Guest Editorial
How to Avoid the Reviewer's Axe: One Editor's View

Editor's Note: Stephen D. Senturia has been a member of the Board of Editors for IEEE/ASME JMEMS since the journal's first issue in 1992 and was named a Senior Editor in 1998. This experience, coupled with his service from 1985–1995 as the Solid-State Sensors Editor for the IEEE TRANSACTIONS ON ELECTRON DEVICES, adds up to 17 years in an editor's chair. Over the years, Steve has kept mental notes on the myriad of problems that authors have with reviewers and has been inspired to compile the following "advice to the author" about ways to keep reviewers satisfied; hence, to keep them "at bay."

***Abstract*—Based on his many years of experience, a JMEMS editor provides guidelines for authors that will, if followed, greatly reduce the risk of a devastatingly negative result from the review process. The premise is that there are certain things that rightfully anger reviewers, and, once angered, the reviewers become both negative and aggressive in their judgments—hence, the imagery of "the reviewer's axe" and how to avoid it.**

I. INTRODUCTION

SINCE this is a personal commentary, I will use the first person, something that no proper writer of scientific discourse would ever do. As an author of many technical papers over my 35-year academic career, I have too often felt the anxiety of opening that envelope from the journal editor, which, from its bulk, obviously contains my precious manuscript, returned to me for either minor revision, massive rework, or—the ultimate wound—assignment to the manuscriptal trashbin.

Now, having spent some 17 years on the opposite side of the table, my cumulative experience with many manuscripts and almost equally many unhappy authors is that the primary reason reviewers attack certain manuscripts is that those manuscripts are genuinely flawed. Many, if not most authors won't agree, at least not at first. So I thought it would be helpful to authors if I were to set down some practical suggestions for preventing the reviewer's axe from giving the authors a whack.

A scientific manuscript is intended to communicate new information and to teach new material to a willing audience. Many authors forget this simple fact; rather, they view the writing process as an opportunity to bolster their own egos and impress the reader, even discomfit the reader somewhat, either with too much material or too little. Since there are many different styles of paper, I will select a hypothetical example of an experimental paper in which the authors make a minor advance in an established experimental method, and they then use this method to obtain some new results that are to be compared with a model that is also a minor modification of already published work. Along the way, some unusual behavior is observed that the modified model cannot explain. The authors believe that they understand why this behavior is observed, and wish to propose their explanation, even though they have not yet done the definitive experiments to prove their hypothesis.

II. SENTURIA'S GUIDELINES

How should the authors think about organizing and writing this paper? I propose a set of simple guidelines. The names are listed below, followed by some discussion in which each guideline is explored in depth:

- (Almost) Nothing is New.
- Rely on the Believability Index.
- Watch for Gambling Words.
- Don't Be a Longfellow.
- Don't Pull Rabbits Out of Hats.
- Mine All the Gold.
- Remember: Reviewers are Inarticulate and Authors are (somewhat) Paranoid.

Violation of one or more of the principles explained under each guideline risks getting the reviewer angry (with cause), and once that happens, the axe comes out and swings with purpose. I don't believe that a manuscript has ever been written that cannot be improved, but an angry reviewer finds many more faults than a reviewer who believes that the author has basically done a highly professional job, both of research and of writing. It's just plain dumb to aggravate a reviewer. Every author's goal should be to keep the reviewer's axe in its sheath.

III. (ALMOST) NOTHING IS NEW

Everyone knows that there is nothing new under the sun. Everyone, that is, except an ambitious author who believes that his or her work is unique. While there are a few truly unique and amazing results published once in a while, most of our work is built on the work of others.

It is every author's obligation to establish clearly the context in which the new work belongs, both by a brief introduction and by the citation of appropriate references (which the author should have read, not simply copied from someone else's reference list). If an author doesn't know any relevant references, then he or she should get on-line and find them—they are there! I used to tell my graduate students: "First, figure out what you have done. Then, go to the library and find it!" They might not find exactly what they themselves had done, but they would find all kinds of relevant material that needed to be sifted to find the critical subset that was so relevant, it demanded citation.

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Along the way, there are some additional principles to follow.

> If you have a manuscript on a closely related topic that is either buried in some conference digest, is still in review, or has already been accepted by a journal but is not yet in print, it is your obligation both to notify the editor and reviewers of the existence of this paper and provide prepublication copies to aid the review process. *This is perhaps the single most significant source of reviewer venom—the discovery of a related paper that the authors have kept hidden from the reviewers.* And the venom is real—the reviewer feels that the author is trying to trick the review process, so out comes the axe.

