LETTERS

How to exchange information

It is clear that the central issue in *A Debate on Preprint Exchange* (Michael Moravcsik, Simon Pasternack, PHYSICS TODAY, June, page 60) is whether the rush to communicate new ideas is more imperative than to verify the technical soundness, true innovation, and clear and accurate presentation of such ideas. Whereas in oral communication the availability of the author to clarify, amplify and, occasionally, to retract a phrase or statement permits the employment of relatively loose language, a written statement should be accurate and lucid to an exceptional degree. Here a reviewer can perform a most important function by pointing out to the author words or passages that fail to do this. There is ample evidence of what happens when proper editing is absent (for whatever reason) in technical journals. For example, how many readers realize that the poor quality of some translations stems not from the translator's but from the original author's ineptness?

For whatever reason, there is a general disdain among physicists for the English language. All that matters, they claim, is to get the idea across—how it is transmitted is secondary. I wonder whether they are equally indifferent to other modes of communication such as noisy telephone connections or bumpy propellor-driven airplanes? The point is that unedited reporting may impede the dissemination of knowledge more seriously than the delays occasioned by strict adherence to standards.

It is also interesting to note that the relatively successful Information Exchange Groups in biology are cited as a suitable precedent for PIE. These groups have been successful because their very limited size and scope assure a measure of control that a larger exchange could not maintain. The broad dissemination of scientific information is the function of journals and the usurpation of that function by prematurely or hastily concocted prepublications should be avoided. It is too often true that the expansion of a carefully contained process to a much larger one results in an operation whose quality is inversely related to its size.

The above comments are not intended so much to support either side in the debate but rather to stress the need for more thorough analysis. Granted that an urgency for communicating innovations within high-energy theoretical physics exists, maybe the talents and resources of the technical information division of AEC should be deployed to devising a novel mode of communications. As a far-fetched example, consider a taped message center which can be reached by telephone. An author records his "paper" on a tape in the central repository; its title is immediately transmitted to a predetermined audience who can subsequently obtain the full text, if interested. The tape could be easily revised or corrected by the author and a removal of all such recordings at predetermined intervals would prevent their becoming a part of the permanent physics lore. Whether this or some other procedure is adopted, the point is to define the real need and then to devise the best means for meeting it rather than to subvert a process designed to accomplish a different task. Physics journals were designed to transmit and also to store scientific discoveries; to enable current and future readers to study the discoveries reported. It is imperative, therefore, that their contents be original and carefully presented. Moreover, their integrity requires that the material contained not have been previously published elsewhere. If it is desirable to carry on concurrent topical dialogues, then a clearly distinguishable impermanent means of communication should be devised for such a purpose. By emphasizing the impermanence of such "preprints" most of the objections already raised would

