The Task of the Referee

Alan Jay Smith
University of California at Berkeley

Computer researchers have a professional obligation to referee the work of others. This article tells you how to evaluate a paper and write a report using common standards and procedures.

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

This article tells you how to evaluate a paper, write a referee report, and apply common standards and procedures. It is intended to replace Forscher's rules,[1] which are distributed by some editors but do not reflect the procedures used in computer science and engineering. This article focuses on 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; separate sections consider research proposals and survey and tutorial papers. Authors might find this material useful for preparing papers for publication. Another recent paper discusses refereeing in theoretical computer sciences; there are some differences between theory and the applied areas considered here.

The referee's task

Your role as referee is to decide whether a paper makes a sufficient contribution to the field. The 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. To quote a referee for this article:

> 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papers which are mostly repetitions of other papers should not; papers which have good ideas badly expressed should not be published but the authors should be encouraged to rewrite them in a better, more comprehensible fashion.

Reading a paper as a referee is closer to what a teacher or professor does when grading a paper than what a scientist or engineer does when reading a published work. In the latter case, the reader presumes that the paper has been checked (refereed) and is thus correct, novel, and worthwhile. As a referee, on the other hand, you must read the paper carefully and with an open mind, checking and evaluating the material with no presumption as to its quality or accuracy. The result of your reading should be a referee report that recommends for or against accepting the paper and lists necessary and suggested changes.

It is important that you walk the uncertain line between being too permissive ("publish everything") and being too restrictive ("nothing is good enough to publish"). If you are not critical enough, you encourage poor research, recognize and honor those who don't deserve it, mislead naive and inexperienced readers, mislead the author as to what is publishable, encourage disrespect for the field, distort commercial development, hiring, promotion, and tenure decisions, and perhaps actually subtract from the general store of knowledge; consider the Piltdown man fraud, which misled anthropologists for years. As has been noted by Thompson[3] and others, unrestrained publication buries the professional under mounds of paper, only a very small fraction of which can be examined, let alone read.

If you are too critical, you block or delay good research from publication, waste the time of authors, damage careers, and perhaps leave journals with nothing to publish and conferences with nothing to present. It is particularly important not to reject new and significant work that runs counter to the prevailing wisdom or current fashion.

If you want to be taken seriously as a referee, you must have a middle-of-the-road view—you must be able to distinguish good from bad work, major from minor research, and positive from negative contributions to the literature. A referee who always says "yes" or always says "no" is not helpful.

The referee report

A good referee report should have several parts. First, you should briefly state your recommendation and the reasons for it. Second, you should summarize the point of the paper in one to five sentences, both for the editor's use and to ensure that you actually understand the paper. Third, you should evaluate the validity and significance of the research goal. Fourth, you should evaluate the quality of the work (methodology, techniques, accuracy, and presentation). Finally, you must provide an overall recommendation for or against publication. If you recommend against publication, you should clearly state why. You should also make the strength of your opinions clear; an equivocal ("maybe") recommendation is acceptable if your reasons for it are clearly documented. In any case, your report must contain enough discussion and information to justify your recommendation.

If your recommendation is favorable, you must list both necessary and suggested changes. If your recommendation is negative, but you think the paper can be salvaged and either submitted elsewhere or resubmitted, then you should provide a similar (but perhaps less detailed) list. Suggestions for alternate places to publish are always welcome.

Refereeing a paper can require considerable time and effort; don't waste that effort on a detailed critique of a badly flawed paper that can never be made publishable. Finding one or more fatal and uncorrectable flaws excuses the referee from checking all subsequent details.

Typically, the author receives the text of the referee report stripped of all material identifying the referee. Thus, while it is important to be clear and explicit, your report should not be insulting. Don't refer to the author as a "fool" or an "idiot" nor to the paper as "trash." Your review should be directed at the paper, not the author. The review of a proposal, though, is also a review of the investigator. In this case, it is appropriate to evaluate the author's abilities as well as the research proposed. In all cases, however, the evaluation should be objective and fair. The more psychologically acceptable the review, the more useful it will be.

How to become a referee

Editors are always looking 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 community. You can also become active in professional activities, such as local IEEE or ACM groups, IEEE Technical Committees, ACM Special Interest groups, and conference-organizing committees. Participating in these activities will help you meet editors and program chairs. Also, editors sometimes actively solicit referees.

