The Task of the Referee[*]

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Abstract

The task of the referee is to evaluate in a timely manner a paper for publication in a specific journal or conference proceedings. This involves determining if the work presented is correct, if the problem studied and the results obtained are new and significant, if the quality of the presentation is satisfactory or can be made so, and what revisions and changes to the paper are necessary and/or desirable. The evaluation must be with regard to the coverage and degree of selectivity of the specific publication.

In this article, we discuss the problem of how to evaluate a (research) paper for publication, and by inference, how to write one. The primary question which is addressed is that of determining whether the paper should be published, and if so, what changes and improvements are needed. The role of the editor, and rules and procedures used by most computer science journals are discussed. Brief discussions of refereeing proposals and survey and tutorial papers are also given.

1. Introduction

There is a constant stream of papers written and submitted for publication to conferences, journals, newsletters, anthologies, annuals, trade journals and newspapers, and other periodicals. Many such publications use referees as impartial, external experts to evaluate papers. This approach is often called *peer review*. Refereeing is a public service, one of the professional obligations of a computer science and engineering professional. Typically, referees learn to produce referee reports without any formal instruction: by practice, by feedback from editors, by seeing referee reports for their own papers, and by reading referee reports written by others.

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In this article, we provide guidance to referees on how to evaluate a paper, how to write a referee report, and how to apply commonly used standards and procedures. It is intended to replace the use of [Fors65], distributed by some editors, which does not reflect the procedures used in Computer Science and Engineering. Another paper, similar to this, considers refereeing in the theory area [Parb89]. The focus of this article is the evaluation of research papers in applied areas of computer science and engineering, such as systems, architecture, hardware, communications, and performance evaluation, but most of the discussion is generally applicable. Some discussion is provided as well on refereeing research proposals and survey and tutorial papers. In addition, authors may find this material useful for preparing papers and submitting them for publication.

2. What is a Publishable Paper?

This entire article is intended to address the question of what is a publishable paper; in this section, we provide some brief comments.

A paper is publishable if it makes a *sufficient contribution*. A contribution can be new and interesting research results, a new and insightful synthesis of existing results, a useful survey of or tutorial on a field, or a combination of those types. In some cases, a paper which merely confirms previously published results (by a different researcher, using different data) may be publishable, if the previously published results were important enough to require confirmation. (Such papers offering confirmation are routine in biology and medecine, but are rare in computer science.) To quote a referee for this article itself: ''small results which are surprising and might spark new research should be published; papers which are mostly repetitions of other papers should not; papers which have good ideas badly expressed should not be published but the authors should be encouraged to rewrite them in a better, more comprehensible fashion.''

The role of the referee is to provide an *opinion* as to whether the paper makes such a sufficient contri-

bution. There is seldom a single correct evaluation of a paper, and equally skilled and unbiased readers will differ.

3. The Task of the Referee

The two major components of a referee report are:

(a) A recommendation for or against publication in a specific publication or presentation at a specific forum. An equivocal recommendation is acceptable if adequate discussion is provided for the guidance of the editor or program committee. If rejection is recommended, and if the paper does contain some publishable research, the report can suggest another place to publish. In all cases, sufficient discussion *must* be provided to justify the recommendation.

(b) A list of necessary and recommended changes and revisions. A recommendation to reject the paper does not excuse the referee from suggesting changes that might permit the paper to be published elsewhere, or after resubmission. The extent of necessary revisions, for journal publication, is largely separate from the recommendation for (eventual) publication; for a conference, the short time available for revisions, and the difficulty of arranging for a second (or n'th) round of revisions generally means that a paper which requires substantial revision cannot be accepted.

It is very important that the referee walk the uncertain line between being overly permissive ("publish everything") and overly restrictive ("nothing is good enough to publish"). If the referee is insufficiently critical, poor research is encouraged, recognition (of a sort) and honors (of a sort) are given to those who don't deserve it, the naive and inexperienced reader is misled, the author is misled as to what is publishable, disrespect for the field is encouraged, commercial development is distorted, as are hiring, promotion and tenure decisions, and the paper may actually subtract from the general store of knowledge; consider the Piltdown man fraud. As has been noted in [Thom84] and elsewhere, one of the worst problems with unrestrained publication is to bury the professional under mounds of paper, only a very small fraction of which can be examined, let alone read.

