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How to Make Finance Scholarship More Creative and Equitable: An Author's Suggestions to Editors and Referees *

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Abstract

Editing high-quality finance journals is challenging, and it also requires careful thought on the optimal governance processes required to ensure equity, particularly given the large number of submissions to these journals. Enhancing and maintaining incentives for authors to be creative is also important. In this paper, I discuss issues arising from subjectivity in referees' reports in finance journals and present a list of editorial suggestions that might improve authors' experiences and enhance the quality of submissions to these journals.

如何使财务学术研究更具创意、更公正：一名作者给编辑和审稿人的建议

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摘要

编辑优质财务学期刊殊非易事，尤其是这类期刊会收到大量投稿，故须审慎思考最佳的管治程序，以确保公正。此外，提升并保持可推动作者创意的激励因素同样重要。本文讨论财务学期刊审稿人的主观评审所引起的问题，并为编辑提出一系列建议，以期改善作者的投稿经验，同时提高这类期刊的投稿质量。

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I. Introduction

It seems to be a trend to serve as an editor and then to either provide advice on how to write and review papers or reminisce about one's experiences as an editor (Spiegel, 2012, 2013; Harvey, 2013). Somewhat interestingly, the advice given is sometimes contradictory: Spiegel (2013) feels that referees can be equivocal in their recommendations if they wish, whereas Harvey (2013) feels that they need to be unequivocal. Spiegel (2012) argues that we should seek to ban long introductions, but at the same time, seek to give authors freedom and autonomy. Now, what should referees do?

More recently, Hirshleifer, Schwert, and Singleton (2013), via a joint editorial, admonished authors for two types of behaviour: (1) submitting papers "too early" and (2) resubmitting to another outlet after a rejection without considering a careful revision in response to a referee's report. While this editorial has useful points to make, I would have liked to have seen a greater sense of appreciation for the creative energies of fee-paying authors, a necessary condition for the existence of journals; a sensitivity to the finite time horizons of many faculties (because of the fear of pre-emption by other work and tenure guidelines) would also have been appreciated. In fact, I am concerned that inexperienced assistant professors will take this joint editorial literally and delay submission to an alternative outlet for so long that it might threaten their livelihood. Alternatively, authors might revise their work for so long that their paper might be pre-empted by other work by the time they revise their work to resubmit to another outlet. Time is of the essence in scholarship.

Another problem with the joint editorial is that it does not seem to be internally consistent: On the one hand, authors are allegedly so irresponsible as to submit papers early to use the refereeing process as a cheap consulting device, but on the other hand, after receiving advice from referees, they are also irrational enough *not* to incorporate it and instead send it off to another outlet! So, do all problems emanate from authors alone in an internally inconsistent way while almost all referees are supreme, rational beings? Since authors are also referees, this seems hard to accept.

The goal in this short piece is to discuss how active authors perceive the review process and how the process could be improved from the authors' perspective. It is worth pointing out that while reviewers' generous input without much reward except goodwill is critical, without the free flow of authors' creative energy, the field will not advance. Creativity is a necessary condition for the existence of journals and needs to be nurtured, not demoralised. The review process should be protective of authors, not evaluators, and it seems only reasonable that authors should be given a forum if editors are since editors are dependent on authors to provide the creative impetus that fills the pages of journals.

II. The Problem: Extreme Subjectivity without Accountability and Governance

One point seems clear to me: There are no generally accepted guidelines on how to evaluate work, no agreement on what constitutes quality, and no agreement on where the revise-and-resubmit line should be drawn. Indeed, we seem to go out of our way to impose no governance mechanisms whatsoever; instead, we have processes that basically perpetuate the notion that screening mechanisms are perfect and editors and reviewers can do no wrong. An extreme example of this is the *Journal of Finance*'s appeal process, which indicates that one can successfully appeal only once every three years and that one cannot dispute whether the incremental contribution of a paper is big enough. This is absurd. First, should this limit not be based on the number of papers submitted? Time seems irrelevant. Second, the easiest way for a referee to hedge against being overruled is simply to claim via one sentence that the incremental contribution of the paper is small. Since such a claim is neither verifiable nor disputable (the latter as per the American Finance Association (AFA)), reviewers' interests are preserved. A criticism that can neither be verified nor disputed seems, in all honesty, vacuous.