Evaluating a research paper

As a referee, you must evaluate a paper's novelty, significance, correctness, and readability. This general set of goals can be broken down into a much more specific series of questions.

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

Is the paper appropriate? Does this paper have anything to do with computer science or engineering? If so, is the research appropriate for this forum? (Authors should not submit papers on queueing theory to *Datamation* or market analyses of the latest MVS release to the *Journal of the ACM* or the *Proceedings of the IEEE*.)

Is the goal significant? For that matter, is the problem real? Does it contradict any physical laws (as do perpetual motion machines) or widely reported measurements?

Is there any reason to care about the paper's results, assuming they are correct? In other words, is the problem or goal major, minor, trivial, or nonexistent? Keep in mind what the Walrus said[4]:

> 'The time has come,'
> the Walnus 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 became boiling hot or an elegant study of the flight characteristics of pigs? Sophisticated mathematical analyses can be applied to models so unrealistic that the effort is useless and the results of no interest.

Is the problem obsolete, such as a reliability study for vacuum tube mainframe computers? Is the problem so specific or so applied that it has no general applicability and does not merit wide publication?

Is the problem, goal, or intended result new? Has the design been built before? Has the problem 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 or she cite that work and give specific distinctions between it and the current work? If the paper describes an implementation, are there any new ideas?

Is the method of approach valid? Is there something about the approach that invalidates the results? Can you tell what the method is, or do you have to ferret it out from 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 sufficient for the purpose? For example, does it matter if the author overlooked available data and used a random-number-driven simulation with unrealistic parameters? "Back of the envelope" calculations are often sufficient.

If this is a new idea, does the author present enough discussion or analysis? 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 technical reports.

Unrestrained publication buries the professional under mounds of paper, only a very small fraction of which can be examined.

Is the actual execution of the research correct? Are the mathematics correct? One or more referees should check the math in detail; you should always tell the editor if you 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 you that the results are valid? For stochastic simulations, does the author give confidence intervals for the results? Are the results consistent with the assumptions or with observed facts or measurements? Have boundary conditions been checked? Are the results plausible or even possible? Did the author do what he or she claims? For example, did the author simulate the original system, a reasonable model of it, or just the approximate mathematical model?

Are the correct conclusions drawn from the results? What are the applications or implications of the results? Does the author adequately discuss why he or she obtained these results?

Is the presentation satisfactory? Is the paper written well enough for you to evaluate the technical content? A paper that is incomprehensible is not publishable. A paper that requires extensive revision is not publishable in its present form and might never be. If the paper is readable at all, you must evaluate the presentation as well as the technical content. (Refer to articles such as that by Day[5] to learn how to write a paper.)

Does the abstract describe the paper? Does the introduction adequately explain the problem and the research framework? 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, legible, and meaningful? Are there too many or too few tables and figures? Are explanations poor or even nonsensical? Is the author too verbose or too terse and cryptic? Is the paper sufficiently self-contained that someone knowledgeable in the field can understand it, or does the reader need detailed knowledge of results published elsewhere? If the author refers the reader to other papers for crucial details, do you believe him or her?

If sections of the paper are missing or incomplete due to a deadline, do you believe 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, does it contain too much material, or has the author been too wordy? 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 committee chair worry about specific page limits.

Does the paper contain typographical errors or problems in grammar, punctuation, and wording? You should identify all such problems you find. Such errors can be a serious problem when an author's native language is not English. It is not your job, however, to rewrite the paper.

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

Making recommendations

After comparing the paper to an appropriate standard (not your own standards, which may be high or low), to the average of the papers that you write, or to the average of the papers that you find worth reading, you should be able to put it into one of these categories:

(1) Major results; very significant (fewer than 1 percent of all papers).
(2) Good, solid, interesting work; a definite contribution (fewer than 10 percent).
(3) Minor, but positive, contribution to knowledge (perhaps 10-30 percent).
(4) Elegant and technically correct but useless. This category includes sophisticated analyses of flying pigs.
(5) Neither elegant nor useful, but not actually wrong.
(6) Wrong and misleading.
(7) So badly written that technical evaluation is impossible.

The next question is: What are the standards of this journal or conference? Is this the *Proceedings of the IEEE*, *ACM Transactions on Computer Systems*, or the *ACM Symposium on Operating Systems Principles* (all quite selective), or is it the Tahiti Conference on Beach Ball and Computer Systems (fictional, but a presumed boondoggle)? You should compare the paper with the average paper in that specific journal or conference, not with the best or worst. Of course, in some cases the average is too low and needs to be raised by critical refereeing. Note that you cannot tell how selective a conference or journal is by the percentage of papers it accepts; far fewer bad papers are submitted to the best conferences and journals.

