

# Current Comments®

EUGENE GARFIELD

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

## Requiring Standards for Scientific Journals Has Come of Age

Number 31

July 31, 1989

Does advertising compromise the issue of peer review audits of journals? In a recent *New York Times* article,<sup>1</sup> George D. Lundberg, editor of *JAMA—The Journal of the American Medical Association (JAMA)*, was reported to be reluctant to provide information on the performance of peer review. Other journal editors interviewed for the *Times* article, however, look more favorably on the idea of auditing procedures—not only for researchers, but also for journals and their editorial policies.<sup>1</sup>

Perhaps one of the most provocative commentaries on auditing was published in *THE SCIENTIST*® by Andrew Herxheimer,<sup>2</sup> clinical pharmacologist, Charing Cross Hospital and Westminster Medical School, London, UK. Herxheimer's article is reprinted here. He wants to know which journals are "quick and efficient" or "slow and erratic." Herxheimer's article has stimulated a great deal of comment in the press<sup>1</sup> as well as at the International Congress on Peer Review in Biomedical Publication in Chicago, May 10-12, 1989.

Stephen Lock, editor of the *British Medical Journal*; Alfred Yankauer, editor of the *American Journal of Public Health*; and Lundberg are willing to participate in audits conducted by scientists under accepted ground rules. However, Lundberg may have inadvertently revealed the essential conflict for scientific journals that also accept advertising. He would be reluctant, as *Times* reporter Lawrence K. Altman stated, "because it could provide crucial data to rivals that compete for articles, media attention and

advertising revenue."<sup>1</sup> This is an incredibly honest and forthright position that reveals the ambiguity in nonprofit professional society journals such as *JAMA*, the *New England Journal of Medicine, Science*, and other nonprofit journals. However, I suspect that none of these three journals would be the worse were they, in fact, to comply with Herxheimer's suggestions as well as with some of the measures outlined in the *Times* article. I'm sure that foreknowledge of even six-month delays would not deter some authors who are eager for the prestige associated with these journals. The Matthew effect<sup>3</sup> is ever present in all these matters. The most prestigious authors will often get the fastest review of their papers. On the other hand, some authors will prefer to guarantee priority of discovery over the value of publication in the highest impact journals.

At a recent centennial meeting of the Johns Hopkins University medical institutions, Baltimore, Maryland, I illustrated for the audience my law of concentration,<sup>4</sup> which, in fact, is an extension of an earlier formulation of Bradford's law of scattering.<sup>5</sup> Since the earliest days of *Current Contents*® and the *Science Citation Index*®, we have reminded readers that a small number of journals account for a large percentage of what is published and an even larger percentage of what is cited. Since my papers in *Science*<sup>6</sup> and *Nature*,<sup>7</sup> a small number of journals have joined this rather exclusive club. (See Table 1.) *Cell*, *Physical Review B—Condensed Matter*, *Biochemistry*, and *Physical Review Letters* are the only four

**Table 1: The 15 most-cited journals from the 1987 SCI® Journal Citation Reports®, with the year that each began publication. A=journal title. B=1987 citations.**

A	B
Journal of Biological Chemistry (1905)	163,605
Nature (1869)	155,736
Proceedings of the National Academy of Sciences of the United States of America (1863)	152,150
Journal of the American Chemical Society (1879)	114,212
Science (1880)	97,700
Journal of Chemical Physics (1931)	79,638
Biochimica et Biophysica Acta (1947)	66,819
New England Journal of Medicine (1812)	66,289
Physical Review Letters (1958)	66,174
Lancet (1823)	66,139
Journal of Immunology (1916)	58,895
Physical Review B—Condensed Matter (1970)	56,994
Cell (1974)	54,778
Biochemistry (1964)	49,684
Astrophysical Journal (1895)	47,575

journals founded in the last 31 years to find their way into the top 15, ranked by total citations.

Of course, newer journals trying to break into the more select group with the highest

impact and prestige will be more open because they have the motivation to compete for high-quality manuscripts. Not surprisingly, *Cell* and other “newcomers” are less reluctant to reveal such “confidential” information on peer review.

With all the concern at the congressional level about fraud in science<sup>8,9</sup> and accountability for tax dollars, there is concern that auditing of the peer review procedure is more than an academic question.

