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Information for Contributors appears on pages 112–114 of the 6 January 1995 issue. Editorial correspondence, including requests for permission to reprint and reprint orders, should be sent to 1333 H Street, NW, Washington, DC 20005.
***Science* World Wide Web address:** http://www.aaas.org
Other Internet addresses: science_editors@aaas.org (for general editorial queries); science_letters@aaas.org (for letters to the editor); science_reviews@aaas.org (for returning manuscript reviews); membership@aaas.org (for member services); science_classifieds@aaas.org (for submitting classified advertisements); science_advertising@aaas.org (for product advertising)

LETTERS

Journal policies

The question of whether press embargoes are beneficial or not, and to whom, is raised in letters reacting to an article describing events surrounding the publication in *Science* of papers about a newly discovered "fat hormone." One writer would like to see the embargo dropped, while two others support it. Additional letters suggest posting supporting sequence data on the World Wide Web and listing journal authors à la Hollywood...

Faltering Press Embargo?

In the article " 'Fat hormone' poses hefty problem for journal embargo" (News & Comment, 4 Aug., p. 627), Wade Roush describes how embargoed, advanced information given by *Science* to the general media evidently "triggered a surge in Amgen's stock." This 10% jump was not surprising. *Science* must realize that early disclosure of information is a natural consequence of its embargo policy. As the financial stakes continue to grow, the pragmatic assumption is that voluntary embargo is destined for failure.

Although *Science*'s desire for accurate news coverage in the lay press is understandable, I am unconvinced that ending prepublication release of information leads to shoddy journalism. Isn't a more likely consequence just *delayed* coverage? It is more credible that *Science*'s policy is related to self-promotion and prestige, as suggested by Teena Lerner, the technology analyst with the Lehman Brothers brokerage firm who gave the alert about the in-press *Science* papers (Reports, 28 July, pp. 540, 543, and 546).

Let's have everyone see information on the date of publication. Besides, I, for one, wouldn't mind reading my copy of *Science* before the local paper tells me what's in it.

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There is an old Yiddish expression that says, roughly, "On someone else, you can see a fly. On yourself, you can't see a hump." Although *Science*'s embargo policy may be partially self-serving, it is not entirely so—the opportunity for reporters to digest technical material cannot but be beneficial to the resulting news stories, thus serving the public interest. Although she doesn't seem to acknowledge it, the actions of stock analyst Lerner appear to be entirely self-serving. Shifts in the prices of stocks can be based primarily on hype. It is ludicrous that Amgen's stock took a sudden jump on the basis of a discovery that is so far from producing a usable product. The primary beneficiary of such manipulations is the securities industry itself.

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The problem of press embargoes is many faceted. The *New England Journal of Medicine*, in particular, has responded to requests of its physician subscribers by enforcing its embargo policy in a vigorous and highly ethical manner. It is not possible for the practicing medical community to answer anxious patients' queries about life-and-death matters if new research results are relayed to the lay press and described before that community can read them.

Scientists and editors, it is to be hoped, have concerns above and beyond Wall Street and its sometimes avaricious brokers. Science as a discipline, under attack from creationists and others, would be well advised to consider its relation to the money-eyed interests.

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Data on the Web?

A direct result of DNA sequencing technology and its attendant proliferation of data is that important data sets are rarely included in scientific manuscripts submitted for publication. This absence of data impedes the critical analysis of manuscripts by reviewers and of published papers by the scientific readership.

Authors should be required to provide supporting data (on diskette) with manu-

scripts submitted for review. Access to the original data would permit a comprehensive review of both the data and the manuscript before a paper is accepted for publication. This procedure would place an increased responsibility on the reviewer not only to investigate the assumptions inherent in the analysis, but also to maintain confidentiality and voice any potential conflicts of interest.

On publication of a paper, the complete data set should be freely available to the scientific community. Depositing data in one or more databases, such as GenBank, a common practice now, might seem to provide an obvious solution. But it does not. For example, in the field of molecular evolution, single-taxon entries do not contain all the information in any intertaxon data set, such as gaps introduced into the sequences to improve the alignment. An option is to request the data directly from the authors, but this is often less than efficient and the information exchanged does not constitute an archive.

A solution would be to publish supporting data on some manifestation of the Internet, such as a dedicated World Wide Web site. This would enable data transfer to be complete and any assumptions of the authors to be tested immediately by other investigators.

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■

Lights, Camera . . . and Action!

Science should be congratulated on the special News Report about aspects of subtle issues in scientific conduct (23 June, pp. 1705–1718). In his editorial in the same issue (p. 1679), Floyd E. Bloom points out that "the real everyday conduct issues in the pursuit of science are much more intricate and complex than those of the famous misconduct cases. These tough questions are hammered out in a gray area."

Many "gray" problems in scientific credit seem to result from a lack of a universally accepted credit system. For a paper with more than two authors, we often have to rely on track records, anecdotes, or hearsay at meetings to guess the relative contributions of each author. We might all benefit from a new convention of authorship, based on that for motion pictures.

In such a convention, the senior author might be regarded as a scientific director and producer (who brings in money); the first author would be the executive director. All the authors could be identified by the actual experiments, analysis, or other services (including ideas or models) they have provided. For example, the authors of the paper "Whisker-related neuronal patterns fail to develop in the trigeminal brainstem nuclei of NMDAR1 knockout mice," which appeared in *Cell* [**76**, 427 (1994)], instead of being given as "Yuqing Li, Reha S. Erzurumlu, Chong Chen, Sonal Jhaveri, and Susumu Tonegawa," would be given as follows:

First author: Yuqing Li
Production of mutant mice: Yuqing Li
Histochemistry and anatomy: Reha S. Erzurumlu, Sonal Jhaveri, Yuqing Li
Electrophysiology: Chong Chen
Senior author: Susumu Tonegawa

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Notes

1. I thank H. Hinds and Y. Li for comments.