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What is Expected from a Reviewer?

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Most scientists are asked to review many papers every year. Many times, even experienced scientists are not totally aware of what is and what isn't a good review. His own analysis follows what he has received from others. Is there a guide to produce good and valuable paper reviews? This summary points out what is, and what is not expected by journal editors from a peer review.

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Why do we review papers? What motivates us or forces us to do this?

In fact, nothing forces us to do it. We review other authors' works hoping that these same authors will do the same for us. It is part of the ethical code of our profession. All professions have a series of rules that make them viable and ethical. Even rent killers have their rules, and with us it is not different. The only ones able to judge our work are our peers, and we are the only ones able to judge theirs. Science has advanced this way for centuries and the peer review process makes what is published reliable. Articles published without review would lack credibility as they would be nothing more than opinions on this or that subject. The endorsement of a reviewer beyond the publisher's endorsement gives support to the author and a guarantee to the reader that the work in front of his eyes followed the rules and procedures recommended by the scientific method. It also certifies that the results presented are coherent with those who already know about the subject of study.

It is an act of collegiality but obviously a courtesy and not an obligation. There are those who prefer not to give opinions nor review papers, but these are a bit self-centered since they

expect from others to do what they do not do themselves. A good reviewer is therefore held in the highest esteem by the editors. But after all, what is a good reviewer? What is expected of him?

Perhaps the most important and paramount thing expected of a reviewer is that he **responds to the invitation** that was made to him. The reviewer who fails to respond to the invitation ends up hindering the whole review process. This is because he is given a deadline to respond to the invitation that is respected by the editor. The non-answering reviewer forces the editor to choose a new reviewer after first invitation deadline, and time-count starts over again. It is normal that an invited reviewer has no time, knowledge, or willingness to review this or that article. Other times, the reviewer has potential conflict of interest, that could bias his review. In this case it is enough to refuse the invitation, but this refusal must be communicated to the editor as soon as possible. In this way the editor can find new reviewers in a timely manner without delaying the review process of the article. It is a lack of respect so much with the editor, who

considered the reviewer a skilled scientist to do the review, so much with the author of the work, that seems to be not interesting enough to deserve reviewer's attention. Besides this, it is contempt for the journal, that seems not to be prestigious enough for the reviewer to wish to donate a little of his time. There is no problem refusing, but please answer.

The second important thing the reviewer should do, before accepting to review a paper, is to **read the article thoroughly**, to be sure he is able to give a valuable opinion on the subject. We are often not 100% sure on certain issues or totally confident with the paper's subject. In this case our opinion can harm more than help. One must issue **opinions only on matters he is familiar with**. At this stage the reviewer can identify any conflict of interest that could potentially bias his report too. Normally the editors in charge avoid inviting concurrent researchers to evaluate their rival's work, but it can happen occasionally, and the reviewer should decline the invitation.

If he feels confident, the reviewer can agree to review it, and hopefully, he will submit his review **as soon as he can**. It is then important to look after similar papers on the web and to **verify what already exists** on the subject, and what the authors have already published in the past. This step is important to evaluate paper's novelty. Furthermore, it is not rare (believe me) to find cases of **double submission**, or cases known as **salami publication**, in which the authors divide the same piece of research into several smaller papers. Both procedures are considered unethical and reprehensible. In case the reviewer finds anything in this sense, or something that configures **plagiarism** or even self-plagiarism, he or she must communicate the fact to the editor alongside appropriate evidence. This is much more frequent than most of us imagine, especially in submissions to less prestigious journals. The reviewer is not intended to be a kind of science detective, but he is the one who usually finds suspicious material during the reviewing process. The editors will know how to deal with such situations.

It is important to **take notes** during this careful analysis, especially about the **unclear points** and **errors**. Errors here mean wrong deductions, uses of wrong models and theories. Experimental methodology must be adequate for the kind of problem under study. Many times, missing steps, obsolete or unsound methodology will lead to mistaken conclusions. The method should be described in a sufficient level of detail to allow the experiments or procedures to be reproduced. Robust data analysis, and methodology is a good indication that the results are trustable. All trends, data analyses, and statistical patterns should involve enough and significant data points. All **conclusions must be supported** by coherent results and harmonious evidence. The conclusions of most papers agree with the literature, but if they don't, the referee should be open-minded, even if the approach or the model used in the paper is not his favorite. Science progresses with disruptive discoveries, and yes, these ones may be present in the paper just in front of you. The reviewer can make a **short summary of the manuscript** to ensure he understood the problem and the proposed solution. He should annotate paper's main qualities and its flaws. At this point the referee will be able to judge the **novelty and the originality** of the results. Good papers are always original, and they always bring novelty.

