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Commentary

How to Review a Paper

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ABSTRACT: Peer-reviewed papers remain a source of high-quality reliable scientific information, especially valuable in the modern world. To help those who are starting to participate in peer-review, I discuss a number of questions related to this process. When should you say yes to an invitation to review? How do you write a report that is appreciated by the editors? How do you provide feedback to the authors that is fair, constructive, and helpful? What are the specific points that should be considered when analyzing the abstract, introduction, results, discussion, conclusion, and Supporting Information? How do you structure your report? How do you make the final recommendation? What ethical and diversity issues are involved?

The flow of information in the modern world continues to increase—instead of rock carvings on a cave wall, we now receive the news instantly and digitally from many sources from computer algorithms to the Internet trolls.

Although, in the ideal world, scientific research should provide the high quality factual information free of bias and errors, the modern mantra "publish or perish" applies pressure on scientists to publish more. There is an explosion of new "commercial" journals ready to publish nearly anything as long as authors pay. Hence, quality control becomes even more important.

If you are reading this journal, you probably know that the Reviews, Case Studies, Letters, and Research Articles that it publishes are "peer-reviewed". For scientists, "peer-reviewed" is an indication of quality, an important factor that gives published work credibility. While we cannot trust everything that we read in social media, peer-reviewed scientific publications remain a source of high-quality reliable information, especially valuable in the modern world.

WHAT IS PEER REVIEW? WHY ARE PAPERS PEER-REVIEWED?

The Oxford Dictionary defines "peer review" as evaluation of scientific, academic, or professional work by others working in the same field. In the ideal situation, this evaluation is timely, fair, impartial, and helpful as it guides the authors in improving their work based on the feedback from the experts.

It is clear how journals, authors, and society benefit from peer reviewing—this process provides quality assessment to the editors and useful critique with tips for improvement to the authors. All of that leads to a higher quality paper for the broad scientific community to benefit from. Furthermore, peer review serves as the first line of defense against bad science by providing impartial and critical prepublication analysis. It allows both scientists and the general public to rely on the published scientific work. Although, as with any other human enterprise, peer-review is imperfect, it provides an important tool to ensure the quality of published work and, if used correctly, increase its value in the process.

However, writing a peer review is also a time-consuming, unpaid, and mostly anonymous activity. Why then would you say "yes" to a request to review a paper?

I will try to answer this question below from my personal experience based on writing >800 reviews (https://publons.com/researcher/548548/igor-alabugin/). Although providing a thorough and helpful review takes time, I generally find this activity rewarding for several reasons.

First, it is an opportunity to learn new science before it is published. Exposure to new areas and ideas is important for staying up-to-date in your field and expanding your knowledge. I have to confess that this selfish reason is one of my motivations. Invitations to review give me a chance to think about interesting scientific problems outside of my own research. To me, the randomness of papers that land in my mailbox is a lottery of ideas that can expose my mind to very interesting and sometimes unexpected problems.

Second, I have benefited enormously from many insightful and helpful reviews of my own papers. It is fair to be asked to pay back, by providing honest evaluation and guidance, especially to the younger scientists.

If you are a younger scientist yourself, being invited to review for a journal is an acknowledgment of your standing in the profession and an opportunity to engage in your field. Reviewing activities can now be easily documented and used in promotion cases as an illustration of service to the broader scientific community. You can get credit for your time and service![1]

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Below, I provide tips and suggestions on how to review in a way that is efficient and helpful to both the editors and the authors.

BEFORE YOU ACCEPT A REVIEW REQUEST

Let me start by discussing what happens after you receive a review request from a reputable journal.

First, one does not *have* to say "yes". If you do not have time or the paper is clearly outside of your expertise, do not hesitate to say "no". Independent of your choice, make it quickly—this will be helpful to the editor who can find a replacement reviewer and to the authors who are waiting for the editorial decision. If you know someone who *can* provide an expert evaluation of this manuscript, share their name with the editor.

If you are *not sure* if you are an expert, communicate with the editor. As very few people can be experts in *all* aspects of a multidisciplinary collaborative work, it is not unusual for editors to use multiple reviewers with different areas of expertise to get opinions on different components of the work. It is possible that your area of expertise is needed to evaluate a specific aspect of the paper.

