Journalology—or what editors do

Jane Smith

Elsewhere in this issue Alex Paton (p 741) and Tessa Richards (p 744) argue that today's *BMJ* is recognisably the same journal as that of the past century and a half.[1,2] But much has changed: the language, and not always for the better; the typography, decidedly for the better; and, most importantly, the science. The attitudes and roles of editors have changed too—and in turn affected the science they publish. For journals are not simply passive vehicles, and editors influence the way that


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science appears in their journals. The key to these changes—both in the journal's pages and beyond—is the imposition of process and structure.

Peer review

For potential authors the important editorial process is peer review; yet it is a recent phenomenon in medical journals. The earliest scientific journals—produced by the Royal Society in England and the Academie Français in the seventeenth century—used peer review in deciding which articles to publish,[3] but Burnham has shown that medical journals did not routinely use referees until after the second world war.[4] Nineteenth century medical editors had other concerns. Firstly, their journals were much more their personal fiefdoms, used for campaigning and proselytising. Secondly, and probably more crucially, many were scraping around for material to publish. Also, much of what was published didn't need expert reviewing. With obvious exceptions (Lister on antisepsis, Spencer Wells on forceps, Manson and Ross on malaria), throughout the nineteenth century the *BMJ*, like other journals, was dominated by anecdotal case reports and series, descriptions of methods and appliances (with no evaluation), and lectures given by the great men (there were few women) of the day.

Nevertheless, Ernest Hart, the *BMJ*'s editor from 1867 to 1898, was one of the few exponents of peer review. Burnham cites him as telling American medical editors, "Every letter received, every paragraph, every cutting editorially dealt with, is referred to an expert having special knowledge and being a recognised authority in the matter."[5] We have no data on the reviewing practices of Dawson Williams (1899-1928) or Norman Horner (1928-47); but Hugh Clegg (1947-65) used referees extensively and collaborated with Austin Bradford Hill in promoting the use of sound study designs in medicine (p 752).[6,7] At the time Bradford Hill was responsible for organising the major Medical Research Council trials of the 1940s and 1950s—for example, of streptomycin for tuberculosis—many of which the *BMJ* published.

Now the idea of a major journal not using peer review is unthinkable, and the *Lancet* has recently taken pains to scotch the myth that the *BMJ* referees everything and the *Lancet* nothing[8]—though a former editor is on record as arguing strongly for such a stance.[9]

How the *BMJ* selects its papers

Today the *BMJ* has about 2000 referees, who are recorded on computer with lists of their specialties and interests and details of their workload and performance. But unlike special journals, which referee nearly all their papers, we referee just over half—because we have full time editors and get many papers that are manifestly unsuitable because they are unoriginal or too specialist. All papers are read firstly by a *BMJ* editor and then sent to either a referee (if potentially acceptable) or another *BMJ* editor (if likely to be rejected). We ask referees whether a study is original, scientifically reliable, clinically important, and suitable for a general journal. According to the referee's opinion, the paper may then be rejected or considered further by the journal's weekly "hanging committee"—a committee of three editors and two or three clinician associate editors.[3] A statistician attends most hanging committee meetings, and papers that look clinically interesting are statistically reviewed. Despite all that has been written about improving the quality of study design and analysis,[10-12] papers continue to fall at this hurdle.

In 1893 Hart identified many of the criticisms still levelled at peer review: "It is a laborious and difficult method, involving heavy daily correspondence and constant vigilance to guard against personal eccentricity or prejudice or—that bugbear of journalism—unjustifiable censure."[5] Vigilance consists in giving our referees guidance on what we expect from them, scoring the quality of their opinions, and listening to feedback from authors in the form of appeals. We even published a paper together with three referees' reports in the face of an author who would not give up and an irreconcilable difference of opinion between him and the reviewers[13] (the subsequent correspondence did not resolve the dispute either).

Hart's "prejudice" is the hardest criticism to shrug off, though the evidence is patchy.[3] Many argue that blinding referees to authors' identities would result in fairer judgments. Indeed, one recent study—the first for a medical journal—showed that opinions given by blinded referees were better than those from unblinded referees,[14] but this is a debate that will continue.