The appeal process described above (tilted heavily against authors) suggests that the AFA thinks that good papers can be unerringly identified, but the notion that we can identify good papers through the refereeing process in most cases is utopian. It is not that hard for a referee to criticise and reject a paper: all one needs to do is to pick every negative point in the paper; or one can make dismissive remarks, such as "This is the worst paper I have ever reviewed" or "The incremental contribution falls well short of [journal name's] standard". However, criticism is cheap, creation is hard. It is not hard to critique a movie; it is, on the other hand, hard to make a movie, even if it is a mediocre one! I agree that our critics tend to be scholars themselves. Nonetheless, to address one's own insecurities, the tendency to carp is strong. Indeed, on many occasions, I have had editors and referees provide critiques that could easily be applied to their own work. Furthermore, given the small field and small pool of reviewers, Harvey (2013) is right to be concerned about referee fixed effects. Referees' personalities and agendas play a big role in the outcome for a paper.

In general, the process seems to work as follows on quite a few occasions: make up one's mind on the basis of some sort of gut feeling, which is a combination of the authors' identity, the paper's reference list, and other amorphous things, such as writing style, whether the paper uses one's favourite technique or has addressed the feedback one provided, and whether the topic threatens one's own research agenda; then, having made up one's mind to reject, carp about how the paper falls short—balance goes out the window; and then, when the author protests, simply say the paper is not well done. This leads to a low-quality process, which, in turn, leads to low morale. The process frequently becomes akin to a company audit with the sole goal of nailing the company (fairly or unfairly), and I wonder how many finance researchers would support such an approach to an audit! Indeed, I am conducting an informal survey into what people think of the refereeing process in our major journals. Although the survey is still ongoing, among hundreds of respondents, a full 88% responded that referee reports were "sometimes" or "frequently" "not thorough and thoughtful": that is a high statistic.

I now turn to the infamous incremental contribution criterion often applied to papers. I argue that the incremental contribution is really small in 90% of the papers that get accepted in any chosen subset of four field journals. As for the impact factor, it can simply be explained by a version of my illustrious colleagues' cascades theory (Bikhchandani, Hirshleifer, and Welch, 1992): When deciding to cite a paper in a journal, one ignores one's own perception of the paper given the rejection rate of 94% for the journal (if the rejection rate is high, the paper must be good!). However, the rejection rate can go up if the number of accepted papers is kept constant by the powers that be, but one gets more submissions simply because secular technological innovations (e.g. in computing speed) allow the production of more papers and submissions. In the extreme, a rejection rate of 94% can be achieved by only accepting every 16th submitted paper. Thus, the rejection rate is not that informative because its determinants are unverifiable. Moreover, the average annual citation count per paper at the high end is barely three or four over two years: not a high number. The majority of the papers we publish fail to achieve even 10 citations after two years. Hence we should exercise due humility when performing editorial duties.

III. A Fictitious Example of Arbitrariness in Reviewing

The previous section mainly builds an argument that the broad (indeed, virtually unlimited) range of criticisms we permit allows us a large degree of arbitrariness in reports and in establishing the revise-and-resubmit boundary. To give a specific example, according to the ISI Web of Knowledge®, there were 63 papers published in the *Journal of Finance* in 2011. I generated a random number between 1 and 63, which was 29. I then read the 29th published paper, which was titled "Threshold Events and Identification: A Study of Cash Shortfalls". Here are two perspectives on the paper (most of us can identify with both for our own work):

(A) I dislike the paper: "This paper is a somewhat meandering, mundane, and technical treatise on the use of threshold events and RDD on corporate studies. As far as I can see, the authors do not invalidate Rauh but rather just show that Rauh's effect is restricted to certain regions of the 'fundedness space'. The rest of the paper is devoted to econometrics that I suspect are well known to econometricians. Any author doing research using RDD could talk to econometricians down the hallway to do this right, and I do not believe a broad-based A-level journal should consider this paper seriously. I recommend the authors submit the paper to the Journal of Econometrics. I have the following comments."

(B) I like the paper: "The paper is important because it clarifies a lot of our thinking on how to do RDD right. The authors demonstrate depth and insight. I recommend a revise and resubmit but have the following comments."

While I am a fan of Toni Whited's thoughtfulness, notice how broad the evaluation is, how much leeway I have to abuse the system, and how small the checks and balances are! Also note how little extrinsic incentive I have to think deeply about the issue of (A) versus (B) above; the costs and benefits of going either way are trivial. It could be something as simple as "Toni trashed my paper at a conference, this is my chance" versus "Toni did me a good turn by not trashing my paper at a conference" that drives my reaction. Indeed, the greater the number of submissions, the greater the possibility that one can be arbitrary and get away with it. So, such broad leeway without accountability is problematic. It is not an excuse to say "it has always been this way"—the goal should be to improve.