Another reason to reject reviewing a manuscript is to avoid a conflict of interest, such as reviewing a manuscript authored by relatives, close friends, current collaborators, or colleagues at your company/university. On the other hand, it is not uncommon to review a manuscript by past co-workers or people you have met at a conference, as long as this relationship does not compromise your impartiality. You should declare any potential conflicts to the journal before accepting the invitation to review.

It is a good idea to check the journal's guidelines for reviewing—often they are included either in the review invitation or in the follow-up email from the journal.

ONCE YOU HAVE ACCEPTED

Keep in mind that by accepting the request to review a paper you also accept the responsibility to provide a helpful, fair, and timely evaluation. You should provide your review on time (or earlier). If you need more time, let the editor know in advance.

BEFORE YOU WRITE

I found that it is beneficial for me to read the manuscript at least 3−4 times. Initially, I give the paper a quick read-through just to get the main message. I carefully read the abstract, browse through the schemes, and make a few general notes. This process also gives me an idea of how much time I need to budget for this particular manuscript—it takes less time to review a short well-written communication than a long poorly constructed review. The second time, I read the full work carefully and look at the data, including the tables and Supporting Information (SI). I take notes, either on the hard copy of the manuscript or in the pdf file. Sometimes, I need to look for additional information and search for related papers, especially if I am not convinced that the reported results are new or if I do not agree with a suggested explanation. After reading (and sometimes rereading) the manuscript carefully, I come back to it for the last time when I am writing my referee report. I never do these readings on the same day—it ensures that my mind is fresh. It also gives me a chance to think about this work in the meantime.

EVALUATING THE MANUSCRIPT

When reading the manuscript I judge it for quality of science (technical accuracy, novelty, impact) and clarity of presentation.

Is It Good Science? First, do the results and methods make sense? This is the most important component of the paper! Do not take suggested structures and reported yields for granted—check the experimental procedures and spectral data in order to determine if the reported findings are real.

Think critically when evaluating the experimental data. Do the results make sense? Would one really expect a 95% yield on a three-step synthetic sequence with two chromatographic separations? Why would this reaction be so chemo- or regioselective? Would a pendant azido group survive reduction with $NaBH_4$?

After analyzing the data, I look at the authors' interpretations. Are the product structures correct? Is the suggested mechanism convincing? In certain situations (e.g., a short communication reporting an unprecedented reaction), it is okay for the reaction mechanism to remain tentative and even speculative. However, even a preliminary mechanistic hypothesis should be based on logic and precedents. If the general rules of chemistry do not apply, one has to point this out.

For example, what would you do if you see that the authors suggest that a van der Waals interaction can be shorter than a covalent bond?[2−6] Can bonds at sp-hybridized carbons easily bend to 120°? Is the octet rule optional, so oxygen can accommodate 9 electrons in a molecule that is "stable in water for weeks"?[7] Alert authors and editors about really unusual observations that suggest rewriting a general chemistry textbook.[8] But also do not be arrogant and assume that you are always right—chemistry is full of surprises!

Another useful strategy is, after getting the main premise of the paper, to set it aside and try to figure out what the main results may be. Make it a game. Can you predict the nature of the products, reaction selectivities, substituent effects, etc.? If the paper describes a new reaction, would you suggest the same mechanism? The value of this approach is that it makes reviewing more interesting by making you "invested" in the process. It is a good feeling when everything makes sense. However, I personally like it more when some of my "predictions" are off because it means that I have just learned something new (and gained a healthy dose of humility). Sometimes, this process allows me to see interesting points that authors missed or took for granted.

Is It Well-Written and Logically Organized? Even good science will not have a full impact if it is not communicated well. One would expect the published work to be logically organized with a good flow among the introduction, results, discussion, and conclusion. A paper will be better understood and make a larger impact if it is illustrated with clear and informative schemes and figures. The large body of data should be organized in tables or, if trends are important, graphically. The work should include literature citations that fairly reflect the history of the field and give due credit to important contributions. On the other hand, minor grammatical blemishes can be checked and corrected later, as long as they do not interfere with the understanding of the discussion.

