

Peer Review

The idea of writing about peer review came to me after having discussed this topic a number of times with colleagues while sipping a good cup of coffee at lunch time. Everybody has a strong opinion about it, and not unexpectedly, since this is something that affects our professional lives almost on a daily basis, either as authors or reviewers. (In addition, some of us also serve as editors or associate editors, and this makes the issue even more relevant.) We receive review requests practically every week and e-mail invitations to contribute to some obscure new publication (that we may never have heard of before) almost every day. Also, if we act as (associate) editors, we frequently have to “hunt” for good reviewers, and this can be a quite frustrating task.

So, what is going on? Do we have a problem, and is there a solution?

Before launching into a long open-loop, and possibly too narrow, monologue, I thought I should ask for the opinions of some my colleagues who I regard as more experienced than I am in this matter. So I contacted some present and past editors of the IEEE Control Systems Society (CSS) transactions and *IEEE Control Systems Magazine* and asked for their opinions and for references they may be aware of regarding peer review. Well, it was quite an interesting and rewarding experience. I discovered a whole world to which I had been only partially exposed.

According to the Oxford Dictionary online (see also *Wikipedia* [1] and the references therein), peer review is the “evaluation of scientific, academic, or professional work by others working in



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the same field.” “Prepublication review” is the preliminary evaluation aimed at deciding about the paper’s acceptance or rejection, while in “post-publication review” the evaluation takes place after the scientific article becomes accessible to everybody.

Prepublication review is typically “blind” (meaning that authors do not know the identities of the persons who review their work) or even “double blind” (the reviewers do not know the identities of the authors of the papers they are reviewing). On the contrary, in “open peer review,” adopted both for prepublication and for post-publication reviews, the identities of the reviewers are public knowledge.

In 2006, *Nature* started experiments of this kind, and some academic journals were launched relying only on open peer review. There are mixed results and opinions about which of these solutions is preferable, but there seems to be consensus about the fact

that under open peer review, more of those who are invited to review decline to do so.

Interestingly, the peer review system dates back to 1665 (see [1] and [2] and references therein) when the founding editor of *Philosophical Transactions of the Royal Society*, Henry Oldenburg, a member of the Royal Society of London, set up a “Committee on Papers” whose task was to solicit and collect expert opinions. It became the standard for scientific publication only after World War II, when the sudden increase in the volume of research articles made it impossible for editors to personally evaluate all the articles they received, and they needed to search for external help.

In the large majority of the papers about peer review I read, the authors were making a case about the fact that the peer review process was a flawed process (see, for instance, [3]–[6]), and several of them were suggesting open post-publication peer review as a/the solution. Reference [2], in particular, claimed that nobody has ever been able to prove that peer review improves the quality of published papers. I personally disagree with this claim. As an author, I recognize that some of my papers have benefited, sometimes in the content, often in the form, from the competent suggestions of peers. As a reviewer, I believe I have always done my best to provide some constructive feedback, not only on the technical aspects of the paper and on the presentation, but also on its meaning and relevance. At least in my biased view, the final outcome seems to make the paper

better and more enjoyable to read than the original manuscript.

When collecting opinions from several CSS volunteers that I bothered for the purpose of writing this column, I discovered, with a mix of satisfaction and relief, that none of them questioned the need for prepublication peer review. Everybody pointed out some problems and had good suggestions about how to address them, but nobody suggested moving to a different model. I regard this as evidence that our community believes the system works or at least can work. Yes, it needs improvement, but in the end it works!

The main problem with the current peer review process, and in this respect I fully agree with [3], is the enormous number of papers that enter the system and require an evaluation. Panos Antsaklis, editor-in-chief of *IEEE Transactions on Automatic Control (TAC)*, made a rough but nonetheless very signifi-

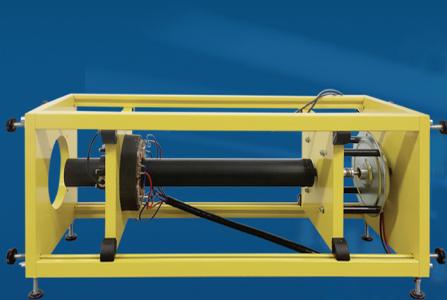
cant estimate: *IEEE TAC* and *Automatica* alone handle about 2500 papers per year, with multiple rounds of reviews for each paper (assume two, to be conservative). The American Control Conference and the IEEE Conference on Decision and Control (CDC), on the other hand, receive about 4000 submissions per year. If we expect three reviews on average for each paper, this means a total of approximately 25,000 reviews. This ignores all the other journals and conferences dealing with systems and control.

