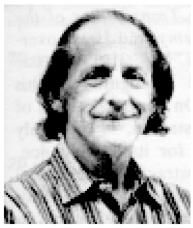
Essays of an Information Scientist, Vol:9, p.230, 1986
Current Contents, #31, p.3-11, August 4, 1986



Current Comments®

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

Refereeing and Peer Review. Part 1. Opinion and Conjecture on the Effectiveness of Refereeing

Number 31 August 4, 1986

Peer review is so much a part of the fabric of scholarly inquiry that it is often taken for granted. I have written many essays over the years that are directly or indirectly related to peer review. These include several on authorship[1-3] and editing,[4] faculty evaluation,[5] identifying Nobel-class science through citation analysis[6-9]—and even a few on various aspects of refereeing itself.[10-12] But I have never before discussed the intricacies of the system in detail. Since the subject is central to scholarly life, we have decided to devote a three-part essay in *Current Contents®* to it.

The first two parts will cover refereeing for publication. Part 1 examines how the refereeing system works and lists some of the common opinions about its advantages and disadvantages. Part 2 will cover scientific studies of refereeing and some proposed alternatives to the present system. Part 3 will follow later and will focus on the peer review of grant proposals. Note that I distinguish between a *referee* (one who evaluates an article before it is published) and a *reviewer* (one who evaluates already published material or, in the case of grant reviews, research-grant proposals). I generally use the term *referee* to mean one who advises editors on the publishability of a scholarly manuscript. The process by which this advice is solicited I usually call *refereeing*, but occasionally *reviewing* or *peer review* seems appropriate. The term *peer review* is also used to denote the evaluation of research proposals; more generally, it can refer to the professional review of patient records by special committees of physicians that many hospitals use to maintain high-quality patient care.

Refereeing: How It Came About and How It Works

Refereeing is meant to ensure that articles submitted for publication meet the accepted standards of their fields. Like editing, refereeing is a complex intellectual, political, and social process; it often involves a spectrum of activities that blend into one another in complex ways, in a fashion similar to the range of practices related to ghostwriting.[3] Among many who have expressed the idea, Peter Amiry, former editor, *Journal of the Operational Research Society*, wrote in an editorial that referees are an editor's insurance policy, providing a reservoir of knowledge that few editors could hope to match.[13]

The practice of refereeing manuscripts prior to publication is now well established, but it was not always so, state sociologists Harriet Zuckerman and Robert K. Merton, Columbia University, New York, in their classic 1971 study of patterns of evaluation in science.[14] It evolved in response to the development of scholarly societies and the scientific journal. I summarized this and other work in an earlier essay on the changes in scientific communication over the past 300 years.[15]

According to David A. Kronick, professor of medical bibliography, University of Texas Health Science Center at San Antonio, "science in the seventeenth and eighteenth centuries...differed in many ways socially, intellectually, and economically from the science of the twentieth century."[16] Although associations and societies promoting scholarly activities had existed for hundreds of years,[17] (p. 46), the social role of "scientist," as well as conventions for doing research, had yet to emerge.[16] In fact, Kronick notes, "individuals did not begin to regard themselves as scientists rather than philosophers until the seventeenth century."[17] (p. 34)

The learned journal as we know it today also traces its origins to the seventeenth century, with the founding of the *Philosophical Transactions of the Royal Society of London* and the *Journal des Sçavans*, associated with the *Académie des Sciences* in Paris.[14] By the early eighteenth century, Kronick says, members of these and other scholarly societies sponsoring official or semiofficial publications began to realize that if scholars were to have confidence in the content of these journals, then material submitted for publication had to be critically evaluated before it was published.[16]

Societies thus began to take measures to preserve their credibility. Some adopted strict regulations governing publication that members had to comply with to retain their membership. And by the mid-eighteenth century, according to Kronick, some—such as the Royal Society of Medicine of Edinburgh, Scotland—had developed techniques of evaluating and approving manuscripts before publication that are almost indistinguishable from today's system of refereeing.[16] Kronick, incidentally, is the author of a recent book on the literature of the life sciences that includes a short section on the refereeing and the publication process in that branch of science.[18]

The procedures involved in refereeing a manuscript vary from journal to journal and from field to field, but there are certain general steps that virtually every paper has to go through before it is published. Among the first steps an editor takes, whether or not the journal is refereed, is to evaluate a submission's compatibility with the scope and style of the journal, according to Robert A. Day, consultant, ISI Press®, and former managing editor, American Society for Microbiology (ASM) journals.[19] Once this is done, an editor must then choose appropriate referees for a given manuscript.