If the referee is overly critical of research, he blocks good research from publication, or causes it to be delayed in publication, wastes the time of authors, damages careers, and perhaps leaves journals with nothing to publish and conferences with nothing to present. It is particularly important not to reject new

and significant work which runs counter to the prevailing wisdom or current fashions.

It is important for a referee who wants to be taken seriously to have a middle of the road view, to be able to distinguish good from bad work, and major from minor from negative contributions to the literature. A referee who always says "yes" or always says "no" is not helpful.

4. How to Read a Paper for Refereeing

Reading a paper for the purpose of refereeing is closer to what a teacher or professor does in grading a paper than what a scientist or engineer does in reading a published paper. In the latter case, the presumption is that the paper has been previously checked (refereed), and is thus correct, novel and worthwhile. The referee, conversely, has to read the paper carefully, checking and evaluating the material. No presumption should be made as to the quality or accuracy of the paper; it should be read with an open mind. The result of such a reading should be a referee report usable by an editor or program chair.

Note that refereeing a paper can require considerable time and effort; that effort should not be wasted on a detailed critique of a badly flawed paper which can never be made publishable. Finding one or more fatal and uncorrectable flaws in a paper excuses the referee from checking all of the subsequent details.

5. The Referee Report

A good referee report should have several parts. First, the referee should state very briefly his recommendation and the reasons for it. Second, the referee should summarize the point of the paper in 1-5 sentences, both for the use of the editor, and to ensure that the referee actually understand the point of the paper. Third, the referee should evaluate the goal of the work both with respect to its validity and to its significance. Fourth, the referee should evaluate the quality of the work (methodology, techniques, accuracy, errors, presentation), and finally, the referee must provide an overall recommendation as to publication. If the recommendation is negative, the referee should always be clear about why he is recommending rejection. The referee should also be clear about the strength of his opinions; an equivocal ("maybe") recommendation is acceptable if the reasons for it are clearly documented. In any case, the referee report must contain enough discussion and information to justify the recommendation.

If the recommendation is favorable, it is essential that the referee provide as long a list as appropri-

ate of both necessary and suggested changes. If the recommendation is negative, but the paper can be salvaged and either submitted elsewhere for publication or resubmitted to this publication, then a similar (but perhaps less detailed) list should also be provided. Suggestions for alternate places to publish are always welcome.

Typically, the text of the referee report is given to the author, stripped of all surrounding material identifying the referee. Thus, while is important that the referee report be clear and explicit, it should not be insulting. Words such as "fool" and "idiot" should not be used to refer to the author, nor terms such as "trash" for the paper. A review of a paper should be directed at the paper, and not be a personal attack on the author. The review of a proposal, though, is also a review of the investigator, and it is appropriate to evaluate his research abilities as well as the research proposed; in no case, however, should the evaluation be other than objective and fair. The more a review can be made psychologically acceptable to the author, the more useful it will be.

The referee must make sure that his report reaches the editor in a timely manner. Computer science journals are notorious for having long delays between submission and publication; the two major components of that delay are the referees and the publication queue for the journal itself. Imagine if it were your paper! In a conference setting, referee reports must reach the program chair well before the program committee meeting so that the material can be assembled and prepared for discussion.

6. Issues in Evaluating a Research Paper

The referee is responsible for evaluating the novelty, significance, correctness and readability of a paper. This general set of goals can be broken down into a much more specific series of questions to be applied to the paper, as we discuss in this section.

What is the Purpose of the Paper?

What is the problem being considered? Is it clearly stated? Does the author make clear what the important issues are? Does the author tell you, early in the paper, what he has accomplished? For example, if this is a system description, has the system been implemented or is this just a design?

Is This Paper Appropriate?

Is this work appropriate for this forum? One does not submit queueing theory papers to *EE Times*, or market analyses of the latest release of Windows to *JACM* or *Proceedings of the IEEE*. Does this paper

have anything to do with computer science or engineering?

Is the goal of this paper significant?

For that matter, is the problem real? Does it contradict any known physical laws (for example perpetual motion machines) or widely reported measurements?

