When I started out, nobody had heard of health economics; now every provider unit in the health service wants one. People seem to want health economists, up to a point, and even epidemiologists because they boast a set of tools to offer managers and doctors for opening what you called the black box. The economists didn't get to this position by hanging back and wingeing from the sidelines. If, as you claim, medical sociology, and your ethnographic methods, can really open up this realm of process and tell us what is going on in the "black box" then you've got to be more entrepreneurial. Change your name to Pandora while you're at it, people might be less inclined to be dismissive!

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Authors have rights too

Michael E Dewey

Without publication research can be of little value. When researchers approach publication there is ample published guidance for them on what their obligations are, and there are well known style guides within each scientific discipline including, in medicine, the Vancouver style. This article gives a series of anonymous examples to suggest that the impact of similar guides for editors has been patchy and to make some suggestions for better communication.

The Vancouver style, "Uniform requirements for manuscripts submitted to biomedical journals,"[1] sets down only the obligations of authors. The wide acceptance of this style guide, and similar ones in other disciplines, suggests that it fills a need. So far there does not seem to be an equally well known guide on the responsibility of editors to authors and to referees.

Below, I illustrate some of the problems authors experience that could be avoided by editors following guidelines. The examples do not identify the article or journal concerned, but each example has happened to me or my coauthors during submissions to what are generally regarded as quality journals. Most of the examples are from Britain but European and the American journals also figure. The order of the points in the article corresponds to the progress of an article from submission to eventual publication not to perceived seriousness.

What if two papers with similar content arrive?

There may seem to be no problem for an editor under these circumstances: each paper is assessed on its merits and published accordingly. I submitted an article which caused disagreement between referees, and after a third opinion was sought it was rejected. A few months later the journal published an article covering similar ground. The published paper was more extensive and a much better article, but an author does not have to be paranoid to wonder what went on. Perhaps editors need to bear in mind what authors may think when this sort of thing happens and keep them better informed.

What if the editor is also an author?

If there are few good journals in a specialty, editors may not be able to publish during tenure unless some mechanism can be found to allow for this eventuality. I submitted an article to the journal of which one of my coauthors was editor. The rules of the organisation which owned the journal outlined a procedure to be followed that used a guest editor. However, this procedure was not explicit to the readership, and only by adding an acknowledgement to the article could we make clear that the article had not been accepted just because the editor was an author.

Banning the editor from publication in the journal seems extreme if there are few alternative outlets. Logically, the editor's research team would also have to be banned, which would probably discourage potential editors even more. Whatever the procedure for dealing with the problem the journal should make it explicit.

How long should the author wait?

When they acknowledge receipt of an article journals sometimes state how long authors should expect to wait before receiving a decision, although few make this information more widely available. I waited two years for the first substantive response to one article despite reminder letters to the editor. (It was then rejected, which added insult to the injury, although it was then accepted by another journal). Another journal has taken a year to respond on more than one occasion.

Journals usually blame slow referees, but if they have not replied within three months are they likely to reply at all? It is no real answer to say that authors could withdraw the article and resubmit elsewhere, as the chosen journal may be the most appropriate one.

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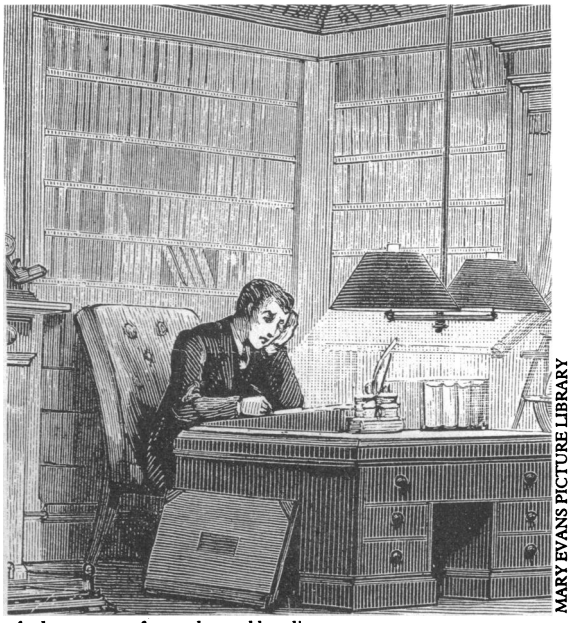

MARY EVANS PICTURE LIBRARY

Authors are too often maltreated by editors

Journals which publish the date of receipt and final revision under articles at least let the prospective author estimate the likely delay.

