Letters

Duplicate Publication

The editors of *Sky & Telescope* agree with Philip H. Abelson's position (Editorial, 3 Dec., p. 953) concerning the multiple publication of original research. In the field of astronomy and space science, for example, so much information is being published at present that we are hard pressed to review even the most important journals and evaluate whatever other news comes to our attention. Duplicate publication only exacerbates the situation.

However, I worry that Abelson's warnings may cause scientists to fear that any mention of their work in media other than *Science* could result in their disenfranchisement from its pages. As editors of a popular news magazine of astronomy, we believe it is our obligation to report new discoveries as quickly as possible and, if practical, before formal publication. In this, we have the tacit support of the American Astronomical Society, which encourages the press to attend its meetings and interview astronomers presenting papers. Also, in the same spirit, the *Astrophysical Journal* has recently rescinded its long-standing prohibition on the issuance of news releases before the date of a paper's formal publication.

Since much research today is carried out with federal funds, it seems reasonable that the taxpaying public have ready access to research supported with its dollars. This is the role of newspapers, the electronic media, and popular magazines.

The essence of the problem can be resolved easily by applying the well-known legal principle that the words of authors can properly be copyrighted, but not their ideas or their work. In this regard, one can have no objection to *Science*'s proprietary right to the publication of original research. However, we take exception to arrangements made with researchers who use data in the public domain, if such agreements preclude them from making their findings known to broader, nonprofessional audiences before publication. Data from the Voyager spacecraft are a specific case in point.

Since public funding is so crucial for the continuation of science as practiced today, it is clearly in the best interest of science to have an informed and enthusiastic public by its side.

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As a scientist and attorney who is particularly concerned with censorship and the suppression of scientific inquiry, I read Abelson's editorial "Excessive zeal to publish" with some disquietude. It raises grave First Amendment issues of "prior restraint."

While editors have, and should have, broad discretionary power, by professional usage and by well-established law, to control the contents of publications for which they are responsible, this power is not without constraints and must be balanced against the rights of scientists who believe they have something significant to say. Certainly publication pressures result in abuses, but I am not convinced that the proper approach to the problem is for two or more editors to suppress publication of the work of a scientist. Such prior restraint could create liability for editors personally, their journals, and the professional organizations that publish the journals. In the past the scientific community has been remarkably free of acrimonious litigation. I am sure we all want it to stay that way.

The AAAS and other professional societies should make a serious effort to develop policies to curb these abuses that fall short of the censorship we all abhor. Nothing can cut closer to the heart of scientific inquiry than the suppression of information for any reason.

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Abelson is right to castigate authors who submit manuscripts to more than one journal, in what amounts to a deceit. I subscribe to his condemnation. The complement to this situation is also a matter of concern. I have lately been polling small groups of professional people about abuses in publishing. I have often received an explosion of anger at the practices of publishers and editors. The power of an editor is virtually absolute. And absolute power can tend to corrupt. Manuscripts can be kept for months, even years, and then rejected with no explanation. Careers can lose their promise, and good prospects for tenure can turn sour in such circumstances.

What is needed is a code of good practice for professional publishing and writing. Many aspects need to be considered, but the two principal affirmations should be (i) a maximum time for consideration of a manuscript—a time that is a function of length; and (ii) a guarantee that the manuscript is being submitted to only one publication at a time. Perhaps a committee of the AAAS could be set up to draft such a code.

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Although I agree with Abelson's editorial position regarding submission of the same article to two or more journals in the same or related subject areas, this poses a problem when an article has a bearing on audiences in two different subject areas, only a few of whom read each other's journals. A solution might be to submit to a journal in one field, and if the manuscript were published, to send a reprint with an abstract to the journal editor in the other field with an explanation of the approach. This would give the editor the option of listing the article's availability (with the abstract if desired), or in certain cases, publishing the information in a newsletter (if the journal is part of a professional/scientific organization). It would also facilitate communication among readers in diverse fields without duplication of effort.

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Any scientist who has spent enough time in a library to become tired and bored there may have wondered if his own publications will end up just collecting dust on the yellowing pages of some old journal. Do those with an excessive zeal to publish think that their own work will have a better fate than that of the thousands of others who have preceded them? Society should certainly not be kept waiting for scientific information that affects it, but perhaps it would be

better for individual scientists to sit back, relax, and wonder if an isolated bit of research is worthy of individual attention. I can appreciate the necessity of establishing oneself in a particular field, but I have never thought that generating long lists of little experiments is the way to do it.

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Funding R & D

Thelma Carter, in her letter "Investing in science" (12 Nov., p. 638), raises interesting and important points concerning the funding of research and development projects through tax-sheltered investment opportunities. I should like to make some comments, particularly with respect to the growing use of research and development limited partnership (RDLP's).

