

Rebuttal:

Some Comments On Pasternack's Criticism

by *Michael Moravcsik*

PIE IS PROPOSED AS AN EXPERIMENT in prepublication communication. Just as with a scientific experiment, the outcome can not be entirely known beforehand; otherwise it would not be necessary to carry out the experiment. If we had definite answers to all the questions one can ask about scientific communication patterns and methods, this and other debates would not have to take place at all. We would simply institute programs that embody the answers, and, barring mismanagement, success would be assured.

We do not have the answers, however, and in deciding whether PIE or any other new experimental program should be undertaken, we cannot demand a priori assurances that the program be a cure-all for the problems that plague scientific communication. Instead, the requirements we should impose are that the new program (a) offer a reasonable prospect of improving some of the present ills, (b) not damage existing beneficial features of communication methods, (c) be flexible enough to be modified or even stopped if the outcome of the experiment demands it. I believe that PIE satisfies these conditions unusually well.

Three kinds of arguments

In discussing such an experimental program, arguments pro and con could be divided into three groups. The first group comprises arguments dealing with facts about the past performance of communication systems. Such arguments are the most suitable for discussion inasmuch as they can be debated on essentially rational and objective grounds.

The second group includes arguments based on

value judgments with respect to past and present patterns of communication. Such arguments are much less objective because they depend largely on the particular background, experience and even emotional commitments of the persons involved. Although there is sometimes a temptation to erase the dividing line between this group and the first one, it is important to resist the temptation; it can easily lead to an overconservative attitude in which no new idea is tried because it is thought to conflict with "solid experience" based on present methods.

Arguments in the third group are even more subjective. They involve predicting the outcome of the experiment in question. The danger here is that guesses will be stated as inevitable consequences of the experiment.

It seems to me that in our debate, the comments presented by Pasternack are divided between the second and third groups. Among the arguments based on value judgments of existing methods, some are in a category that is particularly dangerous in a debate: that category is made up of partly semantic disagreements. For instance, Pasternack claims that PIE material would be no more "selective" than journals because everybody in the world would have access to PIE. What I mean by "selective" is that the PIE material would be *imposed* on only a relatively specialized readership compared to journal material, which is "imposed" on all subscribers. PIE material would be imposed on only the 1000 or 1500 high-energy theorists in the world, whereas the subscriber list of *The Physical Review*, for instance, is many times larger. In my opinion, the problem for many of us is not

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to get access to information, but to enjoy the privilege of not being automatically flooded by information.

Definition of publication

Another semantic disagreement concerns the word "publication." Pasternack claims that "PIE would . . . constitute 'publication' in any reasonable interpretation of the word." At the risk of being considered "unreasonable," let me suggest that the definition of "publication" includes the concepts of permanence, compactness and some prepublication scientific scrutiny, none of which applies to preprints.

In connection with this last point, I might mention that the main underlying fear throughout all of Pasternack's arguments seems to be that preprints will somehow ruin journals. I do not share this contention (which is a combination of value judgments and guesses) because I believe the two formats have different and complementary functions. I explained this in my opening article and hence will not repeat it in detail here.

A third value judgment in Pasternack's argument concerns the analogy of PIE to the biologists' IEG. In my opinion the analogy, if not perfect, is close enough so that some comfort can be gathered from the success IEG has encountered among biologists. Whether the specialty of each IEG group is that much narrower than high-energy theoretical physics is very much a matter of opinion. Also, whether a membership of 600 (with individual distribution) is so different from a membership of, say, 1500 (with a group distribution of about 300) is also debatable. I know of at least one prominent editor whose attitude toward the proposed PIE has been substantially altered by information he has accumulated on the operation of IEG. I believe, therefore, that it is a considerable exaggeration to say the analogy of PIE to IEG is "highly misleading," and that IEG "differs radically in scope and quality from PIE."

Another matter of value judgment concerns the disclosure lag involved in today's journal publication. Pasternack thinks that because of the existence of letter journals, the de facto lag is much less than the four, six or even twelve months that I mentioned. I, in turn, think that his claim is unrealistic because letter journals publish only a small fraction of the material they receive and they are not designed to publish detailed papers on particular calculations, experiments, or theories,

and in my opinion such papers are still the backbone of scientific work. An overwhelming fraction of scientific work, therefore, would suffer a disclosure lag of many months if preprints did not exist.

I might add at this point that some high-energy theorists believe such a lag is not detrimental, considering the low standards of much work in the field, and in fact they would like somehow to do away altogether with 95% of the papers "published" in the field (not including, of course, their own). To some extent I sympathize with this sentiment, but I also believe that the deep reasons for such a state of affairs lie in matters that have nothing to do with publication patterns. I claim that fast communication helps to remedy the problem to some extent, inasmuch as both plainly wrong results and subtly unfruitful ideas die a quicker death if communication is fast. To be sure, a new idea in high-energy theory generates a great fad and furious activity on the part of many researchers, but such an idea is also likely to "burn out" faster in such a climate. I do not need to quote examples of this phenomenon.