Donald Christiansen, editor, *IEEE Spectrum*, conducted a survey of referee selection practices among 26 of the *IEEE Transactions* editors. Common sources from which referees are recruited include widely recognized experts, members of a journal's editorial board, professional acquaintances, previous referees, and scientists cited in the author's references.[20] Sometimes authors are asked to supply a list of suggested referees. A few journals are using manual and computer-assisted bibliographic retrieval methods to select referees. For example, Stevan Harnad, editor, *Behavioral and Brain Sciences* (*BBS*), reports that *BBS* staffers search a microcomputer file of the journal's referees that has been coded by areas of expertise. They also search the current biobehavioral literature through the *Science Citation Index*® and the *Social Sciences Citation Index*® for additional referee candidates.[21,22]

Usually two referees are chosen, according to Claude T. Bishop, director, Division of Biological Sciences, National

Research Council of Canada (NRCC), and editor-in-chief, NRCC Research Journals. "The merits of this system," he writes, "are that it usually provides at least one solid [report], that the two [referees] can be checked against each other, and that one referee may cover points that the other missed."[23] But Harnad notes that, for many journals, the "number of referees [selected for a manuscript] is an empirical matter requiring research."[21] *BBS* uses five to eight referees per paper. In Harnad's experience, such a sample is more likely to produce a balanced review.[24]

Along with the manuscript, referees generally receive a list of instructions and a form for comments and recommendations. Routinely, referees respond within a few weeks, recommending either publication or rejection or requesting modifications; they often include specific comments for both the author and the editor.

A paper is most likely to be accepted, according to Michael Gordon, research associate, Primary Communications Research Centre, University of Leicester, UK, when the referees agree that it meets three criteria.[25] (p. 6-8) First, it should be *sound*. The author(s) should have employed reliable research techniques, drawn valid conclusions, and committed no flaws of logic. It should also be *original*, in the sense that its findings have never before been published. Finally, it should be *significant*, meaning that it should contain some new perspective or observation of potential importance.[25] (p. 6-8) Of course, published articles meet these criteria in varying degrees.

Referees do not always agree with one another, and some authors take this as evidence that the system is unreliable or capricious. But disagreement is at the heart of scientific inquiry. Harnad says that "the current and vital ongoing aspect of science consists of an active and often heated interaction of data, ideas, and minds, in a process one might call 'creative disagreement.' "[26] Moreover, reviewer disagreements are not simply shrugged off; editors generally resolve each dispute on an individual basis. Gordon described some of the options open to editors for dealing with these conflicts.[25] (p. 20-5) When reviewer disagreements are mild, for example, editors may rely on their own judgment to resolve them—with, perhaps, some communication with the author.[25] (p. 21) When differences are profound, editors may reject the paper without further reviewing or they may send the manuscript out for review once again, together with the comments of the disputing referees. Editors may also ask the author to respond to the referees' observations. After the "arbitrating" referee(s) and the author have reported, editors should be in a better position to make a final judgment. When authors take exception to referees' comments and provide editors with a point-by-point refutation, editors often follow a procedure similar to the one just outlined for adjudicating disputes between referees.[25] (p. 22-5)

Research, Pseudo-Research, or Non-Research?

The results of our literature search for this essay support the view that refereeing is an issue clouded with subjectivity and emotionalism—at least for a vocal minority. The dominant vehicle of discussion in the debate about the effectiveness of refereeing has been editorials and correspondence. Some contain incisive discussions, but with little or no empirical evidence to support what amounts to a litany of opinion and anecdote. Indeed, in an endeavor such as science, which depends on dispassionate logic and systematic evidence for much of its credibility, the dearth of rigorous thinking and hard data in the correspondence of many who are critical of refereeing is remarkable. Of the relatively few controlled studies that have been done, many suffer from such severe methodological shortcomings that their conclusions are questionable. More will be said about research on refereeing in Part 2.