IV. Another Governance Issue: Verifiability

Without proper governance, the issue of the verifiability of research results is also vexing. Apparently, as per Harvey (2013), the AFA decided that it is not worth asking authors to provide source programs and data, in part because proprietary data cannot be disclosed and the highest impact journal in economics does not impose such a requirement. This seems amazingly specious. Are we saying that we will just blindly emulate journals with the highest impact as opposed to engaging in a thoughtful discussion about what is the appropriate thing to do? And why does it make sense to have unverifiable findings as part of the scholarly literature? Ideas are not research: it is the findings that are. If findings cannot be verified, we are simply entertaining each other as opposed to building the knowledge base for future generations of scholars. This concern is the first order issue; the proprietary data issue is the second order issue.

V. Issues Arising from the Recent "Joint Editorial"

I will now revisit Hirshleifer, Schwert, and Singleton (2013), which I discussed briefly in the introduction. The best way to characterise my response is to quote an email I received on the editorial (the author of the email, of course, will remain anonymous): "[The editorial] ignores the basic assumption of reciprocity: authors will respect the referees if the referees respect the authors. The editors point out that the *quid* is not working. They do not point out that it may not be working because the *quo* is not working. Referees have to tell authors respectfully why they are rejecting the paper [or not rejecting the paper]" (words in brackets added by the author). I could not agree more. Too often, referees, perhaps without realising, perhaps stricken with insecurity, perhaps being short on time, write something unfair, poorly reasoned, and arbitrary and cause authors much heartache and headache. I always consider the following before hitting “send”: Is this report fair? If we were all nicer to each other, the profession would be a far more pleasant place.

The joint editorial is well intentioned but can potentially mislead. Many a time, feedback is so idiosyncratic that it is not in the author’s interest to actually incorporate it into the paper. Second, as I said in the introduction, the finite horizon for tenure often requires a pipeline that precludes sequential revisions. Third, pre-emption is a valid concern. On at least a few occasions, I have delayed resubmission to incorporate feedback only to get a report recommending rejection because my paper has been pre-empted. When referees get a paper to review that they have seen before at another outlet but which has not been revised, and when editors are informed of this, they could carefully consider the challenges authors face in terms of submission strategies before reacting adversely to such phenomena.

VI. Overall, What Can Be Done?

Before getting into the specifics of what might improve the process overall, I argue that the role of the editor is more critical than that of the referees. Although it is commonplace to criticise referees because of the anonymous nature of refereeing, so that no one person can be critiqued, editors often “hide behind” referees’ reactions to justify their decisions. There are two problems with this “hiding behind” phenomenon. First, editors choose the referees, and so they are accountable for their choices. Hiding behind one’s own choices is not credible. Second, peer review is not about (often arbitrary) reactions; rather, it is about sound and sensible critiques that can ascertain whether a manuscript adheres to a fair and reasonably consistent set of standards as opposed to idiosyncratic tastes. Indeed, the editorial role is even more crucial in light of Welch’s (2014) important finding that the correlation in opinions across referees for any given paper seems to be only 0.3. While, given our experiences, this modest correlation is not surprising, it certainly accentuates the need for editorial activism. Furthermore, imperfections in the refereeing process and the lack of consensus on quality notwithstanding, the impetus to promote and tenure candidates on the basis of their output in the relevant journals does not show any signs of abating. This also emphasises the critical need for editors to intervene to ensure that resources are properly allocated in the profession and that creativity and hard work get their due.

To improve the experience, I specifically recommend the following for papers that do *not* fall into the clearest of clear reject category (e.g. “The CAPM Holds in Transylvania”):

(1) Increase the number of slots in proportion to the increase in submissions rather than making the “club” misleadingly selective by conflating restricted membership with an

amorphous but unverifiable notion of quality.

(2) If a paper is perceived to be at the margin, publish it: far better to let creation get its due than be obsessed about a phantom notion of quality. The more authors get their due, the more motivated they will be to advance knowledge, which is the global goal.

