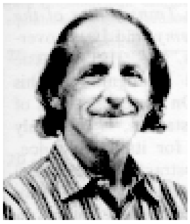
Essays of an Information Scientist, Vol:9, p.239, 1986
Current Contents, #32, p.3-12, August 11, 1986



Current Comments®

EUGENE GARFIELD
INSTITUTE FOR SCIENTIFIC INFORMATION®
3501 MARKET ST., PHILADELPHIA, PA 19104

Refereeing and Peer Review. Part 2. The Research on Refereeing and Alternatives to the Present System

Number 32 August 11, 1986

Continuing our discussion of refereeing, which focused on complaints about the system in Part 1,[1] we now examine the empirical research on the subject, the anecdotal literature supporting the current system, and some of the suggestions for improving it. Part 3 will appear at a later date and will discuss the peer review of grant proposals. Again we will review the considerable literature of opinion and conjecture, but we will give special attention to the large-scale study by sociologists Stephen Cole, State University of New York (SUNY), Stony Brook, and Jonathan R. Cole, Columbia University, New York,[2,3] as well as other papers[4] and special reports.[5]

Editors: The Author's Guardians

Each anecdote purporting to reveal some fault in the present system of refereeing seems to find a ready counterpart in the opinion of a supporter. For instance, many critics claim that some referees do not review manuscripts dispassionately. But editors say that they usually take great pains to ensure that referees are fair. In *Running a Refereeing System*, Michael Gordon, research associate, Primary Communications Research Centre, University of Leicester, UK, recommends the use of two or more referees to reduce the risk of an offhand, frivolous, or biased treatment of a manuscript.[6] (p.13-5) When referees *do* cause excessive delays, return unsupported or capricious reports, or otherwise display "questionable ethics," they tend to be retired from the system, according to Patricia Dehmer, Argonne National Laboratory, Illinois, and member, Publications Committee, American Physical Society (APS) in a "Guest Comment" in *Physics Today*.[7] Whether this is the case in other disciplines is not known.

Critics also suggest that referees sometimes take advantage of the privileged information they are privy to in the manuscripts they review. But Dehmer asserts that many APS editors try to ensure that referees are not working along lines precisely like those of the papers sent to them, to reduce the possibility of conflicts of interest. But this is contrary to the practice in biomedicine and elsewhere. Most editors try to match submissions with reviewers as closely as possible, in an attempt to have the manuscript reviewed by those presumed to be most qualified to judge it. In either case, according to Claude T. Bishop, director, Division of Biological Sciences, National Research Council of Canada (NRCC), and editor-in-chief, NRCC Research Journals, referees ought to disqualify themselves when there is the possibility of a conflict of interest, or when they feel they cannot be objective about the paper or its author. In some instances, however, they might propose simultaneous publication of their own paper and the review paper, or even approach the authors of the review paper and propose a collaboration.[8] (p. 50, 82) As a parallel approach, many editors honor author requests that a paper not be sent to a given referee.[7]

Authors Often Lack Knowledge of Publishing

Editors also point out that authors frequently do not understand the publication process. For instance, many authors charge that referees make up a closed, "elite" group. Yet the number of active referees for a journal can far exceed the number of active contributors.[9] According to *JAMA* editor George D. Lundberg, that journal's list of active referees contains over 3,000 names.[10] The *Journal of the Operational Research Society*, a relatively small journal, used 285 referees in 1982 alone.[11] And a careful study of nine years of materials from the archives of *Physical Review* and *Physical Review Letters* by sociologists Harriet Zuckerman and Robert K. Merton, Columbia University,[12] showed that authors of every rank participated in the refereeing process. Their main finding, which is based on referee reports for both published and rejected manuscripts and which refutes another widely held belief, is that there is no consistent relationship between referee acceptance or rejection of manuscripts and the relative standing of authors and referees.[12] In addition, informed authors know that it is not referees, but editors, who are ultimately responsible for rejecting a manuscript.

