

upon an unassailable *status quo*. For many years, I've tried in vain to convince journals like *Arzneimittelforschung*, the *Comptes Rendus*, *Chemische Berichte*, etc., to give English translation of titles or to provide an English contents page--or merely to use a legible type size and sensible column width, no matter what the language. I often wonder whether many of such journals' authors also read CC, and whether they might exert some influence on their editors.

We have repeatedly stressed that scanning a contents page is easier when titles precede authors. Yet, hundreds of journals persist in giving priority of placement and typeface to authors' names rather than their subject matter. Isn't this really an editor's and publisher's appeal to the vanity of the authors listed, probably a quite unnecessary one? How many names does the average reader recognize? Can it be even 10%? Aren't these journals aware that although authors' names will catch the eye of other members of his invisible college, the *information in a title* will attract many other readers more quickly?

Are you willing to show that you care? If so, send a note to several of your favorite journals, asking whether they comply with

1. Garfield, E. Publications dates--realities or promises? *Current Contents*® (CC) No. 34, 22 August 1967, p. 4.
2. Should journal publication dates be controlled by legislation? CC No. 13, 29 March 1972, p. 5-6.
3. Aesthetics in scientific communication. CC No. 47, 19 November 1963, p. 3.
4. Variety is the spice of life--whether in people, language or the contents pages in CC. CC No. 12, 21 March 1973, p. 5-6.
5. Citations in popular and interpretive science writing. *Science* 141:392, 1963.
6. Numerical vs. alphabetic order for cited references. CC No. 50, 13 December 1968, p. 4.
7. Incomplete citations and other sources of bibliographic chaos. CC No. 24, 17 June 1969, p. 5.
8. Precise bibliographical verification with the *Science Citation Index*®. CC No. 35, 2 September 1970, p. 4-5.
9. Deliberate omission of explicit citations of textbook errors is irresponsible science communication. CC No. 35, 1 September 1971, p. 5-6.
10. Publishing referees' names and comments could make a thankless and belated task a timely and rewarding

generally accepted international standards, or, if it's warranted, complaining of archaic editorial practices. The accepted standards are by no means rigid, and allow great flexibility in aesthetic design, typography, etc. The last thing we could want would be an identical format for all journals.

You might, for example, point out that almost invariably highly successful commercial journals have an attractive and appealing contents page, one calculated to provoke interest--and purchase. A good scientific journal need not scorn that intent, but its contents page must provide information as well: carefully worded titles,¹⁷ authors' addresses, etc.

You might, in addition, comment on the need for legible, unambiguous, and complete citations, and even on the title of the journal itself. Any journal that has more than four words in its title does everyone in science and librarianship a disservice by increasing the cost of handling and using every issue. If the journal doesn't adhere to its publication schedule, or uses unreal publication dates, mention *that* too. *Turn on the heat!* As we've said before--if they can't stand the heat, they should get out of the kitchen!¹⁸

- activity. CC No. 17, 25 April 1973, p. 5-7.
11. English, an international language for science? CC No. 52, 26 December 1967, p. 4-5.
12. Concerning cover-to-cover translation journals. CC No. 17, 29 April 1970, p. 4-5.
13. Cover-to-cover translation of Soviet journals--a wrong solution of the wrong problem. CC No. 29, 19 July 1972, p. 5-6.
14. International science requires cultural as well as economic "imperialism." CC No. 4, 24 January 1973, p. 5-6.
15. The value of article titles in bibliographic citations. CC No. 45, 8 November 1968, p. 7-8.
16. Recently, the prestigious *Naunyn-Schmiedeberg's Archiv für Pharmakologie und experimentelle Pathologie* changed its title to *Naunyn-Schmiedeberg's Archives of Pathology*, at the same time announcing that it will accept only articles written in English.
17. Garfield, E., Unintelligible abbreviations and sloppy words in article titles create magic (invisible) spots for indexers. CC No. 48, 29 November 1972, p. 5-7.
18. If you can't stand the heat, get out of the kitchen! Publishing journals is not kid-stuff. CC No. 10, 7 March 73, p. 5.

**Publishing Referees' Names and Comments
Could Make a Thankless and Belated Task
a Timely and Rewarding Activity**

April 25, 1973

Few journals explicitly admonish their referees (reviewers) to think about the quality or quantity of references cited in a manuscript. The *Journal of Pharmaceutical Sciences (JPS)* may be unique in this respect. Published by the American Pharmaceutical Association under the able editorship of Edward G. Feldmann, the *JPS* has for many years required its referees to determine whether the "author uses too many literature references that are not directly related to the work", or conversely, whether "the references are insufficient, perhaps giving the impression that the work reported is more original than it actually is."

The referee is also asked to consider whether the title and abstract concisely describe the work reported. In pressing for adequate documentation, *JPS* also asks the referee whether the description of experimental procedures is sufficiently detailed and/or documented with references to permit the findings to be easily reproduced.