Refereeing and other forms of peer review have been discussed at length, especially in the four decades since World War II, but discussion alone does not constitute science or scholarship. Since we are all affected by peer review, it is not surprising that so many of us have opinions on the subject. Yet the literature representing controlled studies of peer review is either pitifully small or disgracefully absent, while the body of anecdote and opinion is quite large. We carefully distinguish here between studies, experiments, experience, and opinions.

In researching this essay, we also found that most published opinion on refereeing is negative. But we suspect that this is due, ironically, to the widespread acceptance of and satisfaction with the current system of peer review: most scientists simply do not feel that refereeing needs defending, so positive opinions are relatively scarce. It should also be kept in mind that these opinions on refereeing are themselves unrefereed. Furthermore, the existence and ranking of hundreds of refereed journals is concrete evidence that they are the preferred medium of publication.

Flaws in the System?

In a note published in the *New England Journal of Medicine* (*NEJM*), John C. Bailar III and Kay Patterson, Harvard School of Public Health, Boston, Massachusetts, speculate that current opinion on refereeing seems divided among one or more of four paradigms.[27] Based on their own informal observations, the authors assert that many scientists seem to perceive the process as a sieve, sifting the wheat from the chaff. Many also liken the process to a smithy, in which "papers are pounded into new and better shapes between the hammer of peer review and the anvil of editorial standards." Some seem to view it as a switch, reflecting the widespread belief that a persistent author can eventually publish a manuscript somewhere (although refereeing may determine exactly where). Finally, some scholars seem to consider refereeing a capricious and essentially unpredictable process—a "shot in the dark."[27]

Stephen Lock, editor, *British Medical Journal*, feels that refereeing "favours unadventurous nibblings at the margin of truth rather than quantum leaps."[28] An example supporting his opinion is the reception given the early demonstration, via radioimmunoassay, of insulin-binding antibody by the late Solomon A. Berson and Rosalyn S. Yalow, Veterans Administration, New York. This work was fundamental to the development of the radioimmunoassay into a "powerful tool for determination of virtually any substance of biologic interest," according to Yalow.[29] Although Yalow would share the 1977 Nobel Prize with Roger Guillemin, Salk Institute, San Diego, and Andrew Schally, Veterans Administration Hospital, New Orleans, the initial research concerning radioiodine-labeled insulin was rejected both by *Science* and, at first, by the *Journal of Clinical Investigation* (*JCI*) as erroneous.[29]

Nevertheless, when the paper was revised to meet the objections of reviewers, it was published in the *JCI*.[30] A comparatively recent poll of the authors of manuscripts rejected by the *JCI*, conducted by editor Jean D. Wilson, Department of Internal Medicine, University of Texas Health Science Center at Dallas, found that 85 percent of the rejected papers were subsequently published elsewhere. And Wilson also reported that "most of the authors of the [other] 15 percent...were convinced by the review process that [their papers] were either unoriginal or wrong."[31]

Delays in Publication

In addition to charges that referees make too many serious mistakes, complaints also focus on the delays in publication that many attribute to the refereeing process. While conceding the value of thorough, constructive reports by referees, Richard Shea, editor, *Transactions on Nuclear and Plasma Sciences*, is nevertheless concerned about the time lost during the refereeing process; he is quoted by Christiansen as saying that "the ultimate referee is the reader."[20] And as noted by Kronick, the historical significance of papers ultimately depends on this reader evaluation and readers' willingness to cite what impresses them.[32] But one of the reasons for the existence of the refereeing system is that readers of scientific articles have varying interests and backgrounds; they *must* be able to rely on a high degree of validity in what they read, especially if it is somewhat outside their field.