Keep in mind what the Walrus said [Caro65]:

> "'The time has come,'
> the Walrus said,
> 'To talk of many things:
> Of shoes - and ships - and sealing wax -
> Of cabbages - and kings -
> And why the sea is boiling hot -
> And whether pigs have wings'"

Is this a careful analysis of how the sea got to be boiling hot, or an elegant study of the flight characteristics of pigs?

Is there any reason to care about the results of this paper, assuming for the moment that they are correct? Is the problem or goal major, minor, trivial or non-existent? Is the problem now obsolete, such as reliability studies for vacuum tube mainframe computers? Is the problem so specific or so applied as to have no general applicability and thus not be worth wide publication?

Is the problem, goal, or intended result *new*? Has it been built before? Has it been solved before? Is this a *trivial* variation on or extension of previous results? Is the author aware of related and previous work, both recent and old? Does he *cite* that work? Are distinctions between this and previous work given and are they specific? If this work describes an implementation, are there any new ideas? If this work merely confirms previously published results, is the confirmation significant and necessary?

Is the method of approach valid?

Is there something about the approach to this problem that invalidates the results? Can you tell what the method is, or do you have to ferret it out from the middle of the mathematical formulas? What are the assumptions? How realistic are they? If they aren't realistic, does it matter? How sensitive are the results to the assumptions?

Is the approach used sufficient for the purpose? For example, data is available, but the author has used a random number driven simulation with unrealistic parameters. Does it matter? Back of the envelope calculations are often sufficient.

If this is a presentation of a new *idea* - for example, a new design, or a new concept, is the *appropriate* amount of discussion or analysis presented? There should be neither too much nor too little. Published archival papers are traditionally terse and complete but not cryptic; extensive and detailed discussions, along with voluminous supporting data, are better published as a technical report.

Is the actual execution of the research correct?

Are the mathematics correct? One or more referees should check the mathematics in detail; a referee should always tell the editor if he didn't read or check some part of the paper. Are the proofs convincing? Are the statistics correct? Is the simulation methodology described in sufficient detail to convince the reader that the results are valid, and for stochastic simulations, are confidence intervals for the results given? Are the results consistent with the assumptions and/or with observed facts or measurements? Have boundary conditions been checked? Are the results plausible, or even possible? Did the author do what he appears to claim? For example, did he simulate the original system or a reasonable model of it, or did he simulate the approximate mathematical model of the system?

Are the correct conclusions being drawn from the results?

Are any conclusions being drawn from the results? What are the applications or implications of the results? Is there an adequate discussion of *why* these results were obtained?

Is the presentation satisfactory?

The first question is whether the paper is written well enough so that the technical contents may be evaluated. A paper which is incomprehensible is not publishable. A paper which requires extensive revision is not publishable in its present form, and may never be. Presuming that the paper is readable at all, an evaluation of the presentation is needed, in addition to the technical evaluation. The reader is referred to articles such as [Day77] for how to write a paper; in this section, we concentrate on how to evaluate the way it is written.

Does the abstract describe the paper? (The reader should be able to get the gist of the paper by reading the abstract. The abstract should not only describe the problem but also very briefly summarize the key results.) Does the introduction adequately explain the problem and the framework for the research? Are the remaining sections clear and do they follow in a logical order? Is there too much or too little detail? Are the grammar and syntax correct? Are the figures and tables well labeled? Are the figures legible? meaningful? Are there too many or too few tables and figures? Are explanations poor or even nonsense? Is the author too verbose or too terse and cryptic? Is the paper sufficiently self contained that someone knowledgeable in the field can read it, or does the reader need a detailed knowledge of results published elsewhere? If the author refers the reader to other papers for crucial details, do you believe him? If sections of this paper are missing or incomplete, due to a deadline, do you believe that they will be filled in as promised? Is the paper too colloquial or too formal in style? Is the formalism useful or necessary? Are there many typographical errors? Is the paper too long? If so, is that because it contains too much material, or because the author has been too verbose? That is, is there too much discussion, or too many tables and figures? Could the paper be split into two or more papers without losing coherence? The paper should be long enough to present the necessary material, and no longer. Within reason, let the editor or program chair worry about specific page limits.