Think about the following questions. Do specific parts of the manuscript serve their purpose? Does the abstract accurately represent the paper? Is the introduction helpful or just a sales

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pitch? Are the results presented clearly and accurately? Is the discussion helpful in connecting this work to the broader picture and integrating it within current knowledge? Does the conclusion clearly summarize the findings and highlight the new deeper understanding of the research problem that has, hopefully, emerged from this work?

More specifically, I try to evaluate parts of the manuscript from the following points.

Abstract: An informative abstract plays an important role in guiding the reader. When evaluating it, consider if it accurately describes the main points of the article. It should not include vague, inaccurate, or misleading statements. However, it should include brief answers to the four following questions that mirror the four main parts of the paper.

Why? What is the goal of this paper? What are the research questions?

How? It should outline the approach to answering these questions.

What was done? Briefly summarize the key activities within the main approach

What was found? Provide conclusions addressing the original goal.

Introduction: A good introduction should identify a problem that is sufficiently important for the reader to care. It should present the main topic of the paper, explain why this topic is important, and outline a specific plan to address this topic. Surprisingly, many introductions do not adequately cover these three points.

Specifically, point out if the introduction is too general and does not establish a logical connection from existing knowledge to the specific research questions of the present work. Additionally, mention if the introduction fails to provide a proper context by omitting any important background studies, especially those that might contradict the main premise of the paper or compromise its perceived novelty. A thorough reviewer would check for the relevant recent publications.

Use your judgment to evaluate if authors did not overstate the importance of their work. It is better to approach the claims of importance critically, especially if they are not directly related to the topic of the paper. In particular, I do not find blank statements that "this class of compounds has biological activity" convincing. One can argue that "each organic molecule is biologically active",[9] so a specific example for a closely related compound especially with a well-defined mechanism of biological action is more valuable.

Finally, does the introduction finish with a well-defined goal for the paper? What is the plan of action? Is there a clear purpose to this work and is it outlined in a research hypothesis?

Results: This is the key part of the paper that needs be analyzed especially thoroughly. This is where your specific expertise is likely to be the most important. Do not be distracted by any big claims from the authors—what was really found? Are the results presented clearly and accurately? Are scope and limitations tested and reported?

While checking for accuracy, make notes about clarity. Are the figures and tables used properly to illustrate, organize, and clarify the findings? Are figures and tables integrated in the manuscript and discussed clearly without simply repeating their contents? Are figure captions clear and descriptive? Are axes in plots and columns/rows in tables clearly labeled? If any of the above is missing, mention it in your review. Be specific in describing where the problems are.

Also, is there excessive or less important data that should be relegated to the Supporting Information section? Even if the journal is 100% online, excessive information can lose a reader. In the electronic version, the SI is readily available to those who need more details.

Discussion: Discussion is the place where authors have a chance to connect their results with the broader conceptual picture. It is appropriate to reemphasize the key points and compare them with the related examples. However, the discussion should not simply restate the results without interpreting them critically.

When evaluating the discussion ask the following questions. Did authors interpret their results correctly? Did they miss any important points? Also note if authors overreach: exaggerated claims of the importance as well as speculations not supported by evidence should be avoided.

Conclusions: The conclusions should reflect the manuscript's content but be more than just rephrasing the discussion.

Are the conclusions accurate? Are they redundant and simply repeat the discussion? Or do the conclusions provide cumulative analysis that transcends the specific results without going too far into speculations? Is there a "take-home message" that can be used as a foundation for the future studies?

Supporting Information: More and more important data are now moved to this supplementary part of the manuscript. It is important to check it. In papers that report synthetic procedures, I check them for clarity and attention to detail.

For example, it is not enough to say that "50 mL of reagent X was added"—was it added in one portion or dropwise? How long did it take "to thaw the reaction mixture to room temperature"? What volumes of solvent were used for extraction or crystallization? Was a flask (or a Schlenk tube) kept closed after the introduction of all reagents, or did it remain open under slight positive pressure on an Ar line? Such missing details can ruin someone's day in the lab or even career. If you find the last statement hard to believe, I strongly recommend you read a detective story (disguised as a mini-review) about a "lost" cyclopropanation reaction and its subsequent rediscovery by Chen and co-workers.[10]

It is unacceptable when the authors say "we have used a modified procedure from paper X" without clearly describing their specific modifications.

