

# Editing the *British Journal of Psychiatry*

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## ACHIEVING EXCELLENCE

I began my working association with the *British Journal of Psychiatry* around 1982. In addition, I assisted Professor Shepherd, the editor of *Psychological Medicine*, from mid-1980 until the early 1990's. Then, in 1993 I narrowly won the election for Editor of the *Royal College of Psychiatrists* and, thus, for a decade, I became editor of the *British Journal of Psychiatry* – as well as being responsible for our sister journals, and the Gaskell book programme.

My principal concern – expressed in an early editorial – was to fulfil the requirements of my psychiatric constituents by consolidating the position of the Journal as the leading international journal of general psychiatry and an essential companion in clinical practice (Wilkinson, 1994). I sought to do this in two ways: by ensuring rapid publication of the best clinical science and art; and by enhancing editorial standards and practices. In my view, the Journal had a British base and an international perspective, and I wanted to encourage the involvement, and commitment, of readers, authors, assessors, and editors. I welcomed constructive criticism, and adaptation to changing needs and technological developments. In essence, I would strive for excellence in clinical communication.

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Throughout my editorial career I have been interested in Journalology. For the sake of underpinning the rest of my views on research, in most of the themes that follow I refer to the journalological investigations I have undertaken with colleagues during my tenure.

## IMPACT FACTORS

One of the first issues that I encountered was the impact of the *British Journal of Psychiatry* on the scientific community. This could be estimated by analysing citation data, which provide a quantitative estimate of the influence of a journal on researchers over time. In the event, Louise Howard and I examined citation data for the Journal and other comparable general psychiatric journals over 10 years (Howard & Wilkinson, 1997). Data on three measures of citations (total number of citations, impact factor and ranking by impact factor) were obtained from journal citation reports for 1985-1994.

We found that the Journal ranked sixth of all psychiatric journals when journals were ranked by impact factor. The Journal's impact factor fell between 1985 and 1990 and this was followed by a rise in impact factor after 1991. The Journal did not rank in the top 10 psychiatric journals between 1991 and 1993. *Archives of General Psychiatry* continues to be cited more frequently than any other psychiatric journal, with the *American Journal of Psychiatry* usually ranking second. Psychopharmacology journals were replacing more general journals in the top rankings. Ranking of most journals had become less stable in recent years.

We concluded that the Journal would have to change the nature and number of papers published if it were to improve its impact factor. However, there are a number of limitations to citation data, and such data are only one

of several factors useful in evaluating the importance of a journal's contribution to scientific and clinical communities.

Citation data are not the only way to measure a journal's influence and should not be relied upon as the sole source of information when comparing and evaluating publications. Citation analysis should complement other assessments of journals, such as peer survey and specialist opinion. Citation measures can be skewed by the publication of one article with an exceptionally high citation rate. Review articles tend to be cited more widely than other types of publication whereas shorter communications, such as case reports, are cited less often. Specialised research may not be widely cited but may be important. Popular journals may rank poorly in the citation list but may be very widely read and considered to be important by the scientific and clinical communities. Citation frequency may also depend on variables such as the reputation of authors published and controversial subject matter. Citation analysis probably provides a crude method of assessing the quality of publications. The data presented by us might reflect the size of the different readerships of each journal and, by extension, the relative proportion that might go on to cite papers they have read. Citation analysis is thought to be important in academic life and in the university's research assessment exercise in the UK (in which the quality of publications is used as part of the assessment of the quality of research funded by the Higher Education Funding Council).

Should the Journal aim to increase its impact factor and go for a high profile scientific image but become widely unread? This could mean further reducing the number of articles with low citation rates (short reports and case reports that clinical readers enjoy), publishing more review articles and possibly publishing fewer articles per journal while ensuring only the highest quality research is included. 'Biological' research (I would say all human research is biological) is cited more frequently than psychodynamic work. The Journal's impact factor would surely rise if more 'biological' research was published but this could be at a cost in clinical, professional, educational and training terms.

The Journal is published by the Royal College of Psychiatrists and its members might believe that the type of articles published in the Journal should reflect primarily the interest of the College's membership. This aim is not necessarily compatible with the psychiatric community's expectation for the Journal to publish high quality research, of international significance, which could influence the scientific and medical community at large. Citation data represent a standardised method of evaluating such a policy, but they are not infallible, prescriptive or definitive in effect.

## PEER REVIEW AND EDITORIAL DECISION MAKING

There is little published on how editorial decisions are made despite increasing scientific interest in the peer review process. The most notable recent development has been the opening up of the process, e.g., by the *Journal of the American Medical Association* and the *British Medical Journal*. It is clear that editor's requests of assessors vary substantially, and editorial peer review practices differ between journals. While some journals give readers detailed information on the editorial process, this is the exception rather than the rule. Often, editorial decisions are mystifying.

