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FROM THE EDITORS

SOME REFLECTIONS ON CONTRIBUTION

Shortly after Tom Lee assembled his team of associate editors, he asked each of us to write on a topic of our own choosing for this column. I constructed a first draft of this commentary exactly six months from the day I received my first manuscript as associate editor. It was also New Year's Day, 2002—a good day for reflection, particularly after the events of September 11.

Since September 11, many of my academic friends have spent more than the usual amount of time wondering whether what they are doing with their lives really has much meaning in the grand scheme of things. Like people in many other walks of life, academics wonder, in light of recent events, how they can increase the contribution they are making to others in their roles as teachers, parents, spouses or partners, and members of the broader community.

Although the role of associate editor has not given me any particular expertise in speaking about these broader types of contributions, it has given me reason to think a great deal more about our contributions as researchers. I believe that all of us who pursue research are hoping that some day, in some way, our work will "make a difference" to someone—either to other researchers or to those our research is ultimately intended to help. Yet it is sometimes easy to lose sight of that aspiration in the day-to-day business of generating research ideas, painstakingly gathering and analyzing the data, writing up results and conclusions, having our efforts critiqued, and then, more often than not, rejected in the review process.

Much as most of us want to make true contributions with our research, my general impression after six months as associate editor has been that most manuscripts are rejected precisely because they are not perceived to make enough of a contribution. To test this impression, I decided to reexamine the reviews and decision letters associated with all the manuscripts that I have rejected as this commentary begins to go to press (in mid-February). This closer look at the data confirmed my initial impression. In 25 of the first 32 rejections, limited contribution was either the first- or second-mentioned factor in the decision letter. In other words, most manuscripts are simply not perceived to be contributing very much to the stock of current knowledge and practice.

Although it is true that most rejected manuscripts also have problems in the areas of theoretical development, measurement, or analysis, in many cases the reviewers believed that those problems might be remedied by revision. However, when the judgment was that the "raw materials" of the manuscript could not make a big enough contribution no matter how carefully analyzed or cogently argued, the recommendation was usually to reject.

The notion of contribution—like many other abstract concepts, such as quality or truth—is somewhat subjective and can only be assessed in the context of each unique manuscript. But it isn't very helpful to say that contribution is subjective and contextual, and it certainly doesn't provide much help to researchers trying to improve their future level of contribution. Therefore, I looked more closely at how reviewers evaluate contribution. This examination suggested that assessments of low contribution fall into several general types.

Assessments of Low Contribution

Low incremental contribution. One of the most common observations was that the results had been shown elsewhere before, thus raising the questions "So what?" and "What's new?" A twist on this theme was that reviewers doubted whether a newly proposed construct or theoretical frame was really new. In these cases, reviewers often felt that the author had ignored entire literatures that addressed the same problem using different constructs, terminologies, or frameworks.

Overly narrow contribution. This category differs from the first in that reviewers seemed to feel there was *some* incremental contribution, but not a large enough one. This was a relatively common reaction to work in which one or two mediators, moderators, or interactions were added to previously examined topics. Common phrases categorizing this type of rejection included, "The scope of the contribution is very limited" and "This is a very narrow slice of the problem." Often, the reviewers' comments were made in the context of the very high standards that they set for *AMJ* (noting, for

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example, "The contribution is small for a journal as prestigious as *AMJ*"). In these cases, reviewers sometimes believed that the article might make an acceptable contribution if sent to a journal with a more specialized focus or readership.

Not very surprising. This category differs from the preceding category in that here, reviewers are judging the results not against prior literature, but rather against common sense or the likely reactions of the "person in the street." Illustrative phrases include "This seems almost tautological" or "Is this really surprising?"

Unclear importance. Here, reviewers were unable to grasp the point of the studies. These manuscripts tended to be ones that described *what* was done, but not *why* it was interesting or important. When reviewers recommended rejection for this reason, it was usually not simply because the author had failed to make the case for importance. Rather, the (often unstated) assumption was that the reviewer didn't really think the research *was* important, no matter how it might be reframed. In contrast, when reviewers thought the research was in fact important but the case was not well made, they generally recommended revision and resubmission. In addition, they often gave concrete suggestions about how the authors might better position the manuscript.

In addition to examining cases in which rejection was at least in part due to contribution deficits, I also examined the rejections in which no mention was made of contribution. This small sample (n = 7) appeared to fall into two categories. In the first, manuscripts were perceived to be so fatally flawed in their execution that raising the issue of contribution seemed beside the point. The second set of papers was quite different. These were uniformly agreed to address potentially important issues, but the data sets were deemed insufficient to answer the proposed problems or questions (for instance, a mismatch between theory and data was seen, or authors promised more substance than the data could support).

Enhancing the Potential for Contribution

The major conclusion I draw from these assessments is that the potential for contribution is generally set at the very earliest stages of a research project. By the time the data are collected, it is usually too late to think about how to make a substantially larger contribution.

