

How to review a scientific paper: some guidelines

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Paolo Robuffo Giordano

Senior CNRS Researcher

IRISA, Rennes, France

Some sources

- S. Hutchinson. “Surviving the Review Process”, IEEE Robotics and Automation Magazine, Vol. 17, No. 4, p. 101-104, 2010
- N. Lambert. “How to Review a Paper”. <https://www.natolambert.com/guides/how-to-review-a-paper>
- K. A. Nicholas and W. Gordon. “A Quick Guide to Writing a Solid Peer Review”, Eos, Vol. 92, No. 28, 233-240, 2011
- M. Black. “Novelty in Science. A guide for reviewers” <https://perceiving-systems.blog/en/news/novelty-in-science>
- S. Caron. “Reviewing a scientific paper” <https://scaron.info/blog/reviewing-a-scientific-paper.html>
- “Information for Reviewers”, IEEE Transactions on Robotics <https://www.ieee-ras.org/publications/t-ro/information-for-reviewers>

Why reviewing ?

1) For the research community (quality control)

- > is it worth publishing or not ?
- > fact checking, falsifiability, etc.
- > a (necessary) service to the community

2) To help the authors

- > constructive criticism: can the work be improved and how ?

3) For yourself

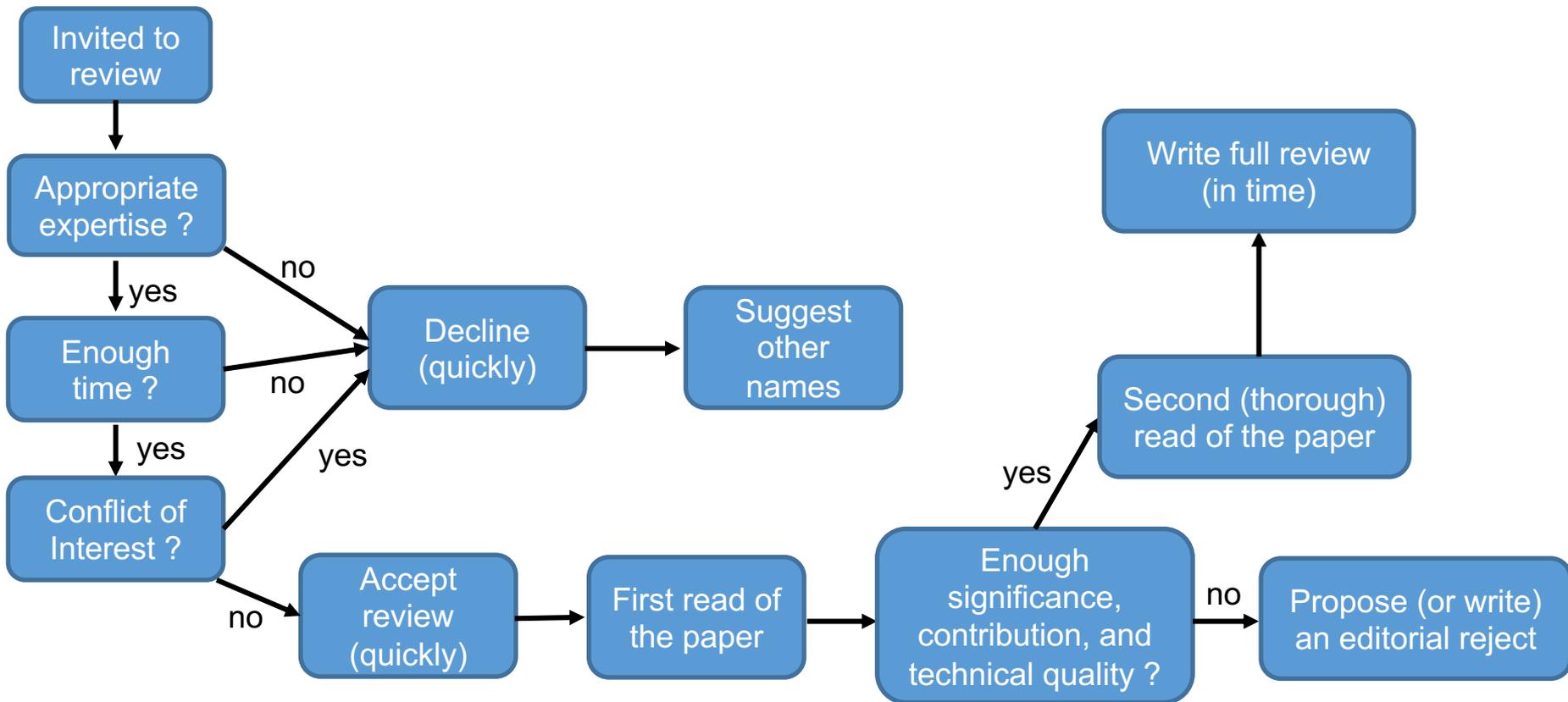
- > to learn more about the work of other researchers
- > to learn how your own work could be critically evaluated
- > to learn how to better present your work