The private sector has been successful in financing R & D arrangements by taking advantage of the appropriate tax laws. An estimated 3 percent of total tax shelter volume during 1981 involved RDLP arrangements (*1*), and evidence from trade and industry publications indicates that the percentage is increasing. The RDLP is a mechanism available to both individual entrepreneurs and corporate entities in funding R & D efforts. The RDLP is an effective alternative to traditional sources of R & D funding, such as retained earnings, stock sale, or borrowed money. No repayment is necessary if the RDLP is unsuccessful. The funds are supplied by limited partners who are usually investors in high tax brackets. These investors anticipate a tax reduction in the first year based on the ability to reduce taxable income from other sources by an amount equal to a substantial portion of their investment in the partnership. The investors also anticipate a substantial return on investment if the new technology is licensed or sold upon completion of the R & D project. If the partnership is properly arranged, the investor's income may be taxable at capital gains rates. These rates for individuals were reduced from 28 percent to a maximum of 20 percent by the Economic Recovery Tax Act of 1981.

Carter's suggested use of professional societies as clearinghouses to maintain registers of projects and patents available for implementation is an idea worthy of further exploration. Clearly, a source of basic information is needed concerning novel mechanisms for financing commercial R & D activities.

The formation of RDLP's involves complex legal and business considerations, and the guidance of experienced and reputable advisers is vital.

To assist the private sector in developing an understanding of RDLP's, the Department of Commerce recently prepared a document entitled "Information and steps to form research and development limited partnerships." Copies may be obtained for $10 by ordering document number PB 83-131516 from the Sales Department of the National Technical Information Service, 5285 Port Royal Road, Springfield, Virginia 22161.

EGILS MILBERGS
Office of Productivity, Technology, and Innovation, Department of Commerce, Washington, D.C. 20230

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1. P. A. Dreyfus, *Money*, August 1981, p. 87.

Interferon Research

Barbara J. Culliton, in her article regarding the settlement of the interferon affair by researchers, the University of California, and Hoffmann–La Roche (News and Comment, 28 Jan., p. 372), does not discuss one essential feature. The initial research was paid for by taxpayers, through the National Institutes of Health (NIH); and it was this research that was elaborated on by the drug company scientists in their attempt to generate large amounts of interferon. The question then is, Who gets the financial reward if the protein proves to be of commercial value? As it now stands resolved both the University of California and Hoffmann–La Roche will get something; but what about the taxpayer? Without the initial funding by NIH, for the establishment of the cell line, Hoffmann–La Roche might have gotten nowhere. Yet, in the final conclusion, any monies generated by the possible commercial success of interferon will not flow back to NIH (that is, the taxpayer). Thus, we simple taxpayers are paying twice; once for the research funded by our monies, and second for the opportunity to buy a product that was generated by this initial research. My suggestion is that the price of commercially available interferon should be the cost of making it by Hoffmann–La Roche, plus a profit, plus the cost of the development by Hoffmann–La Roche, but minus the cost of the research done through NIH funding. Alternatively, let Hoffmann–La Roche set any price it wishes, but a certain percentage should go back to NIH, either to be put back into the general governmental kitty or into specific research funds.

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Culliton's article concerning the settlement of the lawsuit between Hoffmann–La Roche and the University of California contains two statements relevant to the scientific chronology of events that should be clarified.

The statement, "Gallo . . . observed that KG-1 produced modest quantities of interferon" implies that this information was imparted to me before or at the time we received the KG-1 cells. In screening media from a large number of cells growing in his laboratory, we found several that contained interferon (*1*) and requested those corresponding cell lines, which Gallo generously sent to me without restrictions of any sort. The KG-1 cells were among them. We did not know about Gallo's observations until long after we received the cells and developed them into good interferon producers.

After developing conditions for good interferon production with these cells, we isolated messenger RNA from them so that we could clone the "interferon gene." The construction and identification of the first recombinant human leukocyte interferon A clone was accomplished in my laboratory (*2*), not at Genentech. It contained most, but not all, of the coding sequence for leukocyte interferon A. This clone was brought to Genentech by scientists from my laboratory and used to screen their library of clones prepared from messenger RNA we supplied. With this initial clone, my colleagues at Genentech, under contract to Roche, subsequently isolated a full-length clone of leukocyte interferon A and others and efficiently constructed an expression vector for a mature leukocyte interferon for the first time (*3*).

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2. S. Maeda *et al.*, *Proc. Natl. Acad. Sci. U.S.A.* **77**, 7010 (1980); *ibid.* **78**, 4648 (1981).
3. D. V. Goeddel, *Nature (London)* **287**, 411 (1980).

Erratum: Two taxonomic errors appeared in the report "Oak leaf quality declines in response to defoliation by gypsy moth larvae" by J. C. Schultz and I. T. Baldwin (9 July 1982, p. 149). In the first column on page 149, *Quercus rubrum* should have been *Quercus rubra* L., and in the first column on page 150, *Q. nigra* should have been *Q. velutina* Lam.