Such admonitions to referees

are exemplary. It would be interesting to compare them with the instructions to authors and referees used by other journals. A *CC*® reader once suggested that a collection of journals' editorial instructions and conventions would make an excellent and useful reference publication. Greater stress on good documentation in manuscript preparation might save everyone much work and time. I once even suggested that an "approved literature search" be required with any submitted manuscript. I made a similar suggestion about FDA applications. Many examples could be cited to show that much repetitive material might be eliminated from the literature if literature searches were mandatory.

The problem of refereeing is always a popular controversial topic among scientists. Inevitably, it all depends upon how you feel about timeliness. There is no doubt that refereeing does contribute to the almost universal time-lag between manuscript submission and publication. The

source of the problem may be the practice of imposing anonymity upon referees.

In general, the more qualified the referee, the greater the value of his time. Unless he has a special ax to grind, he will probably be less than enthusiastic about the job of refereeing. Anonymity is a dull spur to effort in even the most charitable and energetic of us. Truly anonymous philanthropies are rare. This is not cynical; it is only realistic to recognize the fact.

Some journals, like *JPS*, insist that a referee return a manuscript within two weeks. Others attempt similar time limitations, but rarely are such deadlines observed. Whether or not referees do observe deadlines, however, one of the unfortunate aspects of the refereeing process *per se* is the delay it occasions in publication of the work of reputable and experienced investigators. Many have little to gain from the refereeing process.

Perhaps we should only require refereeing for authors who have never published a refereed paper. It is not irrelevant to mention that more than 25% of such authors are never heard from again.¹ After publishing several refereed papers, can't we assume that an author has joined the fraternity and can be responsible

for his own acts? Most authors I know have their manuscripts refereed *before* submission by seeking comments from colleagues. Why not take advantage of this self-imposed refereeing? Authors could indicate that Drs. X and Y have reviewed the paper and that their suggestions were properly dealt with. It is somewhat ironical that so many journals ask the author to supply the names of potential referees *after* submission.

When I served on the editorial board of the *Journal of Chemical Documentation*, I was asked to review a paper of special interest to me. My comments were so extensive that I suggested to the editor that they be published as a separate commentary. The editor agreed. I think the practice is also occasionally followed by the *Journal of the American Society for Information Science*. If this practice were widespread, I think more scientists would participate in the review process in a timely fashion. The result is little different from publication of the discussion that follows presentation of a paper at a meeting!

As editors know, many rejected manuscripts turn up in another journal. In my experience, most rejections are not based on lack of merit or originality. Rather, the relevance to the particular jour-

nal's scope and audience is paramount. I have refereed a number of articles for *Science* which seemed worthy of publication, but only in a more specialized journal. The authors had made no effort to write for a large multidisciplinary readership.²

The publishing establishment must be prepared to bend a little on refereeing. Otherwise, it will encourage experiments like the defunct information exchange groups³ to become commercial realities. There is always increasing pressure to move in that direction. The large number of "quickie" journals has proven the need for more timeliness. The effort to mechanize the printing time-lag is another indicator. As these methods improve, refereeing will be an even greater bottleneck, unless we give referees "a piece of the action." This can be done by including their names at the end of a manuscript or by including a few separate paragraphs of their comments. If a referee's disagreements with the author are so significant as to warrant a separate commentary, then this too should be encouraged. I have never thought much of anonymous refereeing. I would rather sacrifice its presumed advantages to a system which encourages open and rapid publication and discussion.

1. Price, D.J.D. Personal communication. Professor Price has

done some fascinating studies on annual "turnover" in scientific authorship.

2. A recent article has reviewed the dilemma of editors in considering not only poorly written but also wrongly slanted articles that contain important and original material: DeBakey, L. & Woodford, F.P. Extensive revision of scientific articles--whose job? *Scholarly Publishing* 4(2):147-51, 1973.
3. Green, D. Death of an experiment. *Internat. Sci. Technol.* No. 65, May 1967, p. 82-8. The Information Exchange Groups were originated and directed at the NIH by Dr. Errett C. Albritton. Those interested in the history of the IEGs may find the following useful. Green, D., *Science* 143:308, 1964; Anonymous, *Nature* 211:333-4, 1966; Anonymous, *Nature* 211:897-8, 1966; Anonymous, *Nature* 211:904, 1966; Anonymous, *Nature* 212:3, 1966; Spaet, T.H., *Nature* 212:226, 1966; Anonymous, *Nature* 212:865-6, 1966; Anonymous, *Nature* 212:867, 1966; Anonymous, *Scientific Research* 1(12):15, 1966; Dray, S. *Science* 153:695-6, 1966; Abelson, *Science* 154:727, 1966; Confrey, E.A., *Science* 154:843, 1966. Thorpe, W.V., *Nature* 213:547-8, 1967; Albritton, E.C. *Nature* 213:1065, 1967.