Bishop says that authors also show a lack of understanding when they point to differences of opinion among referees as evidence that the system is capricious and unreliable.[8] (p. 43-9) At the root of some of these reviewer disagreements, in Bishop's view, are differences in the algorithms and paradigms fundamental to every branch of science. For instance, referees less often disagree substantially in well-established fields. But in fields pressing at the frontiers of knowledge, significant differences of opinion among referees are bound to be more common. When editors are confronted with a decision between two equally plausible referee interpretations of a given manuscript, they often employ one of several options that range from publishing the paper without comment to publication of the controversial paper along with comments by referees, invited critics, and rebuttals by the authors.[8] (p. 43-9)

Authors also seem to assume that their submissions are, in general, carefully written and based on substantial amounts of work. "Not so," asserts J.W. Cornforth, Milstead Laboratory of Chemical Enzymology, Sittingbourne Research Centre, Kent, UK, who served as a referee for a dozen journals over a 30-year period.[13] "In my experience," Cornforth continues in his letter to the editors of *New Scientist*, "a regrettably high proportion [of manuscripts] show careless or misleading presentation and meager experimental work, and the majority need some modification. Referees—and, of course, editors—almost invariably improve a paper that passes through their hands; often, they are doing what the authors ought to have done."[13]

The Many Faces of Rejection

Authors should also be aware that the scientific value of a paper is not necessarily the only factor that enters into editors' decisions to publish or not; many manuscripts never make it past the screening process that eliminates papers that are incompatible with a journal's readership or have not been submitted in the required format.[14] Or a journal may reject a manuscript simply because it has recently published another, similar paper, or has one currently under consideration.[10] Rejection rates are also significantly affected by the existence of page charges, which support publication and thus allow for much lower rejection rates. This practice is widespread in physics and chemistry but not unknown even in psychology.

It is also important to realize that rejection rates vary. In their study of patterns of evaluation in science, Zuckerman and Merton compiled a table of the rejection rates for a sample of 83 journals in the sciences, the social sciences, and the humanities.[12] Linguistics, geology, and physics journals had the lowest rate of rejection, turning down only 20 to 25 percent of the papers submitted to them. Biology journals rejected about 30 percent of the papers they received. Journals in experimental and physiological psychology had a rejection rate of over 50 percent, while sociology journals were over 80 percent and history journals hovered at 90 percent. Stephen Lock, editor, *British Medical Journal* (*BMJ*), made an observation that has also been noted by others who have read the study. He wrote that "the more humanistically oriented the journal, the higher the rate of [rejection]; the more experimentally and observationally oriented, with an emphasis on rigour of observation and analysis, the lower the rate of rejection."[15] (p. 17)

Zuckerman and Merton also reported that the editorial staff's attitude concerning its own errors in judgment constitutes an often-overlooked factor influencing acceptance rates.[12] Although editors and referees want to avoid errors in judgment altogether, they recognize that they cannot be infallible; thus, since they must make mistakes, they tend to have preferences for the *kind* of mistakes they are willing to risk. The staffs of some journals—notably those prestigious journals with high rejection rates—seem more willing to reject "unorthodox" manuscripts that the wider community of scholars might eventually consider important, rather than to run the risk of *publishing* a substandard work. The staffs of low-rejection journals, on the other hand, apparently prefer to publish the occasional work that doesn't measure up, rather than reject a paper that later turns out to be significant.[12]

The Research

A research front consists of a group of current papers that, together, cite one or more of a cluster of older, core publications. Since I referred earlier[1] to the paucity of empirical research on refereeing and peer review and the abundance of anecdote and opinion on the subject, one may wonder how a research front of any size might be generated on this subject. But even a large anecdotal literature, through repeated citations of previous anecdotal literature, as well as reputable studies, can form a pseudo-research front. Only a careful analysis of the core and citing literature can determine the nature and extent of the research front—even when very useful core review papers can be found. Since the literature on peer review and refereeing is vast, at the end of Part 2 of this essay I have added a selected bibliography of publications not mentioned in the text.

The 1983 ISI® research front entitled "Objectivity of reviewers in peer review" (#83-8291) consists of but 2 core papers and 12 citing papers. One core paper is the highly controversial 1982 study by Douglas P. Peters, University of North Dakota, Grand Forks, and Stephen J. Ceci, Cornell University, Ithaca, New York.[16] The other core paper is a 1982 editorial by Lock, entitled "Peer review weighed in the balance."[17] In it Lock discusses the conclusions drawn by Peters and Ceci and details some of the flaws in their study. In spite of these problems, however, Lock believes that Peters and Ceci have underscored some shortcomings within the system. Most of the recommendations Lock makes for improving refereeing—particularly double-blind review—are discussed in detail below.