You should then make a recommendation, whether favorable ("publish") or unfavorable ("reject"). Your recommendation is your opinion as to whether the paper makes a sufficient contribution. Generally, this will include all papers in Categories 1 and 2 above and some in Category 3. The strength of your recommendation should be clear to the editor ("wonderful paper, definitely accept"; "useful paper, probably accept"; "marginal paper, see how many better ones have been submitted"; or "wrong and misleading, definitely reject"). If you feel that the paper has something worthwhile to say, but you're not sure it is good enough for this journal or conference, you can say "maybe." You can also recommend that a paper be rejected as inappropriate for the journal or conference. If the paper is inappropriate or marginal in quality for the forum but is suitable elsewhere, you can suggest other places to submit the paper. In any case, you must discuss and justify your recommendation. A recommendation without sufficient justification will carry very little weight.

If the author is asked to prepare a revised version, the revision will usually be sent to the same referees for further review. It is important to ensure that the revisions are satisfactory, but you should avoid comments inconsistent with your first review. You should also avoid harassing the author by unnecessarily recommending revision after revision. It is possible, however, that a revised manuscript still contains serious problems due to things overlooked in the first review, problems that have only become apparent after revision, or new errors introduced in the revision. Such problems must be addressed. The presence of serious problems after a second revision suggests that the author is incapable of fixing the problems, in which case it is often appropriate to recommend final rejection.

For a conference, a paper that requires substantial revision generally cannot be accepted due to the short time available for revisions and the difficulty of arranging for additional rounds of revisions. For journal publication, however, the extent of necessary revisions is a separate issue from the recommendation for (eventual) publication.

Surveys and tutorials

Surveys and tutorials differ from research papers in that most or all of the work reported is not new. Such a paper, however, might include a variety of minor research results that would not stand on their own in separate papers.

Surveys and tutorials are similar but not identical. A pure tutorial explains some body of material to nonexperts, usually novices. It might not cover the entire field, and it might have a specific point of view. A survey provides broad and thorough coverage of some field or body of knowledge. It can be aimed at readers ranging from the novice to the near-expert.

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

For a survey, 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 survey at one time? Is the material correct, and is the author sufficiently expert on the subject to interpret results correctly and provide perspective on the field? Has the author integrated the material in a consistent manner, or is this just an annotated bibliography? Is the author's coverage balanced and thorough? Does he or she cite all important relevant literature, or is the presentation biased, slanted, or unevenly selective? Controversial opinions and evaluations should be identified as such. If the survey includes new research results, do they meet the validity and correctness criteria given above for research papers? (A survey does not have to stand on its own as a research paper, so the research does not have to be so significant as to justify publication as a research paper.) Finally, is the paper well written and clear?

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 also consider the investigator.

The primary difficulty with reviewing a proposal is that the investigator is supposed to tell you what he or she plans to do, in addition to what has been done already. The questions you must ask, then, are: Is the research topic significant? Is the method of approach described, and is it reasonable? Do the investigators and assistants appear to have sufficient expertise to produce useful results? Is the budget reasonable given the proposed research, the qualifications of the investigators, and the typical level of funding provided by the agency in question? Are the necessary facilities available?

The easiest way to write a detailed and specific proposal is to propose research that is already complete or at least substantially underway; this approach is quite common among established researchers. 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 and those who propose new work. Also, a proposal might include a larger scope of work than can be reasonably accomplished with the time and effort specified. This is not a negative factor if the investigators clearly recognize it and indicate that they will choose subtopics, depending on their interest and the availability of assistants to work on them.

A major difference between a research proposal and a paper is that a proposal is speculative, so you must evaluate what is likely to result. Therefore, when you evaluate a proposal by a well-known investigator, a substantial fraction of that evaluation should depend on the investigator's reputation. 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 will probably continue in the same manner, no matter how exciting the proposal, how voluminous their research, or how hot the topic. However, you must also consider the possibility that a well-regarded researcher may propose poor research or that a researcher noted for poor-quality work has decided to do better work.

It is important that you do not discriminate against newcomers who have no reputation, either good or bad. In this case, you must rely much more heavily on the text of the proposal and such information as the investigator's PhD institution and dissertation, academic record, host institution, and comments by his or her advisor or others.

