

LETTERS

More debate on information and the PIE proposal

(The following letters comment on our May article "Is Journal Publication Obsolete?" by Simon Pasternack and our June series called "A Debate on Preprint Exchange" by Pasternack and Michael J. Moravcsik.)

Both Moravcsik and Pasternack seem to lose sight of the fundamental purpose of the exchange of physics information. Ideally a physicist would like to be able to communicate as rapidly as possible with colleagues throughout the world who have interests similar to his own. He would like to receive all recent literature that may be relevant to his own interests without any particular effort on his own part. He would not wish to be burdened with extraneous or worthless material.

A physicist receiving a journal usually finds himself with a large amount of material which is of no interest to him in his own narrow specialty. If he finds one article in ten that is of interest, he is lucky. More typically he will find only about one article in a hundred. Journals overload the physicist with large amounts of extraneous material (I, as well as others, have long since ceased to subscribe to *The Physical Review* because of this large amount of "junk" material taking up valuable shelf space.)

A journal article is usually published after a delay of six to twelve months. Quite often a physicist becomes aware of an article only after reading the abstract in *Physics Abstracts*, where another delay of six months may have occurred. In view of the fact that manuscripts can now be sent over the telephone by appropriate photoduplication processes and could, in principle, be made available within minutes after completion, a delay of many months or even a year or more seems inexcusable today. The ponderous methods of the physics journals do not meet the high-speed requirements of modern researchers.

The process of journal publica-

tion frequently filters out precisely the information that should be communicated. A request that space be conserved frequently makes an author condense his descriptions of experimental equipment or mathematical steps so much that they become unintelligible. Direct communication with the author then becomes necessary to recover the information that was filtered out.

Essentially all important contributions in some areas of research, such as electromagnetic-propagation phenomena, for which lengthy analysis is required, have been circulated "privately" as monographs, journals being completely inadequate to meet even the minimal information-exchange requirements.

The refereeing process used by some physics journals frequently eliminates interesting new ideas since the job of the referee is to see that a manuscript measures up to some standard, and the standard is necessarily based on that which is old and established and therefore not new. New ideas are frequently limited to minuscule steps.

In view of the marked discrepancy between the needs of the physicist, as ideally considered, and the performance of present physics journals, I would like to suggest an upgrading of Moravcsik's proposal as follows:

A central agency (such as the American Institute of Physics) would

receive manuscripts from authors, as well as reprints from the existing journals of the world. A physicist who wanted to receive certain select types of manuscripts (or reprints from established journals) would leave a standing order with the central agency for copies (microfilm, reprints or paper, as requested). The physicist could specify not only detailed areas of interest but also whether he wished to receive nonrefereed and rejected manuscripts (or perhaps nonrefereed and rejected manuscripts by selected authors).

Submitted manuscripts would be duplicated and sent out immediately to physicists who requested nonrefereed copies. At the same time the manuscripts could be refereed by one or two physicists chosen from a panel of experts in the specialized area of research treated. If the manuscript passed the referees, it would be distributed (after possible revisions) to physicists requesting refereed manuscripts. Reprints from established journals would be distributed as refereed manuscripts. A rejected manuscript, not withdrawn by the author, might be distributed to those requesting such manuscripts with the referees' comments included.

The panels of experts might be elected (by mail once every two or three years) from among physicists writing in particular areas of specialization, and their names would be published. (The system of secret referees should be as odious in a free society as secret informers who need not face the accused in a court of law. Referees hiding behind a screen of anonymity are frequently guilty of a supercilious attitude and of doing only a cursory job of refereeing.) A panel of experts whose names were known and respected should improve the quality of refereeing.

To finance the project and distribute manuscripts in an orderly and economic manner, recipients of manuscripts would pay according to the cost of the services they received. For example, they might be charged so



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much a page (perhaps ten cents) for a paper copy of a manuscript, and for journal reprints as determined by the journals themselves. If a recipient found that he was receiving more than he could afford, he could reduce the number of manuscripts sent to him. The recipient might also choose to receive only manuscripts of fewer than some maximum number of pages.

