Reviewing Less—Progressing More

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Editorial: Presumably, academic journals exist and publish articles to disseminate new ideas. Somehow that simple goal has been lost. Today, articles appear in print only after a referee is convinced that all other alternative explanations for its results have been ruled out. In reality, no article can exclude every possible alternative, so this is basically an exercise in futility. The criterion for publication should be that once an article crosses some threshold it is good enough to publish. The problem seemingly lies in our inability to say "good enough." But this is a problem we can fix. (*JEL* G)

Foreword

Because this is an editorial, I want to warn the reader not to expect either the prose or evidence typically found in a research article. Everything in this article is my opinion. I am not going to "prove" or offer direct evidence for every claim that I make. This is an editorial, not a dissertation piece on the editorial process. With these ground rules in mind, I look forward to debating the issues raised here with my professional colleagues at various conferences.

Where We Are

Try the following experiment. Take any article accepted for publication at any journal. Now submit it to another journal. What are the odds it will be accepted as is? Zero. There is even a pretty good chance it will be rejected. Our profession seemingly believes that its published articles are in fact not good enough to publish! Perhaps it is because each new reviewer can offer suggestions that will improve the article and, having been asked for his or her advice, feels compelled to give it. Even if true, that just returns us to the initial problem—no published article is good enough to publish. After all, every article can be improved upon. Perfection cannot be the standard. Nor is it necessary. Post publication, influential articles are thoroughly vetted by the profession in a way that no amount of reviewing can hope to duplicate. The

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criterion for publication should be that once an article crosses some threshold, it is good enough to publish. At that point everyone involved should be willing to do so without further revision. The problem seemingly lies in our inability to say an article is "good enough." But this is a problem we can fix.

Round and Round We Go!

I have now spent thirteen years of my life helping edit academic finance journals: seven at the *Journal of Financial Markets*, followed by six at the *Review of Financial Studies*. I have also been on the other side of the process as an author trying to get his work published. My overall experience has left me wondering what most referees and editors think peer review can accomplish. Perhaps it can guarantee that every result a journal publishes is correct? Clearly some people think so, or at least act as though they do. We have all seen referee reports that demand significant and incredibly time-consuming changes and additions. Presumably these changes will ensure the validity of the article's conclusions. Of course, after making all of the mandated changes there remains the distinct possibility that the referee will come up with additional demands. After that? Who knows? Further time-consuming rounds are not out of the question.

If editors and referees are seeking to guarantee the validity of published results, they are wasting everybody's time. Demands by editors and referees for pages and pages of additional tests rarely expose a fundamental flaw. However, even if they did, think of the incentives to cover it up! Surely even the least creative author can devise a workaround that satisfies the referee without revealing whatever issue may have arisen. Neither journals nor referees have the resources to check an author's code, data filters, or even honesty. In practice, reviewers and editors generally accept whatever numbers are presented at face value. Authors may not even be acting duplicitously. While searching across specifications, they may honestly believe those producing contrary results are flawed in some way, while the confirmatory and thus final specification is not.

Cite ME!

Technology has opened up vast new areas of research. Unfortunately, it has also opened up narrow ways of tabulating the quality of an academic's work: all hail the rise of the easily calculated citation count!

Prior to the rise of electronic databases, an academic's citation count typically went untabulated. It was certainly unavailable to the profession as a whole. That has changed. Nowadays an academic's professional standing and number of citations are closely linked. Consciously or not, referees react to this career pressure by favoring articles that mention their work and rejecting those that do not. Authors are all too aware of this dynamic, and it leaves them

Table 1
The trend in citations per article over time

Year	Journal	Number of Articles Cited	
		mean	Median
1980	*JF*	15.695	13
1980	*RFS*	–	–
1990	*JF*	23.804	23
1990	*RFS*	26.148	25
2000	*JF*	39.91	33
2000	*RFS*	33.639	30
2010	*JF*	48.072	46
2010	*RFS*	47.27	44

Key: *JF = Journal of Finance, RFS = Review of Financial Studies.*

with but one obvious solution: cite every possible referee's body of work. Over time, the result has been bibliographical bloat.[1]

