The Importance and Challenges of Being Interesting

Daniel C. Smith
Indiana University

What is the most frequent reason why manuscripts get rejected? Inappropriate samples? Faulty measures? Weak results? In reflecting on my experiences as a reviewer, the most common reason I see for manuscripts not being accepted is the size of the contribution. Every manuscript has its methods-related limitations, and certainly some of these can render even the most provocative idea unpublishable. At the end of the day, however, the publishability of a manuscript often comes down to weighing the inevitable methods-related flaws against its contribution.

For the purposes of this discussion, by contribution I am referring to how interesting and provocative the central points of a manuscript happen to be. How inherently interesting an idea is has long been recognized as being important in advancing theory in a given field. Social scientist and philosopher Murray Davis (1971) observed that "a theorist is great not because his theories are true, but because they are interesting. In fact, the truth of a theory has very little to do with its impact" (p. 309). But unfortunately, developing interesting ideas and crafting interesting manuscripts is no simple feat. Here, I offer a few humble suggestions to authors, reviewers, and even editors, regarding their roles in elevating the "interesting quotient" of literature in our field.

THE DOMAIN OF THE INTERESTING

To be interesting, an idea needs to challenge the taken-for-granted assumptions in a particular area. Less interesting ideas (or hypotheses) are those that tend to be consistent with what readers already take to be true. They are well within the grasp of intuition and because they reaffirm our beliefs, often elicit a "So what, I already suspected that" reaction on the part of reviewers and readers. In contrast, interesting ideas often elicit a strong visceral emotional reaction in readers. Perhaps one of the best descriptions of an interesting idea is that offered by one of my mentors, Gerald Zaltman, who suggested that an interesting idea is one that, if it were true, would require a large number of people to undertake a substantial change in their beliefs or behaviors (see Zaltman, LeMaster, and Heffring, 1982).

Evaluating how interesting a given idea is carries a degree of subjectivity. However, on the basis of an informal review of the high impact of articles, I do believe it is possible to develop a general categorization of ideas that have the potential of scoring high on the interesting "Richter scale."

1. *Testing the assumptions on which a significant stream of research relies.* In many cases, manuscripts are positioned around the next logical study in a vast stream of work. Instead, consider stepping back and identifying core assumptions on which all or many prior studies rely. This is admittedly not easy because assumptions, by their very nature, are aspects of research that we simply take for granted and are often not even aware of. In this regard, one of the more common set of assumptions associated with streams of work are the research methods that, over time, become an accepted part of convention in a given domain. For example, our initial knowledge base on first-mover advantages was based on a series of studies that examined only surviving firms. When you alter this method convention and include nonsurviving firms, you get a very different set of conclusions (e.g., Golder and Tellis 1993).

2. *Probing the external validity of what we take to be true.* While the internal validity of a given study is certainly critical, in the context of examining a stream of work over time, the price of ignoring external validity is great. As Lynch (1982) so bluntly argued, "If findings 'supporting' one's theory lack external validity, *the theory lacks construct validity*. The theory is at a minimum incomplete, and it is quite possibly just plain wrong" (p. 234). Probing the external validity of a stream of research involves identifying common background factors that might interact with the primary variables routinely examined. For example, in the area of brand extension research, with rare exceptions, our knowledge is grounded on studies of

Journal of the Academy of Marketing Science.
Volume 31, No. 3, pages 319-322.
DOI: 10.1177/0092070303252582

consumer brands. What if most of what we know does not apply to business-to-business contexts? At a more general level, opportunities for testing external validity are readily apparent when we witness customer behavior or successful managerial practice that is counter to the conclusions of academic research.

3. *The next new thing*. Interesting ideas can also come in the form of opening new domains of inquiry. How do you know if a new domain is worthwhile? New domains introduce fundamentally new central variables to our research environment. Akin to examining the assumptions underlying a stream of work, new domains often emerge based on questioning assumptions related to our field as a whole. Bagozzi's (1975) seminal article that questioned the implicit "commercial transaction only" boundaries of marketing activities is a classic example of questioning bedrock assumptions of what constitutes the domain of marketing. More recently, the growing body of work on market orientation is a good example of a stream of work that emerged based on questioning one of our most basic premises, that is, that being market oriented is inherently a good practice.

New domain ideas can also emerge from observed behaviors that we simply have not examined but can have *significant implications* if they were better understood. A good example is the stream of work initiated by Rohit Deshpande and Gerald Zaltman (see, e.g., Deshpande and Zaltman 1982, 1984) on the use and misuse of marketing research information by managers. These initial studies emerged from a widely observed contradiction between textbook principles and common practice. Presumably, marketing research should improve the quality of decisions. Yet, while managers would invest considerable sums in market research, they frequently would not use the resulting information. Understanding this phenomenon had the potential to fundamentally improve the quality of managerial decision making.