Real or perceived, delays in publication resulting from refereeing may be the most prevalent concern among scientists, who may have job security, promotions, or the need to establish priority for a discovery hanging in the balance. In a note in *NEJM*, Thomas P. Stossel, Massachusetts General Hospital, Boston, voices his concern that the commercial potential of many new discoveries, especially in biotechnology, is giving rise to new and particularly taxing demands for rapid publication.[33]

In an editorial, Lawrence D. Grouse offers several explanations, based on his experience as senior editor of *JAMA*, for the lag time between submission and publication: "Excellent manuscripts are often criticized by reviewers with vested interests or contrary views. Overcritical reviewers flay manuscripts for minor or supposed deficiencies.... Reviewers may also cynically delay the appearance of research competing with their own."[34] And in a 1979 editorial in the *Journal of Clinical Psychiatry*, associate editor Marc H. Hollender asked "why it takes three months or longer to review an article that takes three minutes to read and perhaps took less than three months to write.... Does it take the referee that long to come to a conclusion and to dictate comments? It is more likely that the article gathers dust among other low-priority items."[35] In short, if I may use an old, informal phrase, referees should either fish or cut bait.

Bias and Unethical Behavior

Of all the complaints about refereeing, however, some of the most bitter—though not the most prevalent—concern the issue of referee bias (although little *uncontested* empirical evidence exists to indicate that authors' affiliations and the reputations of their institutions affect a referee's evaluation). Assuming that some bias exists, however, historian of science Donald deB. Beaver, Williams College, Williamstown, Massachusetts, suggests that a preconceived suspicion of scientific "have-nots" may be explained in terms of the second part of the "Matthew effect."[36] This concept, introduced by Merton in 1968,[37] draws an analogy between the misallocation of scientific credit and a passage from the gospel of St. Matthew: "Unto every one that hath shall be given, and he shall have abundance: *but from him that hath not shall be taken away even that which he hath*" (emphasis ours). Presumably, contributions from unknown scholars from unrecognized or little-known institutions are less likely to be accepted for publication than occasionally comparable contributions by scholars of great repute.

Some cases of questionable referee ethics have been documented. Perhaps the most publicized example, according to a 1984 article by free-lance medical writer Barbara Fox in *Medical Communications*, the journal of the American Medical Writers Association,[38] was one reported on by former *Science* staff writer William J. Broad.[39] It involved a paper submitted by Helena Wachslicht-Rodbard, NIH, Bethesda, Maryland, to *NEJM*. The paper was assigned to two referees, one of whom recommended acceptance, while the other—Vijay Soman of Yale University, who had similar research in progress—recommended rejection. Arnold Relman, editor, *NEJM*, informed Wachslicht-Rodbard that her paper had "engendered considerable differences of opinion among our referees"[39] and told her the manuscript was unacceptable unless revised.

But the matter was far from over. Soman had photocopied Wachslicht-Rodbard's study and, without informing his coauthor, Philip Felig, vice chairman of the Department of Medicine at Yale, of what he had done, sent their article incorporating the plagiarized data to the *American Journal of Medicine*, of which Felig was an associate editor. By coincidence, the journal sent the article out for review to Wachslicht-Rodbard's superior, who showed it to her. It contained more than a dozen passages, verbatim, from her own manuscript; she wrote to Relman accusing Felig and Soman of plagiarism and conflict of interest in the refereeing of her paper. Relman agreed that it had been highly improper for Soman to agree to even read the paper, which was later published in the *NEJM* under Wachslicht-Rodbard's name.[40]

The abuse of anonymity is a longstanding matter of concern. In an article appearing in *New Scientist*, biochemist Robert Jones, Royal College of Surgeons, London, asserted that "the act of submission of a paper can place the author at the mercy of the malignant jealousy of an anonymous rival."[41] The belief seems to be that, from behind the walls of their fortress of anonymity, referees are free to hurl at authors volleys of invective that cannot be effectively countered. "Anonymity tends to bring out the worst in people," according to Heinz Fraenkel-Conrat, Department of Molecular Biology and Virus Laboratory, University of California, Berkeley, in a letter to the editors of *Nature*.[42] "I was recently asked to review, and advocated rejection of, a paper for a virological journal on the basis of factual comments which I would have been quite willing to sign. The editor sent me, out of courtesy, copies of his rejection letter together with the other referee's sarcastic poison-pen comments, also rejecting the paper. There was no justification for one civilised person insulting another in such a manner.... That outburst was solely the joy of releasing adrenalin with anonymous impunity."[42] While Fraenkel-Conrat's analysis may be correct in this situation, there is little evidence, other than anecdotal, that this is a widespread phenomenon. But it suggests fertile ground for study: do *ad hominem* comments—those leveled at authors, as distinct from strong opinions about the authors' text—occur more frequently in signed or in unsigned reviews?