> *If a reference is relevant enough to your work to cite it, then it is also relevant to your results.* Many authors provide a cosmetic list of references at the beginning of a paper, but never return to compare their allegedly new results with the contents of the cited papers. This infuriates reviewers, and rightly so. Scientific advances are the result of confirmation and comparison among many independent investigators. When results are presented without any comparisons to prior work, reviewers get angry, and they get out the axe.

IV. Rely on the Believability Index

The essence of scientific advance is that results are believable because they have been repeated and checked by independent investigators. By definition then, a truly new result is not scientifically confirmed until it has been repeated by others. This leads me to the concept of a Believability Index.

In creating an outline for this hypothetical experimental paper with modest advances both in experimental method and in the model and with some surprising results that come out, the author should think about the believability of the various constituents of the outline. Clearly, the existence of a cited public record of previously published work (regardless of whether that work is or is not correct) is highly believable. So are the basic laws of physics, well-established theories and models, and widely practiced experimental procedures. All of these have a high believability.

In contrast, any new result has a lower believability. If a result hasn't been confirmed by others, it is not "established" and therefore is intrinsically less believable than a peer-confirmed result. At the lowest level of believability is an author's speculation as to the reason for any new result. (Said another way, "Talk is cheap.") But if a new experimental result is sufficiently documented in a manuscript, reviewers may accept it, even if they don't agree with the speculative explanation for the new behavior.

All of this leads to the principle of the Believability Index, which automatically assigns an order to the contents of the paper:

> *Write the paper in order of decreasing believability.*

The beauty of this approach should be self-evident. If a paper is written in order of decreasing believability, each reader will be led to agree with what is stated at the beginning, because it has high believability, but later might balk at accepting either a new experimental result (if improperly explained) or a speculative explanation. A properly ordered paper will have NO critical high-believability content after the introduction of the first moderate- or low-believability material. And the reader who, at some point along the way, fails to agree with the author, has the benefit of knowing all of the high-believability material at the point of disagreement and thus can focus the disagreement on the right issues.

Sample-preparation methods, which are assumed to be completely factual reports of what an author did, should have a high believability and thus belong early in a paper. A common mistake of authors is to surprise readers relatively late in a paper, well beyond the first low-believability point, with a report of some new sample preparations and the like. That kind of writing makes for choppy papers that are hard to read, and hard-to-read papers irritate reviewers.

If you are reporting a new experimental procedure, in order to keep its believability high, you should trace by example how you go from raw data to reduced data to extracted measured result, and mention such things as calibration (if not based on a commercial instrument specification), the number of samples, and the relation between the error bars on the graph and your data (is it full range? probable error of the mean? what?). Confirmation that the new method gives an expected answer in a well-known case is an obvious believability-builder. This helps to improve the believability of your new experimental results, which was presumably the whole point of writing the paper in the first place.

If you are reporting a new model, you need to anchor the model in high-believability starting points, then make clear when you are jumping off the believability cliff by making an assumption that is not provably correct.

As to whether models or experimental methods go first is largely a matter of taste. If there are new components to both, then be careful of going too far down one road or the other in terms of believability before introducing the other.

The loveliest outcome of this approach is that you, as author, are led to place all speculation after the point at which all more moderate-believability things such as new experimental results are already in hand. This sometimes poses difficulties for authors. The tendency is to dribble out results, then comment (see Section VI, "Longfellow"), then dribble out some more results, and so on. Get the higher believability material on record before speculating. Your reviewer will love you.

V. Watch for Gambling Words

You are probably wondering why I would be interested in gambling words in this context. For this insight, I am indebted to Prof. Arthur C. Smith of MIT who, when coauthoring a paper with me back in the early 1970s, cautioned me against using what he called "gambling words" like "obviously," "probably," "certainly," and "undoubtedly." Art's comment was that if you have to persuade using probabilistic words, it means you can't prove your point and you are speculating. Hence:

> If you find yourself inclined to use gambling words, it means you don't know what you are talking about, and, therefore, such material has, intrinsically, low believability. Replace the gambling words with words that make it clear that you are speculating, and place such comments in the appropriate place in the paper, along with other low-believability speculations.

VI. Don't be a Longfellow

In *Tales of a Wayside Inn*, the poet Longfellow presents a set of stories told by various guests at the inn, sitting around the fire. While Longfellow was a wonderful story-teller, he should NOT be adopted as the role model for scientific writing. It is an alluring temptation to state a fact and then tell a story explaining this fact, then give another fact and tell another story, on and on until one runs out of new facts. (For some reason, chemists, in particular, seem to love this model.) What's wrong with it is that it violates the rule of decreasing believability. *Stories are nice, but might, like Longfellow's, be fiction. Scientific writing, one hopes, is nonfiction.* Resist the temptation to be a modern-day Longfellow until ALL of the high-believability material has been presented and one is ready to telegraph the fact that one is speculating by using headings such as "Discussion" or "Interpretation."