Peters and Ceci

This controversial study involved the resubmission of 12 psychology articles—published by authors from prestigious and highly productive departments—to the journals that originally published them.[18] Peters and Ceci became interested in doing the study after reading about an informal experiment conducted by Los Angeles free-lance writer Chuck Ross.[19] He reports having submitted the untitled, untyped manuscript of Polish-born US literary author Jerzy Kosinski's novel *Steps*[20] under a pseudonym to publishers and literary agents to see if "unknown" authors receive fair consideration. Although the book had won the 1969 US National Book Award, Ross claimed that 14 publishers—including the book's original publisher—and 13 agents rejected it.[19]

In the Peters and Ceci study, the presentation of the data in the original papers was slightly altered. Fictitious names and institutions were substituted for the real ones, but the content of the articles was unchanged. Three of the resubmissions were detected as such; of the other nine, eight were rejected. The authors concluded that the rejections resulted from a systematic bias against unknown authors and institutions. In the commentary section published along with Peters and Ceci's article, however, many commentators pointed out a number of flaws in the study. For instance, according to anthropologist Sol Tax, University of Chicago, Illinois, and Robert A. Rubinstein, School of Public Health, University of Illinois Medical Center, Chicago, the names Peters and Ceci chose for their bogus institutions were far removed from the mainstream of psychology institutions. Thus, what the investigators really demonstrated, say Tax and Rubinstein, is a bias against materials originating outside *appropriate* institutions.[21] Nobel laureate Rosalyn S. Yalow, Veterans Administration, New York, commented, "How does one know that the data are not fabricated?... Those of us who publish establish some kind of a track record. If our papers stand the test of time, it can be expected that we have acquired expertise in scientific methodology.... The work of established investigators in good institutions is more likely to have had prior review from competent peers and associates even before reaching the journal."[22]

Garth J. Thomas, Center for Brain Research, University of Rochester, New York, suggests that referees and editors may have recognized the resubmitted articles as very like something they had seen before, but rather than raise the specter of plagiarism, they fell back on statistical criticisms to justify their negative comments.[23] Janice M. Beyer, School of Management, SUNY, Buffalo, writes that the most likely fate of any submitted article is to be unanimously rejected, as 80 to 90 percent are in the social sciences.[24]

In addition, psychologist Grover J. Whitehurst, SUNY, Stony Brook, notes that Peters and Ceci had no control group.[25] Richard M. Perloff, Department of Communication, Cleveland State University, Ohio, and Robert Perloff, Graduate School of Business, University of Pittsburgh, suggest that, among other controls, Peters and Ceci's study should have included resubmitting articles by authors from low-status institutions under by-lines with equally low-status affiliations, as well as resubmitting articles by high-status authors under equally high-status by-lines.[26] "Without such controls it is impossible to argue that the findings reflect the status bias [that Peters and Ceci] suggest," the Perloffs write.[26]

But *Is* There Bias?

Still, Tax and Rubinstein feel that a bias preventing competent work from being published is much more damaging than one that lets mediocre work slip through.[21] And anecdotal evidence of bias is so widespread that the possibility should not be dismissed by researchers. For instance, in another commentary on the Peters and Ceci article, Robert Rosenthal, Department of Psychology, Harvard, said that as a young member of the psychology faculty at the University of North Dakota, he was unable to publish 15 to 20 articles in mainstream journals in the 1960s. Within a few years of his move to Harvard, however, he says that most of these articles were published in the same journals that had previously rejected them.[27] He does not say, however, whether these were the identical articles, or if they had been substantially revised to meet the objections of reviewers or changed in any other way.

In a 1970 investigation of how attitudes might influence referee judgment, Leonard D. Goodstein and Karen

Lee Brazis, University of Cincinnati, Ohio, mailed abstracts of an empirical study on astrology to 282 members of the American Psychological Association.[28] They were asked to rate the design of the paper. Half were sent an abstract that reflected a conclusion confirming commonly held scientific attitudes toward astrology; the other half received an identical abstract, except that it included a conclusion that ran counter to scientific beliefs. The former was rated by most referees as better designed and having more significance for future research. The latter, which contradicted common wisdom, was rated as flawed.