One difficulty in assessing the quality of peer review is knowing what to measure, which may explain why it has proved easier to get clearcut results about statistical reviews than about clinical reviews. Gardner and

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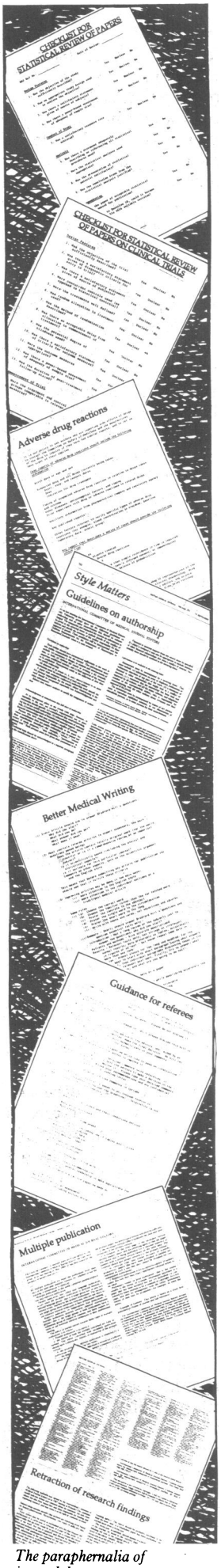


The paraphernalia of journalology

Bond, for example, showed that statistical review did improve the quality of published papers,[15] but they used the quite precise checklists of questions they ask of each paper to measure this.[16] They examined 45 papers published in the *BMJ* in the first half of 1988; on submission only five had been considered statistically acceptable. As a result of statistical review and amendment by their authors 38 were considered acceptable by the time they were published.[15]

Statistics

Nineteenth century editors did not think about statistics, but no contemporary editor (or author) can afford to ignore them. The *BMJ*'s earliest mention of statistics was in 1936, when it published Mainland's paper on chance in clinical medicine[17]—two years after a similar paper in the *Canadian Medical Association Journal*.[18] Nevertheless, the fruitful and fortuitous collaboration between Hugh Clegg and Austin Bradford Hill in the 1940s meant that statistics has played an important part in the *BMJ* from then on. In 1977 we appointed our first statistical adviser, after Gore *et al*'s critique of published papers,[10] and the process has continued with the development of statistical checklists,[16] guidelines for authors,[19] and papers on confidence intervals.[20,21]

These changes have not been confined to journals, of course—or even mainly to do with journals. But they illustrate the way that editors can influence standards—and even enforce them by their practices in selecting papers. The fact that virtually every clinical research paper published in the *BMJ* is accepted subject to revision means that the journal powerfully influences the way that what is published appears.[19]

Confidence intervals are the most recent example. Statisticians have long complained that reliance on a p value to indicate significance can be misleading, arbitrarily designating a difference of, say p=0·049 as meaningful and one of p=0·051 as meaningless. A confidence interval round a mean difference gives readers a more accurate measure of its uncertainty and hence a better sense of the "confidence" with which they can attribute meaning. Since the *BMJ* published Gardner's and Altman's original article in 1988[20] many other medical journals have promoted the message.[22-24] Although few of the papers submitted to the *BMJ* include confidence intervals, an increasing proportion of those actually published do—as a result of requests made by the journal and its statistical reviewers.

Language

Statistics has brought its own language; but even before that the language of clinicians was changing. Gone are the days when a case report read more like a novel by Dickens. Thus Robert Storrs, a surgeon from Doncaster, opened his description of a case of eversion of the uterus in 1841: "Mrs Lowther, aet twenty-two, a stout good looking woman of lymphatic temperament, was delivered, after a tedious labour, of her first child." Like all good novels the story ended happily. After a considerable but successful tussle with Mrs Lowther's everted uterus Storrs reported, "About a fortnight ago I saw her tripping along the street quite well."[25] Such personal description is not now thought to be the language of science, and identifying the patient would be unacceptable (though some journals persist in using initials).

With the rise of "scientific" studies came the rise of an "objective" language—one that the *BMJ* has been trying to stamp out for years.[26] Writers believe that objectivity demands that they obliterate any hint of themselves—Robert Storrs would have appalled them. They thus write largely in the passive with many abstract nouns; the *BMJ*'s technical editors spend much of their time unscrambling this prose, following the rules of Gowers[27] and Strunk and White[28] to make it clearer and more direct for general readers and those whose first language is not English. It is not simply a matter of stylistic clarity but also of responsibility, which gets lost in vague "it was decideds."