In a "Guest Comment" published in *Physics Today*, F. Curtis Michel, professor of space physics and astronomy, Rice University, Houston, calls for referees to back up their comments. "Accountability is now all directed back at the author," he writes.[43] "If there is any dispute, it is entirely the authors' fault because they have 'failed to convince their peers.' Here, the word 'peer' has a nice ring of fairness to it.... However,... when a group of colleagues is permitted to have [their] comments taken as some kind of gospel, [they] are no longer peers but quite definitely superiors insofar as power and influence go."[43] It is in answer to just this kind of criticism, Harnad reports, that *BBS* is conducting an internal, statistical study of, among other things, the relationships among anonymity, referees' ratings of manuscripts, and authors' ratings of the usefulness of referee reports.[24]

Another criticism of the system is of the "Newcomb variety." I have often referred to the career of Simon Newcomb, who proved conclusively—just months before the Wright Brothers took off from the sands of Kitty Hawk—that a flying machine was impossible.[44,45] Sometimes this type of rejection is the result of referees who are hostile to innovative ideas or to those that clash with their own.[41] We don't know how often thoughtful, conscientious scientists—in good faith and in keeping with currently accepted theory—rendered an opinion concerning the implausibility of a given idea or theory, only to see that theory become the basis of a dramatic paradigm shift. Still, referees and journal editors should not consider such rejection experience as sufficient reason for extending some kind of "publication *carte blanche*" to would-be authors who want to prove, for example, that perpetual-motion machines are possible. I continue to be in favor of refereeing that prevents the publication of intellectual atrocities, including papers with inadequate documentation. For those articles straddling the border between science and speculation, there exist publications such as *Speculations in Science and Technology*, which was started specifically as a forum for the publication of ideas lacking support "in established theoretical and experimental work," according to an article by founder William M. Honig, senior lecturer in the physical sciences and engineering, Western Australian Institute of Technology, Perth, in the *Sciences*.[46]

Refereeing and Garfield's Uncertainty Principle

It is easy to "prove" on the basis of anecdotal evidence that the refereeing system doesn't work. From the hundreds of published *Citation Classics®* commentaries—such as those written by Oscar Buneman, Stanford University, California,[47] and Hans Lineweaver, US Department of Agriculture, Washington, DC[48]—or in correspondence with their authors, we know that dozens of significant papers have been rejected by some journals for various reasons. Some of these reasons might be described as "N-I-H," that is, "not invented here." Nevertheless, much scientific quackery is exposed by careful, insightful, constructive refereeing, and this far outweighs the ideas that have allegedly been suppressed because of referees who would not give them a chance to see the light of day.

A scientist's appreciation of the collaborative, communal goal of refereeing—protecting science and the public from errors and inferior work—varies according to a host of factors, including the scientist's age, status, and temperament. Famous, tenured, or established researchers may be better able to weather the occasional rejection notice than scientists just starting their careers and trying to make their mark. No other activity is as fundamental to democratic scholarship as refereeing. From all this, I concluded that there is an Uncertainty Principle of Refereeing: The more we have of it, the less we like it—but the less we have of it, the more we miss it.

We sometimes trivialize what we take for granted. Refereeing has been around for so long that it's easy to forget that it wasn't always there. The present stage of its evolution will be affected by social and technological factors such as funding and electronic publishing. But the public discourse of scholarship, both formal and informal, is essential to the very existence of science. In the modern era of big science—and by that I mean both large-scale projects and large numbers of projects, whether small or large—we must find ways to inculcate new research practitioners with the precepts and ideals that "naturally" were taught in the era of little science. We cannot allow squabbling over limited research funds to cloud the fundamental need to preserve the scientific *process* implied by refereeing. But we must recognize that the very size of the scientific enterprise may make it necessary to modify rigid application of the Ingelfinger rule[49] [promulgated by the late Franz J. Ingelfinger, former editor, *NEJM*, which states that papers submitted to *NEJM* must "have been neither published nor submitted elsewhere (including news media and controlled-circulation publications)"] or other precepts that may have been reasonable before the electronic revolution.