Lock’s book on peer review, *A Difficult Balance*,<sup>10</sup> is cited by Herxheimer. Lock is reported to have warned at the Chicago conference, “ ‘If we don’t put our own house in order, then those chaps in Congress or the House of Commons are going to do it for us.’ ”<sup>11</sup>

\* \* \* \* \*

*My thanks to C.J. Fiscus and James Mears for their help in the preparation of this essay.*

© 1989 ISI

## REFERENCES

1. Altman L K. Errors prompt proposals to improve ‘peer review’ at science journals. *New York Times* 6 June 1989. p. C3.
2. Herxheimer A. Make scientific journals more responsive—and responsible. *THE SCIENTIST* 3(6):9; 11, 20 March 1989.
3. Merton R K. The Matthew effect in science. *Science* 159:56-63, 1968.
4. Garfield E. *Has the information revolution changed research?* Speech presented to the Johns Hopkins Medicine Centennial, 7 June 1989, Baltimore, MD. 22 p.
5. ———. Bradford’s law and related statistical patterns. *Essays of an information scientist*. Philadelphia: ISI Press, 1981. Vol. 4. p. 476-83.
6. ———. Citation analysis as a tool in journal evaluation. *Science* 178:471-9, 1972. (Reprinted in: *Essays of an information scientist*. Philadelphia: ISI Press, 1977. Vol. 1. p. 527-44.)
7. ———. Significant journals of science. *Nature* 264:609-15, 1976. (Reprinted in: *Essays of an information scientist*. Philadelphia: ISI Press, 1983. Vol. 6. p. 573-9.)
8. ———. What do we know about fraud and other forms of intellectual dishonesty in science? Parts 1 & 2. *Current Contents* (14):3-7, 6 April 1987; (15): 3-10, 13 April 1987.
9. ———. Some deviant behavior in science has nothing at all to do with fraud. *Current Contents* (49):3-5, 7 December 1987.
10. Lock S. *A difficult balance: editorial peer review in medicine*. London: Nuffield Provincial Hospitals Trust, 1985. 172 p.
11. Steinbrook R. Diagnosing biomedical publications. *Los Angeles Times* 22 May 1989. Sec. II, p. 3.

## Make Scientific Journals More Responsive—And Responsible

By Andrew Herxheimer

---

Problems with the editorial policies and practices of scientific journals lead to recommendations that journals publish their editorial policies and peer review procedures and that audits of journals for the quality of the editorial process be made. An International Scientific Press Council is proposed.

---

Scientific journals welcome new subscribers without reservation. It is a pity that many are far less welcoming to the other group of people on whom they depend—their authors.

In one sense, it is inevitable that journals do not take pains to accommodate authors. Only a minority of scientists can succeed in having their work regularly published in the most respected and widely read journals in their field. Many papers are rejected by one or more journals before being accepted. What I don't understand, however, is why the editorial process has to be so wearying and frustrating for scientists. All of us probably have stories to tell about editors who took months to acknowledge the receipt of a manuscript, or who failed to publish after agreeing to do so, or who otherwise made things difficult for authors.

Some of us also know of cases of outright editorial misbehavior. In one recent example, the editor of a pharmacology journal invited a senior clinical pharmacologist in the Netherlands to write an article on drug regulation in his country. He did so, received an appreciative acknowledgment from the editor, and subsequently a proof of the article. Having returned the corrected proof, he heard no more. Letters to the editor remained unanswered. When asked, the publisher said he knew nothing about the paper, but confirmed that it had not been scheduled for publication. Then the same journal published an article on the same subject by other authors that contained some material that had been in the original article.

This is by no means an isolated example, and it highlights a major problem in the editorial process of scientific journals. Editors are well defended by their position, by their editorial boards, and by their publishers. An author who has been badly treated stands alone.

I believe, however, that authors can take steps to make mistreatment less likely and to

make the process of having papers published more efficient and tolerable in general.

One obvious tactic is being more choosy about which journals we send papers to. We might get better treatment if we take pains to find out which publications are showing the most interest in our particular topics. More important, we need to learn about the quality of the editorial process at various journals. Where is it quick and efficient, and where is it slow and erratic? Do some editors—and the editorial process—seem intelligent, helpful, and creative, while others are dull and bureaucratically viscous?