Any federal or institutional aid to the program would reduce the cost to recipients. Copies of manuscripts provided free to the central agency would also reduce the cost to recipients.

This system of distributing literature as it becomes available appears to provide as close to an ideal system of information exchange as is possible. It would probably encourage a greater exchange of information; more manuscripts might be handled per unit time than are now handled by journal publication. However, the total amount of paper used, or pages distributed might be far less than is currently distributed since "junk" material would not be distributed.

It would seem that the secondary problems of permanent storage and retrieval of such manuscripts (possibly bound according to subject matter) can be resolved. An abstracting service, such as *Physics Abstracts*, could publish abstracts of refereed papers and possibly just the titles of non-refereed and rejected papers.

The practice of measuring a physicist's worth by counting his publications could be continued if a manuscript passed by the referees were counted as a publication. (A rejected manuscript might be given less weight.) It should be noted that although the evaluation of a physicist in terms of the number of papers he can get by a referee is of interest, such evaluation is of secondary importance compared to the fundamental problem of maximizing physics-information exchange. A referee should not act as a complete censor with the power seriously to curtail or completely cut off certain types of information

exchange. The evaluation of manuscripts is time consuming and, if mandatory, it would seriously impede the proper flow of information. Each recipient physicist should be the best judge of what type of manuscripts he wishes to read; if he wishes nonrefereed manuscripts to save time, they should be made available to him without delay.

I recommend an expanded and upgraded PIE.

James Paul Wesley

University of Missouri at Rolla

I have appreciated the lively controversy in *PHYSICS TODAY* concerning document exchange vs journal publication, particularly the debate by Moravcsik and Pasternack in the June issue. (I agree with Pasternack's distinction between "documents" and "preprints." There is a real distinction between a bona fide preprint—of a manuscript to be published—and a document, which often is a preliminary report of work in progress and may be radically changed in publication or not published at all.) I would like to join Pasternack's side and present an argument that he omitted.

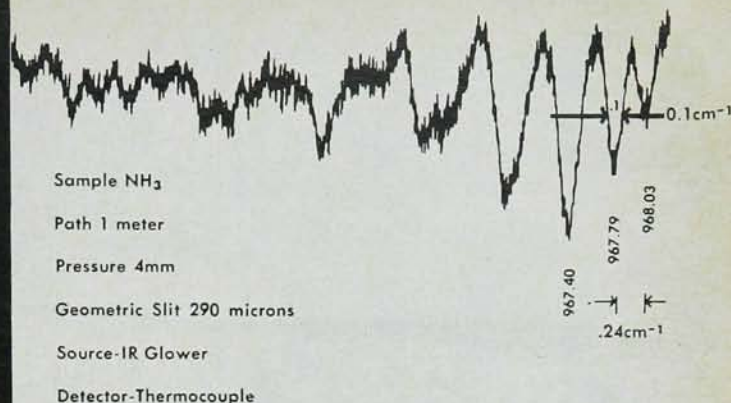
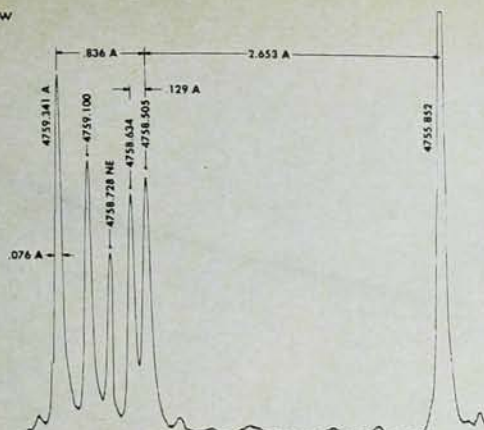
We already have a good system in operation for rapid publication of physics research work: namely, publication in *Physical Review Letters* of "Abstracts of Articles to be Published in *The Physical Review*." I just made a statistical study of the 33 abstracts published in the 20 June *Physical Review Letters*, to determine the time interval between receipt of the manuscript by *The Physical Review* and publication of its abstract. The average is 2.7 months, with quartiles at 3.2 and 2.4 months. The longest interval (I neglect the case of a revised manuscript) is 4.7 months; the shortest interval is 1.9 months. I submit that it is worth waiting 2½ months for an abstract of a publication, instead of developing a new system such as PIE, to obtain documents in, say, 1 month.