It is also important to check spectral data—do they agree with the authors assigned structures? Do they show a significant amount of impurities? Or, on the contrary, do they look suspiciously clean?

One of common problems in this section is the incorrect use of significant figures in describing numbers. For example, computational papers often report results with meaninglessly high precision—there no need to report energies with the 0.01 kcal/mol precision if the computational method used by the authors can, at best, provide a 1 kcal/mol accuracy. For papers reporting experimental NMR data, the ACS recommends giving ^{13}C NMR chemical shifts to one digit after the decimal point, unless an additional digit will help distinguish overlapping peaks.

Finally, potential safety issues should be properly identified and documented.

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WRITE YOUR REVIEW

Summary: Start with a short summary. Describe the main ideas of the manuscript for the field, how this work fits in the overall context of this field, and why it is important. The "executive summary" is useful to everyone involved—the editor will get a general overview of the paper, the authors will be able to see if you have understood their work, and you can use it to organize your thoughts about the paper.

The importance of such a summary is illustrated by the following quote from an ACS journal editor: "the best referee report is *not* a rubber-stamp 'Publish as is' but is instead a thoughtful synopsis of the major points, strengths, and weaknesses of the manuscript reviewed".[11] In fact, editors often do not find the rubber-stamp reviews helpful. Keep in mind that they can rescind an unhelpful report and invite another reviewer.

Even if you were lucky enough to review a perfect paper that does not need any corrections, still summarize why this work is so good. This information will be useful to the editor in case the other reviewers have a different opinion.

The summary should include your general assessment of both the quality of science and how well the science is communicated. I also comment whether I think this work is important or not and how it compares to the state of the art in the field. I understand that my judgment is subjective—I leave it to the editor to agree or disagree with my assessment in the context of the full set of the referee reports. Tastes and visions can differ—it is hard to be always right, especially when judging novelty and impact.

Science: If you are not an expert in some of methods and techniques used in a manuscript (which is not unusual for multidisciplinary collaborative work), state it clearly. For example: "Overall, the results are properly obtained and clearly presented. However, I am unable to comment on the biochemical assays used for the detection of antigens specific for SARS-CoV-2." The editor would know where to seek for an additional opinion.

If you have enough experience, you can also offer your opinion on whether this work is appropriate for the journal, but I often leave it up to the editor.

Presentation: Did you find the paper interesting, stimulating, informative, or thought-provoking? Or was it hard to understand, confusing, overloaded with superfluous information, or missing the key points and literature precedents? When discussing it, keep in mind that your review is your personal opinion—what you find interesting, others may not (and vice versa).

Specific Critique: Next, I start discussing specific points. Often, reviews would follow a "chronological order" by starting with page 1 of the manuscript and continuing to the end. Such organization is fine, but if the review is long, I often separate it into three parts: conceptual, major, and minor.

Conceptual—those are the most important points that address the very heart of the matter.

Are the results reliable? Do data fully support the author's claims? Can the conclusions be trusted? Are there any unanswered questions? Are there any "fatal flaws" that make the manuscript unsuitable for publication?

Make clear statements and provide specific evidence to support your points. For example, if you think that the work is not novel, provide an earlier example that you think is relevant. Avoid general statements and combine your constructive criticisms with specific suggestions for the improvement. It is less helpful to the authors if you say that the "discussion is poor". It is better to say that the "discussion of substrate selectivity is insufficient". It is even better to be specific, for example: "the discussion can be improved if the selectivity profiles are organized according to the substrate type and the origin of selectivity is outlined".

If you think that the work is incomplete, you can ask for additional experiments. This can be necessary when the key question is in doubt, an important chemical structure is not properly characterized, or a possible safety issue is apparent. However, be reasonable in your requests. Keep in mind that making even a few new examples can mean thousands of dollars in additional research expenses and several months of work. Will the additional work add significant new knowledge or simply waste resources and prevent a student from graduating on time? Could the additional experiments be published in the future work?

Major—identify contradictions, gaps, inconsistencies, and mistakes. Here, I also point to the vague or ambiguous parts that can interfere with understanding the paper. Try to offer suggestions and resources for improvement.