When Zuckerman and Merton examined the selection of articles for the *Physical Review*, they found that papers by physicists of great repute affiliated with prestigious institutions were more likely to be exempted entirely from the refereeing process. Their papers were accepted and published more quickly than papers by lesser known physicists.[12] And in a large-scale study of papers submitted to physics journals, Gordon reported a strong bias in referees from major universities toward papers by authors who were also from large, well-known universities.[29]

Lock, however, found no evidence of referee bias in a study of 1,558 manuscripts submitted to *BMJ* between January and August 1979. The study was published in his book *A Difficult Balance: Editorial Peer Review in Medicine*.[15] Of the 246 external referees who were sent manuscripts by *BMJ*, 143 held academic positions, while the rest had nonacademic affiliations; yet the proportion of papers recommended for acceptance did not differ from one group to the other.[15] (p. 56-71) Moreover, regardless of the affiliations of both referee and author, Lock said that referees judged manuscripts "to an equal standard."[15] (p. 61)

Suggestions for Improvement

A few years ago, Norton D. Zinder, Rockefeller University, New York City, sent me the text of a talk he gave to the Society of Editors in 1969, when he was an associate editor of *Virology*.[30] Tongue partially in cheek, Zinder asked, "What would be so terrible if there were no refereeing of scientific papers?... As we now operate, with [the] restriction of publication by reviewing, the *number* of publications becomes a thing in itself.... If we were to cease refereeing papers,...there'd be little bar [to publication, and] quality might reassert its role, since there'd be less pressure to have long lists of publications."[30] The Perloffs write that the "caveat emptor approach [of having no refereeing system at all] might be viewed as a nod to the free market of ideas. Let millions of flowers bloom."[26] Some may feel that the continued growth of the literature may lend support to these views. However, others, including myself, believe that a few non-refereed publications can exist only because the refereed journals set the standards for all the others.

I believe that most scientists would agree that if something is indeed shown to be wrong with refereeing, an attempt should be made to repair the system, rather than to abandon it. Unfortunately, with little or no solid, systematic evidence of refereeing's deficiencies, most suggestions for improvement are as conjectural as the ills they are meant to cure. Among the most discussed options—one that is already prevalent among sociology journals—is that of double-blind refereeing, also called reciprocal anonymity, in which neither the authors nor referees are aware of the others' identities. There is precedent for author anonymity: David A. Kronick, professor, medical bibliography, University of Texas Health Science Center at San Antonio, notes that "maintaining the anonymity of the author was a standard practice in the prize essay competitions (a sort of early form of sponsored research) of eighteenth-century scientific societies, which had elaborate devices to maintain the anonymity of contributors."[31]

The rationale behind double-blind refereeing, as was pointed out in an appropriately anonymous editorial in *Nature*, is that referees could still be frank about a manuscript's shortcomings without fear of ruining working relationships or being subjected to the anger of rejected authors.[32] Such a system would also, in the opinion of J. Scott Armstrong, Wharton School, University of Pennsylvania, Philadelphia, "reduce the prejudice against unknown authors from low-status institutions."[33]

Many justify the present system by citing what Marcel C. La Follette, editor, *Science, Technology, & Human Values*, calls the "crackpot avoidance" theory.[34] According to this idea, an author's record of achievement and the stamp of legitimacy provided by the author's institutional affiliation help referees evaluate manuscripts because they constitute presumptive "proof" that the research described was really done. La Follette says that accepting manuscripts without regard for the potential of misrepresentation or error is unwise, but she points out that a prestigious affiliation is no guarantee against fraud—in fact, it may even help the perpetrator evade detection.

According to John Moossy, editor-in-chief, and Yvonne R. Moossy, managing editor, *Journal of Neuropathology & Experimental Neurology*, a common objection to double-blind refereeing is a widespread conviction that experienced referees can identify authors despite the removal of the authors' names from their manuscripts.[35] In a study conducted to test this contention, they removed the names of authors and their departmental and institutional affiliations from 33 papers sent out for refereeing from May 1983 through April 1984. Each of the 67 referees, who filed a total of 85 reports, was asked to identify the authors and their departments or disciplines; 34 percent were able to make correct identifications. Eleven percent made incorrect identifications, and 55 percent would not even hazard a guess. Interestingly, only 9 referees objected to the double-blind procedure; a surprising number—24—had "no opinion," while 33 favored it, citing such reasons as greater objectivity and less risk of being swayed, either for good or ill, by the author's reputation.[35]