Indeed, the community of science may become even more relevant in the new communications age, and so we have to examine more carefully the consequences for intellectual property rights and methods of adjudicating disputes concerning priority of discovery. If much of this sounds Mertonian in tone it is no accident, since Robert K. Merton is one of the few scholars who has devoted great effort to the definition of the problems involved in research on refereeing. In fact, the work of Zuckerman and Merton will form a significant part of the discussion in Part 2 of this essay.

* * * * *

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

REFERENCES

1. **Garfield E.** From citation amnesia to bibliographic plagiarism. *Essays of an information scientist.* Philadelphia: ISI Press, 1981. Vol. 4. p. 503-7.
2. --------------. More on the ethics of scientific publication: abuses of authorship attribution and citation amnesia undermine the reward system of science. *Ibid.*, 1983. Vol. 5. p. 621-6.
3. --------------. Ghostwriting—the spectrum from ghostwriter to reviewer to editor to coauthor. *Current Contents* (48):3-11, 2 December 1985. (Reprinted in: *Essays of an information scientist: ghostwriting and other essays.* Philadelphia: ISI Press, 1986. Vol. 8. p. 460-8.)
4. --------------. Alternative forms of scientific publishing: keeping up with the evolving system of scientific communication. *Op. cit.*, 1981. Vol. 4. p. 264-8.
5. --------------. How to use citation analysis for faculty evaluations, and when is it relevant? Parts 1 & 2. *Ibid.*, 1984. Vol. 6. p. 354-72.
6. --------------. The 1984 Nobel Prize in medicine is awarded to Niels K. Jerne, César Milstein, and Georges J.F. Köhler for their contributions to immunology. *Current Contents* (45):3-18, 11 November 1985. (Reprinted in: *Essays of an information scientist: ghostwriting and other essays.* Philadelphia: ISI Press, 1986. Vol. 8. p. 416-31.)
7. --------------. The 1984 Nobel Prize in physics goes to Carlo Rubbia and Simon van der Meer; R. Bruce Merrifield is awarded the chemistry prize. *Current Contents* (46):3-14, 18 November 1985. (Reprinted in: *Essays of an information scientist: ghostwriting and other essays.* Philadelphia: ISI Press, 1986. Vol. 8. p. 432-43.)
8. --------------. The 1984 Nobel Prizes in economics and literature are awarded to Sir Richard Stone for pioneering systems of national accounting and to Jaroslav Seifert, the national poet of Czechoslovakia. *Current Contents* (49):3-13, 9 December 1985. (Reprinted in: *Essays of an information scientist: ghostwriting and other essays.* Philadelphia: ISI Press, 1986. Vol. 8. p. 469-79.)
9. --------------. Do Nobel Prize winners write Citation Classics? *Current Contents* (23):3-8, 9 June 1986.
10. --------------. Publishing referees' names and comments could make a thankless and belated task a timely and rewarding activity. *Op. cit.*, 1977. Vol. 1. p. 435-7.
11. --------------. Anonymity in refereeing? Maybe—but anonymity in authorship? No! *Ibid.*, 1977. Vol. 2. p. 438-40.