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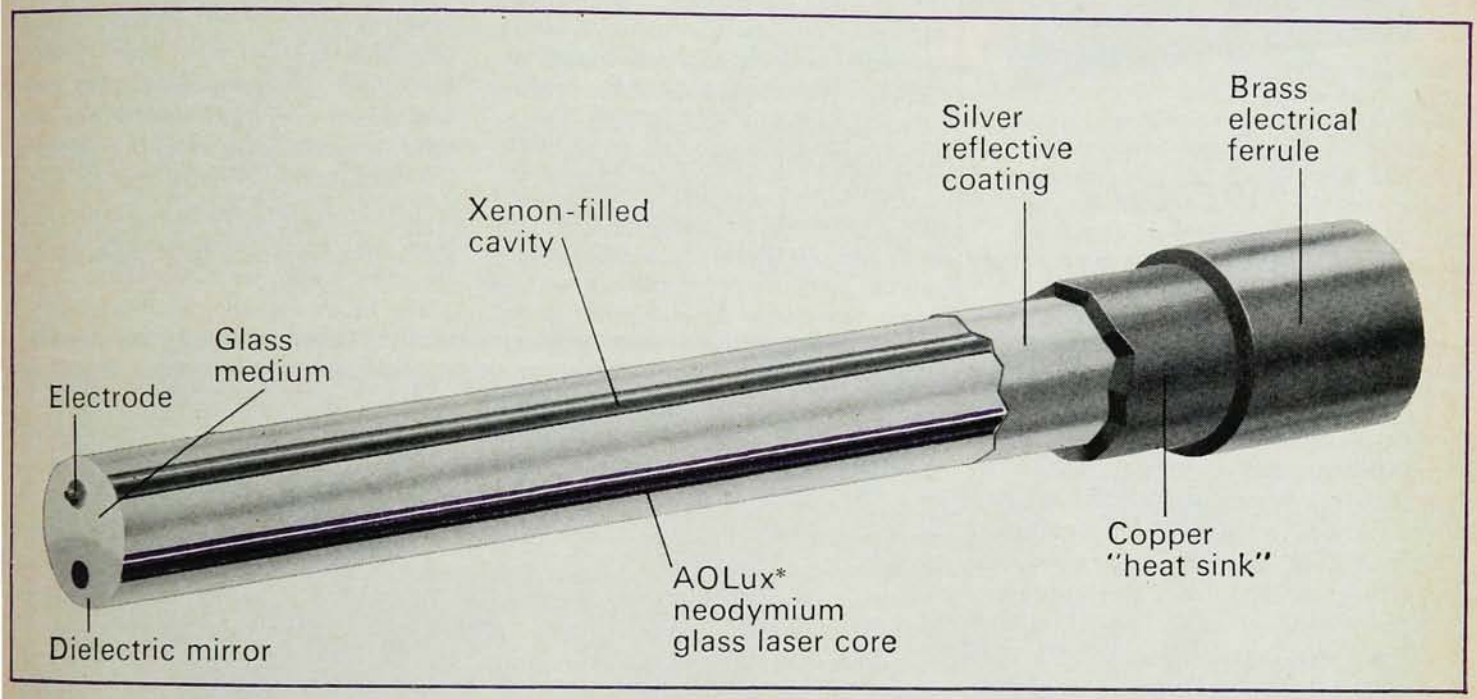

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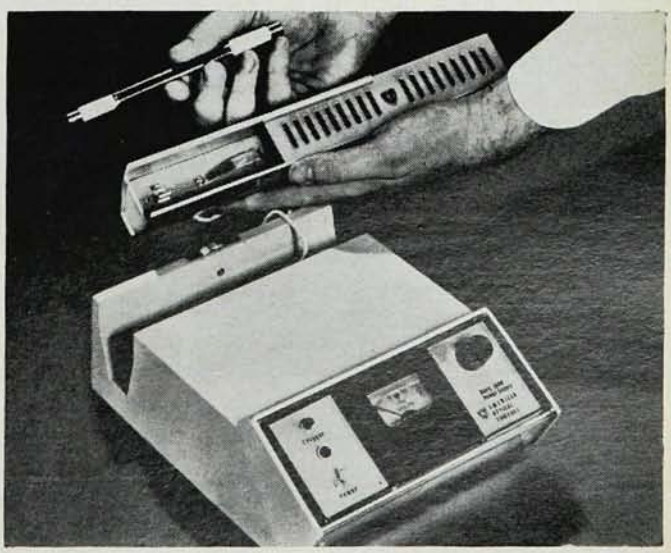

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LETTERS

(Continued)

be overcome. By reducing the transmittal time to hours or days, the real purpose for this innovation would be accomplished.

Leonid V. Azároff
University of Connecticut

The debate is easily won on technical points by Pasternack. High-energy theory is now in a state of hysteria; it needs a dash of cold water, not brandy. An automatic delay period of six months on all forms of publication or disclosure might be a cure.

In his May article Pasternack has said something so important that one need not blush to say it again: *The journal system of publication of papers accredited by authoritative referees is essential to science.* The fact that it arose in the 17th century, with the birth of modern science, and has lasted almost unchanged for about 300 years does not prove that the system is outdated; it shows that this is how scientific knowledge—as distinct from magical incantations, recipes, advertising, love letters, pornography, political speeches, newspaper editorials and other diverse forms of "communication"—needs to be promulgated.

For all their technical cleverness, the "information engineers" are astonishingly barren of intellect. I have yet to read one who realizes what all scientists learn very early—that there is a necessary tension between creativity and criticism in our profession. An idea, an experimental result, an observation, a calculation, however brilliant, is not scientific knowledge just because *I,* with my doctor's degree and expensive apparatus, have said it. It only acquires that status when it has passed through the critical filters of other minds and has become something that *we,* as a community of scholars, have accepted. The balance is delicate. The evils of censorship, academicism and dogma are well known; the opposing dangers of license, crankiness and ideological conflict are just as real.

