

# The Twin Arts of Writing and Revising Technical Articles

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## I. WRITING REVIEW-PROOF TECHNICAL PAPERS

Authors write papers; reviewers review them. The relationship between them is a kind of love-hate relationship. Reviewers are loved when they write nice things; they are hated when they niggle. But there is no way the authors could avoid the reviewers. They are an absolute necessity. Without them there would be no quality publishing. For authors, fame is the spur, with promotion and job security as extras. There are, however, risks. The author discloses professional secrets to a potentially hostile audience, which may or may not know much about the subject. Unfortunately, there is no choice. Sending a manuscript means disclosure.

This article goes into a little more detail about the interactions between authors and reviewers. We have to admit, this is not an entirely new venture to us. One of us (L. Solymar) gave some thoughts to these problems in an article published in this very same periodical some time ago, to be precise in 1963. Let us quote from that article:

“Reviewers are selected from the leading scientists in order to filter the manuscripts reaching the Editor. Unfortunately, leading scientists are

generally busy, have many obligations and administrative burdens as well. They are unable to spend the major part of an afternoon on reading a single paper; but nonetheless, they make comments.

A young author should be aware of this situation and—instead of wasting his time on complaints—should write his paper to satisfy the reviewer, whose sharp eyes detect the slightest irregularity. If the paper is too long the author will be accused of verbosity; if the paper is too short he will be recommended to collect further additional material. If he reports on purely experimental work the foundations will be criticized: if he propounds a simple theory he will be called superficial. If he lists a too long bibliography he will be regarded as unoriginal if he refers to no one at all he will be branded conceited. Thus I would suggest a compromise. The length of the paper should be between 8 and 12 type-written pages (double spacing of course) and about one third of it should be covered with integral signs and special functions. The number of references should vary between 6 and 10, half of them referring to famous (the reviewer has heard of them) and the other half to unknown

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(the reviewer has not heard of them) works. Following the above recommendations the author has a fair chance that his paper will not be rejected *a priori*. On a superficial look the reviewer is favourably inclined."

There were other recommendations too, but they did not manage to overcome the ravages of the iron teeth of time. Looking at the above recommendations from a distance of 55 years, many of them make sense but some others sound odd, not to say derisory. "Special functions," whoever has heard of them? Bessel functions might still be around occasionally but nobody could spot today a "confluent hypergeometric function." All the rage is "simulations," a technique that takes a lot of time, produces nice curves, and nobody could or would want to repeat them. And then there are also two quite ridiculous statements in the above archaic attempt at giving advice: 1) reviewers are selected from leading scientists; and 2) references should vary between 6 and 10. Why are they ridiculous? Surely, there are still leading scientists around, but would they read a manuscript? Not likely. They spend most of their time raising money for their group, for their department, or for their institution.

There is another reason why big shots will be unlikely to act as reviewers: the tremendous proliferation of journals. When we started in our career, at first in industry, in the circuits area there was one IRE journal, the *Transactions on Circuit Theory*. It was published once every three months. It contained papers on circuit theory, filter design, solid-state circuits, and eventually even on digital signal processing. If you read it, you were well informed about activities in the United States and Canada. Admittedly, there was also the *Bell Labs Technical Journal*, and a few German publications which we had to keep an eye on so we did not miss important ideas or trends. Nonetheless, you did not need to spend many hours every

week to keep up with the state of art. Now, you do. So if leading scientists are too busy with fundraising, reading, and teaching to do reviews, who will do the job? Who are the reviewers today? Editors, quite understandably, face a problem of what to do with the plethora of papers they receive. They are glad to get reviewers. So in our time everybody can be a reviewer. Well, maybe not every man on the street, but even an intelligent doctorate student is acceptable.

There are new rules for references as well. Between 6 and 10? Not anymore. The numbers are more likely 50+. We have actually seen papers (*not* review papers) with 150 references. Why? Is it simply inflation? Yes. Everything grows, not only the number of transistors deposited on an area of your thumb. But another reason for authors doing so might be to outfox the reviewers. A possible aim is to confuse them and make them realize how little they know the literature.

A recent problem has been editors and reviewers requesting more citations to the journal that is receiving the submission. The journal editor wants to increase the impact factor or other bibliometric metrics to make him and his journal look better. We have not seen much of this in the IEEE journals in which we publish, but it seems to occur elsewhere.

## II. REVIEWERS' MOTIVATIONS

Reviewers are respectable people, but like everyone else they need some motivation to do their job. Well, the order of importance might be different for different people, but on the whole, we would say that the following reasons play a role.

