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# DROWNING IN A SEA OF REFEREED PUBLICATIONS

The revolution in scientific publishing brought about by the Internet should be an opportunity to make the literature free and to raise standards

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**A**RE THERE TOO MANY JOURNALS, too many papers, even too many meetings these days? Most people seem to think so. There's a dilution effect. More is less.

As referees, we are constantly being pressured to turn manuscripts around faster. Quality must be suffering. New journals are appearing regularly, while old ones die slowly. Libraries can't afford them. Editors are so overloaded that editorial input into the science is now very rare. Authors believe that they have to publish something three times to get heard. The newest scientific unit, the LPU (least publishable unit), is being overused. Exaggeration of novelty and significance is common, and readers seem overwhelmed.

Many scientists complain of being too busy to read the literature. This is not good. The importance and integrity of published work must remain paramount.

**HOW DID** we get to this? Certainly these days, with modern instrumentation and more practitioners, we are producing data at greatly accelerated rates. However, chemistry is no longer an infant science. Not all research requires full publication. The more predictable a result, the less it needs to be published. The case can be made for electronic archives for results that add to the storehouse of knowledge but contain insufficient conceptual advance to warrant publication as a paper. Refereeing would still be required.

From my observations, commercial publishers are driving the problem and have for a long time. Our learned

societies have tended to compete by following along. With a quickness to start new journals, ego-directed invitations, and now (heaven forbid) unrefereed Web preprints (Elsevier's latest play to our all-too-human weaknesses), publishers feed into scientists' desires to get publicity and to extend their publication lists. We need counterbalancing inducements for restraint. We need quality, not quantity.

Much has been written about the "crisis of access" to knowledge as libraries are forced to cancel subscriptions ("A Question of Access," R. K. Johnson, <http://www.dlib.org/dlib/may00/johnson/05johnson.html>). Solutions have been suggested ("Create Change," <http://www.arl.org/create>), but there is a tendency to skirt issues

of our own culpability and the active roles we must take to remedy the problem. Via the Internet, a revolution in scientific publishing is ongoing. We must take this opportunity to make the literature free and to lower the volume by raising standards.

## WHAT CAN be done?

■ Learned societies must take firm control of the literature, minimize the profit motive, and be vigilant about maintaining standards. They have traditionally been better at this than commercial publishers. Let us declare that the "market forces approach" to managing journals is an interesting experiment, now out of control. It has failed because the customers essentially give away the product and have little incentive to exercise buying preferences. If the budgets for chemistry journals were in the hands of the chemistry departments rather than libraries, chemists would be motivated to do something.

With significantly lower costs being realized by electronic publishing (the American Chemical Society has done well by moving fast in this direction), learned societies must move rapidly toward making their journals free, at least electronically. After all, authors do all the work and get the paper in publication-quality format, only to give it away free of charge or even pay page charges to a publisher that turns

around and charges others pay-for-view. Only refereeing, provided free, and prestige are value-added. Editorial and production costs are large only for the print edition—now a luxury item and nearing obsolescence. I think it is a mistake for *Chemical Abstracts* to begin abstracting unrefereed Web preprints (C&EN, June 5, 2000, page 15). We will soon be drowning in a sea of unrefereed papers.

■ Regarding copyright, one deliciously seditious thought keeps occurring to me: Why don't we all conspire to stop signing away copyright? Are we that desperate to publish? Publishers should ask only for a *Consent to Publish*. Lawyers and profit-makers don't belong in the free knowledge business.

■ As authors, let us all pause before we write a paper and examine all of our motivations for publishing. How close do they match the ideals of the pro-



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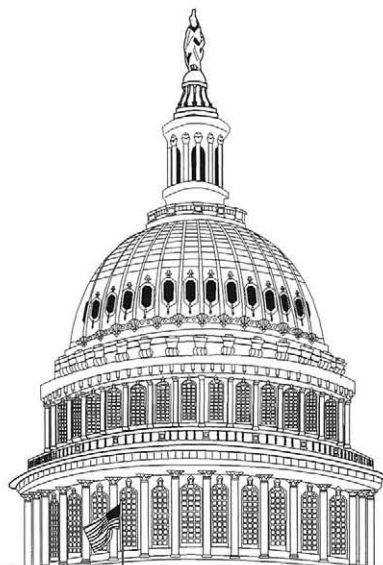
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fession? Would a little self-restraint and self-refereeing help unburden the system? It is sobering to learn that Nobel Laureate Robert B. Woodward published fewer than 90 papers in his illustrious career. Let us all stop sending our work to journals that gouge libraries. Attractive as it might seem, let us quickly abandon the idea of simply publishing on websites. To remove refereeing is to remove quality control. Posting pdf files of our papers on personal websites after refereeing will help solve the problem of access.

■ Referees have real power (and responsibility) in this whole process. As reviewers, we must be thorough and have high standards, but we must never be unfair or unkind. Furthermore, we can easily deny our free services to low-quality, overpriced journals.

■ New journals (and the flattery of being asked to serve as editor or editorial board member or to contribute a paper) should be resisted with great fortitude until necessity is absolutely certain. Libraries should continue to cancel subscriptions to high-priced, low-impact journals and should deliberate long and hard about taking new ones. In chemistry, the crisis is more one of quality than access.

■ Funding agencies should consider making grants longer in duration (for example, five years) and doing away with annual reports. The one thing basic science needs is long-term funding. The pressure to crank out LPUs will be diminished. With less frequent renewals, investigators may find more time to read the literature. Grant renewals could be more accomplishment-based, with the emphasis on quality, of course, by submitting five best papers for the referees to actually read and listing no more than five others. The National Science Foundation is moving toward this approach.

■ Academic promotions could similarly be based on a very small number of scholarly papers. We need some real incentives for scholarship that is measured by quality and impact over time, rather than "promotion by page counting." We probably need promotion criteria to explicitly state

that publication in low-quality journals will count *against* promotion. Editorships or editorial board memberships have become institutionalized criteria for academic promotion. What if such appointments to low-quality journals also counted against promotion? In my experience, departments are rather good at measuring scholarship, but other influences tend to subvert the promotion process.

**Attractive as it might seem, let us quickly abandon the idea of simply publishing on websites. To remove refereeing is to remove quality control.**

■ Conference organizers should resist publishing conference proceedings for all but the most exclusive or unique meetings. Proceedings are often misused to pad curriculum vitae, to slip unrefereed data into the literature, and to publish the same results twice. Abstracts do not belong on a publication list.

■ Research mentors, teach your students well. Students tend to adopt your values. Set standards high. No one is done a favor when a weak Ph.D. gets sent out.

**WHY SHOULD** we worry about all this? Quite simply, huge savings can be realized if we act collectively to decrease the volume and get the profit out of publishing.

More important, we work in a noble, vibrant, and centrally significant profession. Integrity demands that we aspire to the highest standards. In fact, the whole scientific enterprise relies on it. (Check out the movie, "The Insider," in which a chemist is the hero because of his integrity.) Publication quality must remain the true measure of a scientist.

We tend to forget that students, the "seed corn" of our profession, are quite idealistic. If they start to feel collectively that the research enterprise is just a publication game, it will get very much harder to turn the best and brightest young minds on to science. A fair amount of cynicism already exists.

Finally, if the notion arises in the public arena that scientists are spinning their wheels, some politician is going to conclude, wrongly, that there is too much money in science. The consequences could be truly disastrous.

This is a very serious business.

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