4) For the society at large

- > contribute to advance the scientific knowledge and to establish the credibility of the scientific method and community

Not a perfect system but also better than alternatives (and you can **concretely help in making it better**)

Review flowchart



A Review Template

1. A brief summary of the paper, to convey your understanding of the paper to the authors
2. Overall comments that summarize your opinion but **do not include** your publication recommendation in the review text. (The **final decision** is taken by the **editorial board** and can differ from your recommendation)
3. A (possibly bulleted) list of more minor details, such as grammar or notation corrections, suggestions to improve figures, etc.

Some guidelines for the review

- What is the contribution of the paper ?
- Does the author explain the significance of this paper ?
- Is the paper clearly written and well organized ?
- Does the introduction state the purpose of the paper ?
- Are the references relevant and complete ? Supply missing references
- If the paper is not technically sound, why not ?
- If the paper is too long, how can it be shortened ?

Some Suggestions

- Review the **whole paper**, not just parts of it
 - > (classical) example: the reviewer mainly focuses on the **literature review** (that she/he criticizes) and doesn't comment about the rest
 - > In the revision, the authors will be led to believe that they just need to fix the literature review...
- Reviews are supposed to be **anonymous** -> Do not identify yourself !
 - > Especially relevant when suggesting related works. **Try to resist the urge** to have your own work cited/considered at all costs. When suggesting other related works remain balanced
- Reviews are supposed to be **respectful** -> don't be harsh, unrespectful, sarcastic, etc. You are talking to your peers who can (often) be **students** or **young researchers**
Some simple rules of thumb:
 - > Read and review the paper as if you were a close friend or colleague who was asked for feedback
 - > Would you be happy to receive your review, were you one of authors ?
- Try also to highlight the **positive points** of a work and/or to **encourage the authors** to pursue what could be a promising direction

Some Suggestions

- Try to be **specific/concrete/factual** in your comments
- Examples:
 - > The idea/algorithm has already been considered in many previous works. **Which works ? What parts of the idea/algorithm and why ?**
 - > The idea/algorithm should be compared against alternatives. **Which alternatives ? (and would the comparison be feasible ?)**
 - > The paper is too long and should be shortened. **Where and how ?**
 - > The technical content is not correct. **Where and why ?**
 - > etc

Some Suggestions

- A note on the issue of “comparison against the state-of-the-art”. Comparison is essential in science but it should be **reasonably feasible**
 - > Don’t ask the authors to compare against **too many different algorithms/datasets** (unless special cases)
 - > Is the algorithm implementation (that you propose to compare against) **publicly available** ? Or should the authors read the paper(s) and re-implement the algorithm(s) from scratch by themselves (which can quickly become unfeasible) ?
- Related point: you can recommend (or encourage) the authors to make their **algorithms implementation open source** (see, e.g., the the data repository [IEEE DataPort](#) and the executable code platform [Code Ocean](#))

Some Suggestions

- Don't let your judgment be affected by
 - > the authors' names, labs, affiliations... (in single-blind reviews)
 - > the **technical complexity** (generally speaking), e.g., a highly intimidating mathematical formalism, an overcomplex problem formulation, etc.
- Try to balance your judgment between **technical correctness** and **significance** for the robotics community
 - > as well explained in "S. Hutchinson. Surviving the Review Process", young reviewers usually excel in judging the **technical content** but may fail in assessing the **significance** or **novelty** of a work
- A paper can bring a contribution in many forms, e.g.,
 - > it formulates and proposes a method/algorithm that solves an open problem or improves over the existing algorithmic solutions
 - > it revisits a known problem from a different angle that brings important insights to the community
 - > it proposes a novel (and thorough) comparison or (experimental) validation that can be of interest for the community
 - > it describes a new (robotic) system that can bring an added value to other researchers in the field