Another frequently proposed reform is "open refereeing." It is the exact opposite of double-blind refereeing: the referee's name is revealed to the author, who in turn is made known to the referee. Proponents argue that open refereeing might reduce the number of careless and superficial reports, on the presumption that referees will take more care with their reports if they have to sign their names to them. And in fact, I noted long ago that the time of the more qualified referees is of proportionately greater value; thus, they may sometimes be less than enthusiastic over the prospect of a manuscript to evaluate.[36] Anonymity is a dull spur to effort; "Aren't we all more likely to do something properly if our name is attached to it?" asks Ronald Mirman, Department of Physics, Long Island University, Brooklyn, New York, in a letter to the editor of the *American Journal of Physics*.[37]

Armstrong proposes that referees might designate a portion of their report to be signed and published along with the manuscript. He believes this would provide useful information to scientists because few readers can devote the kind of attention to a paper that a referee gives to it.[33] However, a number of problems might be encountered were referee anonymity abolished. For instance, the late Franz J. Ingelfinger, former editor, the *New England Journal of Medicine*, believed that "the referee who is several steps below the author on the status ladder" might be put in an uncomfortably vulnerable position and might even be unwilling to criticize candidly the manuscript in question.[38] Some reviewers might soften their objections to manuscripts, rather than jeopardize working relationships with the authors.[6] (p. 16) Identifying referees would also enable authors to get in touch with them. This might foster a communication process that excludes the editor, or even exposes referees to verbal attacks.[31]

The Perloffs have another suggestion for promoting a greater sense of responsibility among referees. They argue that paying referees would encourage them to perform their task more thoroughly and impartially.[26] Although they do not say how much referees should receive, they suggest that such fees could come from "authors' institutions, their research funding, or their personal resources."[26] They present no empirical evidence supporting their argument, but the notion of paying reviewers, like other ideas reported in this essay, could form the basis of an interesting study. In this case, the questions might be, "Do paid referees perform better than unpaid ones?" and "How much money does it take before a significant effect is noticed?"

Conclusion

It is difficult to draw substantive conclusions about how well the refereeing process functions. But Lock makes an interesting observation: the validating of experimental results and theoretical conclusions is ultimately not through the refereeing process but through the broader evaluation that articles receive over time at the hands of a larger, informed scientific community.[15] (p. 128) Of course, refereeing does not always detect fraud, plagiarism, errors, and muddy thinking. Still, it is probably impossible for most journals to switch to a system of in-house evaluation: despite its faults, real or imagined, refereeing is probably the most efficient and effective method for distinguishing the promising from the meretricious—at least, until it is *proven* otherwise.

In assessing refereeing's supposed flaws, one of the key issues seems to be delays in publication. Much of the accumulated anxiety about refereeing in many fields seems traceable to the tedious process that is often made out of what should be a straightforward decision. At the heart of many delays are referees who allow manuscripts to gather dust on their desks without informing editors that they cannot complete a review in a timely fashion.

As I see it, at the root of many of the alleged deficiencies of peer review are the attitudes of the scientific community itself. Were quality valued over quantity, and spurious "productivity" deplored instead of rewarded with tenure and promotions or research grants, then the incentive to publish shoddy or half-finished research would diminish. This might reduce the burden placed upon editors and reviewers because of the publish-or-perish syndrome. Unfortunately, we have not yet emerged from the stage of regarding the sheer number of publications as significant,[39] but there is a growing tendency to limit the number of papers to be listed on nominations for awards, grants, and so on.[40] And in fact, one of the often-stated goals of citation analysis is to encourage quality, high-impact work, rather than publication for the sake of pure output.

Of the myriad comments about refereeing, it is difficult to find one brief, all-encompassing statement that says it all. But John Ziman, Imperial College of Science and Technology, London, UK, and editor, *Science Progress*, has come close. In a commentary on Peters and Ceci, he wrote:

> Informed discourse on the primary communication system of science takes for granted the basic utility and reliability of the peer-review process, at least up to some modest practical level of human competence. The height of this level should not be exaggerated: It is not an indicator of permanent scientific worth. Acceptance for publication by a reputable journal implies no more than that the work is superficially sound, mildly interesting, and moderately original. The opinion that it should at least be taken into consideration by other scientists is only a preliminary assessment, likely to be contradicted and entirely superseded in the light of further study. Nevertheless, this weak and uneven standard of quality appears real enough to the authors, editors, and reviewers who tussle endlessly to establish and maintain it. Specific accusations of prejudice, inquiries concerning systematic bias, and demands for institutional reform have all been addressed to imperfection of performance around and about this hypothetical benchmark.[41]

The question of refereeing must be discussed in the larger context of peer review for funding research. In the next part of this essay, I hope to review the anecdotal as well as systematic information available. But refereeing and peer review are ethical and sociopolitical issues scientists must review periodically. Democratic institutions are dynamic. We want to retain the best of what we have had, but we must be willing to change that which no longer satisfies the needs of a changing world.