Of course, this is not an original observation. Many others have written on this point. Perhaps many of you have a favorite article or book that you refer to as you think about designing a new study with contribution in mind. I also have a few such favorites, two of which I will pass along to you. I discovered (in one case, rediscovered) these sources after two particularly discouraging bouts with the review process. Although I was still disappointed about the rejections, reviewing these sources helped me to realize that I could decrease the chances of future rejections by applying greater discipline to my thinking at the earliest stages of the research process.

The first of these sources is chapter 4 ("Antecedents of Significant and Not-So-Significant Organizational Research") from Campbell, Daft, and Hulin's 1982 book, *What to Study: Generating and Developing Research Questions.* In this chapter, the authors report the results of asking 29 prominent researchers to think about the conditions that led to the development of their most significant, and their least significant, research projects. Campbell and colleagues' main goal in doing this was "to develop criteria for predicting significant research *in advance*—in other words, what should an investigator look for when choosing a research project in order to enhance the probability that it will make a significant contribution to knowledge?" Although I urge you to read their findings for yourself, here are six factors they found to be associated with respondents' most significant research projects:

Activity. "Frequent interactions, being in the right place at the right time, chance, and contact with management and colleagues are related to the beginning of good research ideas."

Convergence. "There is a sense that several activities or interests converge at the same time (e.g., an idea and a method). Convergence seems related to the notion of activity because it is through activity and exposure that the investigator is able to be at the convergence point of several events."

Intuition. "The importance of the research and the interest in it seem to be guided by intuition and feeling rather than by logical analysis. Investigators often expressed a feeling of excitement or commitment, a perceived certainty, as if they 'knew' at a deeper level they were doing the right thing. A great deal of intrinsic interest is also present."

Theory. "A concern with theory also seems to be important. A primary goal often was to understand or explain something about organizational behavior. The investigator was curious, was concerned with a puzzlement, or wanted to clarify something that was poorly understood."

Real world. "Often the research problem has an applied, real-world flavor to it. The ideas often were tangible, useful, and pertained to ongoing organizational activities. Often the idea arose from contact with laymen in organizations."

Personal commitment and motivation. "Significant research is more likely to be undertaken because of personal interests of the investigator rather than because the research is acceptable to the discipline (and publishable). The investigator is likely to have strong beliefs about the expected outcome" (which often conflict with conclusions from existing research).

Joining the Conversation

A second favorite source of mine is Anne Huff's 1999 book, *Writing for Scholarly Publication.* One of my favorite take-aways from this book is Huff's concept of research and writing as conversation: "Scholarly work is rooted in the lively exchange of ideas—conversation at its best." In order to maximize the value of this conversation, she recommends working through four crucial questions at the very beginning of each research venture:

- Which conversations should I participate in?
- Who are the important "conversants"?
- What are these scholars talking about now?
- What are the most interesting things I can add to the conversation?

She goes on to say: "Let me be very clear about the reason I am urging you to do all this work: You should anticipate making an impact on the scholarly conversation of your field, from the very beginning of your career. . . . Sometimes authors are too hesitant. They think they will have to wait until they have more experience before joining the conversations that interest them most. Don't be that tentative. Begin by thinking in terms of the people and the ideas that interest you most. Assertive, well-grounded responses to those who have captured your interest are rewarding and fun to make . . . They are also the most likely to be published and cited by others." Somehow, the imaginary exercise of sitting in a room with the people whose research I most admire, trying to explain what my new research might contribute, never fails to illuminate the weakest aspects of my thinking.

In closing, I'd like to quote from Campbell et al.'s respondents about what the process of working toward a significant contribution "feels like":

> I threw out an idea in a doctoral seminar to which a student responded. [There was a] sense of great excitement, engagement in task, reading, thinking, interacting. Continuous interaction to test ideas against one another—couldn't let go.
>
> Idea originated in a seminar where diverse backgrounds led to stimulating clashes. Connections plus enthusiasm.
>
> The idea occurred as a result of studying literature relevant to this problem. Also playful, exploratory intellectual climate—lots of "what if?" conversation.
>
> A colleague walked in one day and tried to explain a new concept from operations research. Suddenly I realized significance for organizations. Multiple implications blossomed right in front of us. We talked for a year, were very excited, and finally wrote everything down.
>
> It was a real world problem that could have policy implications. Also involved my personal values—concern over the Viet Nam war.
>
> Worked (outside academics) and my personal experience contrasted with academic theory. Findings were politically relevant for understanding motivations of poor people."
>
> It was a plight—I didn't believe in X and wanted to show it.

Excitement, engagement, interaction, enthusiasm, playfulness, exploration, newness, intellectualism, commitment, consistency with personal values, significance, and implications—these are the signals that a significant contribution is in the making. I wish you the best of luck in creating such moments, and I encourage you to submit them to *AMJ* when you do!

Sara Rynes
Iowa City