Letters

"Conference Literature"

The editors of *Biological Abstracts* announced in the issue of 15 November 1964 their future policy of not abstracting individual papers from the proceedings of conferences, congresses, and symposia. Only a single descriptive abstract of the published proceedings will be given, except for certain selected conferences. Inasmuch as this decision of the abstracting service may discourage the excessive publication of conference proceedings, the editorial and advisory boards of the *Journal of Lipid Research* wish to record their endorsement of this decision.

In our opinion, the main function of conferences is to bring workers with similar interests into personal contact. The delivery of many talks on related themes widens the vision of the auditors and enhances the value of each individual contribution. The great value of oral interchange during the succeeding discussion in clarifying old issues and raising new ones is well recognized. But we are opposed to the view apparently held in some quarters that the proceedings of a conference should, without further consideration, necessarily be published.

Let us consider first the advantages of publication. For certain conferences, the reader's convenience is indubitably served by collecting all the delivered papers into one volume. Conferences dealing with an entirely new field, or with an area of research in which workers from different disciplines meet together for the first time, are examples. Symposia may provide useful "refresher courses" for those of us who are involved in teaching subjects not central to our research interests. Sometimes the regular publication of symposia may add prestige to the tradition of a society; but at times this advantage may accrue only to the editor, the publisher, or the sponsors of the meeting.

The disadvantages are more numerous. First, there is needless duplication: almost all the worthwhile work presented at a conference has been or will be published elsewhere. Second, there is a drain on library and research funds that is out of all proportion to the return in information: the titles of conference books often turn out to be more impressive than their contents, while the contents, already standing on the journal shelves in more satisfactory form, have an even shorter half-life than other scientific literature. Third, the threat of publication inhibits the presentation and discussion of really new work at a conference, the primary purpose of which may be to describe preliminary or incomplete work and to allow the imagination full scope during discussion periods. Since scientists are generally agreed that it is always inappropriate and usually harmful to publish unfinished work or ill-considered assertions, the conscientious participant in a conference may refrain from presenting data and ideas that could well be enlivening and thought-provoking when he knows that they are to be enshrined for all time on the printed page.

Our main objection to the publication of "conference literature," however, is that the individual contributions are seldom subjected to critical review. The system of scientific publication that has emerged as the most satisfactory, and in which we strongly believe, is one in which every paper is subjected to critical evaluation by specialists in its subject matter. Too often the proceedings of a conference are used as a vehicle for publication of unrefereed work. Even though there is no check whatever on its scientific quality, the fact that the work is published means that it can be cited by other authors as though it were on a par with journal articles which have been through the fire of informed criticism. The standard of scientific literature is inevitably lowered.

Unrefereed *abstracts*, which serve a different purpose, are of course not to be disparaged under this head. Nor does this criticism apply to symposia for which the customary refereeing system has been used. Unfortunately, the reviewing process delays still further the notoriously slow publication of proceedings.

We do not mean to imply that articles submitted to a journal are uniformly of higher quality than contributions to a conference. But we do believe that the refereeing system provides a valuable control over scientific standards and that scientific publication without its aid should as far as possible be avoided.

Because there are sometimes special advantages, as outlined above, in publishing a conference as a whole, we do not suggest that the process should be abolished altogether. But we are opposed to making conference publication an established custom, for we believe that it is a custom with ill-defined purposes, prompted too often by commercial rather than scientific motives, and frequently harmful to the progress of science.

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(This letter reproduces, in slightly modified form, an editorial appearing in the April issue of the *Journal of Lipid Research* over the names of the members of the editorial and advisory boards and the foreign corresponding editors of that journal.)

Reprints Abroad

While I can appreciate the motives of Alan Hofmann *et al.* in their letter "Reprints: A proposal" (4 Dec. 1964, p. 1251), I am not sure their plan would be entirely advantageous. Certainly it would save the author money, something I would dearly like to do in my own case, but some of the personal touch may be lost. While I have a number of blunt requests for reprints, without any obvious reason for the requests, I receive as many requests setting out reasons and some account of the correspondent's research in that field. This puts me in touch with workers of similar interests to my own, something which I doubt would occur if I merely received a bunch of reprint-request cards from the publisher, as suggested in the letter.

There is also the problem that most publishers will print only the number of reprints specified in advance, after which the type is dismantled. How are publishers to decide how many reprints they should retain in stock, and can publishers be expected to hold large stocks of a paper for which there may be little public demand? I still receive requests for a paper I wrote 7 years ago. It must be remembered that in less fortunate countries, like those

in Africa and Asia, where library facilities are not readily available, a reprint may be the only means by which a research worker can have the relevant material close at hand. Asian and African students are not particularly affluent and can ill afford the cost of reprints.

