

There are many books and articles giving guidance to the writer and I have prepared a list of my favourites. In them you will find not only good advice but so many warnings of the pitfalls that all but the most daring will, I hope, be put off. Don't forget that much can be absorbed with pleasure from one's everyday reading. But in the final analysis nothing succeeds like repeatedly doing a job yourself and, to leave you with a few crumbs of comfort, I pass on the words of a respected journalist friend who, when I asked how he managed to write with such ease, replied: "The first million words were the worst."

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## How I referee

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The arguments in favour of refereeing are:

(1) No editor can know his subject well enough to be an expert in all its aspects. This must certainly be true for a general medical journal, such as the *BMJ*, but I think it is true even for specialist journals. My particular interest is in diabetes. That sounds a narrow subject but there are two English-language journals, each containing about 100 pages an issue, devoted entirely to this one subject. A quick look at the list of contents shows how varied are the papers: clinical, biochemical, pathological, statistical, and immunological. I do not know anyone who would claim to be an authority on all these aspects of diabetes. My view seems to be shared by the editors of *Diabetes* and *Diabetologia*; both these journals use referees.

(2) It takes a long time to establish a journal's reputation, but it may soon be lost if a few bad or hastily written papers or papers without proper acknowledgment of other work are published. It is the ease of making bad mistakes and their disastrous consequences that support the need for expert refereeing. (Referees makes mistakes too—there is only one sure way of not publishing bad papers, which is not to publish any.)

(3) Most manuscripts can be improved by advice from referees. This may have nothing to do with grammar or style but may concern a reference that has been missed, a conclusion which is over-bold, or a technique which needs description. The referee may see, in a way that an editor cannot, how a paper can be improved by amplifying or explaining part of the work, or that the paper would be better if deferred until more material had been collected or more experiments done.

The arguments against refereeing are:

(1) It causes delays. A paper can be killed by long delays in publication. Recently the process of publication has been speeded up in most of the more general medical and scientific journals (*BMJ*, *Lancet*, *Nature*); refereeing takes time, so omit it. But referees can be prompt. In practice the time taken to referee a paper is only a fraction of the whole submission-to-publication time.

(2) Refereeing does not lead to the best selection of papers. A general editor can do just as well. My bias is against this, and I think poor selection of papers shows, at least to the expert reader.

I have set out some of the pros and cons of refereeing, but why must we come to any definite conclusion? Why not have variety? I am, in general, in favour of refereeing for medical journals but I am glad that there are some editors who never referee and some who break their own rules. The editor of *Nature* in 1953 cannot have needed a referee to advise him to accept that paper by Watson and Crick.\*

If I were Chairman of the Journal Committee of the BMA I would say to the editor: "I hope you will go on using referees but I also hope that you will use your own judgment, not merely on bad papers, which I am sure you can easily reject without advice, but also on good papers, whoever the authors may be. It may be easy to decide to accept a paper by Peter Medawar or Cyril Clarke, but you may also get a paper by someone you have never heard of which you like, and then I hope you will take it."

### How to referee

(1) The editor must know what he wants from his referees: straight advice on whether to accept or reject or, in addition, criticism of the paper and, if so, in detail or only in outline?

The editor must choose his referees and they must have certain qualities—they must be reliable and punctual (unpunctuality is an incurable curse). An editor soon learns whose judgments cannot be trusted. My guess is that most referees tend to err on the side of recommending rejection and the editor may have to put on a slight bias to compensate for this. On the other hand, a referee who recommends acceptance of a paper which is then demolished in correspondence should probably be dropped. A man may have been a good referee once but cease to be so because he does not keep up with his subject or takes on too many other commitments. He should be dropped.

Should the editor use one referee or more? If he uses a second referee, either simultaneously or after the first has reported and they disagree, what then? Use a third, or disregard them both? It is probably better, as a rule, to use only one referee but there will be exceptions. Indeed, a referee may himself suggest that the editor takes another opinion because he is unsure of his own judgment or is not familiar with the whole scope of the work being considered.

Should the editor transmit the referee's comments verbatim to

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\*But even that great paper could have been improved! The first seven words of the famous last paragraph—"It has not escaped our attention that the specific pairing we have postulated immediately suggests . . ." are superfluous.

the author? This question has been exercising the editor of the *New England Journal of Medicine*,<sup>1</sup> who fears that rude comments such as "waste of time" or "useless work" will offend the authors. Of course they will and there is no need to pass them on. All authors, whatever their protestations of indifference, are as sensitive as mothers at baby shows and just as protective. If an editor wants to reject a paper he can do so politely and, unless there is a special reason—for example, that the paper would be better in another journal—without giving a specific reason. Some may disagree with this advice on the ground that it lacks courage or is secretive, but I see no point in rubbing salt into the wounds of rejection—and another editor may accept the paper.

