

Concerning "An Evaluation of the Peer Review Process at *Angewandte Chemie*"

By Paul F. Ross*

Research leading to improved practices in peer review in the sciences is badly needed. Daniel's work ("An evaluation of the peer review process at *Angewandte Chemie*"^[1]) can contribute to understanding influences on decisions. My work^[2] and the demonstration by Peters and Ceci^[3] showing that current practices are seriously deficient are a decade old; yet few studies have been stimulated by our work. Daniel's work and *Angewandte Chemie*'s willingness to be examined are welcome.

Daniel's work addresses the validity, reliability, and biases of decisions and follows up on rejected work using a single sample of manuscripts, an approach essential to understanding outcomes of decisions by reviewers and editors. However, as presented, Daniel's work ascribes to scientific value outcomes that are actually caused by circumstances unrelated to scientific content. 1) Daniel's work assumes works published in German only have the same opportunity to be cited as works published in both German and English. That is like expecting the same monthly sales total for a \$ 200 fashion item available only in Berlin and an equally fashionable \$ 200 item available in both Berlin and New York.

2) Daniel's work assumes that the visibility of the journal in which an item appears has no influence on whether the item is cited. No executive deciding where to publish an advertisement would accept that premise. 3) Daniel's work assumes that the author's reputation, and the reputation of the author's employer, have no effect on the editor's decision to publish and 4) no effect on the citing author's decision to cite. Research shows otherwise (e.g., Peters and Ceci^[3]). 5) Daniel's work counts citations for items published in *Angewandte Chemie* over a somewhat longer time period than for items rejected by *Angewandte Chemie* and published elsewhere, exaggerating the observed differences (see Daniel's Figure 1). Daniel's data need to be reexamined, controlling for these and other influences using the statistical method of factor analysis.

For these reasons, I strongly suspect Daniel's conclusion that the 1984 editorial decisions at *Angewandte Chemie* accepted for publication the manuscripts of highest value to scientists from among those it received is an incorrect conclusion.

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[1] H. D. Daniel, *Angew. Chem.* **1993**, *105*, 247–251; *Angew. Chem. Int. Ed. Engl.* **1993**, *32*, 234–238.

[2] P. F. Ross, unpublished manuscript "The sciences' self-management: Manuscript refereeing, peer review, and goals in Science", 1980.

[3] D. Peters, S. Ceci, *Behav. and Brain Sci.* **1982**, *5*, 187–195, 246–255.

Reply:

By Hans-Dieter Daniel*

In his letter to the editor of *Angewandte Chemie* Ross^[1] argues that my work^[2] on the peer review process fails to control for the language in which the work was published, the visibility of the journal, the author's reputation, and the reputation of the author's employer, and that citations were not counted for equal-length post-publication periods for accepted and rejected-and-published-elsewhere communications.

To start with his last point: the analyses of covariance carried out to test for publication bias and validity of editorial decisions of course took into account the delay period from time of rejection by *Angewandte Chemie* to publication in another journal. The mean citation rates for accepted and rejected-but-published-elsewhere communications were

compared, with number of months since publication of the manuscript held constant. It was not necessary to test for bias related to language in which the work was published, since very few of the rejected manuscripts subsequently published elsewhere appeared in German (cf. Table 4 in [2]). Prestige ratings of scientists, departments, and universities are very popular, and are published regularly in the United States. Unfortunately, there has been so far no analogous prestige rankings for most of the 21 different countries from which *Angewandte Chemie* received manuscripts during 1984. Therefore, it is not possible to test for bias related to prestige of author or employer. Moreover, how should one test the provocative thesis that the same paper published in *Angewandte Chemie* and in another journal will be cited more times from *Angewandte Chemie* than from the other journal?

To control for bias variables, Ross proposes a factor analysis with dummy variables. Factor analysis, however, as-

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