Of course, once the abstract of a *Physical Review* paper is published, any physicist can readily obtain a bona fide preprint of the paper by the simple expedient of sending a postcard to the author. There are three

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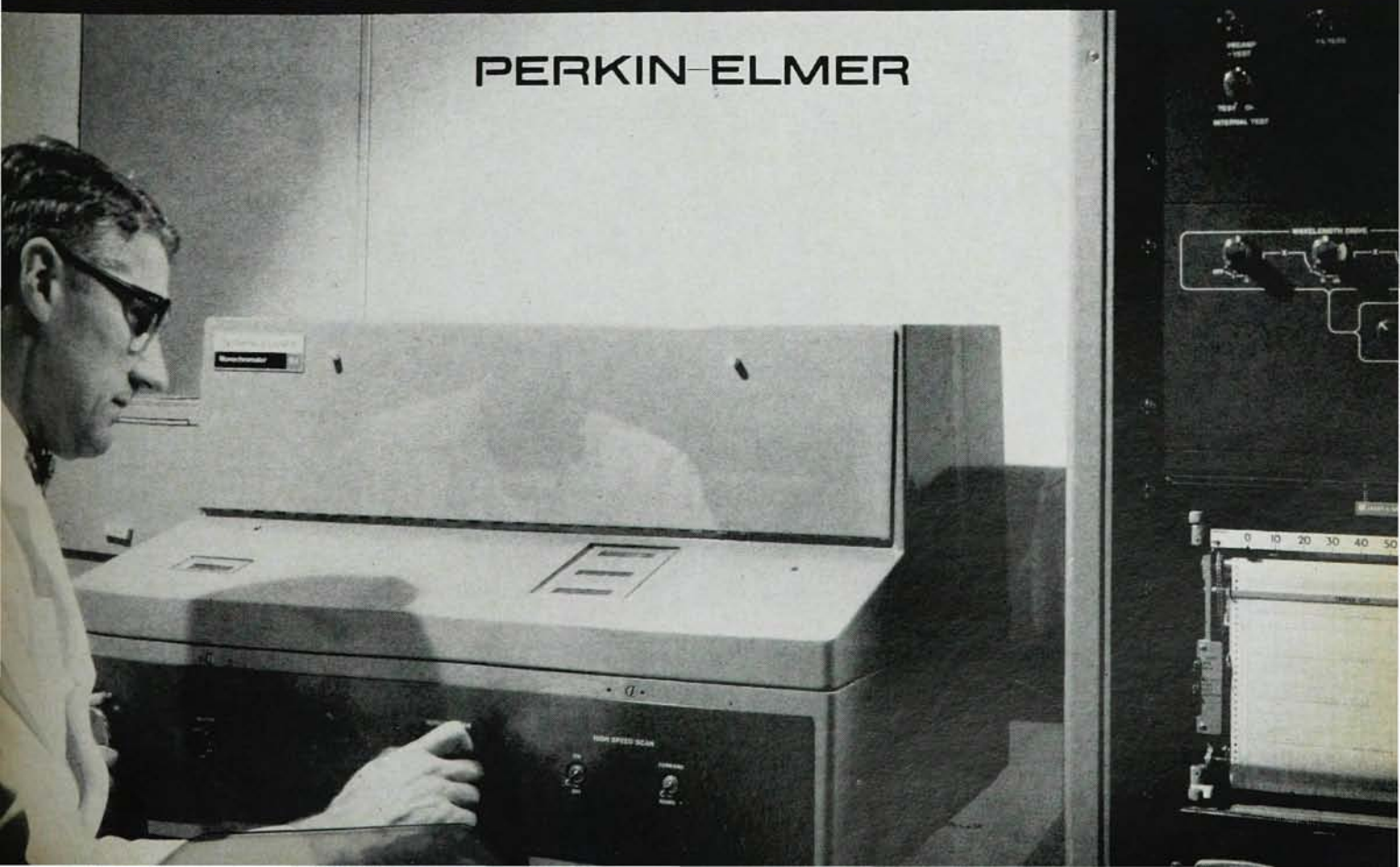
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great advantages in having bona fide preprints, rather than miscellaneous mimeographed documents: First, one can use data from a bona fide preprint with the assurance that the author is publishing these data. If one uses data from a "document" he runs the considerable risk that the data will have changed considerably by the time the paper is published, if indeed it ever is. Second, Pasternack's problem of "private communication" references is automatically solved. Further, *Physical Review* abstracts, and the corresponding manuscripts, are available to the entire physics community, rather than the selected few (or selected many) on a given preprint list. This feature of public access to published work is very valuable, and should be maintained.

