Recommendations for reviewing papers in engineering-related research areas

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Abstract

All professional researchers are from time to time asked to carry out some reviewing. Although it is impossible from a practical point of view to achieve perfect reviewing, it is certainly mandatory for a responsible professional to try to do it in a good way. Good practices in peer-reviewing are of paramount importance for a proper operation of the research communities. In this paper an effort has been devoted to identify a series of criteria that can guide unexperienced reviewers to produce their first reviews in a way as objective and constructive as possible. Some parts of the paper are especially focused on the specific characteristics of engineering-related research areas. The paper discusses how important it is to have a constructive attitude, describes the main aspects to be analyzed when assessing the correctness and quality of a paper, discusses an important difference between the reviewing process of journal and conference papers, and finally makes some practical suggestions on the structure that a review report should have.

1. Introduction

Peer-reviewing is a controversial issue. Some critics dare to write statements such as "[it is a] non-validated charade whose processes generate results little better than does chance" [2]. This is perhaps an extreme view. However, it is true that peer review is not perfect. In fact, any researcher should be aware that just because a paper has passed peer-review, this does not mean that the paper cannot contain errors. But even so, research evaluation is absolutely indispensable for a proper operation of the society and peer-reviewing seems to be the best among possible systems to evaluate and filter out research results.

All professional researchers are from time to time asked to carry out some reviewing. Although, as mentioned above, it is impossible from a practical point of view to achieve *perfect* reviewing, it is certainly mandatory for a responsible professional to try to do it in a *good* way. For this, it is very relevant to be at least knowledgeable about the subject of the paper. It would then be wise to only accept invitations to review papers for which one has a previous background, although it is not always possible to find experts for each and every one of the papers submitted to a conference or journal.

There are subjective aspects in every task humans undertake. Therefore, the rest of this paper describes a series of recommendations for peer-reviewing that correspond to our personal point of view on what a good review is.

2. The *good* attitude

Reviewing somebody else's work is a serious responsibility. In the modern research career, one's publication record is the basis for any promotion. Judging must always be careful and respectful since it certainly impacts the author's life.

Respect is indeed the mandatory attitude for a reviewer. Any work of research deserves respect. Publications are not anonymous, so at least the authors indicated their names and affiliations and there is nothing more honest than taking responsibility in such a way, allowing everybody to see how good or how bad one's work is [1].

It is not the fault of the reviewed paper's author that the reviewer has a lot of other duties to perform. Reviewing is not an honor that one accepts because it allows one's name to appear with those of relevant researchers in public lists of Program Committees (PC) for conferences or Editorial Boards for journals. Reviewing is work and responsibility. If one thinks it would be impossible to do it properly due to some excessive work load, it is better to decline the invitation to review.

Taking advantage of the anonymity that always characterizes high-quality reviewing processes for letting off steam is surely one of the gravest errors that the reviewer can make. Not only is it rude and even despicable, but the reviewer can also be held accountable for it, since an increasing number of authors report unfair reviews.

Providing constructive criticisms should be the top priority for the reviewer. Furthermore, not all comments should be intended to point out incorrect aspects of the paper. A part of the review should be centered in providing suggestions for improvement. No paper can be perfect, and asking

for perfection as a requirement for acceptance is not only unrealistic, but also extremely unfair. It is also true that some papers are so weak or incorrect that they make it difficult to identify specific improvements. For those cases, it is perfectly fair to clearly state the bad quality of the paper, but always in a respectful manner.

3. The goal of the game

The main goal of a review is always to prevent wrong research to be published. By *wrong* research we mainly mean research that could, at least in principle, be proven to be factually incorrect at the time of review. A lot of the conference or the journal prestige is linked to the effectiveness of its reviewing processes for rejecting incorrect papers. In fact, some of these processes have been tested by persons submitting a dummy paper and, shamefully, not always said processes have succeeded in detecting the fraud [4].

However, note that wrong research is not only the one presenting erroneous results or conclusions, but also the one that does not fulfill originality or novelty requirements. Indeed, any published paper should report findings made by the authors and these findings need to be unpublished until the submission of the paper. Incremental research is not uncommon, thus it is quite normal that a significant part of a paper is actually a revision of previous work by the same authors. Most conferences and journals have no problem with that, although typically some journals may request that a certain percentage of the paper has to be new material. Measuring this percentage is always a complex issue.

Obviously, perfect detection of wrong papers is extremely difficult. In order to completely guarantee the correctness of a paper, it would be necessary to reproduce all the experiments, calculation, measurements, etc. that the paper could be reporting. Moreover, a thorough examination of previous work should be carried out to ensure originality and novelty. No practical review purports to do that. Therefore, the whole task becomes a best-effort one, and the amount of effort that each reviewer is willing to devote is very variable.