(3) When rejecting a paper, articulate clear and careful reasons why it falls short of generally accepted guidelines and the existing literature. In going against an author, it is simply not enough to just critique the paper because any paper, including a published one, can be critiqued. It is necessary to detail exactly why a paper falls short of some generally accepted standard when rendering a decision. [This, by the way, does NOT mean looking for reasons to reject because such reasons can be found for 99% of published papers.] Thus, editors need to develop a sound thought process that can separate fair from unfair criticism.

(4) In rejecting a paper, it is not enough to simply amorphously claim that multiple people have "trashed the paper". For the maintenance of editorial credibility, it is the rationales for the rejection recommendation that are important, not the number of people opining; this is because authors know that recommendations depend on who their paper goes to. As an extreme example, a descriptive empirical paper on return anomalies sent to two theorists will likely be recommended for rejection, and an esoteric theory paper sent to empiricists will likely meet the same fate. So, referees' recommendations alone cannot justify a decision. Thus, the editorial letter should focus on the validity of the rationales as per (3) above for the field of the paper. Even allowing for taste variations, given human nature, it is easy to get multiple people to trash a paper: hark back to the Salem Witch Trials. The phrase "give a dog a bad name and hang him" exists for a reason.

(5) And yes, if other people have used a technique in the same or similar journals, that *is* a good reason for using that technique unless a reasonably compelling argument can be made that the situation does not warrant its usage. If the editor sees a referee objecting to a technique used elsewhere (possibly because the referee has not read the related literature), intervention is necessary before the editorial letter is written. Consistency is important.

(6) Clearly advise the author on revise and resubmits: which points are and are not to be addressed; if the peer review seems unduly idiosyncratic in parts and particular points need not be addressed, one needs to say so. Editors need to feel free to tell authors "This is the part of the review that is important, and this is what is not". Also, the editor is accountable: If the author acts in good faith and does not address certain referee comments on the basis of editorial advice, the editor should accordingly overrule the referee if he/she insists that those points be addressed. All too often, editors simply restate the report and state their helplessness given the report. That is not why they are editors! Editors may often be concerned about the political ramifications of overruling

referees. They will, however, find that they command greater respect if they do so.

(7) Give authors the benefit of the doubt when establishing the revise and resubmit boundary. For example, give authors the option of a *de novo* submission when the topic is promising but the referee who originally reviewed the paper is not willing to work with the author. The referee's willingness to work with the author should play less of a role than the promise of the paper in determining whether the author gets a second chance. The objective is to be fair to journal readers and authors, not referees!

(8) Yes, do consider asking *all* authors to provide details and data so that results are verifiable: no excuses, no exceptions.

(9) When there is an appeal or a letter of criticism from the author, listen to the author and respond to the specific points raised.

  a. Note that the author can only respond to the feedback received. Hence, the response to the appeal should be based on the appeal and the information originally provided to the author, not nascent or covert communication.
  b. If the author's claim that the referee has made a material error in reviewing is correct, admit it to the author. There is no point in being indirect. Many a time, referees simply miss an econometric treatment of a topic or the consequences of relaxing an assumption described somewhere in a paper. If the referee is fair-minded enough to admit the issue, great; if not, the referee needs to be held accountable and thus changed. The best-respected editors (and referees) are always the ones who are broad-minded and right-minded enough to be able to listen to authors' specific concerns.
  c. On the basis of experience and the way it is described, and given that it is characterised by the fact that the editor does not intervene much, the *Journal of Financial Economics* (JFE) dispute process, while not perfect, seems the fairest to me.

The benefits of implementing the above points are as follows: (1) authors get some freedom from the "bonded labour" nature of revisions, where the referee *must* be made happy to get a paper accepted; (2) the fresh submission option allows the authors, through their creative forces, to use their own judgment on which comments would improve their paper; (3) the process addresses the demoralising perception that referees are more likely to work with authors on papers that cite their own work or advance their agendas or papers that are written by authors they are affiliated with in some way.

VII. Conclusion

Most importantly, the efforts of referees and editors (for which we all are grateful) notwithstanding, the active creative forces that are necessary to advance the field should not be forced to accept the amorphous criticism that a paper "falls short". This does not seem to be in the interests of building the knowledge base in the profession. To build *ex ante* motivation for creativity, there needs to be editorial activism, a clear articulation of equitable concerns (not critiques on the fly), a commitment to fairness, a willingness to listen to rebuttals without ego, and the recognition that while scholarly endeavours are a two-way street, it is the creative energy of *authors* (and not reviewers) that provides impetus to advance the field.

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