At the *Journal of Finance (JF),* bibliographies now cite three times as many articles as they did in 1980 (Table 1). Things are no better at the *Review of Financial Studies (RFS),* where bibliographies have doubled in length since 1990. (Exactly how many articles are relevant to any single paper?) Whether this trend has improved our understanding of financial markets is questionable. But one thing is certain; it has helped ramp up author citation counts as well as increase the impact factors of the profession's journals. Unfortunately, this statistical increase benefits both of these groups, which only incentivizes them to push for ever-longer reference lists. So, absent some dramatic changes in the profession, I suspect this means that we have yet to see the end of the ever-expanding bibliography.

Referees not only want authors to cite their work, but they want the text to laud it as well. Whatever the motivation—and we can all easily think of several—referees cannot directly demand that others affirm their publication record. So, instead they tell authors to do a better job of "placing the article in the literature." Authors have responded accordingly. Nowadays, papers routinely try to review every article by every potential referee, no matter how tangentially related to the subject at hand. Unfortunately for the reader, a standard introduction is not long enough to really accomplish this, so many articles now have two introductions. The first is still called the introduction; the second, the "literature review" section. In the end, it can easily mean wading through thirteen or more pages of introductory material before arriving at the article's actual results. Whatever else these long preambles may do, they certainly distract from whatever point the author is actually trying to convey.

Introductions used to be quite short.

[1] Since this is an *RFS* editorial, data were, of course, pulled for the *RFS*. To obtain data going back to 1980, it was necessary to add the *JF* because the *RFS* only began publication in 1988. For the most part, the trends for both journals from 1988 to 2010 are similar.

Table 2
The trend in the length of introductions over time

Year	Journal	Length of Introduction (words)	
		mean	median
1980	*JF*	481	408
1980	*RFS*	–	–
1990	*JF*	697	627
1990	*RFS*	944	846
2000	*JF*	1,242	1,187
2000	*RFS*	1,486	1,411
2010	*JF*	1,673	1,665
2010	*RFS*	1,823	1,572

Key: *JF* = *Journal of Finance*, *RFS* = *Review of Financial Studies*.

Since 1980, introductions at the *JF* have increased more than fourfold (Table 2). The *RFS* has not been around as long, but it shows a similar trend, going from an average of 846 words in 1990 to 1,572 in 2010. These figures actually understate the amount of growth over time because they exclude the now ubiquitous "literature review" section. Including this section would no doubt substantially increase the total word count for the year 2010 since literature reviews are often as long as the introduction itself.

What exactly has changed since 1980 that necessitates vastly longer introductions and bibliographies? Have articles become better as a result? Short, succinct introductions lay out an article's main thesis in a way that makes it easy for people to figure out what the author seeks to prove. If 408-word introductions were long enough to acknowledge prior contributions in 1980, they should be long enough now. Are thirteen pages really needed to "properly place an article in the literature"? These are professional journals. Let the author cite the immediately relevant literature and move on. Skip the long discussions. Readers can go back and look up whatever material they like. Rambling, defensive introductions are distracting. Unless you are steeped in the area's minutiae, it has become increasingly difficult to find the passages that actually detail an article's contribution. We can easily solve this problem. Journals should discourage, if not outright ban, literature review sections. At the same time, they should limit introductions to four pages. Doing so would make it far easier for readers to figure out what the article at hand actually seeks to show, which would benefit nearly everyone involved.

Did You Check to See What Happens If You Include My Favorite Variable in Your Regression?

I recall that when I started in the profession back in the mid-1980s, my senior colleagues complained about how much tougher and more tortuous the submission process had become since the "old days" (meaning the 1970s).