Should referees be named or anonymous? In theory referees should be named: the authors know whom they are dealing with and everything is open and acknowledged and the referee is restrained from indulging his whims and prejudices. I believe this is a facile argument and that referees will usually give better opinions if their identity is protected. They are spared the embarrassment, for example, of being seen to recommend rejection of a paper by a senior man or a personal friend. Of course, the editor has to choose referees carefully when they are privileged by confidentiality and reject those with obvious bias, but that is part of good editing.

How to choose referees? In most subjects there are many experts in the country and from them good referees can soon be selected, but in some highly specialised fields an editor may have to reach across the world to find the right man.

If an editor rejects a paper he should be ready, if the author challenges him, to think again and perhaps consult another referee. A referee must accept that he is not the only adviser an editor may use and that he is giving an opinion, not making a decision.

### The referee

Some simple rules:

(1) Don't lose the manuscript. A former chief of mine had a bad few hours before his secretary found a manuscript he had accidentally thrown away on the town rubbish tip. If you lose the author's only manuscript I advise immediate emigration.

(2) Be prompt. If you cannot read and comment on the manuscript (which does not usually take long) within two, or at most three, weeks return it at once. It doesn't take any longer to read the paper today than in a fortnight, and it won't go away if you put it in the bottom of the "in" tray.

(3) See what exact questions the editor is asking you. The editor of the *BMJ* asks specific questions about a paper: is it

original? is it reliable? is it clinically important? and is it suitable for the *BMJ* or would it be better in another journal?

(4) If in doubt add a bias in favour of recommending publication. A borderline paper published is not a sin, but a reasonable paper rejected is a shame. The temptation is for the referee to be superior and advise rejection. It should be resisted. The purpose of medical journals is to convey information, not to block it.

(5) Don't nitpick. There is a strong tendency of referees to find little faults. A referee may prefer one way of expressing results but if the author prefers another there may be no harm in that. The referee is not the author. In short, don't be bitchy. Your opinion may be confidential but write it in such a way that if it were published you might be embarrassed but not ashamed.

(6) Don't be overawed by the authors: famous men can do bad work and write bad papers. And papers from famous departments may be badly prepared and may not (or so one must suppose) ever have been read by some of their illustrious authors.

(7) Don't ask silly questions of the author. Don't ask him if he has collected results which it is obvious he has not. If the absence of those results invalidates the paper advise rejection; if not keep quiet. Likewise don't suggest doing new work. You are judging this paper not the next.

(8) Don't get bogged down in details. At the first reading take the paper at speed to get its general feel and then turn to points of technique or detail.

(9) If you have comments which you specially do not want the authors to see make that clear to the editor.

(10) Try to resist the temptation to advise acceptance of a paper merely because it makes frequent (and favourable) reference to your own work.

(11) Don't get in touch directly with the author, least of all by telephone. If referees are meant to be anonymous they should stay so.

### Conclusions

I come back to a few points: referees usually improve a journal; they should be anonymous, but they should write as if they were not; the editor should not usually give detailed reasons for rejecting a paper.

Finally, there are no absolute rules in this matter; variety is of the essence of progress, which is what medical publishing is for.

### Reference

- <sup>1</sup> *New England Journal of Medicine*, 1975, **293**, 1371.

## How editors survive

STEPHEN LOCK

Editors survive by accepting good articles. Obviously there's more to a general journal than the original articles, but I believe that, however good and important the other sections are, its quality must depend on the quality of the papers, originals, and medical practice articles. What is a good article? It's one that has a definite structure, makes its point, and then shuts up. Its English uses nouns and verbs and not adjectives and adverbs, while the scientific structure is crisp and each individual section does what it is supposed to and no more.

How do we get good articles? We believe we can do this only

by refereeing, or peer review. We ask our referees four main questions. Is the article original? Is it scientifically reliable? Is it clinically important? And is it more suitable for a general journal, or a specialist journal?

In depending so heavily on refereeing the *BMJ* of course differs largely from its distinguished contemporary the *Lancet*. Its last editor, Ian Douglas-Wilson,<sup>1</sup> is on record as saying that it is opposed to peer review, because it is too slow, because it tends to be conservative and élitist, and because it may be bigoted.

Given that a prime function of the editor is to monitor and control his referees, none of these objections seems to me very serious. With respect, the point about delay is trivial, because, as a recent article in the *New England Journal of Medicine* has shown,<sup>2</sup> the average time lapse between having the original scientific idea and publication of the final article is four years—