How to review a scientific paper: some guidelines

IEEE RAS YRP Online Event

Paolo Robuffo Giordano

Senior CNRS Researcher

IRISA, Rennes, France

IEEE Robotics & Automation Society

IEEE Advancing Technology for Humanity

Some sources

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- 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

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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

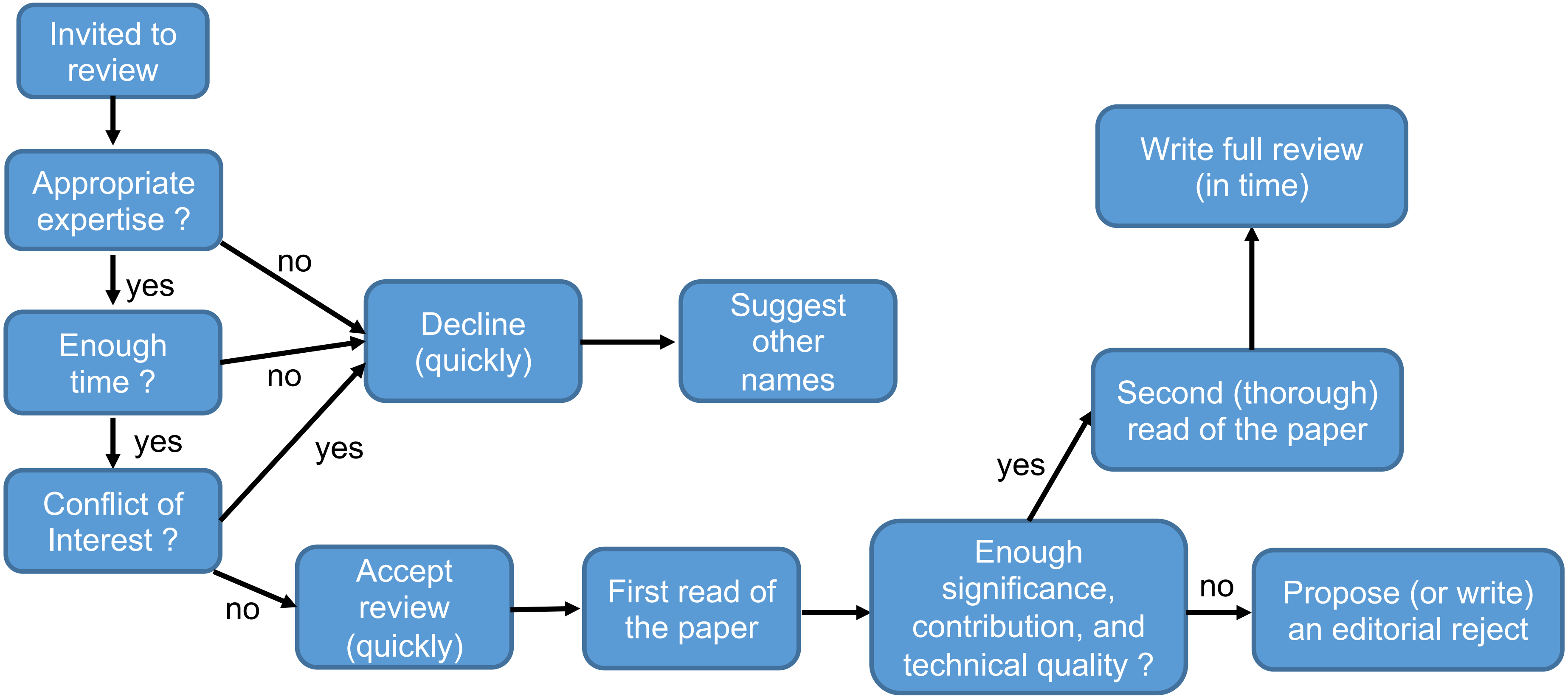
> to learn how to better present your work

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



Adapted from “A Quick Guide to Writing a Solid Peer Review”

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 ?

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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

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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 ….

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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)

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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

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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)

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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?

The Young Reviewers Program

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