Table 3
Trend in article page length over time

		Number of Pages	
Year	Journal	Mean	median
1980	*JF*	11.411	11
1980	*RFS*	–	–
1990	*JF*	17.511	17
1990	*RFS*	25.370	26
2000	*JF*	31.202	30
2000	*RFS*	31.278	31.5
2010	*JF*	34.275	34
2010	*RFS*	38.041	37

Key: *JF = Journal of Finance, RFS = Review of Financial Studies.*

Back then, referees would either reject an article or accept it subject to some minor changes, but beginning in the mid-1980s there were constant demands for extensive revisions. Have things gotten worse since then? The answer is yes. Referee demands for robustness checks and the exploration of secondary issues have seen ever-increasing growth. In the past, an article might propose a hypothesis, offer evidence in its favor, and then conclude. If others thought there was a reason to suspect the results, whether due to a lack of robustness or because the article had the causality backward, they would prove their argument and publish it.

Now, it seems authors are supposed to cover all possible alternatives themselves. It is a hopeless quest. More importantly, why bother? How frequently does a particular robustness check find anything? Based on the reports and articles I have seen, almost never. But one thing all these additional demands have accomplished is the production of far longer articles.

At the *JF*, papers have increased from eleven to thirty-four pages since 1980, and at the *RFS* they have gone from twenty-six to thirty-seven (Table 3). What does all of this additional material buy? Are articles now more influential? Do their results stand up better to the profession's scrutiny than in the past? I do know that journals are constantly receiving submissions arguing that past published results are suspect in one way or another. Over my thirteen years as an editor I saw little evidence of a decline in such critiques. Indeed, there appears to be no evidence that all of the additional material that articles now include makes them more likely to prove timeless and influential.

Having made the publication process ever longer and more tortuous, the profession has reacted accordingly. Like it or not, tenure decisions often come down to simply counting the number of articles somebody has published. But, for whatever reason, our profession does not divide the credit for a publication by the number of authors. Thus, if an article is authored by two people, each receives the same credit as if it had been solo authored. Combine this with a more arduous editorial process and what do you get? An increase in the number of authors per article, of course!

Table 4
Trend in the number of coauthors over time

		Number of Authors	
Year	Journal	mean	median
1980	*JF*	1.632	2
1980	*RFS*	–	–
1990	*JF*	1.870	2
1990	*RFS*	1.963	2
2000	*JF*	1.955	2
2000	*RFS*	1.778	2
2010	*JF*	2.348	2
2010	*RFS*	2.418	2

Key: *JF = Journal of Finance, RFS = Review of Financial Studies.*

While the median number of authors has remained at two, the mean has been creeping up over time. Since 1980, the average paper at the *JF* has added 0.8 authors. Since 1990, the *RFS* has seen its mean author count increase by a bit under a half (Table 4). Some may argue that multiple coauthors bring multiple talents to articles and thus make them better. These days, title pages routinely list three or four authors. Is this really due to economies of scope, or a desire to pad vitas? Shortening the editorial process will help reduce the pressure to coauthor articles in order to hit some publication count for tenure. Ideally, the review process should neither encourage nor discourage coauthorship. Inducing people to coauthor simply to lengthen their vitas benefits no one. At the very least, with fewer editorial rounds, people can experiment more, and that will hopefully increase the chances that we collectively produce pathbreaking discoveries.

The Current State of Affairs

So where are we today? Over the past few decades we have ended up with longer articles, longer bibliographies, and longer introductions written by ever more coauthors. In exchange, has finance been progressing at a faster and faster rate? My answer is unequivocally no. If anything, it seems like we have gummed up the works and progress is slower than ever.

Even if you think the current editorial process has reduced the chance that erroneous results are published, that achievement is nearly worthless. There is almost no reason to worry if a particular article is "right." What horrors will befall us if a paper with a mistaken conclusion is published? Not many. The vast majority of articles are quickly forgotten. Who cares if their findings are accurate? The profession will not use the material regardless.

What if an article is important—that is, people read and cite it? In that case, academics will dissect its every aspect. Some will examine the article's data filters; others will check for coding errors; still others will look for missing factors or reverse causality explanations. The list is endless. But, that is the

point. The list *is* endless. Authors, referees, and editors cannot even scratch the surface. Nor do they have to. The fact that our colleagues will stress test any important publication means our profession's received canon of knowledge has a self-correcting mechanism built in. We have faith that important articles are "right" because their results have been tested over and over again in myriad ways. Instead of trying to preempt this by demanding that authors perform ever more tests, we should instead just let the academic process work by publishing articles if they are likely to be interesting. After that, let the profession determine if the author's conclusions are accurate—or rather, that they are interesting enough to even make checking for problems worthwhile.