The referee should identify, to as great an extent as possible, typographical errors and necessary corrections in grammar, punctuation, and wording. Such errors can often be a serious problem when the paper has been written by an author whose native language is not English. It is not the job of the referee, however, to rewrite the paper for the author.

What did you learn?

What did you, or what should the reader, learn from this paper? If you didn't learn anything, and/or if the intended reader won't learn anything, the paper is not publishable.

7. Overall, how good is it? What do you recommend?

Can you put the paper into one of these categories?

1. Major results - very significant. (fewer than 1% of all papers written.)

2. Good, solid, interesting work; a definite contribution. (fewer than 10% of the papers you will see.)

3. Minor, but positive, contribution to knowledge. (perhaps 10% to 30% of the papers submitted.)

4. Elegant and technically correct but useless. This category includes sophisticated analyses of flying pigs, as mentioned above.

5. Neither elegant nor useful, but not actually wrong.

6. Wrong and misleading.

7. The paper is so badly written that a technical evaluation is impossible.

In putting a paper into one of the above categories, it is important to normalize to an *appropriate* standard, not to your own standards, which may be high or low, or to the average of the papers that you yourself write, or to the average of the papers that you find worth reading.

After categorizing the paper, the question is: what are the standards of this journal or conference? Is this the *Proceedings of the IEEE* or the *ACM Transactions on Computer Systems* or the *ACM Symposium on Operating Systems Principles* (all quite selective) or the Tahiti Conference on Scuba Diving and Computer Systems? (Fictional, but a presumed boondoggle.) You should compare this paper not with the best or worst in that specific journal or conference, but with the average. Of course, in some cases the average is too low, and needs to be raised by critical refereeing. Note that you cannot determine the selectivity of a conference or journal by the percent of submitted papers which are accepted; the best conferences and journals have far fewer bad papers submitted.

You should then make a recommendation. It can be favorable ("publish") or unfavorable ("reject"). The strength of the recommendation should be clearly stated to the editor (e.g. "wonderful paper, definitely accept"; "useful paper, probably accept"; "marginal paper - see how many better ones have been submitted to the conference", "wrong and misleading; definitely reject"). It is permissible, but not desirable, to say "maybe"; if you feel that the paper has something worthwhile to say, but you're not sure if it is good enough to publish in this journal or conference proceedings, you can give an equivocal response.

Your recommendation is your opinion as to whether the paper makes a *sufficient contribution.* Generally, this will include those papers in categories one and two above, and some of those in category three.

You can also recommend that a paper be rejected for being inappropriate for this journal or conference. If the paper is inappropriate, or marginal in quality for this forum, but suitable elsewhere, you can also suggest someplace else to submit the paper.

In any case, you *must* provide sufficient discussion and justification for whatever recommendation you make. A recommendation which is without sufficient justification will be given very little weight by the editor or program committee.

If the author is asked to prepare a revised version of the paper for a journal, the revision will usually be sent to the same referees for further review. It is important to ensure that the revisions are satisfactory, but the referee should avoid comments inconsistent with the first review, and should avoid harassing the author by recommending revision after revision unnecessarily. It is quite possible, however, that there may be serious problems in a revised manuscript, either due to things overlooked in the first review, due to problems that have only become apparent after revision, and/or due to new errors introduced in the revision itself. Such serious problems must be addressed. Note that if there are still serious problems after the second revision, it will often be appropriate to recommend final rejection, as the author would appear to be incapable of fixing the problems.

8. Surveys and Tutorials

Surveys and tutorials are different from research papers. Most or all of the work reported in such papers is not new, and is not expected to be new. Such a paper, however, may be a convenient place for an author to include a variety of minor research results which would not stand on their own in separate papers.

Survey and tutorial articles are similar but not identical. A pure tutorial is intended to explain some body of material to the non-expert, usually novice, reader. The tutorial may not cover the entire field addressed, and may have a specific point of view. The survey should provide broad and thorough coverage of some field or body of knowledge, and may be aimed at a reader ranging from the novice to the almost-expert.