Pasternack's remarks, for all their good sense and firmness, are pitched too low. It is not a question of orderliness; the heart of the issue is the nature of scientific activity itself. To remove the critical checks would be as disastrous as to allow everyone the right to print his own banknotes or to avenge himself against his neighbors by the force of arms. Science is a democratic community whose success depends as much on coöperation and the rule of law as it does on free speech and individual enterprise.

Let me go further and say that what we need is *more* criticism, far more effort to make scientific knowledge coherent and many more reviews, books and encyclopedias. The little bits and pieces that the information boys want to store for us are only the scaffolding of the house of intellect. In the end, surely, *understanding* must take the place of *memory,* so that we shall know what we know by its place in the scheme of things, as revealed by science, not by the magnetic-tape address of the microfiche reproduction of the piece of paper on which it was first announced to a disbelieving world. As whole nations now comprehend, the five symbols $E = mc^2$ are more potent than 50 Pentagons stuffed with preprints, reprints, offprints, data tables and other garbage.

J. M. Ziman
University of Bristol, England

The arguments of Pasternack are appealing, honest, and quite realistic. I agree with his view in general, and believe that the practice of putting the "Abstracts of Articles to be Published" in *Physical Review Letters* is a very useful compromise to the time-lag situation. Persons sufficiently interested can get a real preprint. As to introducing further haste in communication it would surely mean that the noise level would increase, and we all would become even more resistant to unfiltered "communication." On the scale of the human enterprise the entire issue is uncalled for, but surely the methods which Pasternack advocates, supplemented by other present methods of personal communication, will result in the most orderly, efficient, and effective route to advance. I firmly believe that the delay involved in producing a well prepared and thought-

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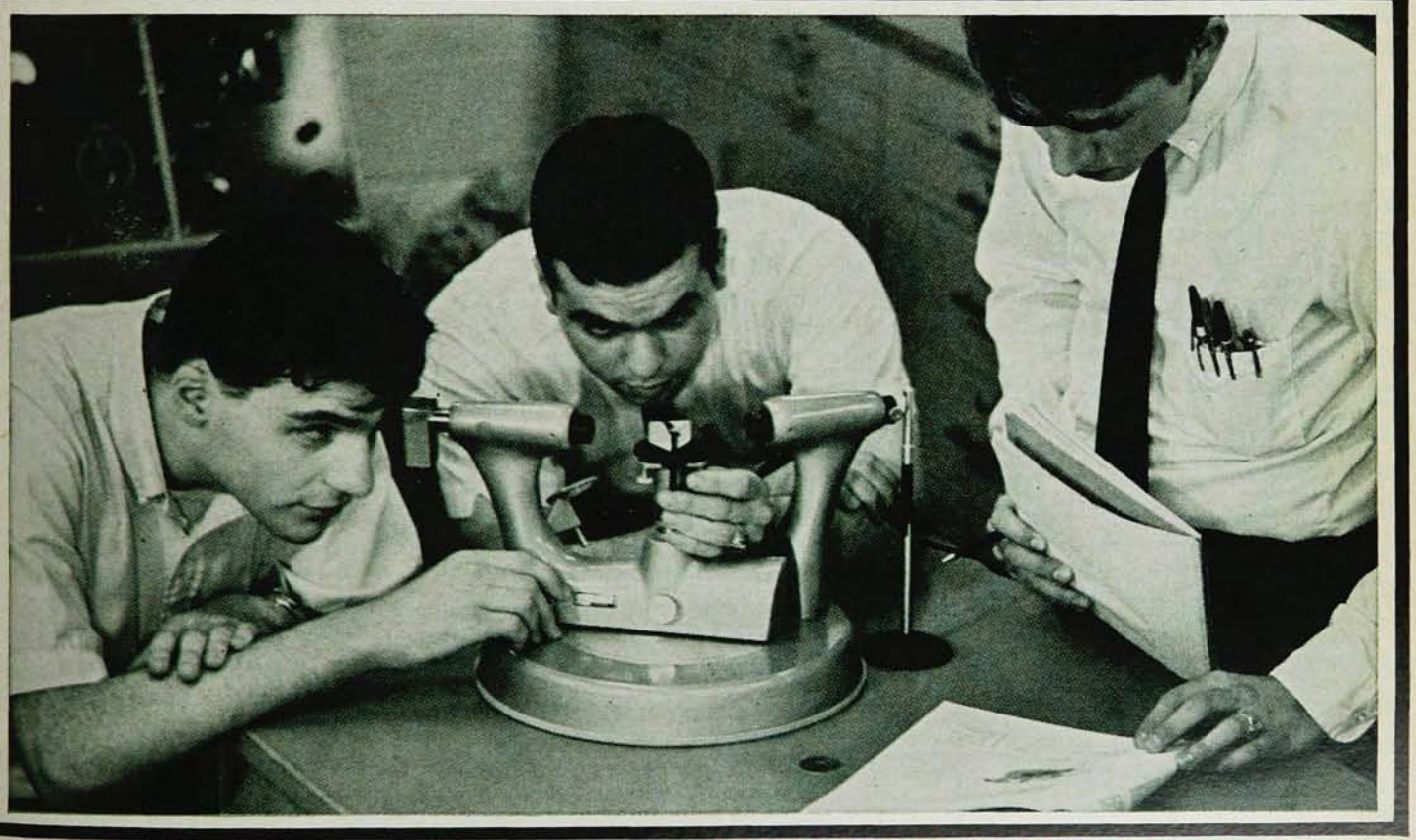