Postscript

Since it is a primary mission of ISI Press® to publish books on the process of scientific communication, it has published several such works mentioned in this essay. Several more, including Lock's *A Difficult Balance: Editorial Peer Review in Medicine*,[15] will be printed or reprinted by ISI Press in the fall. They are: *Medical Style and Format: an International Manual for Authors, Editors, and Publishers*[42] and *How to Write and Publish Papers in the Medical Sciences*,[43] by Edward J. Huth, editor, *Annals of Internal Medicine*; *How to Copyedit Scientific Books and Journals*,[44] by Maeve O'Connor, CIBA Foundation, London, UK; and *An Insider's Guide for Medical Authors and Editors*,[45] by Peter Morgan, scientific editor, *Canadian Medical Association Journal*. Incidentally, Lock's book contains a bibliography of over 200 references—some of which appear following the references in this essay in the selected bibliography. In a review[46] of Lock's book, Alfred Yankauer, editor, *American Journal of Public Health*, says it is "an invaluable reference for all those interested in the editorial process." In his review, he quotes a passage from Alexander Pope[47] that he feels "captured the essence" of Lock's views on refereeing and the editor's role. Yankauer suggests that for the word "critic," the reader should substitute "editor" or "referee/reviewer."[46]

> But you who seek to give and merit fame,
> And justly bear a Critic's noble name,
> Be sure yourself and your own reach to know,
> How far your genius, taste and learning go;
> Launch not beyond your depth, but be discreet,
> And mark that point where sense and dullness meet....
>
> Careless of censure, nor too fond of fame;
> Still pleas'd to praise, yet not afraid of blame;
> Averse alike to flatter or offend;
> Not free from faults, nor yet too vain to mend.
>
> Alexander Pope
> *An Essay on Criticism*

* * * * *

My thanks to Stephen A. Bonaduce and Terri Freedman for their help in the preparation of this essay.

REFERENCES

1. **Garfield E.** Refereeing and peer review. Part 1. *Current Contents* (31):3-11, 4 August 1986.
2. **Cole S, Rubin L & Cole J R.** *Peer review in the National Science Foundation: phase one of a study.* Washington, DC: National Academy of Sciences, 1978. 193 p.
3. **Cole J R & Cole S.** *Peer review in the National Science Foundation: phase two of a study.* Washington, DC: National Academy Press, 1981. 106 p.
4. **Russell A S, Thorn B D & Grace M.** Peer review: a simplified approach. *J. Rheumatol.* 10:479-81, 1983.
5. **Sanders H J.** Peer review. How well is it working? *Chem. Eng. News* 60(11):32-43, 1982.
6. **Gordon M.** *Running a refereeing system.* Leicester, UK: Primary Communications Research Centre, University of Leicester, 1983. 56 p.