While I can see the merits in the scheme suggested by Hofmann *et al.*, it does not fully answer the problem. I would suggest a footnote to all papers saying the author will send reprints only to those who write personally, explaining their interests. I, for one, am suspicious of printed request cards.

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The Information Race

I wish to call to the attention of your readers an increasingly widespread practice which I believe to be detrimental to the advancement of science. For lack of a better name, I choose to call it "publication by preprint." A typical example of this practice might occur when a scientist completes a project, writes up his results neatly in the form of a preprint or preliminary report, circulates the results among a small number of his colleagues working in the field, and then allows 1 or 2 or 3 years to elapse before submitting the material to a recognized scientific journal.

Although the desire to inform one's colleagues of new developments rapidly is a commendable one, the practice of letting these preliminary communications usurp the role of the recognized journals is not a commendable one. In fact, this practice systematically isolates younger, unestablished scientists from the mainstream of progress. It encourages stagnation of thought by restricting participation in the scientific dialogue which accompanies any discovery to a select group of individuals whose ideas probably already dominate the field. The communication of scientific information by preprint, progress report, or report at a scientific meeting can in no way substitute for publication in a recognized scientific journal where access to the information is available to all.

To remedy the problem, there are steps which could and should be taken. One of these might be the establishment of a clearing house which would make preprints accessible to all, as suggested by Moravcsik (*Physics Today*, March 1965). More than likely, however, the ultimate solution must lie with those scientists who now engage in "publication by preprint." Their realization of the dangers of such practices is essential.

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I have before me a reprint-request postcard from a medical department of a Southwest university—or so I gather from the printed form. It is unsigned, the name of the requester is not given even by stamp, and in fact the entire card has been untouched by human hands except for the addressing, presumably by the departmental secretary. From the list of addresses I infer that it is a request for a "reprint" of a paper that has not yet been submitted for publication in regular channels, although the preliminary work on which it is based has been issued as a handsomely printed brochure by the commercial firm with which one of the coauthors is associated.

The most telling argument for this sort of prepublication is that, among workers in the particular area involved, advances will have been made well beyond the information in the paper by the time it could possibly appear in any journal. There are fallacies in the argument, and the value of information for which this may be true is rendered somewhat dubious, but the fact remains that in most instances the rapid dissemination of information can save large chunks of time and minimize duplication of effort. In modern-day "competitive research" the costs are so high that the economic bill for duplication and poor communication can attain fantastic proportions, even though the corresponding scientific and sociologic gain may be minuscule.

Until recently, one could feel justified in writing off as facetious the recurrent suggestion that journal publication in rapidly moving fields might eventually be completely bypassed with an adequate system of cross-indexed titles under which an author merely provided privately reproduced copies of his work upon request. Such a system is only a step from the Documentation Institute idea, in which "publication" consists simply of filing a paper or a body of verifying data with a central indexing agency, which undertakes the chore of providing copies upon request. Sober reflection indicates that some combination of the two ideas may be the only logical response to the steadily mounting costs of conventional publication and to our decreasing ability to read the journals to which we subscribe. But the one crucial question in all such proposals remains unanswered: What will be the function of the institutional library—if it can survive?

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Fred Griffith Memorial

In an article published in the *Journal of Hygiene* in 1928, the late Fred Griffith described how he had transformed live Type 2 pneumococci into Type 3 by injecting them into a mouse along with dead Type 3 pneumococci. This experiment, the results of which were received with considerable scepticism, was successfully repeated by American workers in O. T. Avery's laboratory at the Rockefeller Institute. A series of papers from this laboratory culminated 16 years later in the demonstration of deoxyribonucleic acid as the transforming principle. This fundamental work proved to be the starting point of a vast field of genetic exploration in which many scientists of different training and background are now engaged. Griffith did not live to see the fruits of his original observation, for he was killed by a bomb in London in 1941. The 25th anniversary of his death will fall next year. To commemorate him, a fund is being raised by some of those who were privileged to know him. Probably many workers in the fields of genetics will not have heard of Griffith or of his great contribution. It is to these, as well as those to whom his name is familiar, that an appeal is being made. In the United States a memorial is already being prepared for Avery, and it seems only fitting that the part Griffith played should not be forgotten. Checks should be made payable to the Griffith Memorial Fund, Westminster Bank, 154 Harley Street, London, W.1, England.

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