VII. Don't Pull Rabbits Out of Hats

We all recall the thrill when, as children, seated on the floor of a crowded school auditorium, we would see the visiting magician pull a rabbit out of his hat. Some of that thrill seems to stick, because many scientific writers seem to want to imitate the magician. They store up a confirming experiment until after they have led their readers down a particular garden path, and then, and only then, do they reveal that they did this extra experiment that (the authors hope) proves their point. There are two problems with this: first, it clearly violates the Believability-Index rule by placing (presumably) high-believability material after some lower-believability explanations of earlier results and second, it opens the possibility that there is really a flaw in the reasoning. *Reviewers get tenacious searching for the flaws when confronted with rabbits out of hats.* The rule is simple: *Don't do it.*

VIII. Mine All the Gold

Imagine that you have hiked up a desolate canyon in mountainous country, took a few shovelfuls of promising-looking dirt, dumped them in the gold pan and, in the nearby stream, washed it down until you found a few nuggets of gold. You are elated, and decide to rush to the nearest mining office and stake a claim. Then, inexplicably, you announce your claim to the world, but never return to mine the gold.

Everyone would think you a fool if you were to do this, but in reality, many scientific writers, in effect, fail to "mine the gold." It costs real time and effort (and often significant sums of hard-to-get money) to get good data. The data represent the shovelfuls of earth that yield a few nuggets. The analog of "staking the claim" is writing a paper—it is through this process that you announce to the world that "there is gold around." Given the cost of those data, however, it would be foolish not to try to get every single nugget out of the dirt, or, at a minimum, every nugget out of the shovelfuls of dirt you have already collected.

Many authors, regrettably, in my opinion, are too quick to give up on what they can learn from data. (This is the opposite of overspeculation on what poorly supported results mean. That is a different sin which was covered under the general "Believability" heading.) While it may not be essential to the publication of the nuggets you did find, your chances of success with reviewers goes way up when you are able to demonstrate a DEEP understanding of what your data do and do not show. For example, many authors look at the signal they are able to measure and fail to note that the noise spectrum may provide information on fundamental processes that might limit detectability. Other authors fail to search for correlations buried in their results that give hints of things that may be new or important. In short, be tenacious. *Try to mine all of the information from data, even if it pushes you in the direction of speculation and other low-believability comments.* As long as such comments are clearly labeled as speculative and are potentially interesting, reviewers will applaud both the diligence and the forthrightness.

IX. Remember: Reviewers Are Inarticulate and Authors Are (Somewhat) Paranoid

I close this article with guidelines on how to deal with the reviewers' comments, once they have been received.

When a reviewer complains about something in a paper, the chances are very good that there is a problem with the paper. Not every comment by every reviewer is a correct or proper criticism, but I would say that more than 90% of the criticisms that I have seen have some degree of merit.

But, reviewers are inarticulate. Reviewers often state their objections badly, and that makes their reviews look arbitrary, even whimsical. The authors' anger and paranoia are then provoked. Now what?

As an author, it is your obligation to respond to each and every reviewer criticism. The manner with which you do this has a great effect on the smoothness of the road to publication. If you try, as some have, to bully the reviewer (or the editor) into submission without making a constructive response, both the reviewer (and probably the editor) will do the equivalent of tossing you out on the street. I have seen cases in which brilliantly written polemics from angry authors that effectively rebut a reviewers' point failed in their goal because the authors wouldn't incorporate the essence of their rebuttal into suitable modifications of their precious manuscript. *Ego interferes with constructive action, and paranoia cripples it.*

Asserting scientific correctness of your own work is a task to be undertaken with some humility and with respect for the established knowledge that has preceded your work. Difficult as it may be, *hold your temper and your polemics when you get a review, and try instead to think "why is the reviewer really bothered at this point?"* If you, as author, can figure out why the reviewer was led to a particular comment, you will find a pathway to improving the paper and satisfying the reviewer at the same time. Often, the failing of the paper is not at the precise point raised by the reviewer but rather arises somewhere else, such as through a non-optimal order of topics or comments, or an omitted few words of explanation elsewhere else in the paper. A remarkably open mind is required to read reviewers' criticisms in this vein, but it is vastly productive and greatly shortens the time to publication.

Of course, some reviewers' comments are simply wrong. If you handle the proper comments with courtesy and professionalism, the editor is much more likely to agree with you about the comments that you reject. So, my advice is to submit a complete restatement of the reviewer comments with your own comments added on how you have responded to each and every criticism. If you do a good enough job on this, the editor may find that he or she can make a publication decision without going back through the review process, saving many weeks in publication time. And the reputation you develop by being mindful of the realities of referee inarticulateness will serve you well throughout your career.

And, next time, you will write a better paper.

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