There are however some minimum tasks to carry out. First, it is necessary to carefully read the whole paper. This seems obvious, but more frequently than expected, reviews show that the reviewer did not truly understand or even read all sections of the paper! Second, it is necessary to perform some basic check on the references provided. This is not such a difficult task nowadays, since even if one cannot access the original papers through the official sites of the publishers, simple web searches using common engines are likely to provide us with copies of preliminary versions of those papers. It would be advisable to use the same search engines to see if some similar paper that is not cited pops up. In any case, a thorough and properly written (convincing) related work section helps to build trust in the correctness of the work.

It is of paramount importance to be able to identify the key contributions of the paper. When these are not clearly highlighted by the authors, it should be criticized. Once the contributions are identified, it is also important to find the motivation for them. The authors are thus expected to justify why these contributions were needed or how they help to improve the state-of-the-art in the area. Deciding how relevant the contributions of the paper are for the community is always subjective and such subjective criteria could do much harm by preventing relevant research from being published [3]. Thus, we prefer to base reviews, and especially final recommendations (e.g. acceptance or rejection), on more objective aspects.

Another fundamental aspect to review is the logical correctness of the reasonings presented. When flaws are detected, we can say that the paper is not convincing. A typical error in many papers is that the claims made by the authors at the beginning of the papers are later on not substantiated by the reasonings and evidences presented in the rest of the document. This should be always detected in a good review.

Writing should also be clear. This includes the structure of the paper (e.g. sections and subsections), which should help to communicate the main message in an effective manner. One usual mistake of many submissions is what is called *lack of focus*. Indeed, a good paper should almost always be focussed on one central idea or contribution. This idea should be clearly stated at the beginning of the paper and the rest of the document should be devoted to elaborate on that idea and its implications. Trying to cover many different ideas in the same paper tends to produce unclear and confusing documents.

Even if the writing is acceptable, a good review should always identify parts that are poorly written and thus should be improved or rewritten. These can be not only parts of the main text, but also parts of the figures, section titles, references, etc.

Failure to achieve correctness in any of these aspects represents a clear reason for recommending rejection of the paper. Practical experience shows that when a reviewer decides to recommend rejection due to some fundamental mistake in the paper, this should be indicated to the PC Chair in no ambiguous terms. In most conferences, the final score of the paper is calculated as the average of the scores proposed by different reviewers. If one of them has detected some relevant mistake that makes the paper unacceptable, it is advisable to mark the paper with the minimum score. This will not only seriously affect the average score, but it will also raise a flag on the serious concerns about the paper.

In some cases, reviewers are asked not only to reject incorrect papers but also those that are not *good enough*. When this happens, the chances of fair reviewing are severely reduced since subjectivity gains ground in the reviewer's

mind. Even if this kind of quality check is not explicitly requested, a certain amount of it should be part of any review. In our opinion, these aspects should not be used as grounds for rejection, but they should affect the score that the paper receives. One aspect that is worth to mention is the generality (i.e. applicability) of the contributions. From an academic point of view, general results that can be applied to multiple situations or systems always deserve a higher score. However, in engineering this could also mean that the result is far from an actual and real application, and application-oriented papers are considered very interesting in some conferences.

3.1 The industrial papers issue

Some conferences welcome papers reporting on industrial practices or industrial developments. It is very interesting for that kind of events to attract authors from industry in order to foster networking between companies and universities. How to review those papers is usually a complex issue. On the one hand, the reviewing standards should be the same for all papers. On the other hand, very few papers produced by company employees correspond to the quality criteria that are applied for universities. However, this does not mean that they do not deserve publication. They tend to be more practical and application-oriented, and thus very interesting for practitioners and also for academics interested in real-life aspects of engineering. Moreover, they are useful to point out lines of research that could have an industrial impact.

There is no final solution to this problem. Obviously, it is always possible to ask for guidelines to the PC Chair, but it is common to relax the reviewing standards for those papers. This should never imply to accept papers that present erroneous results, but it does mean to be more flexible in aspects such as novelty, relevance, number and generality of the results, and quality of the presentation.

3.2 Journal papers

There is not the same tradition in journal publication in most areas of engineering as there is in sciences. This makes reviewing journal papers an even more complex matter since there is no complete consensus on what are the differences between both kinds of publications.

It is commonly accepted that journal papers should be *better* than conference ones, but what this means is unclear. Some researchers invoke the archival value of journals to claim that a given journal should publish a submitted paper which is just a simple compilation of several conference papers. However, the fact is that most journals call for papers that report novel research, or at least that include a percentage of new material, as already indicated above. Some journals even ask prospective authors to submit the related previously-published conference papers together with the new article to facilitate the novelty check by the reviewers.