Is this sustainable? Most people believe it is not, and I agree with them. Reference [7] claims that the number of researchers has grown in parallel with the number of submissions, and the burden of peer review has not significantly increased. I disagree with this conclusion. Even if the community of researchers has grown over the years, the number of people who submit papers is much higher than the number

of people willing and able to provide a careful review.

Let us look into these two adjectives: willing and able. It is a fact that a lot of people decline to review papers. Yes, occasionally one is asked to provide an evaluation of something that is really marginal or even outside of his/her expertise. Turning down such review requests is understandable. It is also true that some review requests really come at a bad time. Okay, this may happen occasionally. The problem, however, is when people *always* consider themselves too busy with better and more important things to do than providing "service" to the research community. Some colleagues refer to these persons as "takers" or "freeloaders." This is a real problem because a research community works well if everybody contributes in some measure. If we ask the community for a service, when we submit our manuscripts, it should be fair to be willing to

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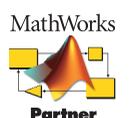
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provide the same service when asked. Nobody wants to start using a jewelry scale to evaluate whether each of us is giving back as much as we receive, but there are situations in which the unbalance is blatant.

Some Societies have decided to put policies in place to address this unbalance. For instance, the Board of Governors (BoG) of the IEEE Information Theory (IT) Society passed the following resolution at its July 1, 2012, meeting (see [8]): "In view of its concerns about excessive reviewing delays in the IT Transactions, the BoG authorizes the EiC in his sole judgment to delay publication of papers by authors who are derelict in their reviewing duties." Most of the colleagues I know do not support this kind of policy, but a few others do. It is a fact, however, that if the peer-review process of a journal becomes seriously disrupted by the reviewers' behavior, some firm action needs to be taken.

And now let us consider the second adjective, "able." Each of us, at the beginning of our career, did not know how to produce a useful review. A novice reviewer typically interprets reviewing the paper as checking whether or not the math is correct. There is much more than this to reviewing. Of course, correctness matters a lot. A wrong result may be published, quoted, and subsequently give rise to incorrect papers. Repairing this damage may take substantial effort, but a reviewer should not focus just on correctness. Is the paper meaningful? Is the paper relevant? How is the paper positioned in the current literature on the topic? Some journal review forms explicitly pose these questions. Often it is hard to give a precise answer, but to experts on the topic of the paper, the answers to these questions should come reasonably easily. Perspective requires experience but also is the result of the will to gain this experience.

Reference [7] quotes the head of a major research institution who says that training students to evaluate papers is a fundamental and even enjoyable experience, which will help them also in writing papers and in addressing reviewers'

comments when their own papers are reviewed. This learning process does not end with the review submission but continues with the evaluation ex-post of its effectiveness by comparing the student's comments with those provided by the other reviewers.

I am fully supportive of this viewpoint. As a Ph.D. student, I was trained to review papers. This made a big difference to me. But what matters is not only the technical aspect of the job but also the perception that what we are doing matters, is a duty, helps us learn things, and is not wasted time. I strongly believe that if I am serving as IEEE CSS president now, it is because I was a committed and dutiful reviewer at the beginning of my career.

My careful work as an *IEEE TAC* reviewer was the reason why an outgoing associate editor (AE) (who I did not know personally) recommended me to be his successor. My commitment as an AE was the reason why Christos Cassandras nominated me to be registration chair of the IEEE CDC 2004 and why I became an appointed BoG member. Then, further years of committed service to the CSS as vice president (Member Activities and Conference Activities) led me here. Was it worthwhile? Definitely so. Did I quit reviewing papers just because I am now deeply involved in Society governance? No, I did not. Sometimes it may be harder, but I still regard this as a duty and something that keeps me aware of the literature, thus helping my research. Indeed, in reviewing papers, we not only provide a service to our community but we also keep track of the research evolution and learn things we do not know. So we also receive benefits.

There are definitely too many papers in the system, and the "publish or perish" culture imposed by research funding agencies and senior people in universities shares some of the blame for this [3]. However, there is something we can do, both as a Society and as individuals. Surely it would be appropriate to focus more on the concept of "meaningful/suitable contribution," in addition to the standard concepts of

"original/correct paper," both when we write a paper and when we judge it. In this sense, editorial boards and editors-in-chief could be given some discretion in the preliminary evaluation to determine whether a submitted paper seems to bring a "meaningful/suitable contribution." But independently of this, it is our individual contribution to the peer review process that is fundamental. As a community, if we all do our due share, we can make the peer review system work, or at the very least make it better.

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