I would like to see the efforts of physicists channeled in the direction of cutting the time interval for publication of *Physical Review* abstracts say from 2.7 months to 2.0 months or even less. (I remark parenthetically that I do not understand why it takes almost as long to publish abstracts in the *Bulletin of the American Physical Society* as it does to referee papers and then publish their abstracts in *Physical Review Letters*. It should be possible to publish abstracts for meetings in 1.0 month.)

Joseph S. Levinger
Rensselaer Polytechnic Institute

As a rather senior member of the computing fraternity, I would like to offer a word of reassurance to Pasternack. The enthusiasm for giant data base systems and especially for the establishment of a national document retrieval system for all of physical science and technology, is beginning to diminish. Military experience with command and control systems of comparable magnitude has been most unfavorable—I might say bluntly, scandalously unfavorable. And a few of us who are seriously concerned with the philosophy of our trade now believe that there are deep theoretical

reasons why such systems can never be brought into full or effective operation.

Leaving these philosophical arguments aside, however, systems like SAGE were never able to keep up with constant changes in objectives, new evaluation techniques, improvements in software, and constant upgrading of computer hardware. As many as half a dozen major programming fixes would be in the engineering-change pipeline at one time, each fix depending on the current state of the fantastically complicated system, plus all the changes ahead in the pipeline! I firmly believe that the same problems would arise in giant document-retrieval systems. Revised objectives, expanded subject matter, improved classification schemes, new fashions in programming languages, and a flood of urgent hardware improvements will combine to make impractical not only the system under consideration, but any system that attempts to relate all things to all men.

It does not follow, of course, that abandonment of the effort to automate information on a national or global scale would alleviate all the other problems of publication. The central one is certainly that of stemming the torrent of worthless or ephemeral material. But I cannot claim to be an expert in that regard, only a hapless victim!

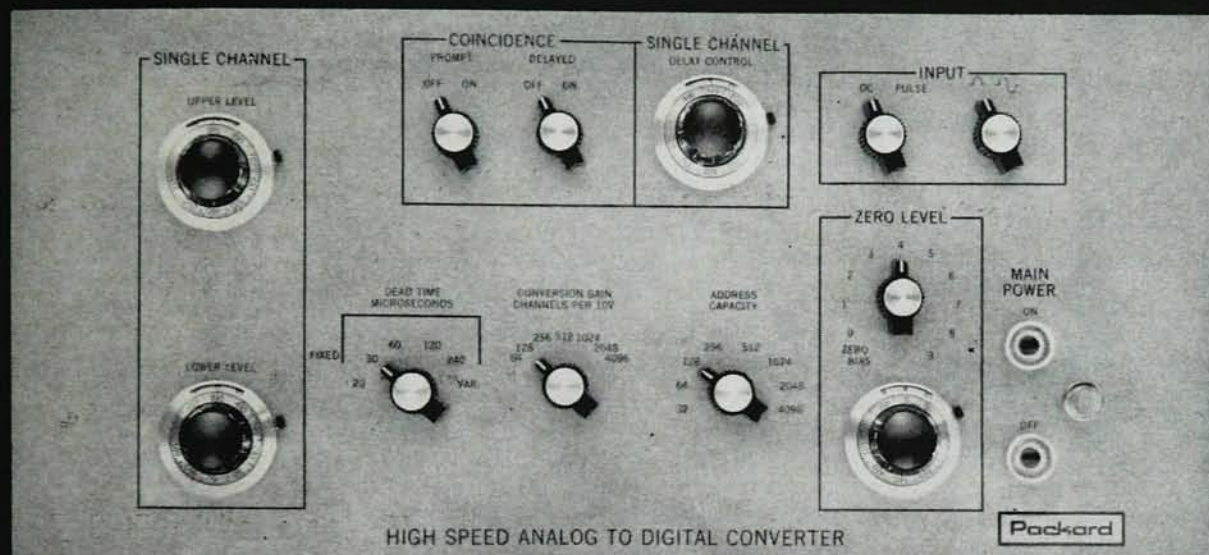
H. R. J. Grosch
General Electric, Santa Barbara,
California

