

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

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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.