In reviewing a tutorial paper, there are some specific issues to address: Does the paper cover the material promised by the title or abstract? Is this a reasonable body of knowledge to be covered by a tutorial article? (Is the scope too wide, too narrow, or too bizarre to be useful?) Is there a consistent theme to the paper? Is the material in the article correct? Is the level of coverage excessively simple-minded or excessively sophisticated, given the likely audience? Is the paper well written and clear? This last is a crucial issue for tutorials, but journals that publish tutorials, such as *IEEE Computer* and *ACM Computing Surveys*, often have editors and a professional staff who help with revisions.

For a survey paper, many of the same questions apply. Does the paper cover the material promised by the title or abstract, and is this a reasonable body of knowledge to be surveyed at one time? Is the material in the article correct, and is the author sufficiently expert on the subject that he is able to correctly interpret results and provide perspective on the field? Has the author integrated the material in a consistent manner, or is this just an annotated bibliography? Has the author provided a balanced and thorough coverage of his topic? Does he cite all of the important relevant literature, or is the presentation biased, slanted and/or unevenly selective? Controversial opinions and evaluations should be identified as such. To the extent that the survey includes new research results, do those results meet the criteria given above for research papers for validity and correctness? A survey does not have to stand on its own as a research paper, and so the research presented does not have to be sufficiently significant as to justify publication as a research paper. Finally, is the paper well written and clear?

9. Proposals

A proposal is a request to a funding agency, company or foundation for financial support, supposedly to do the research described in the proposal. Reviewing proposals is quite different from reviewing papers, and some special considerations apply. Reviews of papers address only the science; reviews of proposals must consider the person as well.

The primary difficulty with reviewing a proposal is that the investigator is supposed to be telling you what he *plans* to do, in addition to what has been done. The questions to be asked, then are: (a) Is the topic (or topics) of research significant? (b) Is the method of approach described (briefly) and is it reasonable? (c) Do the investigator and assistants such as students appear to have sufficient expertise to produce useful results? (d) Is the budget reasonable, given the proposed research, the qualifications of the investigator (and his students) and the typical level of funding provided by the agency in question? (e) Are the necessary facilities available?

The easiest way to write a detailed and specific proposal is to propose to do research that is already complete, or at least substantially underway; this approach is quite common for an established researcher. Unfortunately, that isn't the purpose of a research proposal, and requiring a high level of detail and specificity in the proposal discriminates against newcomers to the field, and also against those who propose new work. Thus, the most serious difficulty faced by the reviewer of proposals is to evaluate a

proposal and not to expect to see a research *report.* Similarly, a proposal may include a larger scope of work than can be reasonably accomplished with the time and effort specified. If the investigator clearly recognizes this, and indicates that he will pick and choose sub-topics within the area of research, depending on their interest and the availability of someone such as a graduate student to work on them, this is not a negative factor.

A major difference between research proposals and papers is that a proposal is speculative, and the reviewer has to evaluate what is likely to result. Such an evaluation, as noted below, should rely strongly on the personal reputation of the investigator, *when that reputation exists.* People with a consistent history of good research will probably do good work, no matter how sloppy or brief their proposal. People with a consistent history of low quality research (no matter how voluminous, nor how hot the topic) will probably continue in the same manner, no matter how exciting the proposal. Therefore, a substantial fraction of the evaluation of a proposal by a *well known* investigator should depend on the reputation of that investigator. Is is very important, however, *not to discriminate against newcomers to the field*, who have no reputation, either good or bad. In the latter case, one must rely much more heavily on the text of the proposal, and on other information such as the investigator's Ph.D. dissertation, his academic record, his host institution, his Ph.D. institution, comments by his advisor or others who know him, etc. It is also important to take into account the possibilities that a well regarded researcher is proposing poor research, or that a researcher noted for poor quality work has decided to do the better quality work of which you believe him capable.

Reviewers are asked to comment on the proposed budget. Keep in mind that many factors affect the size of the budget other than the proposed scope of research, such as the agency providing the funding, the availability of facilities and staff, etc. Note particularly that for a new investigator, there is a major difference between no funding and minimal funding (two months summer salary, amounts for travel, supplies and computer time). Funding a new investigator at a low level is often a good gamble; two or three years later, the investigator will have a track record, and if it is a good one, higher levels of funding can be justified. Such small grants are often called *initiation grants* and should be much easier to get than regular grants.

