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# Last Reflections of the Editor

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As I write this editorial, it was just about 47 months ago that I took over as editor-in-chief of *Marketing Science*. Over that period of time, I handled 585 manuscripts in one stage or another of the review process. (I still have responsibility for approximately 12 papers, plus another 20, which are associated with the special issue on Managerial Decision Making.) During the 38 months that I was the editor, the journal received 410 new submissions, including those for the special issue. To date, I accepted 59 of these papers, while the average turnaround for a paper was just over 75 days.

What did I learn from this experience, besides the fact that carrying home manuscripts every night can lead to a bad back? First of all, I have a much deeper appreciation for the complexity of the review process. One would like to think that the process is always reliable and valid, as well as constructive and speedy. As an editor, I felt I had only marginal control over the first two attributes, and then mainly by carefully selecting the review team. Where I felt I had the most impact was maintaining a "speedy" review process. In hindsight, almost all long review processes were directly linked to one slow evaluator. What I should have done to get around this problem was to process a paper within some cutoff date with whatever information was available at that time. For one reason or another, I had difficulty doing this and instead hoped (often to no avail) that the tardy individual would eventually respond and I (and the authors) would gain valuable new information.

I am less sanguine about influencing the reliability and validity of the reviews. It is probably true that we publish very little that is truly wrong and/or not interesting to a significant segment of our readers. Thus, the review process is reasonably good at screening out papers with technical problems and marginal contribution. That we catch most mistakes is not all that surprising when you realize that reviewers have been "trained" to look for errors and that only one of the (4) evaluators used for each paper has to catch a "fatal"

flaw for the paper to be rejected. What is of more concern to me is the other type of error, i.e., reviewers suggesting rejection for a paper that actually should be published (albeit after some revisions).

## The Problem

This raises a series of questions. What is the correct definition of a paper that should be published? Why are so many papers screened out by reviewers and what, if anything, can an editor do to rectify the situation?

Defining the characteristics of a paper that should be published is very difficult unless one uses abstract terms such as "relevant" or "makes a significant new contribution". Still, we all have seen papers that have changed the way we look at a field. Interestingly, there is strong empirical evidence that many of these papers did not have an easy time in the review process. For example, Akerof's paper "A Market for 'Lemons'" (1970) was initially rejected by three leading economics journals. Bob Lucas's 1972 paper introducing rational expectations concepts into macroeconomics and the 1973 paper by Fischer Black and Myron Scholes introducing their option pricing formula are other examples of papers with huge impacts that were originally rejected (Gans and Sheppard 1994). For more examples of classic papers that were rejected on first submission, see Campanario (1995). One might explain this by noting that these papers represent new ideas and challenge existing paradigms. They also often require the evaluator to think about the problem in a new way. Finally, because they are breaking new ground, there may initially be some "errors" in the author's logic, or presentation. Each of these characteristics can lead reviewers to suggest rejection.

If "classic" papers have a tough time in the review process, what about the solid paper that deserves to be published but may lack the "home-run" quality mentioned above? More generally, how reliable are review-