

Letters

Referees: Anonymity and Other Problems

I take exception to only one of Forscher's suggestions (twice made) in his "Rules for referees" (15 Oct., p. 319): "If the editor transmits [the referee's] comments verbatim to the author, then the name of the referee should also be transmitted." Scientists are prone to react rather violently to criticism, or suggestions, no matter how valid or mild. Lasting and destructive enmities can be established by providing names of referees to authors. I could not obtain objective reviews from referees if they knew beforehand that the authors would have access to their names. Indeed, I might not be able to obtain *any* reviews! If I provided a referee's name without forewarning him, I would probably never have his assistance again. Some referees do allow their names to be included with their reviews, and in such instances I make their names available.

Forscher offers the editor the alternative of rephrasing (or taking excerpts from) the referee's comments and transmitting them to the author without identification. Such revision is senseless if the statements are made clearly and without rancor. An editor may occasionally lose important nuances of meaning by rephrasing a specialist's statements.

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... As the editor of a journal that has traditionally relied on a rigorous reviewing system, I have grave doubts about the wisdom of having referees sign the comments that are transmitted to the author. Anonymity encourages frank, forthright expression of opinion. If the issues are purely scientific, I do not see why it is necessary to identify the source of the criticism. Of course,

the comments of referees, even when unsigned, are not always models of objective criticism. It is the editor's responsibility to see that the comments he transmits contain nothing irrelevant, illogical, or unnecessarily abrasive. This is a difficult job and the editor will not always succeed. Therefore, our policy is that the author who considers his manuscript unfairly treated is allowed an opportunity for rebuttal and for a fresh review if this seems indicated. In the exchange of views that such an appeal procedure entails, the signing of reviewers' comments would only complicate matters. The indignation of the author at bay before his critics is notorious, and in the acrimonious clash of personalities that might well ensue it would surely be difficult to arrive at a fair and dispassionate editorial decision.

Forscher's suggestion that referees ought to be allowed to append critical statements to published papers also seems to me questionable. When a reputable scientific journal publishes a paper it should be understood that neither the journal nor its sponsoring society guarantees the ultimate correctness of the author's thesis. Publication means only that after careful review by experts in the field the final manuscript is believed to be competent and significant enough to warrant serious attention. The journal in publishing the contribution is merely offering the reader assurance that there are no obvious flaws in the paper which vitiate its claim to consideration. If a manuscript has been required to meet certain standards of quality before acceptance, I see no reason why it should have to be challenged in print by official caveats from the reviewers. It doesn't seem to me fair to require that the author share the stage with the reviewers.

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... Several additional points may be worth considering:

1) In view of the recognized possibility of "conflict of interest" and of the undue influence that may be exerted by the author's reputation or institutional affiliation, ought it not be suggested that manuscripts placed in the referee's hands be entirely anonymous, so they can be judged solely on their merit?

2) If the author's identity is made known to the referee, then—whether the editor chooses to quote from the referee's comments or to forward the report to the author in its entirety—would it not be reasonable to make mandatory the naming of the referee, so that the author can make his rebuttal with full knowledge of his critic? In my view, if "truth" is to be best served, either both or neither of the correspondents should be identified.

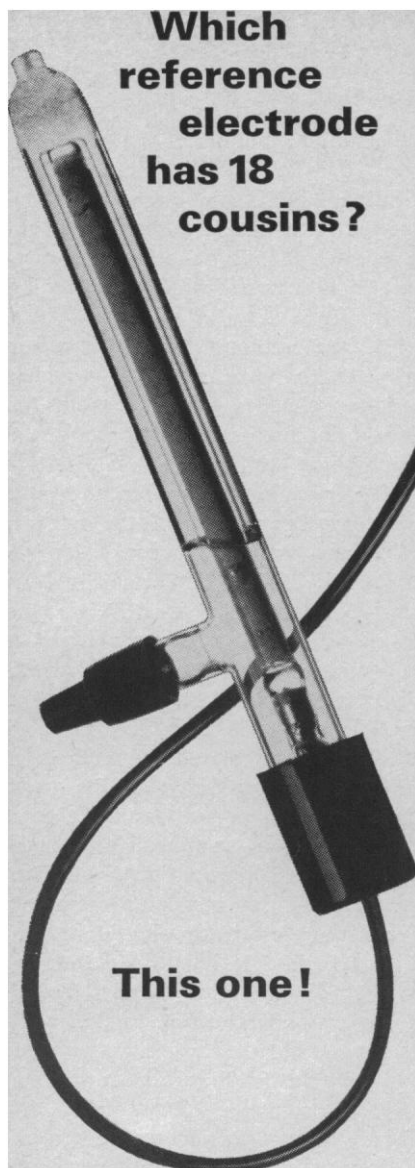
3) The policy of most journals and publishers, either stated or implied, is to disclaim responsibility for the views and statements of authors. Yet most editors are loath to publish controversial opinions, speculations, ruminations, and pure conjecture on the grounds that discussion should be limited to the data at hand. Also missing are the illogical but often fruitful thought processes leading to the methods of investigation adopted, and the potentially fascinating accountings of the errors, agonies, frustrations, and joys of discovery encountered along the way. The published paper is a model of conformity in thought and style, adhering rigidly to the "scientific method" and to a sterile and unimaginative prose that belies the frequently unorthodox thinking, individuality, and literary capabilities of its author.