12. --------------. Reducing the noise level in scientific communication: how services from ISI aid journal editors and publishers. *Ibid.*, 1980. Vol. 3. p. 187-8.
13. **Amiry P.** Refereeing for *JORS. J. Oper. Res. Soc.* 34:1025-6, 1983.
14. **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.]
15. **Garfield E.** Has scientific communication changed in 300 years? *Op. cit.*, 1981. Vol. 4. p. 394-400.
16. **Kronick D A.** Authorship and authority in the scientific periodicals of the seventeenth and eighteenth centuries. *Libr. Quart.* 48:255-75, 1978.
17. --------------. *A history of scientific & technical periodicals.* Metuchen, NJ: Scarecrow Press, 1976. 336 p.
18. --------------. *Literature of the life sciences: reading, writing, research.* Philadelphia: ISI Press, 1985. 219 p.
19. **Day R A.** *How to write and publish a scientific paper.* Philadelphia: ISI Press, 1983. p. 82.
20. **Christiansen D.** Peer review reviewed. *IEEE Spectrum* 18:21, 1981.
21. **Harnad S.** Personal communication. 25 June 1986.
22. --------------. Commentary on "Computer-assisted referee selection as a means of reducing potential editorial bias" by H. Russell Bernard. *Behav. Brain Sci.* 5:202, 1982.
23. **Bishop C T.** *How to edit a scientific journal.* Philadelphia: ISI Press, 1984. p. 53.
24. **Harnad S.** Commentary on "Peer review and the *Current Anthropology* experience" by C. Belshaw. *Behav. Brain Sci.* 5:201, 1982.
25. **Gordon M.** *Running a refereeing system.* Leicester, UK: Primary Communications Research Centre, University of Leicester, 1983. 56 p.
26. **Harnad S.** Creative disagreement. *Sciences* 19(7):18-20, 1979.
27. **Bailar J C & Patterson K.** Journal peer review: the need for a research agenda. *N. Engl. J. Med.* 312:654-7, 1985.
28. **Lock S.** Letter to P.B.S. Fowler. 4 December 1984. *Brit. Med. J.* 290:1560, 1985.
29. **Yalow R S.** Radioimmunoassay: a probe for the fine structure of biologic systems. *Science* 200:1236-45, 1978.
30. **Berson S A, Yalow R S, Bauman A, Rothschild M A & Newerly K.** Insulin-I^{131} metabolism in human subjects: demonstration of insulin binding globulin in the circulation of insulin treated subjects. *J. Clin. Invest.* 35:170-90, 1956.
31. **Wilson I D.** Peer review and publication. *J. Clin. Invest.* 61:1697-701, 1978.
32. **Kronick D A.** Personal communication. 20 June 1986.
33. **Stossel T P.** Speed: an essay on biomedical communication. *N. Engl. J. Med.* 313:123-6, 1985.
34. **Grouse L D.** The Ingelfinger rule. *JAMA—J. Am. Med. Assn.* 245:375-6, 1981.
35. **Hollender M H.** Authors, editors and referees. *J. Clin. Psychiat.* 40:331, 1979.
36. **Beaver D D.** On the failure to detect previously published research. *Behav. Brain Sci.* 5:199-200, 1982.
37. **Merton R K.** The Matthew effect in science. *Science* 159:56-63, 1968. (Reprinted in: *The sociology of science.* Chicago, IL: University of Chicago Press, 1973. p. 439-59.)
38. **Fox B.** Peer review and the public's right to know: a look at the Ingelfinger Rule. *Med. Commun.* 12:33-7, 1984.
39. **Broad W J.** Imbroglio at Yale (I): emergence of a fraud. *Science* 210:38-41, 1980.
40. **Wachslicht-Rodbard H, Gross H A, Rodbard D, Ebert M H & Roth J.** Increased insulin binding to erythrocytes in anorexia nervosa. *N. Engl. J. Med.* 300:882-7, 1979.
41. **Jones R.** Rights, wrongs and referees. *New Sci.* 61:758-9, 1974.
42. **Fraenkel-Conrat H.** Letter to editor. (Is anonymity necessary?) *Nature* 248:8, 1974.
43. **Michel F C.** Solving the problem of refereeing. *Phys. Today* 35(12):9; 82, 1982.
44. **Garfield E.** Negative science and "The outlook for the flying machine." *Opt. cit.*, 1980. Vol. 3. p. 155-72.
45. **Newcomb S.** The outlook for the flying machine. *Independent* 55:2508-12, 1903.
46. **Honig W M.** Science's Miss Lonelyhearts. *Sciences* 24(3):24-7, 1984.
47. **Buneman O.** Citation Classic. Commentary on *Phys. Rev.* 115:503-17, 1959. *Current Contents/Engineering, Technology & Applied Sciences* 15(37):16, 10 September 1984.
48. **Lineweaver H.** Citation Classic. Commentary on *J. Amer. Chem. Soc.* 56:658-66, 1934. *Current Contents/Life Sciences* 28(11):19, 18 March 1985.
49. Definition of "sole contribution." *N. Engl. J. Med.* 281:676-7, 1969.