4. *Work backward in the causal chain*. Over time, nomological networks of variables and propositions develop around a given research issue. Important ideas can emerge from identifying the most central variables in a network and treating them as new dependent variables. The result is that you will be examining a variable that, if understood in greater depth, has significant implications for a vast web of related relationships. For example, dozens of studies have shown that a major determinant of the success of new products is the degree to which they are unique in a way that is meaningful to customers. This suggests a substantial contribution may reside in understanding factors that affect a firm's ability to develop meaningfully unique new products.

5. *Intervene in an accepted causal chain*. As noted above, in any nomological network, certain relationships are central and become taken for granted. But what if they are not what they appear to be? What if there are other constructs that intervene and, if controlled for, would result in the disappearance of a well-established relationship? For example, in the area of advertising, Murry, Lastovicka, and Singh (1992) found that the previously held direct effect of television program liking on consumers' attitude toward a brand is mediated by their attitude toward the ad. That is, program liking affected consumer attitudes toward the ad but had no direct effect on attitude toward the brand after controlling for attitude toward the ad.

6. *Challenging conventional managerial practices or beliefs*. In the area of marketing management/strategy, the practices and, perhaps more critically, the beliefs of managers are often excellent sources of interesting ideas. Referring again to the study by Murry et al. (1992), noted above, many managers believe that it is not a good idea to advertise on television programs that elicit intense feelings of sorrow or fear. Drawing on this belief as a source of motivation, the authors demonstrate that it is not program content per se that affects how consumers processed ad information but rather the degree to which they enjoyed the program. The moral of the story? If viewed through your "research idea lens," popular press books touting the latest marketing/management fad, teaching in executive programs, and periodic consulting engagements can be rich sources of provocative ideas.

7. *Resolve inconsistent findings*. In many streams of work, inconsistencies in findings will emerge where some studies find support for a particular relationship and other studies do not. Such results suggest a contingency model in which the focal question should shift from "Does variable X affect variable Y?" to "When does variable X affect variable Y?" A good example of this shift can be found in recent studies on the effects of market orientation on firm profitability (e.g., Matsuno and Mentzer 2000).

This list of opportunities for interesting ideas is certainly not exhaustive. However, it is important to note one approach that is intentionally absent yet often plays a subtle cornerstone role in how we teach doctoral students to develop research ideas, that is, come up with the next extension in a well-worn stream of research. In contrast, the common thread that passes through all of the above noted approaches to identifying research ideas is that authors need to step back and look at a stream of work in a *holistic* fashion, focusing on subtleties that may have not so subtle implications.

SUGGESTIONS FOR AUTHORS

Having a sense of potential types of ideas offering a high likelihood of significant contribution may be helpful, but ultimately, authors need to position their work to clearly highlight its importance. How interesting an idea is often has as much (or more) to do with how it is presented as it does with the inherent importance of the idea itself. While there is no hard-and-fast formula, it is worth reflecting on how authors can elevate the interest level of two of the more common motivations for research that come across my desk.

1. *Positioning "gap-filling" work.* In motivating a manuscript, it is not uncommon to find that authors open with a review of relevant literature, point to a gap, and say something like "To date, the study of Topic X is noticeably absent. Therefore, we are studying X." The mere presence of a "gap" in the literature does not constitute the basis for a contribution. Indeed, there may be a very good reason why researchers have not examined Topic X—it is simply not important or interesting. Instead, take the reader through a story of prior work and *explicitly* note the *implications* of the gap you are proposing to address. Ideally, the implications of addressing this gap should be related to both theory development and practice. Regarding both of these areas of contribution, try to *explicitly* articulate how current thinking and behavior would change as a result of your work.
2. *Positioning boundary condition work.* Examining the boundary conditions of prior work is another common motivation for research. Here, authors are proposing to examine a set of relationships that are well established in Context X but have not been tested in Context Y. As noted above, examining boundary conditions and the generalizability of well-established findings is potentially interesting. But authors must clearly articulate *why* they believe the results of prior work will not generalize to the proposed context. To do this, one must focus on the underlying theory that was used to support the predictions in prior studies and suggest why that theory would give *different predictions* in the proposed context. For example, let us assume that one basis for predicting that brand extensions are more effective than new brands is that the former reduce perceived risk. This finding has been demonstrated in consumer product markets. If you want to argue that it is worthwhile examining brand extensions in business-to-business markets, you would need to argue that perceived risk is lower and hence, the findings may not generalize. You need to provide readers with a credible argument that makes them feel that there is a reasonable chance that what they take to be true may not hold under certain *frequently encountered* conditions.

At a more general level, remember, as Peter and Olson (1983) observed 20 years ago, science is marketing. Be a passionate advocate for the merits of your idea. Your goal is to do your best to elicit excitement in the reader. While it is common to fill a manuscript with qualified language, at least in positioning your idea in the Introduction section and when you revisit it in the Discussion section, write with passion and commitment. You should be in a "sell mode." Note the quote by John Lynch (1982) cited earlier; he is not the least bit tentative in his stance on the risks of ignoring external validity. The entire article from which this quote was taken is written with similar conviction and is particularly notable because it triggered a debate that spanned multiple issues of the *Journal of Consumer Research*.