What happens when the referees report?

If the opinions of the referees agree there is no real problem, but sometimes they do not, and even when they do agree the editor's decision may not appear to agree with the opinions. It is hard to give an example without reprinting the whole correspondence, but we have had articles rejected when our reading of the referees' reports was that the article merited publishing. To be fair we have also been offered the chance of revising articles which attracted substantial justifiable criticism.

Of course the editor must retain the right to decide on what fits into the journal and what does not, but the high esteem in which refereed journals are held is surely based on the assumption that the referees' reports will be the main feature in the editor's decision. A further worrying issue here is the place of confidential reports, for the editor's eyes only, which most journals use. What is the role of these? If the referees' confidential comments disagree with their comments to the author then they are denying the authors useful feedback; if both sets of comments agree should the editor not have to justify why they have been overruled?

What does the editor do about aggressive referees?

Because persuading people to be unpaid referees is difficult it could be argued that editors have to pass the comments on. In one of my papers I used an abbreviation in a non-standard way, partly because my printer did not produce mathematical symbols. The referee assumed that I was ignorant of the difference between the two concepts and launched into a torrent of abuse.

Such aggression could be very disturbing to young workers submitting their first paper, although experienced researchers are used to it and it seems to be something which most people are prepared to put up with. Editors could return the comments to the referee for rewriting, obtain a report from a fresh referee, or suppress the report and tell the authors why.

How should feedback be given?

Some journals give clear feedback and ask for a list of changes if the paper is resubmitted, but this is not true of all. I once had to have the gnomic response of one respected editor of a major journal interpreted for me by a senior colleague.

No one expects the editors to commit themselves before receiving a submitted paper, but some indication of the degree of pleasure with which a resubmission would be received would be welcome. Clarity costs nothing.

What happens when the paper is resubmitted?

Most journals send the referees' reports to the authors, and authors try to take them into account, assuming they are true. Even if all of the points have been met there can still be problems when the paper is resubmitted.

I revised a relatively brief article in line with the referees' comments only to receive a new set of comments which raised points that could have been made at the first submission. Sometimes this problem seems to arise because the paper has been submitted to new referees but the typeface of referees' comments suggests that this is not always true. Journal policy seems to vary. Some editors decide whether the first set of comments have been met and send for fresh reports only for major revisions, whereas others seem to obtain fresh referees' reports routinely.

What happens when the editor changes?

This should not be too much of a problem, but sometimes delays mean that it can be months before the feedback arrives. I was encouraged to resubmit a substantially revised article to a journal. Although the article was resubmitted during the same editorship, by the time the referee's report arrived the editor had changed and the new incumbent had a different perception of the needs of the journal. I have even managed to persist so long that the final work on an article was dealt with by the third editor, but that owed something to my delays as well.

Some journals expect outgoing editors to retain responsibility for work in progress when they leave. This also encourages them to chase up the referees.

What happens in proof?

Everyone accepts that journals have a house style about how to spell, and how to write abbreviations, but some articles come back with major changes. One article which had been revised and resubmitted appeared in proof partly returned to its original form and partly left in the resubmitted form. Within the constraints of proof changes I could not disentangle it. The most extreme changes in an article which I have experienced involved alterations to the title and summary and major changes within the article itself, including making the most important tables into text.

I should have complained about the first case, but I was inexperienced then. In the second case the changes were reversed with the comment that nobody had ever objected before.

Most authors have been baffled after the leisurely progress of their article through the system to find that they have to return the proofs in such a hurry. Their bafflement is increased when they find such major changes made. What is the point of sending the article to learned referees if it is subsequently going to be altered without warning?

Do referees have rights too?