My Contribution to the Cause

As a journal editor I tried to adhere to the goal of reducing the time and effort that the editorial process forced upon authors. In 2009, of the over one thousand submissions that arrived at the *RFS*, I personally edited 153. Of those, only four went into a third round, six ended in two, and none took four or more rounds. Of the ten articles that were sent back for a second round, seven were accepted for publication.

In many cases, my first-round editorial letters would let authors know that I did not plan to return to the referee for a second review. "Why bother?" was my view. The referee presumably had his say, and if the authors were reasonably responsive I figured that was good enough. My only goal was to publish articles people would read. I did not seek perfection. Nor did I want to take an initial submission and have it rewritten in whatever way I or the referee might have if we were its authors. Neither editors nor referees are authors. The article's authors are its authors. They should be allowed to say what they want, however they want.[2]

Along these same lines I also tried to accept one article per year "as is." (I did not always succeed.) The *RFS* receives hundreds of submissions per year. Is anyone going to seriously claim that none of them is good enough to publish upon arrival? Not even one? Come on. It has to be the case that editors are just afraid to pull the trigger without making some additional demands. We should knock it off. Be brave! What do you have to lose? An article may get published that is not as "good as it could be"? No article is ever as good as it could be. Under this metric the process could go on forever.

By the same token, editors need to rein in the referee pool. There is tremendous cross-sectional variation in how people review articles. It is up to the editors to control this. Why should most referees be asked to conduct second-, third-, or fourth-round reviews? A good report offers up a recipe, and following it should overcome whatever significant concerns may exist.

[2] Of course, nobody should be given free rein to publish prose that is insulting, vulgar, or makes claims that few would view as reasonable.

Assuming the authors do as asked, what is the point of another review? Is it just to force the authors to make more changes? Yes, the article will be different. But different is not better.

How much damage did I do to the *RFS* by trying to shorten the editorial process? Based on the standard metrics used by most journals, the answer would appear to be not much. I took over as its executive editor in 2005 and left in 2011. During that time submissions increased from about 500 per year to over 1,200 (at times over 1,300). The impact factor went from 2.20 to 3.55, moving the *RFS* from third among the "big three" finance journals to number one.[3] The immediacy factor (think of it as a journal's future impact factor) went from 0.42 to 4.47. Overall, this does not strike me as such a bad record, at least statistically speaking.[4]

My Advice

We can accomplish a lot more as a profession if we all do less as referees and editors. Referees should just read the article at hand and try to figure out if anybody else would. If the answer is yes, make at most a few suggestions. Limit them to ones that are editorial in nature. Help make the article more readable. Then sign off.

Editors can streamline the publication process by exercising their authority over it. Let referees have their say, but let authors have their say too. A referee's "suggestions," even if made in good faith, may not be beneficial. Editors can help by routinely paring down referee demands. Let authors know that they are free to ignore various comments. After all, in the end, the article's authors have to live with what is ultimately published. Nobody will ever know who the referee was, and even the editor will earn little in the way of credit or blame for what appears in any one article. By letting authors publish the article they want to publish, editors will, from my experience, attract more and better articles. Just as importantly, by being bold, editors greatly increase the odds they accept the very few submissions that ultimately impact the profession. That, in the end, is what our journals all seek to do.

Reference

Ellison, Glenn. 2011. Is Peer Review in Decline? *Economic Inquiry* 49:635–57.

[3] See what I mean about the pressure to increase a journal's citation count!

[4] Ellison (2011) points out that the *Quarterly Journal of Economics* (*QJE*) shortened the time from submission to publication over the past few decades. During the same time frame, its competitors lengthened theirs. Simultaneously, the *QJE* saw a general increase in the tendency of people at "top" economics departments to publish articles in it. This seems consistent with the idea that when a journal makes the review process less onerous, authors react by sending over more of their work.