Structure is a different matter. In the 1940s articles started to fall into the familiar shape of introduction, methods, results, and discussion (IMRAD), with a summary (later an abstract) at the end (later the beginning). Now this format is a worldwide convention, enshrined in instructions to authors like the Vancouver style.[29] Again we are indebted to Austin Bradford Hill for his elegant explanation of its rationale, given in a speech to the World Medical Association reported in the *BMJ*.[30] Each of the four main sections of an article should, he argued, answer the four questions a reader needs to know: Why did you start? (introduction). What did you do? (methods). What did you find? (results). What does it mean? (discussion). Medawar criticised the convention, claiming that science did not fall into the neat sequence implied by the IMRAD structure.[31] Medawar was right, yet IMRAD provides a convention that both authors and readers understand. To write comprehensibly about science as it really happens demands literary skill. The IMRAD structure also implies that a study was planned and thus provides a good model. For studies planned in advance, with hypotheses to be tested, stand the best chance of saying something sensible. Such are the dangers of post hoc analyses[32] that some have advocated that journals should accept papers in advance on the basis of their protocols,[33] arguing that if the question is important a journal should be interested in publishing the answer—whatever the answer is.

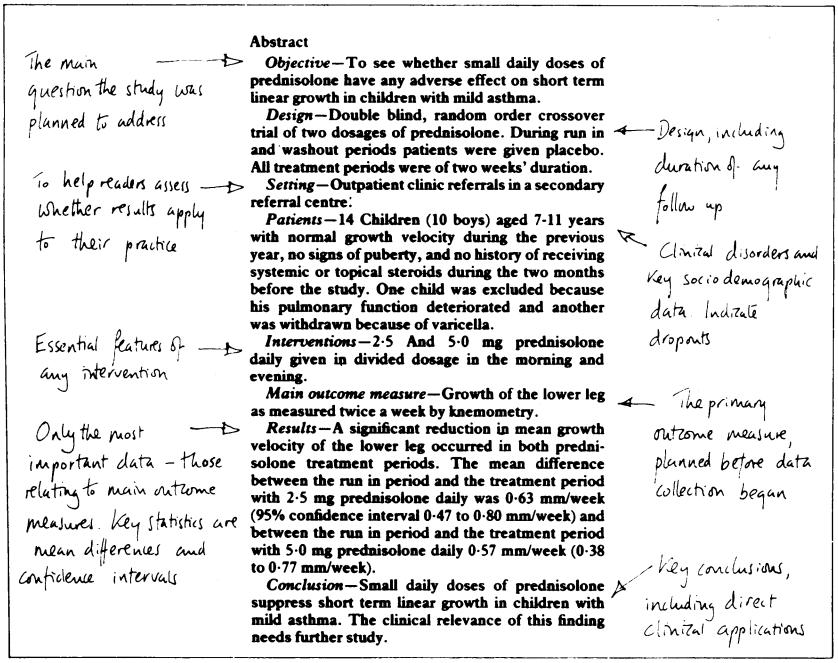
But journals are interested in the answers. Purists criticise them for not publishing negative results, thus undermining the meta-analyses that rely on published studies.[34] More seriously, there is evidence that authors, anticipating this prejudice, do not bother to write up negative findings. This is one reason why Chalmers has gone to such lengths to trace all trials—published and unpublished—for his database of perinatal trials.[35] Nevertheless, general journals are both scientific journals and medical newspapers[36]—and newsworthiness is important to them. A study with a positive result is saying to readers, you must change your practices; here is evidence that this drug or this way of organising care is better than the existing conventional drug or method. A study that shows something new to be no better than the old needs to be published—if only to stop others testing the question again—but editors of general journals would often think a specialist journal more appropriate.

IMRAD and abstracts

Bradford Hill also takes credit for persuading Hugh Clegg to introduce abstracts in the 1940s—but other journals were doing so at the same time, under the pressure of secondary abstracting agencies. Abstracts

1960s

- **1967: Christiaan Barnard starts transplanting hearts**
- **1967: John B Gurden produces the first clone of a vertebrate—the South African clawed toad**
- **1969: First coronary bypass operation is performed**



Abstract

***Objective*—To see whether small daily doses of prednisolone have any adverse effect on short term linear growth in children with mild asthma.**

***Design*—Double blind, random order crossover trial of two dosages of prednisolone. During run in and washout periods patients were given placebo. All treatment periods were of two weeks' duration.**

***Setting*—Outpatient clinic referrals in a secondary referral centre.**

***Patients*—14 Children (10 boys) aged 7-11 years with normal growth velocity during the previous year, no signs of puberty, and no history of receiving systemic or topical steroids during the two months before the study. One child was excluded because his pulmonary function deteriorated and another was withdrawn because of varicella.**

