

Distorting the Practice and Publication of Research

I am leaving the editorship of *AIChE Journal* after six years with satisfaction in our accomplishments. Papers routinely appear in print within a year of the date first submitted; the average elapsed time from submission to publication was eight months for the past year. This achievement is due to the exceptional diligence of many reviewers and in particular to the efforts of Haeja Han, the Managing Editor, who has succeeded in halving the production time. Rapid publication has been achieved while maintaining the highest standards of quality, again because of the unselfish work of the reviewers and the Consulting Editors. During the transition period beginning in January, 1991, the Consulting Editors have borne the major responsibility for the selection of reviewers and subsequent editorial decisions. While we have succeeded in broadening the content of the *Journal* to reflect the scope of current chemical engineering research more closely, I regret that too many of our colleagues still prefer to publish all of their work in places that are not accessible to the general body of chemical engineers, even work that is sufficiently nonspecialized to make it of interest to the broad chemical engineering community.

I wish to take this opportunity to make some observations about publication practices in chemical engineering. They are really comments about academic practices, since most papers published in the *AIChE Journal* and similar journals are written by university professors and their students. Firstly, far too much is being published, resulting in a proliferation of journal titles and pages, and a serious depletion of library budgets. Today's accepted practice is to write a series of progress reports, with the next manuscript often submitted before the prior one has appeared in print, and to publish everything that seems "publishable," whether needed or not. Secondly, what is being published is frequently broken apart and spread among several journals ("salami publishing" or use of "the least-publishable unit"), leading the reader through a maze to acquire a complete picture of a piece of work. Indeed, publication has become an end in itself, rather

than a means of communicating information; how often have we heard this damning commentary on a piece of research: "At least I got a paper out of it." (The complementary practice of presenting the same work at meetings of more than one major professional society is the reason that our schedules are filled with more and more meetings of diminishing substance.)

The twin devils that have led us "to satisfy [our] hunger like a nanny goat" (Kazantzakis, *Report to Greco*) are the perceived demands of funding agencies for quantifiable evidence of "productivity" and the present structure of the university reward system. The former is regrettable and damaging to scholarship, but is at least understandable; there is little excuse for the latter. I know of several chemical engineering departments where the measure of faculty effort and accomplishment rewards the *number* of refereed publications, and I know of departments where assistant professors are told the *number* of papers in refereed journals expected for tenure. Is it any wonder that we are deluged by excessive publication of incomplete work, often in "safe" areas where results are assured?

My Canute-like effort to deal with this phenomenon as an editor has been to decline to consider papers that depend in a substantive way on work still under review, to insist that multipart papers be combined, to reject work that has appeared in any form in the multitude of proceedings volumes that are choking our libraries, and to publish most "follow-up" manuscripts only as short notes. This has simply slowed the wave. The problem will not be solved until the academic community and funding agencies come to grips with the way in which they have distorted the practice and publication of research, and adopt new measures of excellence and new expectations of accomplishment.



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