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 and the availability of facilities and staff. Also note that for a new investigator, there is a major difference between no funding and minimal funding (two months summer salary plus 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 record, 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.

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Other issues

Simultaneous submission, prior publication, and unrevised retries. If an author submits a paper simultaneously to two or more places, he or she must have the approval of all editors or program chairs.

All referees should also be notified. Submitting a paper simultaneously without notification is unethical and a sufficient basis for rejection. There is a good chance that a simultaneous submission will be detected through the review process.

If a paper has already been published (in conference proceedings, for example) and is submitted for republication (perhaps in an archival journal), the editor and referees must be notified. Some associations such as the IEEE and ACM permit republication in their journals, but the paper generally must meet a higher standard than if it had never been published. Significant extensions or major revisions are often a sufficient reason for republication. As a reviewer, you should be alert to the author who tries to publish the same work in all its various combinations, permutations, and subsets, and to the author who adds the "least publishable unit" of new material to each paper. Also note that if the first version of the paper was published elsewhere, copyright restrictions might require the first publisher's explicit permission to republish the paper.

You will sometimes receive a paper to referee that you have previously recommended for rejection 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 that review along with a blunt note suggesting that the author comply with referee reports.

Acknowledgments and plagiarism. Authors must not plagiarize, and they must fully acknowledge joint work and the contributions of others. You should explicitly point out any such problems.

Timely response and returning a paper. You should return your report promptly. For a conference, referee reports must reach the program chair well before the program committee meeting so that the material can be assembled and prepared for discussion. Reports received after the program committee has met are useless.

Journals do not generally have firm deadlines, but preventing consideration of a paper by taking a long time to review it is unethical. Computer science journals are notorious for long delays between submission and publication; the two major bottlenecks are the referees and the publication queue for the journal itself. Imagine if it were your paper. If you can't read the paper in a reasonable amount of time, typically four to eight weeks, send it back to the editor or at least get the editor's agreement to the delay. Dante probably had a place for referees who promise to do reports and then don't do so.[6]

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 a paper back to the editor should not be abused. Editors can choose not to handle papers by authors who don't fulfill their refereeing responsibilities.

If you are sent a paper that you are not qualified to referee, you can send it back to the editor. However, if the editor has selected you to provide an "outside" view of the field, you should provide a limited opinion (see the section on the editor's role).

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

The author's reputation. Should the authors' reputations influence the evaluation of a paper, as opposed to a proposal? There is no consensus. In my opinion, you should consider the author's name and reputation only with regard to ambiguities, unclear points, and references to work that isn't presented. If the author is justifiably well regarded, you can probably assume that any problems will (and must) be corrected on revision. If the author has a well-earned bad reputation, you can reasonably assume that omissions and ambiguities probably represent concealed (deliberately or otherwise) errors. Because they usually have insufficient time for rereview, conference program committees must make assumptions about whether problems can and will be corrected; for journals, assumptions are generally not necessary.

Confidentiality. In computer science and engineering, editors generally send the verbatim referee reports to the author, usually as photocopies without the referees' names, institutional letterheads, etc. If you don't want to be identified, don't put identifying information in the text of your report. There is no easy solution to the delicate problem of asking the author to cite your own work without giving him or her a hint of who you are.

Papers submitted for publication are not necessarily public. You should neither use material from a paper you have refereed nor distribute copies of the paper unless you know it has been made public, for example, as a technical report.

Conflicts of interest. You should tell the editor of any conflicts of interest. If the conflict is severe, you should not referee the paper and should retum it to the editor. For example, if you have a professional feud or a significant personal disagreement with an author, you should send the paper back. If you are reviewing a proposal, and you are competing with the author for funding, you should tell the program officer.

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

The editor's role. The editor has several tasks.[7] ("Editor" in this section refers to both the editor-in-chief, who typically decides 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 and maintains correspondence with the author, selects the referees, sends them copies of the paper with suitable instructions, and awaits their results. He or she should check up on tardy referees and should find new referees if no response has been received after a certain period.

> An author who feels insulted and ignores referee reports wastes an invaluable resource and the referees' time.

The editor should select referees who are knowledgeable in the subject and can be relied on to provide a fair and objective evaluation. Unfortunately, this is not always possible; there are too many papers to be reviewed and too few people who are sufficiently expert and responsible.