10. Other Issues

10.1. Simultaneous Submission, Prior Publication, Unrevised Retries

If a paper is submitted simultaneously to two or more places, all editors and/or program chairs should be advised of, and approve of, this, and all referees should be so notified. It is unethical to submit a paper simultaneously without notification, and that is a sufficient basis for rejecting the paper. There is a significant chance that if a paper is simultaneously submitted, the simultaneous submission will be detected through the review process.

If a paper has already been published (as in the proceedings of a conference) and is then submitted for republication (for example, in an archival journal), it is essential that the editor and referees be so notified. Some associations such as the IEEE and ACM permit republication in their journals, but generally the paper must meet a higher standard for republication than if it had never been published. Significant extensions or major revisions are often a sufficient reason for republication. The referee should be alert to the author who tries to publish the same work in all of its various combinations, permutations and subsets, and to the author that attempts to add the "least publishable unit" of new material to each paper. Note that if the first version of the paper was published by a different publisher than that considering the current version, copyright restrictions may make it illegal for the paper to be republished without explicit permission of the copyright holder.

It is not uncommon to receive a paper to referee which you have previously recommended be rejected by some other publication. If the paper has not been rewritten to comply with your previous review, it is appropriate to return a copy of the previous review, along with a blunt note suggesting that the author might try making revisions in accordance with referee reports.

10.2. Acknowledgements and Plagiarism

It is important that papers not plagiarize, and that joint work and contributions of others be fully acknowledged. Referees should explicitly point out any such problems discovered.

10.3. Timely Response, and Returning a Paper

It is important that referees respond reasonably promptly. Conferences have deadlines, and reports received after the program committee has met are useless. Journals do not generally have deadlines, but taking a long time to review a paper, and preventing its consideration for publication through delay, is professionally unethical. Dante probably had a place for referees who promise to do reports, and then don't do so [Dant49]. If you can't read the paper in a reasonable amount of time, typically 4-8 weeks, send it back to the editor, or at least get the editor's agreement to the delay.

Keep in mind that if you expect to have your own papers published, you have a responsibility to referee a reasonable number of papers. It is part of your job as a researcher. The option of sending it back to the editor should not be abused. Editors may choose not to handle papers by authors who don't fulfill their refereeing responsibilities.

If you are sent a paper which you are not qualified to referee, you may also send it back to the editor or program chair. Note, however, that you may have been specifically selected to provide an "outside" view of the field (see section 10.7), and that fact may qualify you to provide a limited opinion.

If you are going to send a paper back without refereeing it, do so *immediately*. Be sure to return the manuscript.

10.4. Does the Author's Reputation Matter?

Should the reputation of the author be allowed to influence the evaluation of a paper, as opposed to a proposal? There is no consensus. In my personal opinion the referee should consider the author's name and reputation *to a small extent, and only in some circumstances*.

A research paper must stand on its own. The only time to take into account the reputation of the author is with regard to ambiguities, points that aren't clear, and reference to work that isn't presented. If the author is justifiably well regarded, one may be inclined to assume that any problems will be corrected, and must be corrected, on revision. If the author is poorly regarded, through a well earned bad reputation, then one can reasonably assume that omissions and ambiguities probably represent concealed (deliberately or otherwise) errors. Note that assumptions about whether problems with the paper can and will be corrected are an issue primarily for conferences, for which there is usually insufficient time for rereview; for journals, assumptions are not necessary, since the paper can undergo as many review cycles as are necessary to resolve problems.

10.5. Confidentiality

It is the practice in Computer Science and Engineering for the editor to transmit to the author the verbatim referee reports, usually a photocopy, without

the referee's name and without surrounding identifying information such as the institutional letterhead. If you don't want to be identified, you should not put identifying information in the text of your report. Note that there is the delicate problem of asking the author to cite the referee's own work, without giving the author a hint of who the referee is; there is no easy solution to this problem.

Papers that are submitted for publication are not necessarily public. You should not use the material in a paper you have refereed, nor distribute copies of the paper, unless you have knowledge that the paper has indeed been made public, for example by being distributed as a technical report.