The main point of Pasternack's articles is that the referee system as practiced by journals today is one of the major forces in maintaining the standards of scientific work. He is, of course, in possession of more information than is accessible to me, but I have an argument which shows that in most circumstances such a system should have just the opposite effect.

Consider a typical referee: apart from doing his average of refereeing 2.6 papers per year, he also does his own research, and perhaps teaches. He therefore spends more time on the last two activities. He, his colleagues and students write papers that

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they send to the same or similar journals. The better the school where the work was done and more the time that was spent on a paper, the more likely is it to be reviewed by a referee who is not as well up in the subject as are the authors.

Now consider the case in which a teacher or a colleague insists on some improvements, requiring more effort. Fairly soon he will come up against the suggestion, "You may be right, but let us try it out on a journal and see whether they publish it." The referee is never in a position to be aware of this. Even if he is a better expert, he has not spent as much time on the particular question.

Now the anonymity of the referees and the impersonal bureaucratic, judgmental tones of the pronouncements of the system are certainly very impressive, and they must convince a lot of people to suspend their own judgement in favor of what will pass the system. Also, just to be fair to one's colleagues and students, one can not insist on a standard higher than those which are universally recognized—at the moment, presumably, those enshrined in the referee system of the journals. Since referees in turn are picked from the same milieu, this must constitute a negative feedback. Provided money was forthcoming, this situation would also result in an increase in the number of publications and reinforcement of the system.

As with all social phenomena, the deleterious effects would be felt and actions would be taken to ameliorate them before the problems were fully recognized and articulated. Hence it may be significant that the preprint system, which now threatens to supersede the journals, first arose in establishments that were better placed to appreciate the new needs and to break away from the reliance on journal publishing. Even where the preprints are badly produced, and in spite of all the shortcomings pointed out by Pasternack, it remains a fact that one would rather read a preprint that comes from a well known source or is specially recommended than read

a paper just because it has been published in a journal. It can not be otherwise. It is true that the circulation of preprints also has increased enormously, but they are selected according to a more drastic procedure.

The refereeing system at best is a poor substitute for proper scientific discourse. Perhaps in reality what is so disturbing to Pasternack and many others is just this dissipation of discourse whose last vestiges—in the journal referee system—seem to be on the way out. But discourse by its very nature is a matter for small groups. Perhaps it was wrong to suppose that so many people could participate in a discourse. It is certainly too much to require that at the same time they should also engage in the tedium of publishing.

Kailash Kumar

Australian National University

Two cultures and the gap between

In your July editorial, "Two Cultures and Alienation," you disagree with C. P. Snow's statement that western society's intellectuals are being divided into two groups, the "literary intellectuals" on one hand and the "scientists" on the other. The assertion of the editorial is that between these two extremes there are a significant number of "grays and pastels."

To me it seems plausible that by the phrase "intellectual life of . . . society" Snow is referring to a relatively small group whose utterances and actions most affect the attitudes, standards, and material environment of western society—that is to say, the effective politicians and political thinkers, their critics, the creators and critics of art and literature, and, at the other pole, the scientists, engineers and technicians.

If these words accurately represent Snow's meaning then it seems that there is, in fact, evidence to support his view. The nonscientist intellectuals have generally done little to persuade one that they have any deep understanding of the spirit, attitudes and methods of science. The scientist and engineer fare no better, I suspect, in that those who know—not to mention understand—the factors that most