7. **Dehmer P.** APS reviews refereeing procedures. *Phys. Today* 35(2):9; 95-7, 1982.
8. **Bishop C T.** *How to edit a scientific journal.* Philadelphia: ISI Press, 1984. 138 p.
9. **McCaffery M.** Peer review—or sneer review? *Can. Fam. Physician* 29:857, 1983.
10. **Lundberg G D.** Appreciation to our peer reviewers. *JAMA—J. Am. Med. Assn.* 251:758; 817-23, 1984.
11. **Amiry P.** Refereeing for *JORS. J. Oper. Res. Soc.* 34:1025-6, 1983.
12. **Zuckerman H & Merton R K.** Patterns of evaluation in science: institutionalisation, structure and functions of the referee system. *Minerva* 9:66-100, 1971. [Reprinted as: Institutionalized patterns of evaluation in science. (Merton R K.) *The sociology of science.* Chicago, IL: University of Chicago Press, 1973. p. 460-96.]
13. **Cornforth J W.** Letter to editor. (Referees.) *New Sci.* 62:39, 1974.
14. **Day R A.** *How to write and publish a scientific paper.* Philadelphia: ISI Press, 1983. p. 82.
15. **Lock S.** *A difficult balance: editorial peer review in medicine.* London: Nuffield Provincial Hosptials Trust, 1985. 172 p.
16. **Peters D P & Ceci S J.** Peer-review practices of psychological journals: the fate of published articles, submitted again. *Behav. Brain Sci.* 5:187-95, 1982.
17. **Lock S.** Peer review weighed in the balance. *Brit. Med. J.* 285:1224-6, 1982.
18. **Peters D P & Ceci S J.** A manuscript masquerade. *Sciences* 20 (7):16-9; 35, 1980.
19. **Ross C.** Rejected. *New West* 4(4):39-43, 1979.
20. **Kosinski J.** *Steps.* New York: Random House, 1968. 147 p.
21. **Tax S & Rubinstein R A.** Responsibility in reviewing and research. *Behav. Brain Sci.* 5:238-40, 1982.
22. **Yalow R S.** Competency testing for reviewers and editors. *Behav. Brain Sci.* 5:244-5, 1982.
23. **Thomas G J.** Perhaps it was right to reject the resubmitted manuscripts. *Behav. Brain Sci.* 5:240, 1982.
24. **Beyer J M.** Explaining an unsurprising demonstration: high rejection rates and scarcity of space. *Behav. Brain Sci.* 5:202-3, 1982.
25. **Whitehurst G J.** The quandary of manuscript reviewing. *Behav. Brain Sci.* 5:241-2, 1982.
26. **Perloff R M & Perloff R.** Improving research on and policies for peer-review practices. *Behav. Brain Sci.* 5:232-3, 1982.
27. **Rosenthal R.** Reliability and bias in peer-review practices. *Behav. Brain Sci.* 5:235-6, 1982.
28. **Goodstein L D & Brazis K L.** Psychology of scientist: XXX. Credibility of psychologists: empirical study. *Psychol. Rep.* 27:835-8, 1970.
29. **Gordon M D.** The role of referees in scientific communication. (Hartley J, ed.) *The psychology of written communication.* New York: Nichols, 1980. p. 263-75.
30. **Zinder N D.** *Editing without reviewers; or the review process—a protection from what?* Unpublished speech presented to the Society of Editors, 19 May 1969. Cambridge, MA. 6 p.
31. **Kronick D A.** Personal communication. 19 June 1986.
32. In defence of the anonymous referee. *Nature* 249:601, 1974.
33. **Armstrong J S.** The ombudsman: is review by peers as fair as it appears? *Interfaces* 12(5):62-74, 1982.
34. **La Follette M C.** On fairness and peer review. *Sci. Technol. Hum. Val.* 8(4):3-5, 1983.
35. **Moossy J & Moossy Y R.** Anonymous authors, anonymous referees: an editorial exploration. *J. Neuropathol. Exp. Neurol.* 44:225-8, 1985.
36. **Garfield E.** Publishing referees' names and comments could make a thankless and belated task a timely and rewarding activity. *Essays of an information scientist.* Philadelphia: ISI Press, 1977. Vol. 1. p. 435-7.
37. **Mirman R.** Letter to editor. (For open refereeing.) *Amer. J. Phys.* 43:837, 1975.
38. **Ingelfinger F J.** Peer review in biomedical publication. *Amer. J. Med.* 56:686-92, 1974.
39. **Garfield E.** How to use citation analysis for faculty evaluations, and when is it relevant? Parts 1&2. *Op. cit.*, 1984. Vol. 6. p. 354-72.
40. **Angell M.** Publish or perish: a proposal. *Ann. Intern. Med.* 104(2):261-2, 1986.
41. **Ziman J.** Bias, incompetence, or bad management? *Behav. Brain Sci.* 5:245-6, 1982.
42. **Huth E J.** *Medical style and format: an international manual for authors, editors, and publishers.* Philadelphia: ISI Press. (In press.)
43. --------------. *How to write and publish papers in the medical sciences.* Philadelphia: ISI Press. (In press.)
44. **O'Connor M.** *How to copyedit scientific books and journals.* Philadelphia: ISI Press. (In press.)
45. **Morgan P.** *An insider's guide for medical authors and editors.* Philadelphia: ISI Press. (In press.)
46. **Yankauer A.** Review of "A difficult balance: editorial peer review in medicine" by S. Lock. *CBE Views* 9(2):51-2, 1986.
47. **Pope A.** *Pastoral poetry and an essay on criticism.* (Audra E & Williams A, eds.) London: Methuen, 1961. p. 244; 326.