Louise and I aimed to describe and analyse the editorial process of the *British Journal of Psychiatry* in an attempt to make the process more open and available for scrutiny (Howard & Wilkinson, 1998). There are many factors which will influence the editor, including the number of manuscripts submitted and the nature of the journal in question. Editors may need to override the recommendations of assessors, and it has been suggested that editors can be super reviewers in view of their exposure to papers submitted. It should also be recognised that editors decide which assessors to ask for assessments, and this decision itself might be biased. Although there are no objective criteria for this process, we aimed to investigate how assessors' recommendations influenced editorial decisions and whether the comprehensiveness of the assessment made by the assessor also had an influence on outcome.

Four hundred consecutive manuscripts submitted over a four-month period to the Journal were examined prospectively for assessor's comments, and editorial decision on acceptance or rejection. I sent 248/400 (62%) manuscripts to assessors for peer review. Kappa for reliability of assessor's rankings was 0.1 - indicating poor inter-rater reliability. Assessors agreed best on whether to reject a paper. A ranking of 5 (indicating rejection) had the greatest association with my rejection, and the mean ranking of assessments was also significantly associated with my acceptance or rejection.

We concluded that assessors and editors tend to agree on what is clearly not acceptable for publication, but there is less agreement on what is suitable for publication. When I received the assessors' rankings I appeared to be most influenced by a clear rejection from any assessor. I was also influenced by the average score of the assessors but less influenced by clear recommendation for acceptance, or by the comprehensiveness of the assessment. This probably reflects my scepticism of solitary high rankings. The rankings appear more important in influencing me than the comprehensiveness of the assessment,

but a comprehensive assessment can be very useful for the authors of the manuscript.

Peer review is a time consuming task, with our assessors usually working for more than one journal, reviewing approximately 12 manuscripts per year and spending one to two hours on each, with little recognition of their work. The assessors we studied produced impressive reviews and manuscripts, which helped me greatly in assessing submitted papers. However, the assessment process itself should be subject to further review and scientific evaluation, and while there has been increasing scientific interest in peer review in recent years this is usually focussed on the assessments rather than the whole editorial decision-making process.

Publication of the peer review process in journals and on the Internet may facilitate further improvements by making peer review more open to the reader. Editors should make explicit the judgements that are required of the assessor and ensure that, when appropriate, authors have the opportunity to respond to an assessor's comments. The success or failure of the peer review process ultimately will be judged by the readers of the journal.

There is an inevitable, cruel sting in the tail - because of space constraints, less than a quarter of the papers submitted to the Journal are accepted for publication. In such constraining circumstances I have had to exercise editorial choice in which papers to accept, and a variety of factors probably influence my decision: fairness, openness, accountability and transparency compete with hubris and human error. An alternative would be a 'hanging' committee, which I investigated and found to be impractical.

## **OPEN PEER REVIEW – A RANDOMISED CONTROLLED TRIAL**

All Journal submissions reveal the names of authors. Because of occasional concerns received from authors, one of my initiatives was to signal to prospective authors that they could choose to have their work peer reviewed anonymously. In practice, a tiny minority have chosen this option. By contrast, nearly all reviewers remain anonymous, and apparently reluctant when asked to sign reviews: albeit no significant differences in review quality, recommendations regarding publication or times taken to review have resulted from signing. The British Journal of Psychiatry has heretofore operated closed peer review. For an open system to be practical, reviewers would have to be in favour of signing, and there should be no loss of review quality.

Reviewers for the Journal were asked whether they would agree to have their name revealed to authors (Walsh *et al.*, 2000). A total of 245 reviewers agreed to

sign. Four hundred and eight manuscripts assigned to reviewers who agreed to take part were randomised to signed or unsigned groups. We measured review quality, tone, recommendation for publication and time taken to complete each review. We found that signed reviews were of higher quality, were more courteous and took longer to complete than unsigned reviews. Reviewers who signed were more likely to recommend publication.

Our study supported the feasibility of open peer review and identified such a system's potential drawbacks. Of the referees approached, 76% agreed to sign their name. This figure compares with 43% and 70% in previous studies and is the highest reported to date. When considering opening the review process on this basis, the results suggest that one might lose up to one-quarter of reviewers, making the editorial process more difficult and increasing the workload of the remaining referees.

With regard to the review quality, at the outset of the trial a difference between the groups of 10% was chosen as editorially significant. Although signed reviews are a statistically significantly higher quality than unsigned reviews, the mean difference was only 0.21, representing a percentage difference of only 5.5%. This compares with a mean difference of 0.3, representing a percentage difference of 0.75% in the largest randomised trial conducted before this one. With such a wide range it is difficult to draw any firm conclusions about improved review quality. What can be inferred from these results is that review quality did not suffer as the result of signing. Although we found a statistically significant difference in that named reviewers provided higher quality reviews, it is doubtful whether this reflects an important difference in quality. Signed reviews were found to be significantly more courteous. However, it was clear that the majority of reviews were at the courteous end of the scale in both groups.