When **references are too old** in general (more than 10 years) or when there are few references the paper may not be in accordance with the **latest discoveries in its field**. Outdated references are many times far behind the state of the art. This can be dangerous even for the author, that may think he did discoveries that are in fact re-discoveries. A good bibliography

should bring the classical papers and reviews, but also, the most up-to-date publications. When it is not the case, the reviewer may **suggest some references**, that would contribute to attack the problem (He should **avoid asking to cite his own papers or those of his associates**, it is quite unethical and could be understood as a manner to increase his own citations).

The main issue for non-native English speakers is the **language**. Writing in English is not easy, and the wrong form may compromise the meaning. If the reviewer feels comfortable, he or she can suggest some improvement in this sense, but not in the form of detailed and extensive spelling and grammar check. Normally, a suggestion to the editor to have the manuscript language edited should suffice.

Figures, tables, and illustrations should be sufficient. Not too many nor too few, just the essential, and enough to illustrate what is said in the paper and to support the author's conclusions. Those figures or tables that serve to confirm obviosities, or what is already known, should be removed, or placed in the electronic supplementary information section. The quality of the figures is important too. Clumsy or polluted figures, and too much white space may cause complaints. The graphical abstract should be adequate, clear, and self-explaining.

Eye-catching, concise, and precise **titles** are best. They should communicate the whole idea in a few words. The authors should avoid colons in the title, or the words *novel*, *new*, *unprecedented*, and so on. Serious reviewers may be inclined to think the authors are over-estimating their results by using these expressions.

The **length** and, above all, the quality of the **abstract** should be verified. Remember that an abstract is intended to summarize the paper, it should contain the work's context, a brief description of the way the problem was addressed, and the main results or highlights. Good keywords are also important, as well as an eloquent **graphical abstract** if the journal asks for one. The length of the whole paper is important too. The shorter, the better. All unessential parts (for understanding) must be suppressed or moved to E.S.I. section (electronic supplementary information, that must be reviewed too!).

It is obvious that any unpublished manuscript is a privileged communication, and it is authors' property. It is, then, private, confidential and secrecy violation will harm the authors. The reviewer should **never use the information he read before its publication**. The material should not be shared by any means, nor used, unless they have the permission to do so.

Finally, the **reviewer's report** should be **straightforward, clear, and polite**. It should present the title of the manuscript, and the short summary showing that the reviewer got the idea the way it is. The reviewer must remember that it is not the case to criticize the authors, but their work! The referee must find flaws in the paper, but he has not to "teach" the authors about their profession. If a mistake is discovered, it should be demonstrated. The reviewer should avoid generalizations and be objective, **pointing out specific errors** one-by-one. After listing the issues (major or minor) the reviewer will estimate if it worth to get it published. If it is original enough, if it brings important results, if it will contribute to its research field, and principally, if research was conducted in conformity.

At the end, the reviewer will make a **recommendation**, keeping in mind that it is not a decision. The decision is taken by the editor based on the recommendations and his own opinion. It is not taken by the reviewer. The paper should be

rejected when it is too bad, irrelevant, or totally out of context. When it happens, the reviewer must explain his recommendation and, if it is the case, indicate a more appropriate journal for the manuscript. The paper may need only **minor revisions** like correcting references or some sentences, providing more accurate explanations, including more results, shortening the paper, or correcting typo mistakes. It can sometimes need **major revisions**, when the referees are not totally convinced, and they ask for new experiments or even to rewrite the paper totally. Immediate **acceptance** is very rare, and most accepted papers must make minor corrections.

Some journals may give **specific instructions** to the reviewers, but it is rather rare for chemistry journals, except for crystallography data presentation. These journals can also ask you to **score** some critical items like originality, language, etc. to rank the submissions.

Once the referee has reached his decision, he can **submit his review**, and if a second review round is needed, he may be asked to analyze the manuscript once again.

In summary, **making** good, accurate, and fast **reviews** is a **learnable skill** as any other. Many young researchers, especially non-native English speakers, may feel uncomfortable with this task, but who knows if after reading these short lines and the very good papers cited in the bibliography¹⁻⁵, the process might not become less complicated and challenging?

References and Notes

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