Some Suggestions

- Related to the “novelty” consider the following points (see <https://perceiving-systems.blog/en/news/novelty-in-science>)
 - > **Novelty vs. difficulty** : something “new/interesting” doesn’t need to necessarily be difficult or complex. A good (and novel) idea may be a **simple one** that nobody has thought of before
 - > **Novelty vs. surprise**: a novel idea can quickly become “obvious” when one learns about it. But the novelty was in **having the idea in the first place...**
 - > **Novelty vs. usefulness**: “the idea is novel but I can’t see how anyone would need/use it”. **Be careful in using this argument**, since you may never know
- Still about novelty, be careful when reviewing an “**evolved paper**” (from previous conference versions)
 - > Carefully read the **journal policy** to get a feeling on what is expected for evolved papers
 - > Don’t blindly reject a paper because it shares content with a previous conference paper

Some Suggestions

- **Use the confidential comments** to the (Associate) Editor. Use these comments to share any opinion, doubt, piece of information that you may find relevant
- Some Examples:
 - > Inform about possible plagiarism (the Editorial Board will check)
 - > Inform about other works that should be considered (but which you are unwilling to list in your review)
 - > State concisely your candid opinion about the paper (which you may have smoothed in the review)

Common Questions

- Should I be fully confident in the paper topics in order to review it ?
 - > **It depends.** Example: you may not be an expert in the technical details, but you may know well the field (and thus judge the **impact/significance** of the work).
In general, if in doubt use the **Confidential Comments** to inform the Editorial Board
- The could be good but it is written in a **barely understandable English** (that the authors seem to consider as a “proper English”). What should I do ?
 - > If the paper is hardly understandable this is a **(major) flaw**: you can ask for a revision by pointing out the possible merits but by urging the authors to improve the English
- How much time should I invest in a review ?
 - > Again, it depends on many factors: your experience, the type of paper (conference, journal). For young, inexperienced reviewers, expect the reviewing task to be sometimes long (more than 1 day of work)
- Should we change our review standards/style based on the conference/journal the paper was submitted to ?
 - > In general yes, at least for the **conference vs. journal case**. A journal paper (in a major journal) clearly needs to meet a higher bar than a good conference paper.
In case of doubt, check the **journal/conference instructions** (or **discuss with the AE**)

Questions from the Audience

- How long does the review process typically take, and what factors might influence the duration ?
 - > **It depends.** For **large conferences** (such as ICRA), finalizing the program can be a long process. For **journals**, one common reason is the **reviewers' unreliability** (delayed reviews, unsubmitted reviews, etc)
- Could we use Large Language Model to train young people to write their reviews ?
 - > Given the advancements in this field, who knows ? Maybe it could be used as a verification tool...
- What didn't you think about, when reviewing for the first time ?
 - > Probably to consider the significance of the work besides the technical correctness
- In the case of interdisciplinary research, how do reviewers ensure that they possess the necessary expertise to adequately evaluate the paper ?
 - > It should be the (Associated) Editor's job to invite the right set of reviewers
- How do reviewers handle potential conflicts of interest, and what steps should be taken to maintain the integrity of the review process ?
 - > Every journal/conference has (or should have) clear policies on what constitutes a CoI. Read the policies and, in case of doubt, check with the Editor

Questions from the Audience

- What is the most tactful way of approaching a suspicion of plagiarism in a paper you are reviewing ?
 - > A good solution is to use the “Confidential Comments” and openly explain your concerns to the Editor. The board will then look into the issue and decide how to proceed

- Why does the overall process take so long? Notification of acceptance is much later than the review deadline...
 - > The process is, in general, not that long. It can range from 6 to 9 months (depending on the venue, conf/journal)
Always consider that the whole process is **voluntary work (for free)** and many people are involved (reviewers, associated editors, editors, the whole board). Every step needs some time, and the sum can grow large.
Also, again one main source of “delay” are most often the reviewers themselves....

Questions?

The Young Reviewers Program

For high-quality science