***Interventions*—2·5 And 5·0 mg prednisolone daily given in divided dosage in the morning and evening.**

***Main outcome measure*—Growth of the lower leg as measured twice a week by knemometry.**

***Results*—A significant reduction in mean growth velocity of the lower leg occurred in both prednisolone treatment periods. The mean difference between the run in period and the treatment period with 2·5 mg prednisolone daily was 0·63 mm/week (95% confidence interval 0·47 to 0·80 mm/week) and between the run in period and the treatment period with 5·0 mg prednisolone daily 0·57 mm/week (0·38 to 0·77 mm/week).**

***Conclusion*—Small daily doses of prednisolone suppress short term linear growth in children with mild asthma. The clinical relevance of this finding needs further study.**

not only embody the recognition that most people do not read most articles in full but also form part of attempts to "organise the literature" to cope with ever increasing numbers of journals. In the 1960s and 1970s the National Library of Medicine in the United States computerised its bibliography, *Index Medicus*, including abstracts to improve the ability of searchers to identify relevant papers.[37]

A good abstract should be an accurate précis of the paper, reflecting in miniature the answers to Bradford Hill's four questions. But even abstracts that do answer these questions (and many do not) may do so inadequately, leaving out information to enable readers to assess them properly. The structured abstract aims to overcome this defect by ensuring that all necessary information is given.[38,39] The set of structured headings—seen in any *BMJ* (save this one)—forces authors to provide information on the objective of their study, its design and setting, the numbers of subjects studied (including drop outs), the interventions, the main outcome measures, the results, and the conclusions. Moreover, it also imposes a controlled vocabulary so that readers can distinguish prospective from retrospective, controlled from uncontrolled, and cohort from case-control studies. Structured abstracts were clearly designed with randomised controlled trials in mind but are applicable to any original article "dealing with the cause, course, diagnosis, and treatment of health care problems and the quality and economics of health care."[38]

Ethics and fraud

That most studies published in medical journals are ethical—with informed consent and approval from ethics committees—is largely due to concerns expressed after the second world war and moves such as the Declaration of Helsinki to make researchers, editors, and the public aware of good practices. Indeed, the declaration is another of Clegg's achievements.[40] Although ethics committees do not always work as well as they might,[41] the need for them is accepted, and contemporary concern has moved on to ways of preventing and dealing with fraud.[42] As the Vancouver group has pointed out, editors individually cannot do much about detecting fraud, though they can help in raising standards and by printing retractions.[43]

The Vancouver Group itself—an informal grouping of the editors of major English language general medical journals—reflects the rise of journalology. It started off by producing the Vancouver style in 1979—a uniform set of instructions covering format and reference style so that authors would not continually have to keep altering the presentation of their papers to meet the whims of different journals.[29] But now the Vancouver style also includes strictures against duplicate publication, guidance on statistical aspects of trials, a request for declarations of interest and acknowledgment of financial or material support, and a definition of authorship.

The definition of authorship arose from one of the most notorious cases of medical fraud—that of John Darsee. He fabricated data and published them in several papers with distinguished coauthors. When the frauds came to light it was clear that these coauthors had played little part in the work that bore their names. The guidelines on authorship underline the idea that authors must be able to take responsibility for their work, and they have proved an effective tool when editors have wanted to challenge single case reports with 12 authors. If, nevertheless, fraud does occur, there is a recommended format for retractions, ensuring that they can be linked to the original papers by indexing agencies.[29] Fraud, however, is not, as Lock has pointed out, an isolated event: it is one end of a range that starts with sloppy science.[42] One of the guardians against sloppy science is detailed peer review,[42] which does lie in the hands of editors, but preventing it demands mechanisms in the institutions where research is generated and performed.

Conclusions

The general journal is an anachronistic beast in an increasingly organised age. Theoretically, its scientific base should have become whittled away as specialties and subspecialties have burgeoned. Yet, as Stephen Lock, the *BMJ*'s editor, has argued, general journals probably stand the best chance of surviving the threatened demise of printed journals and their takeover by computerised databases holding individual research papers.[44] They do so because they do not serve a single purpose, but rather a series of purposes—of informing, educating, entertaining, and amusing.

