Publication Guidelines for IEEE Transactions on Reliability, and Perhaps Everyone

IN THE HOPES of guiding authors to write better papers for our Transactions on Reliability, I've compiled some thoughts which I hope will help. Perhaps this will help the authors of all archival journals.

An author first has to determine whether their work is of sufficient importance to warrant publication. Then if so, where should they submit the work, for whom should they write the paper, and how?

I. What Constitutes a Sufficient Contribution Worth Publication?

To do meaningful work for consideration in an archival journal, you have to extend the state of the art in a meaningful way.

While it is difficult to specify sufficiently meaningful work for publication, we can define some criteria for what is not worth consideration. Here are some perhaps obvious criteria missed by some authors.

- Has the work been published elsewhere before? If so, there is no need to publish it again.
- Is the new work extending previous work in a way that is obvious? Then is there enough value to the science to make the work publishable? Perhaps others have done this work without publishing it, because it is too obvious to consider.
- Is the effort simply an exercise in search of a problem? Then perhaps there is no reason to publish the exercise[1]. Readers would not take interest in it without a reason to use your work. There must be some reason to do the work, and the reason must be strong enough to have readers read it.
- Are you simply solving a problem already solved, just in a different way, or with a different method? Then before you proceed further, you should find obvious value in the work:
  - It had better show merit over existing methods, to gain the interest of practitioners and researchers alike.
  - Or it had better demonstrate why a significant area of work can be ignored from further investigation or use, to gain the interest of researchers in the area or practitioners who may misapply tools otherwise.
- Are you bringing science from one field to another? If so, perhaps this purpose does not warrant a new publication. Perhaps we should simply bring the attention of the related work to those in the other field using methods other than an archival journal. Maybe a newsletter, or a conference are more appropriate venues.

A. Comment Papers Specifically

Comment papers should do more than correct errors. If that is what you have to contribute, consider whether the errors are obvious, or unimportant. If yes to either or both, there may be no reason to bring the errors to the attention of anyone other than the original author. In fact, that is usually the best course of action, regardless. If after contacting the author you find the corrections are important and valid, you should issue an erratum with that author.

Corrections with important extensions are notable exceptions. If in the course of extending work in a meaningful way you find an error in another paper, you should perhaps still contact the author for verification. You should also complete your extension, and submit it as a regular paper. That way you are able to make corrections to previous work, and add additional value from the extension.

Because a published paper is meant to represent a significant contribution to the state of the art, a simple correction is not enough to justify publishing a paper, even a short one. Instead, be sure you are adding value most of all; then be sure you point out the correction. If there is no value to be had, it may be best to contact the author to issue an erratum, if there is an appropriate action at all.

II. Matching a Paper to a Journal, When the Paper Is Ready

An archival journal should publish the important, significant extensions of the state of the art. Its scope is what determines whether the qualifying work belongs in a particular journal, or another. Authors should carefully select a proper journal for their submission which they believe meets the archival publication qualifications. Authors who do not consider their audience carefully will run the risk of immediate rejection of their submitted work, or worse yet experience a long, failed review process.

A. Choices for Consideration

Journals have defined scope. Some journals have as their scope specific, defined areas of context around a supportive science. Other journals are more technology or business area context based, which I'll refer to as domain specific areas. Reliability is the context of Transactions on Reliability, a supportive science for all the domain specific areas. There are other examples such as Operations Research, Systems Engineering, or Management Science. IEEE has many examples of domain specific journals such as Transactions on Broadcasting, or Transactions on Applied Superconductivity. Reliability should be their concern as well.

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[1]Some may argue that research does not need to be grounded in application to be important. While true, without at least a basis for interest by researchers, work will go unnoticed, or not properly considered. Authors of such papers must explain why the work is still important in this case, which is easy enough to do. Research for research's sake does not remove this responsibility from the author!

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B. How to Select Among the Choices

Given the choices, where is the appropriate place to publish a work; and therefore, where should an author send their work for consideration? A reliability_oriented paper could extend the science in one or more domain areas, or in one or more supportive science fields, or both. So there is no immediate, obvious choice in such cases. But let's dig deeper.

1) Readers' Interest: Readers of a domain specific journal would have no interest in a work that extends the reliability field exclusively, just as readers of a supportive science field take no interest in work that extends the state of the art in a domain area exclusively. Now our choices are a bit simpler.

In these cases, the decision is simply which is the best journal in the domain area, or the supportive science area, for the paper.

2) Where There is Overlap: But where there is overlap between the domain and the supportive science, there is opportunity. A work could contribute to both areas, but it is still important to find the best audience, even when both are good audiences.

It is common for smaller publications or conferences to focus on a domain, and to focus on a supportive area within that domain. For example, there are numerous conferences on communication network reliability, which should be a focus of the support area of reliability within the domain of communication networks (the Design for Reliable Communication Networks (DRCN) conference [1] is one such example).

A paper that extends the state of the art both in the domain and the supportive science area could find its highest interest in a venue that specializes in the intersection of the two areas, but not find the broadest audience, nor find the audience perhaps in most need of the knowledge. Therefore, authors should once again consider the audience they need to reach, then decide accordingly.

3) New to Some, Old to Others: It is always good when the knowledge of one area is carried over to extend the other, but that is not always the best contribution to put fourth, and not enough to justify publication of work, as the work is not actually extending science in a meaningful way. But if the sharing of knowledge does extend the state of the art in one or both areas, the area where the state of the art is extended is where the work should be presented.

C. Decision: Go Where There is Most Value

In the end it is most important to write to your audience, and write the paper for the journal you intend to use for your work. Further, the journal for your paper should be the one which your work furthers the most in terms of knowledge and state of the art. Select the best avenue for your audience, where your work will have the highest impact.

To do this, obviously the author must consider this question before even writing the paper. Therefore, think before you write the paper. Write the paper from the work in a way that will make your work the most valuable. Write for the audience who will benefit most from your work, and write for the journal they read.

Be sure to follow the logic soundly. Do not write a paper generally reporting your results, expecting the world to labor over your "notes" to determine where the value lies. Papers like that will not even be reviewed, if you are lucky. If you are not lucky, the paper will waste the time of reviewers, as well as yours, as many labor over the work only to reject it.

Instead, develop the science, determine who needs to care, find a way to reach that audience, and write the best message to carry the science forward. Choose and use the right venue to help you meet that goal.

III. In Conclusion, Is There Anything Else?

Beyond having a sufficient body of work to publish, and targeting the best way to your audience, you must write a clear, correct, concise, complete, and convincing contribution. Failing these additional criteria, the 5-Cs, great work may not be published, and therefore go unnoticed.

For guidelines on how to write effectively, there are many resources; but one I like is "Writing Under Pressure: The Quick Writing Process" by Sanford Kaye [2]. Here are a few more simple rules to follow.

- Write the simplest sentences you can while still being sufficiently precise.
- Avoid unnecessary flowery language and terms when possible.
- Use first person and active tense so that the reader knows who is doing what.

If scientific writers would follow just these simple rules, their work would be so much more valuable.

Graphics are equally important, if not more. The same rules for writing well apply to graphics too. For guidelines on great graphics, see the work of Edward Tufte (such as [3], [4]), and websites such as http://www.visual-literacy.org/ [5] for a great starting point.

Jason Rupe, *Managing Editor*

References

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