In the final analysis, if a gracious editor permits the publication of a questionable manuscript, it is the author, not the editor or journal, that will stand or fall in the judgment of his peers.

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... A referee should not only assess the scientific value of the manuscript, but he should ensure that it tells a story or conveys an argument which the majority of readers of the journal can follow and understand. On this aspect of the problem Forscher spends



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only nine lines. Much of the literature published today is obscure and ungrammatical, and these faults will only be corrected when the rules of grammar are applied and clarity of expression is set high on the list of standards by which a manuscript is judged.

Forscher omits to mention that the referee has a responsibility to the author, who is entitled in this highly competitive age to prompt consideration of his manuscript. If a referee is too busy or too lazy to fulfill his obligations within a reasonable time, he should not be entrusted with the responsibility. . . .

If it is the editor's responsibility to make the final decision about publication, it should also be his responsibility to weigh the advice of his referees. He need transmit to the author only those comments that he deems necessary for the improvement of the manuscript or for justifying its rejection; this he can do without disclosing the referees' names.

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Forscher's suggestion that verbatim comments from the referee be accompanied by his name appears to offer one way to curb the tendency of some referees to make unsupported judgments such as "naive," "superficial," or, in an extreme case, "stupid."

Moreover, communication of the author with the referee could in some instances prove to be mutually beneficial.

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Competence in the Universities

With regard to the Reuss subcommittee's inquiry into the relation between federal support for basic research and the quality of university science teaching (News and Comment, 22 Oct., p. 464), I offer the following observation: Having completed some 20-odd undergraduate and graduate courses in botany and genetics at four institutions (Washington University, the University of Michigan, Claremont Graduate School, and the University of California at Berkeley) during the pre- and post-Sputnik era, I have encountered teaching ranging from excellent to very poor. In every case the good-to-excellent teachers were those who were actively engaged

in significant basic research, whereas the poor teachers were also ineffectual as researchers. Thus my experience does not bear out the assumption that teaching and basic research are antagonistic duties of the university scientist. One might better regard poor teaching as simply one more aspect of professional incompetence.

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Antiunion

In the issue of 15 October (p. 292) there is a letter headed "No antineoplastic effects." Now, what can *an-īn'-ē-plas-tīk* mean? I get it! It means *anti-neoplastic*.

Why is the hyphen so avoided? The dashed little dash makes for clarity. How can one pronounce and divine the meaning of *picornaviruses* without hyphens? It's easy when you write it right: *pico-RNA-viruses*. That does for many another inelegant formulation born out of the modern, hasty need for neologisms and nonce words.

There ought to be a law: Dash it! As for acronyms: To hell with them.

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Erratum

The 22 October issue presents the wildly improbable coincidence of containing both a letter about parapsychology and "spontaneous cases" (p. 436) and a "spontaneous case." For on page 463, as part of my comments on the 1965 Nobel Laureates in Medicine or Physiology, there appears the phrase "The operator 'loses'. . .," though what I had actually written was "The operator 'closes'. . . ." Now since I happen to have some doubts about the validity of the operator concept, doubts that I certainly would not consciously have wished to introduce on this happy occasion [though I did voice them earlier in *Science* 144, 816 (1964)], this strange error can be explained only as a Freudian slip by a member of the editorial staff of *Science* acting under the telekinetic influence of an author's psyche.

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