Journal papers should indeed be more complete than average conference ones. This means that they should be more self-contained and that they should provide a wider and deeper point of view on a subject. Ideally, they should state the last word on the issue they discuss. Obviously this is again a difficult quality to assess. It is a good hint to demand more results from a journal paper. If for a conference paper it could be acceptable to present a simple case study to validate the main ideas, in the case of a journal paper, it should provide a more complete set of evidences, experiments, figures, etc. that serve as proof of the paper's thesis. This is why it is not uncommon to consider that the amount of work needed for completing the results presented in a journal paper is equivalent to the one needed to complete a PhD thesis.

4. Performing a review

In order to carry out a good review it is important to choose a paper that corresponds to one's expertise. This is not always possible. For instance, if you are member of a conference's PC you are bound to review the papers assigned to you. Fortunately, an increasing number of conferences allow the PC members to bid on the papers they prefer to review.

Once the paper has been assigned, the first task to do is to carefully read the paper. It helps to take plenty of notes on any aspect that is found relevant, from the justification of the main assumptions to the typos that most papers include. These notes are the basis for the later review and they can already be written in a way that they will be later on easily classified in the different aspects to cover (contributions, motivation, novelty, originality, reasoning, substantiation of claims, related work report, adequacy and accuracy of the experiments, paper structure, writing, typos...).

Once all this material has been collected, then it is the time to start writing the report. Both conferences and journals provide their reviewers with templates for review reports. These templates usually include some fields in which a numeric score for some aspect of the paper (e.g. acceptance, relevance for the usual audience of the conference, novelty, scientific soundness, quality of the presentation, reviewer's confidence on her own opinion on the paper, etc.) needs to be indicated and some fields in which the comments for the authors and for the PC should be written.

Some reviewers feel that at this stage they are already prepared to score the papers in the different aspects. However, it could be more advisable to first write the comments in their final form before taking decisions on the numerical scores.

4.1 Journal vs. conference reviews

When performing a peer review, there is an important distinction in the peer-review process between journal and conference papers. Journal papers have several rounds of review. This means that authors can be asked to change aspects of their paper and these changes can subsequently be verified by the reviewers before the final acceptance. Conversely, conference papers have only one round. As a consequence, there is no chance for the reviewers to make sure that mandatory changes have been applied by the authors. In fact, once a conference paper has been accepted, it often happens that the authors do not apply changes requested by the reviewers. For this reason, if an important error is detected in a conference submission, this should be clearly indicated in the review report and the paper should be rejected.

4.2 Suggested structure for a review report

As indicated above, all review templates include a field for the comments to the authors. It is not always explicitly requested by the template, but it is a good practice to start these comments with a very brief summary of the main subject and contributions (and claims) presented in the paper.

After that, the issues found in the paper should follow. It is of course possible to comment on these issues in the same order as they are found in the paper. However, it is much better to sort them to present them from more relevant to less. The author will typically read these comments after reading the scores given to the paper, and it is probably annoying if the paper has been rejected and the first comments are irrelevant, as in the case of typos.

A possible structure for these comments could be the following:

1. Main concerns about the paper
2. Some minor concerns
3. Some suggestions for improvement
4. Some typos

It is a good practice to include explicit justifications of the values given to the different scores in the adequate sections among these. The aspects to be dealt with in each of these sections are the ones described in Section 3. A non-exhaustive list of the aspects to be considered is:

- Novelty
- Originality
- Clarity of the motivation
- Clarity of focus
- Relevance and generality of the contributions
- Correctness of the previous work description
- Technical soundness/Correctness of reasonings
- Substantiation of the initial claims
- Correctness of the presented experiments and results
- Quality of the presentation (structure, writing, etc.)

Many review templates include a field for confidential comments for the PC chair. This field should be used for that, and not for repeating the comments addressed to the authors. Some potential confidential comments could be about the detection of plagiarism or about some information you have that you cannot give to the authors without indirectly disclosing your identity. This field is also very appropriate to insist on some important matter such as that the paper has a extremely high quality and thus could be proposed for the best-paper award, or that it *must* be rejected because a fundamental flaw has been detected. However, if the latter is the case, it is very important (as indicated in Section 3) that the paper has been marked with the minimum score.

5. Conclusions

Good practices in peer-reviewing are of paramount importance for a proper operation of the research communities. Reviewing is more an art than an exact science since it is very influenced by the personal views of the reviewer. Nevertheless, in this paper an effort has been devoted to identify a series of criteria that can guide unexperienced reviewers to produce their first reviews in a way as objective and constructive as possible.

Although most of the content of this paper applies to any scientific area, some parts are especially focused on the specific characteristics of engineering-related research areas.

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