There is also another problem: people who work in area X tend to believe that area X is inherently worthwhile. Referees who work in area X will usually evaluate papers in area X by the standards of area X; they will seldom, if ever, say that work in area X is pointless and should be discontinued. This is why an editor who wants to debunk a paper on alchemy sends the paper to a chemist, not an alchemist. Someone has to say the emperor has no clothes.

After receiving a sufficient number of referee reports, typically three, the editor must read them and decide whether to accept the paper and what revisions are required. The editor does not simply count the referees' recommendations as votes. In theory, he or she 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 justify their recommendations; their reasons count as heavily or more heavily than the recommendations themselves.

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

The program chair's role. For a conference, the program chair selects referees and collects and tallies their reports. Typically, the program committee will decide which papers to accept by majority vote. The chair may or may not have a larger vote than the others, but he or she seldom has the authority to overrule a majority of the committee. Because they handle a large number of papers in a very short time, program committees usually do not give referees and authors the personal attention provided by editors who handle only a few papers per month. The committees often use numerical scores to prepare ranked lists of papers; such scores should be assigned carefully and viewed skeptically by the committee.

When you are the author

This article has been directed at potential referees, but instructions to referees are also instructions to authors. When starting research, writing a paper, finishing the paper, and deciding where to submit it, ask yourself: How will this paper stand up when refereed according to the criteria given here?

You should also consider if you're submitting your paper to the right place. Some journals and conferences will not consider material outside a specific scope; why waste three months to a year to find out your paper wasn't appropriate? Likewise, if you know your paper is minor, why send it to a highly selective forum? Send it where it has a reasonable chance of being accepted. If you suspect that further work will be needed before publication, do the work before submitting the paper; it may turn an unpublishable paper into a publishable one, without the delay. You can answer many of these questions by looking at an issue of the publication. You should also look at the information the journal sends to prospective authors.[8, 9]

Keep in mind that a good referee report is immensely valuable, even if it tears your paper apart. Remember, each report was prepared without charge by someone whose time you could not buy. All the errors found are things you can correct before publication. All the mistaken interpretations could have been made by the final readers. Appreciate referee reports, and make use of them. An author who feels insulted and ignores referee reports wastes an invaluable resource and the referees' time.

Some authors suspect that a negative referee report indicates that the editor, program committee, program chair, and referees are incompetent, biased, or otherwise unfair. While this is sometimes the case, it is the exception. There is seldom a single correct evaluation of a paper, and equally skilled and unbiased readers will differ. However, a set of negative referee reports is an accurate indication that your paper should be rewritten or reworked before resubmission or discarded as unpublishable or embarrassing. A reader forms an opinion of you based on your paper; if your paper's quality would reflect badly on you, you should not even submit it for publication.

Authors should note that Day,[5] Levin and Redell,[10] Manola,[11] and Wegman[12] discuss how to write technical papers. Refereeing is also a good way to learn to write better papers; evaluating the work of others will give you insight into your own.

Scientific progress relies heavily on the peer review process—the evaluation of research for publication and funding by researchers qualified to evaluate the work. Referee reports are essential to that process. The referee's task is necessarily a matter of opinion; as a referee gains experience, the quality of the evaluation should improve. The guidelines and instructions presented here should be particularly useful in training and instructing novice referees. ■

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Alan Jay Smith is a professor in the Computer Science Division of the Department of Electrical Engineering and Computer Sciences at the University of California at Berkeley, where he has been on the faculty since 1974 and was vice chair of the department from July 1982 to June 1984. His research interests include the performance analysis of computer systems and devices, computer architecture, and operating systems. He received the IEEE Best Paper Award for the best paper in the *IEEE Transactions on Computers* in 1979.

Smith is an associate editor of the *ACM Transactions on Computer Systems*, is a subject-area editor of the *Journal of Parallel and Distributed Computing*, and is on the editorial board of the *Journal of Microprocessors and Microsystems*. He was program chair for the Sigmetrics 89/Performance 89 Conference and is program cochair for the 1990 Hot Chips Symposium.

Smith received the BS degree in electrical engineering from the Massachusetts Institute of Technology, and the MS and PhD degrees in computer science from Stanford University. He is an IEEE fellow and a member of the ACM, SIAM, the Computer Measurement Group, Eta Kappa Nu, Tau Beta Pi, and Sigma Xi.

*Readers may write to Smith at the Computer Science Division, EECS Department, University of California, Berkeley, CA 94720.