Now let me discuss some of Pasternack's arguments that belong to the third group, the group of guesses. It may very well be that I will also be judged to err in the direction of stating *my* guesses as "extrapolated facts;" nevertheless I think Pasternack is guilty of stating *his* guesses that way. Let me cite a few examples:

Preprint circulation

I believe it is unwarranted to state that the number of preprints in high-energy theory today, when extrapolated to the operation of PIE, "must be multiplied by a factor ranging from 2 to 10 to allow for reports not intended for publication. . . ." The experience of IEG has been that a large majority of the material processed through it has consisted of journal-article preprints. I personally would like to see more informal discussion within the framework of PIE than we have today, but it seems highly unlikely that such discussion would double the number of documents passed through PIE, and a multiplication by 10 seems extremely far fetched indeed. However, the ultimate answer in this respect can be provided only by the experiment itself.

An even wilder guess, to my mind, is Pasternack's contention that the estimate of 300 preprint libraries "is a gross underestimate, by a factor of 5 or 10." The figure 300 was arrived at by combining all existing theory-preprint mailing lists in the Berkeley-Livermore area. Those lists have been handled very liberally in the past and hence

they can be expected to include most institutions around the world where high-energy theory is investigated on a research level. Combining the lists provided some 250 addresses. It is conceivable that by sending two copies to large groups and adding institutions missed, the total number of addresses would eventually be raised to 400 or even 500. To make a plausible claim that PIE would need a mailing list of 1500 to 3000, Pasternack would have to produce a list containing such a number of addresses. I do not think he can do so.

The amount of money that PIE would save, compared to the present system, has been the subject of a number of "guesstimates," and the results differ considerably. Only the experiment can enable us to arrive at a reliable figure. I do agree with Pasternack, however, that PIE could be successful only if it replaced, and not added to, the present system of preprint distribution. I might mention at this point that recent discussions concerning the details of PIE operation suggested that it might be advisable to supplement the group mailing list by sending about 20 copies of each paper to its author. He would be at liberty to distribute the copies to the few individuals whom he judged to be so intimately involved with his work that they warranted personal copies. Of course nothing would prevent others from using duplicating machines for making personal copies of the very few papers in which they had a very strong interest. In this respect the situation would be similar to that prevailing today.

Effect of PIE on journals

PIE's effect, if any, on journals has also been subject to conflicting speculations. As one who serves as an occasional referee for journals, I can say that preprints do not decrease my enthusiasm for refereeing papers. In fact they make my job easier because by the time a paper reaches me for comment I have already read the corresponding preprint; thus it takes less time for me to prepare a referee's report. As to Pasternack's contention that the existence of PIE would make editors more lenient toward accepting papers, because refusal or demanding revision "really would not be worth the fuss," I have the testimony of another editor who said he would be more strict if PIE existed because he would not have the nagging fear of depriving a worthwhile idea of *all* channels of disclosure by refusing publication. Again, only experience with the PIE system could tell what the actual effect would be.

There are two other guesses Pasternack makes which I feel are implausible. He claims that the

problem of referring to preprints or other informal documents, tracking them down and retrieving information from them would be "greatly exacerbated by the proposed PIE system." Pasternack gives no explanation as to why this should be so. Equally unexplained and undocumented is his guess that priority problems "would be multiplied many times by PIE." The experience of IEG does not seem to indicate such multiplication at all, and as far as I can see there is no reason to expect it.

Of the alternative suggestions Pasternack makes at the end of his article, I might mention that a document registry in high-energy physics is in fact already in existence. It is published by the Deutsches Elektronen-Synchrotron (DESY) in Hamburg. At present the registry is circulated only in a very limited way and in only a few copies, but it might be possible to increase its circulation so that the DESY index could serve as an automatic table of contents for each preprint library in the PIE system.

A possible pitfall

I would like to take this opportunity to emphasize a real pitfall that PIE might fall into, but which it must avoid. The operation of PIE should be kept simple and flexible so that changes could be made in it and the possibility of its termination could be maintained as a realistic alternative. Thus it should not develop into a self-perpetuating empire, at least until it has proven satisfactory and has found its optimal format. I suggested in my opening article that the proposed structure of PIE is well suited for such flexibility. I believe that in preserving its vitality PIE would also be helped by the fact that its success would depend on *voluntary* cooperation by high-energy theorists; it must in fact cater to the *de facto* communication patterns that dominate high-energy physics.

In conclusion, I want to reiterate my belief that PIE is a worthwhile experiment—one that might help solve some of our communication problems, that is unlikely to harm existing beneficial communication methods and that is flexible enough to serve as an experiment. Its two main goals are to make preprint distribution more efficient and more equitable. It is hardly a revolutionary program, but experience with it might lead us to an increased understanding of the underlying problems. This understanding might in turn generate other experimental programs. The success of PIE will depend crucially on the active interest and cooperation of the high-energy physics community. I can only hope that the present debate will contribute to arousing such interest. ■