(SUPPLEMENTARY BIBLIOGRAPHY FOLLOWS)

SUPPLEMENTARY BIBLIOGRAPHY ON REFEREEING

Armstrong J S. Peer review of scientific papers. *J. Biol. Resp. Modif.* 3:10-4, 1984.

Beck C W. Trouble in the hedgerows. *J. Archaeol. Sci.* 12:405-9, 1985.

Crane D. The gatekeepers of science: some factors affecting the selection of articles for scientific journals. *Amer. Sociol.* 2:195-201, 1967.

Dixon G F, Schonfeld S A, Altman M & Whitcomb M E. The peer review and editorial process: a limited evaluation. *Amer. J. Med.* 74:494-5, 1983.

Fox T. *Crisis in communication.* London, UK: Athlone Press, 1965. 59 p.

Gardner M J, Altman D G, Jones D R & Machin D. Is the statistical assessment of papers submitted to the "British Medical Journal" effective? *Brit. Med. J.* 286:1485-8, 1983.

Harnad S. Rational disagreement in peer review. *Sci. Technol. Hum. Val.* 10(3):55-62, 1985.

--------------. Review of "A difficult balance" by S. Lock. *Nature* (In press.)

--------------, ed. *Peer commentary on peer review: a case study in scientific quality control.* Cambridge, UK: Cambridge University Press, 1982. 71 p. (Reprinted from: *Behav. Brain Sci.* 5:185-255, 1982.)

Juhasz S, Calvert E, Jackson T, Kronick D A & Shipman J. Acceptance and rejection of manuscripts. *IEEE Trans. Prof. Comm.* PC18:177-85, 1975.

Koshland D E. Memorandum to Universal Science Foundation. *Science* 229:921, 1985.

Light R J & Pillemer D B. *Summing up. The science of reviewing research.* Cambridge, MA: Harvard University Press, 1984. 191 p.

Lloyd J E. On watersheds and peers, publication, pimps and panache. (An editorial abstract.) *Fla. Entomol.* 68:134-9, 1985.

Maddox J. Privacy and the peer-review system. *Nature* 312:497, 1984.

Mahoney M J. Open exchange and epistemic progress. *Amer. Psychol.* 40:29-39, 1985.

Meadows A J. The problem of refereeing. *Scientia* 112:787-94, 1977.

Miller A C & Serzan S L. Criteria for identifying a refereed journal. *J. Higher Educ.* 55:673-99, 1984.

Morgan P P. When reviewers disagree. *Can. Med. Assn. J.* 129:1172-3, 1983.

--------------. Anonymity in medical journals. *Can. Med. Assn. J.* 131:1007-8, 1984.

--------------. Author, editor and reviewer: how manuscripts become journal articles. *Can. Med. Assn. J.* 124:664-6, 1981.

Patterson K & Bailar J C. A review of journal peer review. (Warren K S, ed.) *Selectivity in information systems: survival of the fittest.* New York: Praeger, 1985. p. 64-82.

Shils E. The confidentiality and anonymity of assessment. *Minerva* 13:135-51, 1975.

Silver S. Ethical questions in the peer review system. *ASM News.* 46:302-6, 1980.

Smith B M & Gough P B. Editors speak out on refereeing. *Phi Delta Kappan* 65:637-9, 1984.

Stossel T P. Reviewer status and review quality: experience of the *Journal of Clinical Investigation. N. Engl. J. Med.* 312:658-9, 1985.

Strasburger V C. Righting medical writing. *JAMA—J. Am. Med. Assn.* 254:1789-90, 1985.

Suppa R J & Zirkel P A. The importance of refereed publications: a national survey. *Phi Delta Kappan* 64:739-40, 1983.

Whitehurst G J. Interrater agreement for journal manuscript reviews. *Amer. Psychol.* 39:22-8, 1984.

--------------. Interrater agreement for reviews for *Developmental Review. Develop. Rev.* 3:73-8, 1983.

--------------. On lies, damned lies, and statistics: measuring interrater agreement. *Amer. Psychol.* 40:568-9, 1985.