The important role of the referees in improving the quality of published science is often overlooked. For many journals the referee receives a typescript in the

post, sends comments off, and receives no other feedback than to see the article appearing in print later. Some journals routinely give the referee feedback, usually on the editorial decision and often including a copy of the other referee's report. Producing a good report, especially on a very technical paper, can take many hours, and editors do not seem to appreciate how rewarding referees would find the knowledge that their comments were taken note of and that the other referees agreed with them. A further concern is that some journals do not transmit the referees' comments to the authors; this wastes the referees' time and fails to appreciate that they want to improve published articles not just act as gatekeepers.

Final comments

The practice of editors varies substantially, and these points are intended to suggest that some simple changes could improve the ways in which they communicate about the process and their decisions. I would not want anyone to think that all my interactions with editors and referees have been unrewarding, many of my papers have been improved substantially as a result of the feedback I have received.

1 International Committee of Medical Journal Editors. Uniform requirements for manuscripts submitted to biomedical journals. *BMJ* 1991;**302**:338-41.

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Medical Education

Making change happen

Stella Lowry

This is the last in a series of nine articles examining the problems in medical education and their possible solutions

The problems that have been identified in British medical education are not unique, and many of the proposed solutions have already been implemented elsewhere. Although new medical schools like McMaster in Canada and Maastricht in the Netherlands have had considerable success (in terms of staff and student satisfaction) with courses based on self directed, problem based learning, these models may be dismissed as difficult to implement in an existing course. One example of how major curriculum reform can be introduced into an established and traditional medical course is the recent experience at Harvard.

Harvard's new pathway

Harvard has a reputation as the premier medical school in North America, and the fact that it has chosen to introduce sweeping changes in its course is likely to make other schools take stock of what it is doing. I asked the dean, Daniel Tosteson, why such a successful school had decided to revolutionise its course. Like many recent reforms in medical education the changes had started with the dean's concern at the effects of the traditional course on the students in his faculty. He knew from interviews with students at entry and graduation that many were demoralised by the course. He did not think that they were adequately prepared for their roles as modern doctors.[1] In particular he thought that competence in computer literacy and manipulating information technology, which would help them to be "lifelong learners," were neglected. He was also concerned that the traditional course overemphasised factual knowledge and paid too little attention to the attitudes that modern doctors need to develop towards their patients, their colleagues, and their work.

SEEDS OF CURRICULUM REFORM

The traditional medical course at Harvard was a postgraduate entry, four year one with the first two years spent studying the basic sciences and the second two devoted to clinical subjects. The main teaching method was the traditional large lecture. In 1979 the school hosted a "symposium on medical education," which sowed the seeds of curriculum reform in the minds of many of the staff. By 1982 the dean was proposing introducing a "demonstration project" which motivated students could enter (with no specific academic prerequisites) at the end of their second college year. The course would run for seven years, at the end of which graduates would enter the second year of residency programmes. Within the course half the time would be allocated to a compulsory core curriculum and half to self directed learning. Basic and clinical sciences would be interwoven during the course, but with the clinical sciences predominating in the final three years.

A report on these ideas appeared in the medical school newsletter and was picked up by the *Boston Globe* and the *New York Times*. The school soon found itself inundated with applications from college students around the country wanting to enrol on this innovative course.[2] In response to this enthusiasm Tosteson set up a planning group to design an acceptable curriculum for an experimental track within the school.

A major departure from his original vision was the rejection of a seven year course—but other concepts were accepted. The "new pathway" was to emphasise basic concepts rather than facts, topics were to be integrated, and clinical contact was to be introduced early. Initially there was considerable opposition from members of the faculty who feared that the proposals would undermine their own positions. Hence it was decided that the pathway should be set up as a small demonstration project only and be fully evaluated before its concepts were more widely introduced into the school. Guarantees of outside funding from sources including the Josiah Macy Jr Foundation, American Medical International, and Hewlett-Packard also smoothed the introduction of the scheme, which was not seen to present any financial threat to the traditional course.

In the new pathway the formal lecture time was reduced to 60% of the total available, the remaining time to be used by students to pursue topics that interested them. Most of the teaching was offered in small tutorial groups with close association between staff and students. Formal departmental boundaries were lost, and clinical teachers were involved from the beginning of the course. Each student on the parallel track was given a personal computer to use for electronic mail communication with tutors and other students and for access to bibliographic information. All students were also allocated to a librarian at the library of medicine who would help with the self directed parts of the course.

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