10.6. Conflicts of Interest

If you have a conflict of interest, you should make it known to the editor. If the conflict is severe, you should not referee the paper, but should instead return it to the editor. For example, if you have a feud with an author, or a significant personal disagreement, it would be wise to send the paper back. If you are competing with the author for funding, and this is a proposal, you should make that known to the program officer.

The opposite type of conflict also occurs - you are being asked to referee a paper written by a friend, colleague, former or current student, boss or subordinate, or former advisor. If you feel that you cannot provide an objective review, then you should return the paper to the editor.

10.7. Role of the Editor or Program Chairman

The editor has several tasks [Bish84]. Here we refer to both the editor in chief, who typically has the authority to decide whether to accept a paper, and the associate editors, who solicit the referee reports and recommend to the editor in chief whether to publish. The editor receives the paper from the author and maintains correspondence with the author. The editor selects the referees, sends them each a copy of the paper with suitable instructions, and awaits their results. The editor *should* remind tardy referees, and find new referees after a certain period if no response has been received.

The editor should select referees who are knowledgeable in the subject matter of the paper, and can be relied upon to provide a fair and objective evaluation. Unfortunately, it is not always possible to do this - there are too many papers to be reviewed, and too few people known to be sufficiently expert and responsible. There is also another problem - by definition, people in area X believe that work in area X is worthwhile. A report received from someone in area X will evaluate the paper in area X by the standards of area X, but will seldom, if ever, say that work in area X is pointless and should be discontinued. It is, however, quite possible that such a response is appropriate; if one wants to debunk alchemy, one sends the paper to a chemist, not an alchemist. If you receive a paper to referee which is outside your area, you should consider whether it has been sent to you deliberately, and for that reason. Someone has to say that the emperor has no clothes.

After the editor has received a sufficient number of referee reports, typically three, the editor must decide whether to accept the paper, and if so, to what extent revisions are required. *The editor does not simply count the referee reports as votes.* The editor must read the referee report recommendations, and their reasons, and must decide, using his own judgement, whether to accept the paper. An editor, in theory, can overrule the unanimous recommendation of the referees; in practice, the editor can and sometimes does side with a minority of the referees. It is important that the referees state the reasons for their recommendations and justify them; those reasons count as heavily or more heavily than the recommendations themselves.

The editor must also resolve conflicting recommendations, and should tell the authors to what extent they must comply with the referee comments in making changes. A wise editor will transmit copies of all referee reports to all referees, both to educate the referees, and to be fair to the author in the case of conflicting reviews.

In the case of a conference, the program chairman is responsible for selecting referees and collecting and tallying their reports. Typically, the program committee, in a meeting or conference call, will decide which papers to accept by majority vote. The program chair may or may not have a vote that is larger than that of the others on the committee, but he seldom has the authority to accept or reject papers over the opposition of a majority of the program committee. Due to the large number of papers to be handled in a very short time, referees and authors are not usually given the personal attention provided by an editor who handles only one or a few papers per month. Note that program committees often use numerical scores to prepare ranked lists of papers; such scores should be assigned carefully and should be viewed skeptically by the committee.

11. When You Are The Author

This article has been directed at the referee, but instructions to the referee are also instructions to the author. When starting research, when writing a paper, when finishing the paper, and when deciding where to submit it, ask yourself: how will this paper do when refereed according to the criteria given here?

Some specific things to think about are: Are you submitting the paper to the right place? Some journals and conferences will not consider material outside a specific scope; why waste 3-12 months to find out that your paper wasn't appropriate? Likewise, if you know that your paper is minor, why send it to a highly selective forum; send it somewhere where it has a reasonable chance of being accepted. If you suspect that further work is needed before publication, do that work; it may turn an unpublishable paper into a publishable one, without the 3-12 month extra delay. A look at an issue of the publication to which you are considering submission will answer many of these questions; it is also helpful to look over the information provided by the journal to prospective authors; e.g. [CACM89, IEEE84].

Keep in mind that a good referee report is immensely valuable, even if it tears your paper apart. Consider - each report was prepared without charge by someone whose time you could not buy. All the errors they find, all the mistaken interpretations they make are things that you can correct before publication. Appreciate referee reports, and make use of them. Some authors feel insulted, and ignore referee reports; that is a waste of an invaluable resource.