Signed reviews took significantly longer to complete than unsigned reviews. Having to sign his or her name appears to make a reviewer spend longer on the review, possibly by checking references or reading about the statistical methods more carefully. The extra time spent would be expected to enhance the quality of the report. This finding does, however, suggest that the time commitment involved in reviewing might be too arduous for referees if the peer review process were opened up, especially when one considers the increased work load resulting from the loss of reviewers who refused to sign their names.

Although signing appears to make reviewers more likely to recommend publication and less likely to recommend rejection of papers, it is important to remember the role of the editor in this process. At the Journal it is not unusual for a manuscript to be sent to four or more

referees and for divergent opinions to be expressed regarding suitability for publication. I frequently have to make difficult judgements in the light of these disagreements. If the process were opened up, I might have to modify my practice and become more autonomous in making decisions, perhaps necessitating rejection of more manuscripts than recommended by reviewers. After all, publication remains an editorial rather than a scientific determination, and the question asked of reviewers is whether there is any major impediment to publication on scientific grounds.

As an aside, we looked at the quality of the 57 reviews completed by those who declined to have their names revealed to authors. The quality was significantly lower than that of both signed and unsigned reviews from reviewers participating in the study. Only the signed reviews, however, were of editorially significantly higher quality. These findings may suggest that the loss of 'decliners' from an open peer review process may be beneficial in terms of review quality. However, it is possible that the Hawthorn effect played a role as both signed and unsigned groups were aware that they were participating in a study.

Increased accountability in the reviewing process is essential. This is because it has become so important to publish in good journals, not only for the careers of individuals but also for the funding of institutions. Reviewers give their valuable time free of charge and with little credit, yet they are performing an important job, which plays a part in shaping our scientific future. It is critical that they do this job in the best possible way. By signing their name to a review they become more accountable. A closer examination of the possible adverse effect that open peer review may have on professional relationships in our close-knit field is warranted.

## DECLARATION OF INTEREST

Declaration of interest has emerged for obvious reasons as a powerful editorial tool, and was adopted by the *British Journal of Psychiatry* some years ago. Editors are not immune and, recently, I became involved in a mini-controversy flowing from this issue.

The following – misleading – comment appeared in a *Lancet* editorial (Editorial, 2002):

“... the editor of the *British Journal of Psychiatry* was recently questioned about his membership of a drug company sponsored “educational organisation”, for which he received £2,000 annually, together with his decision to publish a paper favouring a drug manufactured by the same company. Only after receiving the letter questioning his behaviour did the editor change his journal’s pro-

cedure, excluding himself from decisions about work sponsored by that same company. He avoided the issue about whether he should have any commercial liaisons while acting as editor of a supposedly independent medical journal”.

Subsequently, *Lancet* published a letter from me, which I would commend also to readers of this journal:

“Your anonymous editorialist refers to issues that have been aired openly and comprehensively in our own journal, the *British Journal of Psychiatry*. I suggest that readers look up the original correspondence for the full facts” (Correspondence, 2002).

Intriguingly, I found that my behaviour was more correctly reported by a well-known satirical magazine, *Private Eye* – to which I subscribe, than by *Lancet*. I believe that observation reflects unfavourably on the parochial standards of medical journalism.

## CONCLUSION

I have been privileged to deal with thousands of colleagues worldwide. I did not anticipate that three-quarters of my correspondence would deliver disappointment. In spite of that, I have been gratified to be treated by authors, reviewers, and readers with almost universal courtesy and professionalism. The Journal is changed as a medium of communication. One constant is that – impressionistically – 50-75% of our submissions are publishable but suffer rejection because of a lack of space. I leave believing that the volume of published psychiatric research is disproportionate to the growth of material knowledge in the clinical domain; and, the methodology of peer review remains inadequate to face the challenges of the electronic Journal – and beyond.

## REFERENCES

- Correspondence (2002). *British Journal of Psychiatry* 180, 88-87.
- Editorial (2002). Just how tainted has medicine become? *Lancet* 359, 1167.
- Howard L. & Wilkinson G. (1997). Impact factors of Psychiatric Journals. *British Journal of Psychiatry* 170, 109-112.
- Howard L. & Wilkinson G. (1998). Peer review and editorial decision making. *British Journal of Psychiatry* 173, 110-113.
- Walsh E., Rooney M., Appleby L. & Wilkinson G. (2000). Open peer review. A randomised controlled trial. *British Journal of Psychiatry* 176, 47-51.
- Wilkinson G. (1994). The British Journal of Psychiatry: achieving excellence. *British Journal of Psychiatry* 164, 1.