

In defence of the anonymous referee

SINCE Professor Fraenkel-Conrat's plea for an end to anonymity in the evaluation of scientific manuscripts three months ago we have received several more letters on the subject. One or two have been published, but the majority have said little more than 'me-too' in tones ranging from the reasonable to the outraged and lengths ranging from one paragraph to ten pages. The question of anonymity in refereeing appears regularly in the correspondence columns of scientific journals, usually associated pejoratively with a cloak, and it is desirable that scientists give more than a passing thought to it every few years. In most other fields the arbitrator is not anonymous—justice, sport, criticism of the arts, industrial relations all maintain highly visible decision-makers. Can science alone remain aloof from requiring personal accountability in its judgements?

Those who advocate the removal of the cloak point to two benefits which, it is claimed, would immediately accrue. Referees would have to take their job more seriously since their judgement would be semi-public, and the stab-in-the-back opportunity for the malevolent referee would disappear. There cannot be an editor who wouldn't be glad to eliminate careless or mischievous refereeing, although its incidence may well be greatly exaggerated. On the other hand the debit side of removing anonymity is rarely mentioned.

One of the commonest misconceptions about refereeing is that it is done by a small elevated community. A careful study by Zuckerman and Merton (*Minerva*, 9, 66–100; 1971) showed very clearly that this was not true for *The Physical Review*, and although we have not the same impressive statistics to parade, the file of referees for *Nature* in 1973 contained more than a thousand names. Could we call on such numbers if the identities of referees were revealed? Only, we maintain, when the job was going to be the agreeable one of recommending the acceptance of excellent manuscripts. Large numbers of neither careless nor mischievous referees would send back, unreviewed, manuscripts which they believed to be inadequate and with those authors they had no desire to engage in combat.

Three points need emphasising.

First, referees should not be asked to endure face-to-face or even telephone encounters with irate authors. The sort of verbal roughing-up that even *Nature's* office staff occasionally have willed on them makes it clear that referees should not be placed in a position such that they can be got at by authors. The presentation of science is and must remain ultimately a written activity. This is not to deny the obvious power of the spoken word in education, exposition and elucidation. It is to deny the use of verbal battles, in which forcefulness, slick

presentation and quick wittedness are prized assets in resolving scientific disputes. It would be unfair to ask a referee to engage in conflicts on our behalf, and in any case most would quite reasonably decline to do so. This would result in the decision-making process falling back on a smaller core of conflict-hardened reviewers.

Second, not all the unpleasant things that are said from behind the cloak of anonymity are said from a desire to do mischief. The rather common home truth 'this is the tenth paper based on the same data that the author has attempted to get into the literature in the last year' may be motivated less from malice than a genuine dislike of the practice of multiple publication. Such things would be less easy to say without anonymity—some forms of truthfulness would be positively impaired by identification.

Third, the anonymous opinion, to the conscientious author, cannot easily be discounted. The referee serves more than as just the expert vouching for technical plausibility. He is also the potential reader vouching for relevance, wide appeal and readability. He is the one sample of 'readership' opinion that an author will normally go on. The value of these functions can be diminished by knowing the referee's identity and therefore dismissing his critique on purely personal grounds.

Would identification even do what its advocates claim: bring more conscientious and honest reviewing and deter mischief-making? The latter, probably yes, to a certain extent, although most of those already involved in mutual back-stabbing are well enough known to each other not to worry much about anonymity. Some of these combatants are also well enough known to us not to be allowed too frequent a battle. The former, probably no. The tendency would be just as much towards pulling punches and being less critical.

The tradition of depersonalisation in the assessment of manuscripts has, we believe, served scientific communication well. There are obviously flaws in the system, but radical steps to remove these flaws might remove instead a lot which is good.

100 years ago



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ON the 16th inst., at a congregation held in the Senate House, Cambridge, the Cavendish Laboratory was formally presented to the University by the Chancellor. The genius for research possessed by Prof. Clerk Maxwell and the fact that it is open to all students of the University of Cambridge for researches will, if we mistake not, make this before long a building very noteworthy in English science.

From *Nature*, 10, 139, June 25, 1874.