Although there are still plenty of case reports in the *BMJ*, distinguished from their predecessors of the past 150 years by greater formality of language and variety of investigations and treatments, increasingly our scientific papers are epidemiological papers, randomised clinical trials, meta-analyses, and reports of health service research.[45] Indeed, Haynes has argued that clinical journals should print only randomised clinical trials or structured review articles[47] (Haynes RB, symposium to mark the retirement of Ed Huth, Philadelphia, 1989).[46] As a member of the McMaster group that promoted critical reading techniques[47] and developed structured abstracts, he argues that case series and uncontrolled studies are merely anecdotal communications between colleagues that might be the starting point for a trial but should not be published in clinical journals because they are an inadequate basis for practice. His scepticism about clinicians' ability to distinguish between the evidential weight of different types of papers might be justified: he and his colleagues discovered that clinicians using literature searches on Medline tended to base management decisions not merely on the abstract of a paper (perhaps not so bad with structured abstracts) but sometimes simply on the title.[48] Thus, if in the next 150 years the *BMJ* changes the habits of a lifetime and drops case reports it will be—like most things that editors do—for the benefit of the reader.

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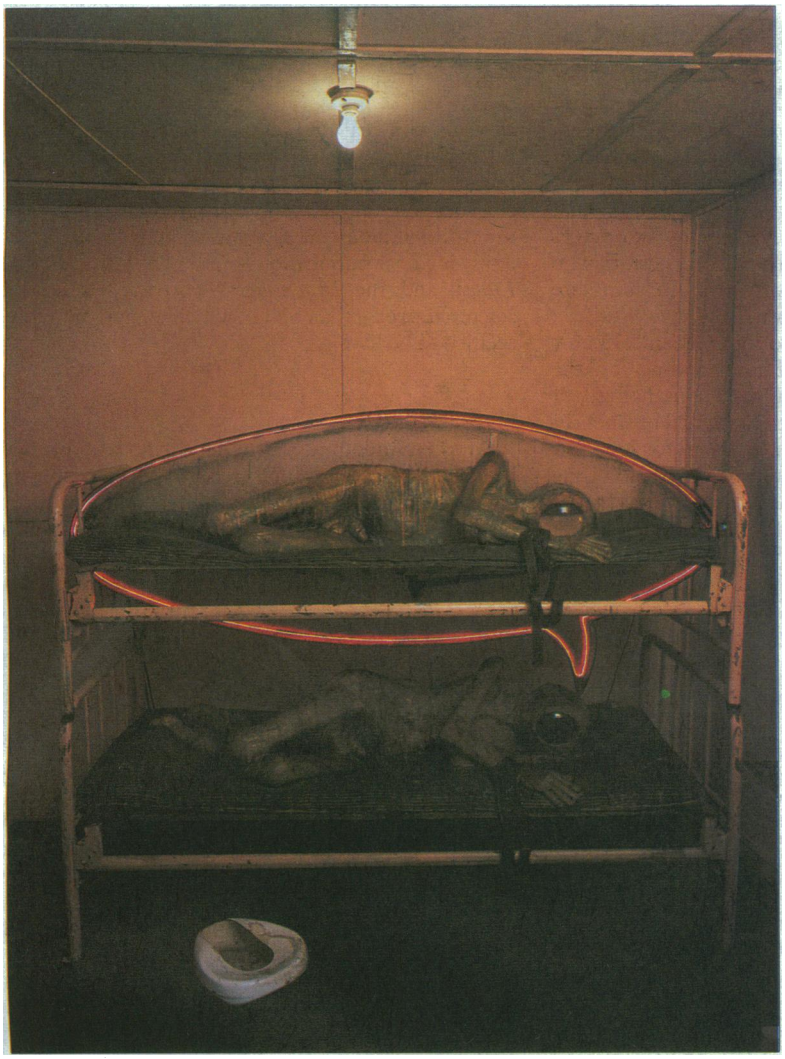

The State Hospital (1966)
Edward Kienholz (b 1927; American)

During the 1960s Edward Kienholz was a prominent member of the Californian school of funk art, sometimes called sick art. In an attempt to penetrate beneath the glitzy façade of American life he created several gruesome and shocking tableaux. "The State Hospital" is one of these, showing a cell containing two emaciated patients strapped to their bunks. These figures, with their discoloured, leathery skin and goldfish bowl heads complete with fish, are effigies of the same man. In the upper bunk enclosed in a strip cartoon balloon is the patient's own self image. If the aim of this grotesque vision of a living hell is to shock it succeeds. Anything more repellent is hard to imagine.

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