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ful paper for publication (in the traditional sense) is worth far more than a "brainstorming physics" attitude.

T. J. Rowland
University of Illinois

I would like to lend whole-hearted support to Moravcsik on this laudable venture. I can illustrate the importance and usefulness of the proposed service to people like us who work in remote areas far away from any active center of research by a story that I hope will clear the doubts from the mind of Pasternack.

Last winter I received my own copy of the *Physical Review Letters* in which the important papers of Adler and Weissberger appeared. This was almost six months after these papers were available in Europe and America in *printed form,* but being completely unaware of the spate of activities that had in this period been reported in preprint form, my colleagues and I merrily set about doing some of the things that had already been done! I believe that such waste of efforts can and should be avoided.

It might be thought that delivery of journals to outlying areas might somehow be speeded up, but after writing a number of angry letters to the secretary of the American Physical Society in the last few years, I am convinced that this can not be done. Preprints are therefore the only means of keeping oneself abreast of current developments in a rapidly changing subject, and if you do not believe in the inevitable supremacy of one country or continent in the pursuit of science but would rather see science develop as a common human endeavor, I have no doubt that you will have to consider seriously the scheme suggested by Moravcsik.

A. M. Harun ar Rashid
Atomic Energy Centre, Dacca

Wait for volume 3

The review of my translation of A. I. Markushevich's *Theory of Functions of a Complex Variable* by J. Gillis (PHYSICS TODAY, June, page 96) creates a misimpression. The reviewer concludes by calling attention to two alleged omissions from the course: (1) a discussion of Bieberbach's conjecture, (2) proof of Picard's theorem based on the elliptic modular function. In fact the absence of these topics in volumes 1 and 2 leads him to refer to a "disinterest in spice shown by so many textbooks of our time."

Unfortunately Gillis has forgotten that Markushevich's course consists of *three volumes,* a fact prominently displayed on the dust jackets of volumes 1 and 2 as well as in the translator's preface. Both of the topics whose absence is deplored by Gillis are treated in volume 3, now in press. Bieberbach's conjecture is in section 3 of volume 3 called "Basic Properties of Univalent Functions," and proof of Picard's theorem using the modular function is in section 49 of volume 3 called "The Modular Function: Picard's First Theorem."

That the form of Picard's first theorem in volume 2 is a weaker version of a result to be proved in volume 3 is pointed out in the footnote on page 268 of volume 2, which should have served as a warning that much more was to come.

Richard A. Silverman
Jamaica, New York

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In the article "National Laboratories, Universities and the AEC" (PHYSICS TODAY, April, page 45), you omitted any indication that New Mexico State University is a member of the Associated Rocky Mountain Universities (ARMU). Since PHYSICS TODAY is a publication for all of the physics community, the physics department at New Mexico State University considers this a rather serious oversight. . . .

Harold A. Daw
Head, Department of Physics
New Mexico State University

The article was very interesting, and I share the author's views. The only exception I must take is to the omission of New Mexico State University as a member of ARMU. . . . □

Richard H. Duncan
Vice President—Research
New Mexico State University