Why the pressure to publish?

Despite technological change and the increasing difficulty of managing the scientific literature, there seems to have been little change as yet in the pattern of publication. But that could change quickly.

MOST people will agree that there is far too much of the scientific literature, but nobody seems to have a convincing remedy. Indeed, the general opinion is that the numbers of journals, and the physical bulk of the successful among them, will continue to grow more or less as they have done for the past several decades. But there is just a chance that present conventions will change under the influence of previously unknown forces, among which new techniques for processing information are only one.

That technology will have an important influence is not open to dispute, although its influence is easily exaggerated and the timing of change remains guesswork. The benefits, while still largely unrealized, are easily imagined. When it becomes commonplace for manuscripts submitted for publication to be handled electronically until the very act of putting ink on paper is reached (and even that may not be necessary), the process of publication could be much faster than at present. But that is only the most obvious feature of what may be in store. When galley proofs are eliminated, or at least replaced by electronic signals which are checked by electronic comparison of the original and the edited data-streams, much aggravation will be avoided. So too, with the help of laser printers and an electronic version of the published paper, will be the annoyance of waiting for reprints to arrive. But none of this will, in itself, diminish people's wish to be published.

Nor will the developing technology eliminate the printed page. Both readers of, and contributors to, journals have a common interest in the survival of ink on paper. For readers, the interest is that of assimilating a variety of information quickly; people's reading habits differ, but there must be something in the view that the most useful information is gathered with the corners of the eyes, while the sight of a printed article is much more effective than the title in telling whether it can be safely left unread.

Contributors mostly welcome printed versions of their articles both because they also have an interest in seeing that what they write should be assimilated and for the natural (and forgivable) warm glow that these tangible proofs of effort bring. So, on the face of things, the volume of the published literature could continue indefinitely to grow a little more quickly than the number of active researchers.

But the forces in the other direction, far from negligible even now, may be growing even more quickly. One is the large but uncounted cost of the printed literature. It can easily be seen how the world could be spending far in excess of \$1,000 million a year on publications; it is simply a matter of believing that there are 5,000 journals selling 1,000 subscriptions at a cost of \$200, modest by many standards.

If the cost (including page charges, where appropriate) of producing and supplying the world's basic research journals were greater than, say, the budget of the National Science Foundation in the United States, nobody would be surprised, but a great many people (not all of them administrators) would be asking whether all that money might not be diverted to more practical uses. And that arithmetic cannot take account of the largely unsung efforts of referees and the honorary editorial boards whose work keeps the journals going.

The problems of the management of the literature exert similar pressures. No doubt the great abstracting journals such as *Chemical Abstracts* would be unusable were they not already available in a form that can be searched by machine, unsatisfactory though the product may often be. For librarians, the challenge is not so much that the bulk of the literature is growing, but that the complexity of, say, finding an item, or telling which items are to be found, increases still faster.

Even when the search for a particular article has succeeded, there remains the difficulty of telling what it says. It is not so much that science has been balkanized by specialization, but that the conventions of the trade impede communication even with would-be readers. Mostly, the conventions are well-meant, but their consequences can be disastrous. People whose results disagree with others already published do not say so openly, no doubt from fear of seeming trouble-makers. Descriptions of experimental procedures are copied in full from laboratory notebooks, but without saying how they differ from well-known precedents (which would often be sufficient), perhaps from fear of being hauled before some investigating committee to face allegations of incomplete disclosure. And there is an increasing and distressing tendency towards pos-faced reticence about the motives and meaning of even the most interesting work. It is as if people fear that if they say

directly what they think the significance of their work may be, they will be black-balled by all the promotion boards and prize-award committees before which it is their ambition to appear.

Problems of comprehension are likely, in the years ahead, to force even the most severe journals into different habits. Already there are essentially academic journals that have taken to publishing reviews of current topics, some of which are as snappy as if they were journalism. The result is to make the literature a little more accessible. Only the most staid will be able indefinitely to resist this trend. At the same time, there is bound to emerge a clearer distinction between material of topical and less topical interest.

But, the realists will say, none of this will seriously dent the impetus that chiefly sustains the publication process — the determination of the appointments and promotions boards with which the academic system is littered to judge people primarily by their publications. But is that necessarily the case?

Publications have understandably been the chief grist for these mills — they are tangible products of people's work, they can be counted objectively and assessed with at least the appearance of objectivity. But there is also a growing sense of frustration with published output as a measure of a person's standing. It is not so much that a bibliography may give a false impression of what a person has accomplished but that, with the laconic conventions of the trade now practised, it may also underrate him or her.

Yet there seems little prospect that moderate changes in the present system will be quickly accepted: a modest proposal, at Harvard, that candidates for appointments should be judged on the basis of a handful of published works only has apparently led to the cry "We want to see the poor papers too!" Those who distrust too much reliance on bibliographies should welcome this reaction; it can only hasten the time when people are judged by their colleagues by what is known of them personally. But since the realists are probably correct in their opinion that the eagerness to publish, not to mention the use of the Minimum Publishable Unit as the quantum of what is made public, is sustained by the activities of the boards, radical change there could quickly bring about radical change in the pattern of publication.

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