An author receiving a negative referee report often suspects that the editor, program committee, program chair, and/or referees are incompetent, biased, or otherwise unfair. While this sometimes happens, it is the exception; individual referee reports are often wrong, but a *set* of negative referee reports is usually an accurate indication that your paper has a problem, and needs to be either rewritten or redone before resubmission, or discarded as unpublishable or embarrassing. Note particularly that the reader of a paper forms an opinion of the author; if the quality of a paper is such as to reflect badly on the author, it should not even be submitted for publication.

Also keep in mind that the decision making procedures of editors and program committees are not failsafe (or idiot-proof). I have more than once had a paper rejected based on what I considered to be reasonable and competent referee reports, only to later discover that papers that I considered to be far inferior to mine were accepted. A competent editor or program chair will not just add up the referees' scores, but will consider the content of the reports and the individual expertise of the referees.

Authors are particularly referred to [Day77], [Levi83], [Mano81], and [Wegm86], which provide discussions of how to write technical papers. Refereeing is also a good way to learn to write better papers; evaluating the work of others gives one insight into one's own.

12. Conclusions

Scientific progress relies heavily on the process of peer review, the evaluation of research for publication and funding by the researchers in the area, or by researchers qualified to evaluate the work. Good quality reviews, referee reports, are essential to this process. The task of the referee is to evaluate in a timely manner a paper for publication or a proposal for funding. For a research paper, this involves determining if the work presented is correct, if the problem studied and the results obtained are new and significant, if the quality of the presentation is satisfactory or can be made so, and what revisions and changes to the paper are necessary and/or desirable. This evaluation is necessarily a matter of opinion, and as a referee gains experience the quality of the evaluation should improve. The guidelines and instructions for refereeing presented in this paper should be particularly useful in training and instructing novice referees.

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How to Become a Referee

Editors are always on the lookout for qualified and responsible referees. The easiest way to become a referee is to write a paper, thus bringing your name and expertise to the attention of the relevant community. You can also become active in professional activities, such as local IEEE or ACM groups, IEEE Technical Committees (TCs), ACM Special Interest Groups (SIGs), conference organizing committees, etc; participation in these activities will enable you to meet editors and program chairs. Sometimes, editors will actively solicit referees.

Guidelines for Referees for IEEE Computer

Following are excerpts from the Guidelines for Referees distributed by the editor for IEEE Computer.

''**Computer** covers all aspects of computer science, engineering, technology and applications. It is aimed at a broad audience. **Computer** publishes technically substantive articles that are referenced extensively in the literature. Articles in **Computer** are often survey or tutorial in nature and cover the state of the art and important emerging developments. One of the most important purposes of **Computer** is to act as a technology transfer conduit to bring results and formalisms from university, industry and government research and development centers to the general practitioners in the field.

''All articles should be comprehensible to readers actively working in a technical discipline. In so far as possible, manuscripts should be written in a style similar to that of articles appearing in **Scientific American**.

''Refereeing reports should be returned on the **Computer Review Form.** Because sections of the review form and the marked up manuscripts will be sent to the author(s) as they are, it is important that no identification of the referee should appear on them. Inappropriate remarks will be deleted before any material is sent to the author(s).

''It takes a good deal of time and effort to develop a manuscript that is technically relevant and readable. A detailed review of a manuscript can be an invaluable aid to the author(s) in improving its overall technical quality, utility and readability. Please provide constructive comments that will help the author(s) to: (1) correct errors and misconceptions; (2) state appropriate, accurate and relevant conjectures and results; (3) employ better definitions, diagrams, tables, graphs and examples; (4) use a maximum of 12 contemporary, relevant and essential references; (5) make the article technically consistent and complete; and, (6) organize the material to help the reader understand the issues presented.

''If major revisions are recommended, you should point these out as specifically as possible and should differentiate optional changes from those you judge mandatory. If the revisions required are extensive, it is perhaps best to reject the paper and recommend preparation of a new, heavily revised manuscript for resubmission to **Computer.** If you reject the manuscript mainly on the basis of reader interest, please suggest a more appropriate journal to the author(s). Manuscripts with little or no salvageable material should be rejected outright and later submission discouraged.''

The Author

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