- 1) Some altruistic types feel that they owe it to their society, since they know that other sufferers will have reviewed *their* papers.
- 2) The reviewer likes to pass judgment over his/her fellow scientists. It makes her or him feel important.

- 3) To be a reviewer (provided the journal is sufficiently prestigious) is good for the CV.
- 4) The authors often make the fundamental mistake of not referring to the reviewer's publications. That mistake needs to be corrected in the interest of the general public.
- 5) Something might turn up, an idea, a model, a reference, that is of interest for the reviewer's research.
- 6) The author is a good friend, who deserves a favorable review.
- 7) The author is one of those people who had harshly criticized the reviewer's presentation at a recent conference. The reviewer will find reasons to reject the paper.

## III. REVISING REVIEWED TECHNICAL PAPERS: A CASE HISTORY

There are few tasks more odious than trying to revise a paper reviewed by a trampling horde of brutal reviewers. We have to tell the story of what happened to one of us (G. Temes), a story about authors and reviewers, full with surprises. It was a paper of seven(!) authors, jointly written, and never adequately proofread. It is said that a camel is a horse designed by a committee. This paper was written and rewritten by seven authors. Each corrected the errors made by the others, and introduced new ones. The resulting manuscript was then reviewed by 13(!) experts. Their opinions were about evenly split, and their overall assessment of the contribution ranged from "insignificant drivel" to "a gem, full of new ideas." Most of them agreed, however, that the paper needed major surgery to make it readable. One reviewer even made his/her writeup very personal: how could G. Temes allow his name to be associated with such careless and sloppy manuscript? G. Temes was understandably upset when reading this. How did the reviewer have the temerity to make such a

personal reproach? At this stage, however, G. Temes decided to reread the paper; carefully, word-by-word, more than once. And the result? He agreed that it was a sloppy drivel!

What lessons can be drawn from this incident? 1) Do not write a sloppy drivel. 2) There is no need ever for 13 reviewers; four are plenty. 3) Never make personal comments; it is not nice even if it is justified. 4) Personal attacks might occasionally yield some benefits. 5) Recognize your own drivel, early if possible, but certainly before it goes into print.

Another issue raised by our horse/camel paper is the proper selection of coauthors included. There is a tradeoff here: clearly, the fewer the authors, the larger share of the credit goes to the main author. On the other hand, when including a friend as a coauthor, this is a way of helping his/her career. Also, that friend may return the favor when he/she is the main honcho! There should perhaps be some guidelines created by the journals about the appropriate level of contribution by coauthors.

#### IV. REVISING REVIEWED TECHNICAL PAPERS (GENERAL)

When it comes to revision, eating a humble pie is the rule. Arguing with a hostile reviewer is foolish. The editor will not usually want to act as the arbiter between an irate author and a hostile reviewer. There are more than enough papers with unanimously

positive reviews to fill that journal until probably the December 2022 issue. Hence, you should not rely on the appeal of a negative review. So how can you rescue your paper? Here are some ideas.

- 1) Start your reply by thanking profusely the reviewers for their valuable comments.
- 2) Write a paragraph on each of the reviewers' recommendations.
- 3) If the reviewer asked for the expansion of some parts of the paper, do the expansion. Be verbose. Limitations on volume do not apply anymore. By adding plenty of new materials, you do only what the reviewer asked you to do. And, of course, verbosity has another advantage as well. The reviewer probably forgot why he/she criticized one of the conclusions, and cannot really decide whether the author's corrections are adequate. Even if the author's response is too long or too elaborate, the reviewer will not bother to read it.
- 4) If your command of the English/American language is inadequate, ask a native speaker to make corrections. Although many of the reviewers are not concerned about this, do it for your readership's sake. Clarity is a good thing.

The above advice is still relevant even if you have several hundred

papers behind you, with one exception: If the reviewers contradict each other, you may point that out with a glee, and your revised paper will be quickly accepted.

#### V. AVOIDING CATASTROPHE AND OTHER PENALTIES

We are now returning to the great explosion in the number of technical journals and conferences: Since it is nearly impossible to read all journals and conference proceedings immediately, or even soon after their publication, the chances of the fact that you may be unaware of an earlier similar contribution which you are inadvertently duplicating in your paper is frighteningly high. This is especially true if you are working in a popular area. Fortunately, the IEEE Xplore Digital Library can give you a way to reduce the probability of this catastrophe. If you scan the likeliest forums and keywords relevant to your work, you may catch the culprits who dared to get the same idea before you. This finding will be painful, but trying to publish, or even actually publish, other people's ideas may damage your reputation beyond repair.

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