and justify the value-added of a contribution is necessary to remove doubt.

The second test is whether the content of the contribution is imitable. The concern is whether the contribution is specific to the theory that it is trying to extend or revise. Are all reasonable competing explanations of the proposed advance controlled for, either through the logical consistency of the theoretical model or through controlling for rival theories in the research method? The basic idea is, Is the contribution different from what can be explained using another theory? I encourage authors to identify how their contributions are specific to a particular perspective and to articulate how those differences are meaningful (the "valuable test").

The final test is rareness. This screen concerns whether a contribution is surprising and unexpected. Is the contribution more of a common sense derivation, or does it represent a novel and unique insight? Originality is a critical concern.

Putting these tests together produces my second recommendation: I suggest that authors identify, explain, and argue for how their manuscripts' contributions are valuable, specific to the theory they advance, and original. Do more than just *mention* the contribution—articulate and defend it.

My experiences as an author and, now, as an editor suggest that authors can and should take a strategic approach to their submissions. There is merit to viewing the review process as a competitive exercise. I believe that authors will benefit from thinking strategically about how to position their manuscripts and will not benefit from taking a product-oriented approach—like thinking a good methodology will win the day. To borrow an old coaching cliché, failing to prepare is preparing to fail. Authors, prepare your manuscripts with respect to the dynamics of the competition. Keep two strategic points in mind when crafting a manuscript: (1) ensure that its contribution is aligned with a receptive journal and (2) identify and evaluate the content of the contribution so that you can present and argue for it most effectively. Building a better methodological mousetrap may be part of making an *AMJ* contribution, but your theoretical contribution is ultimately the deciding factor.

Donald D. Bergh
University Park, Pennsylvania