To the major points, I often add the lack of awareness for the already published work. Make your own judgment as to whether what the authors claim to be "new" is really new. Unfortunately, one cannot always rely on authors in identifying and properly discussing the novelty of their work. I generally do a quick literature search—it is especially important for quickly developing fields. It is also important to give credit to the early original research instead of simply citing the most recent review.

Minor—identify typos, grammar, and things that are unclear but do not compromise the main message. Occasionally, one can spend a lot of time on correcting style and typos. Keep in mind that the editorial staff can correct minor grammatical issues. I do, however, try to identify and list all typos in schemes—those are not as easy to catch.

Even if you find that poor grammar is distracting, try to look beyond language. Keep in mind that not every scientist in the world is a native English speaker. However, if problems are so bad that the paper is hard to understand, it may be fair to return the paper to the authors. It would be an unreasonable burden for you to rewrite the manuscript for the authors' benefit.

Be critical but do not make your review personal. Avoid discouraging and disparaging remarks. Instead, be constructive and concentrate on the facts and on the scientific content of the article. Treat others as you would like to be treated yourself. Do not let the bad reviews you had in the past to color your own reviewing. Be impartial and fair.

A FEW GENERAL POINTS ABOUT WRITING YOUR REVIEW

Once more, be specific in your critique. Instead of saying "this manuscript is a mess", tell what can be done to make it better. For example, you can say that the manuscript would benefit "if the main concept is clearly outlined and the goals of the work were stated in the introduction" or "if the origin of selectivity was discussed critically by considering the available kinetic data", "the results for the catalyst optimization studies would be easier to understand if they were presented in a table", etc. Instead of saying the "discussion is hard to follow", say "the discussion section can be improved if the data are organized

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according to the substrate type" or "the kinetic data would be easier to follow if the results are presented graphically as a function of time and concentration".

Even when the paper has flaws, be kind. Remember that it is hard to do science—students are learning, PIs are under stress, funding pressures mount, etc. Be respectful and courteous. Before sending a very critical review, I often let it sit for a day on my desk and come back to make sure that my critique is not based on me having a bad day.

Even if you and the authors have a different interpretation of their findings, this is not always a reason to prevent or even delay publication. If the results are valid and the author's interpretation cannot be ruled out by the available data, their interpretation can still be published. There is a value in new data - especially in experimental science. And it is possible that the community will be stimulated by the discussion.

FINAL RECOMMENDATIONS

Typically, one of the following recommendations is made.

"Publish as-is"—no changes. The white whale. Rarely but happens. Still, summarize the key findings and tell why the paper is great to the editor. Do not simply write "it's a great paper—publish 'as is'".

"Minor revisions"—these revisions include matters of style and grammar, reformatting data, clarifying statements, and providing more details for experiments.

"Major revisions"—rerun experiments, perform additional experiments, extensive restructuring and rewriting the manuscript.

"Reject"—the flaws can be scientific or the work is not suitable for this journal (e.g., in terms of importance or scope).

ETHICAL AND DIVERSITY ISSUES

On rare occasions, you may suspect a possible breach of scientific conduct. When you see evidence of plagiarism or data manipulations, share your concerns with the journal editors. Keep in mind that such situations can have serious implications for the authors' careers. In such instances, you can use the option "Comments to the Editorial Office" or its analogue.

If you are reviewing controversial work with significant commercial, societal, or political implications, it is worth taking a look at the sources of funding. Be aware that conflicts of interest can also result from financial incentives. Although this is a rare situation for fundamental science, it is worth considering if authors may be under pressure from their funding bodies to produce a certain kind of results.

Be aware of the possible bias against non-native English speakers who may find it more difficult to communicate complex ideas in a foreign language than a native English speaker.

Finally, it is important to be cognizant of possible unconscious biases when making your judgment regarding the paper. Good science is gender-blind, age-blind, and color-blind.[12−16]

In summary, peer-review allows us to build a reliable foundation for future science. Not only it is an important service to society, but it can also be professionally rewarding. It can give you an opportunity to learn something new while helping your colleagues, protecting the quality of published work, and advancing your field.

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Notes

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