SUGGESTIONS FOR REVIEWERS: ROLL UP YOUR SLEEVES AND HELP OUT

If authors do their part and bring interesting and thought-provoking ideas to the table, reviewers need to do their part and read with a developmental eye and an open mind. Taking a developmental approach places the reviewer in a mind-set of raising concerns followed by *concrete* suggestions for improvement. It also requires a significant commitment of time and mental dedication. You may need to rework an author's conceptual model, provide better logic for hypotheses, write step-by-step suggestions on how to position an idea, lay out alternative data coding schemes, and the like. However, if reviewers sincerely follow this model, they often find that concerns that initially seemed fatal often have possible "fixes" that will keep the manuscript in the running. Moreover, if you are dedicated with this approach and cannot find solutions to serious concerns, when you reject a manuscript, you can sleep well knowing that you acted in the author's best interest throughout the process.

A developmental mind-set is particularly important when reviewing highly interesting and provocative manuscripts. Why? As noted, provocative work often calls into question methods and assumptions of well-established knowledge. Given the way reviewers are selected, as experts in a given domain, your work may be among the sacrificial lambs on the block. A developmental approach is no less rigorous than a "read-to-reject" approach. But a developmental approach does help keep a reviewer's ego out of the picture. I have seen multiple cases where initially provocative manuscripts were sterilized by the review process if they made it through at all. A developmental mind-set not only helps to ensure that you give such manuscripts a fair and impartial reading but also allows authors to write with a compelling voice that has the potential to stir the emotions of readers.

THE IMPORTANCE OF EDITORS

Just as provocative manuscripts require reviewers to approach them with open minds, they also often require editors to play the role of shepherds. As Grover and Srinivasan (1992) indicated in their reflections on the review process associated with their O'Dell award-winning article, one reviewer was pathologically negative and never signed off on the article. This is not an uncommon occurrence. It is difficult to get three academics to agree on where to have lunch let alone on the merits of a manuscript. Without the intervention of the editor, this article may have never seen the light of day.

I also recall a more extreme case when I was a doctoral student at the University of Pittsburgh. C. W. Park, Debbie MacInnis, and Bernard Jaworski (1986) submitted an article to the *Journal of Marketing* that called into question a number of commonly held beliefs about product evolution. The reviewers were merciless. Shelby Hunt, the editor at the time, found a kernel of an interesting idea and encouraged the authors to substantially rework the article. The outcome was an article titled "Strategic Brand Concept Management" that not only received the Alpha Kappa Psi Award in 1986 but is widely acknowledged as the piece of work that launched the whole area of brand extension research.

As these examples illustrate, interesting ideas often depend on editors who are active participants in the review process, editors who work with authors *and* reviewers toward a common aim—to craft the most thought-provoking manuscripts possible. Much of our progress as a discipline can be traced to the tireless effort and dedication of conscientious journal editors. To them I dedicate this essay.

REFERENCES

Bagozzi, Richard P. 1975. "Marketing as Exchange." *Journal of Marketing* 39 (October): 32-39.

Davis, Murray S. 1971. "That's Interesting: Towards a Phenomenology of Sociology and Sociology of Phenomenology." *Philosophy of the Social Sciences* 1 (December): 309-344.

Deshpande, Rohit and Gerald Zaltman. 1982. "Factors Affecting the Use of Market Research Information: A Path Analysis." *Journal of Marketing Research* 19 (February): 14-28.

——— and ———. 1984. "A Comparison of Factors Affecting Researcher and Manager Perceptions of Market Research Use." *Journal of Marketing Research* 21 (February): 32-39.

Golder, Peter N. and Gerard J. Tellis. 1993. "Pioneer Advantage: Marketing Logic or Marketing Legend?" *Journal of Marketing Research* 30 (May): 158-171.

Grover, Rajiv and V. Srinivasan. 1992. "Reflections on 'A Simultaneous Approach to Marketing Structuring." *Journal of Marketing Research* 24 (November): 474-477.

Lynch, John R. 1982. "On the External Validity of Experiments in Consumer Research." *Journal of Consumer Research* 9 (December): 225-239.

Matsuno, Ken and John T. Mentzer. 2000. "The Effects of Strategy Type on the Market Orientation-Performance Relationship." *Journal of Marketing* 64 (November): 1-16.

Murry, John P., Jr., John L. Lastovicka, and Surendra N. Singh. 1992. "Feeling and Liking Responses to Television Programs: An Examination of Two Explanations for Media Context Effects." *Journal of Consumer Research* 18 (November): 441-451.

Park, C. Whan, Deborah J. MacInnis, and Bernard Jaworski. 1986. "Strategic Brand Concept Management." *Journal of Marketing* 50 (November): 135-146.

Peter, J. Paul and Jerry C. Olson. 1983. "Is Science Marketing?" *Journal of Marketing* 47 (November): 111-126.

Zaltman, Gerald, Karen LeMaster, and Michael Heffring. 1982. *Theory Construction in Marketing: Some Thoughts on Thinking*. New York: John Wiley.