Some of this information is easily available. Most journals print dates of acceptance, and many also include submission dates, thereby disclosing how long papers have taken to pass the editorial screen, to be revised if necessary, and to appear in print. Unfortunately, however, these printed dates do *not* tell us how long it may take for a paper to be rejected. And this can be extremely important—especially in a competitive field. Other things being equal, I myself would prefer to offer my work to a journal that decides promptly, even if the chance of rejection is high. I'm sure many other scientists feel the same way.

It would help, therefore, if more journals were as considerate of their authors as the *Annals of Internal Medicine*. In a four-page document, "Information for Authors," this publication provides not only guidance about format, style, circulation, audience, and availability, but also details of manuscript processing and evaluation. It describes its policies for acknowledgment of receipt of papers, internal review by editors, peer review, acceptance or rejection, time to final decision, scheduling of papers, prepublication release of information, and complimentary copies. As a result, someone writing for the *Annals of Internal Medicine* knows exactly what to expect.

Editing would be much easier and authors far happier if all scientific journals printed such clear guidance.

But even if they did, there would still be room for improvement. The title of a journal, for example, can be misleading to authors. For several years now, the *Journal of Physiology* has published mainly papers on neurophysiology and on muscle. Perhaps few other papers are submitted to this journal, but the editors do not publish submission and acceptance rates for the various subject areas within the journal's scope. In fact, I know of no journal that does. It would help authors, and readers too, if editors of specialist journals were to provide this information regularly.

Authors would benefit also from knowing more about the referee process. Major contributions to the editorial process also come from referees, who may help authors enormously or who may be obtuse, obstructive, and even offensive. In many journals, referees advise the editor, who takes final responsibility for accepting or rejecting an article. In others, the editor hardly ever goes against the referees' decisions. As the *British Medical Journal's* editor Stephen Lock points out in his book, *A Difficult Balance* (The Nuffield Provincial Hospitals Trust, 1985), one of the editor's important functions is to act as ombudsman, protecting the author from unfairness by referees. But this is easier said than done. Authors rarely know what an editor expects from referees, nor whether the editor ensures that they follow the journal's guidelines. Quite frequently, referees themselves don't even know what the editor wants.

Of course, referees for most journals are unpaid; they are really doing their journal a favor by spending time reading and criticizing manuscripts. But there is a serious disadvantage to this arrangement. Referees often do not keep to the editor's deadline. They have their own priorities and may feel that, with a publication delay of a year or more, there seems no hurry—forgetting that the author may urgently need an acceptance letter for a job or grant application. Editors can impose no sanctions on referees, apart from ceasing to use them.

How can authors deal with these potential problems—ones concerning not just referees but other aspects of the editorial process as well? I believe that we need regular audits of journals to show how well or badly, how effi-

ciently or inefficiently, their editorial processes are working. All journals should perform at least some simple audit annually and publish information on, for example, how often during the past 12 months they have achieved their aim of communicating decisions to authors within a specified period of time. The information that editors present to their editorial boards and to the business meetings of their scientific societies should also be systematically shared with a journal's readers—and authors.

At the moment, information of this kind, where it does exist outside editorial offices, is scattered or buried. A recent editorial in the *British Journal of Pharmacology* (volume 94, 1988, page 277) is a striking exception. This reported a "remarkable and sustained increase" in the number of manuscripts submitted to the journal, discussed likely causes, and explained how the editors planned to cope with it—by, for example, asking authors to reduce the length of their papers. Information of this sort should be made easily accessible in all journals, through their "Information for Authors" section, and their indexes. When occasions arise—for example, if an important change in editorial policy is being considered—it might even be discussed in a "house-keeping" section of a journal. Information about the efficiency and author-friendliness of editorial processes and on how long journals take over their decisions is essential when choosing where to send papers. But we won't get this information unless we keep asking.

Finally, authors need a mechanism to combat actual editorial misconduct, such as the case of the clinical pharmacologist from the Netherlands whose paper was allegedly plagiarized by a scientific journal. Such an aggrieved individual has nowhere to publish the story, let alone get any redress. To provide a means of reviewing cases of this sort, I suggest that we set up an International Scientific Press Council, which would be comparable with the bodies in most countries that respond to public grievances over the conduct of the media.

Meanwhile, feel free to draw this article to the attention of any editor or chairman of an editorial board who you think deserves to read it. □

---

*Andrew Herxheimer is a clinical pharmacologist at Charing Cross Hospital and Westminster Medical School in London.*