affect the modern dramatist or the contemporary political thinker are very rare birds.

With the ever increasing need for the effective person to devote more and more attention and time to the details of his professional tasks, it seems unlikely that he will find more time to study the problems and attitudes of other professions. Perhaps we will see the rise of a profession devoted to increasing and maintaining communication and understanding between segments of the intellectual community.

Richard Hutson
University of Colorado

Individualists and publication

Every successful technical journal receives a plethora of manuscripts made worthless by faulty logic, incorrect or incomplete postulates, or some other or others of scores of possible reasons. But occasionally a valuable paper is mistaken for one of these hopeless ones because it uses a currently unpopular postulate, or unconventional mathematics, or uncommon nomenclature, or cetera. A mark of the wisdom with which the journal is directed is its policy with respect to papers that depart from the conventional, for, as is commonly acknowledged *in the abstract,* far-reaching new ideas often look strange, so strange that papers on them may be dismissed lightly, as only more hogwash. This is quite understandable in view of busy editors having quite enough to do with overseeing and controlling papers that are more conventional and therefore "safer".

Now our modern plan of programed, communal scientific thought is relatively new. But communal living is as old as the culture; and our problem-solving techniques have evolved over many centuries. One of them is that an accused has the right to defend himself before his judges.

Publication is as important to the individual scientist, and to the scientific community, as many legal controversies. Yet in protecting publication rights we are still in the stone age. A submitted paper goes to an editor who, in turn, sends it to one or more reviewers, one of whom might be the editor himself. If negative reviews result, the author need never be informed why the paper is rejected. He is never informed who the reviewers are; there is no way for him to prove to his judges whether the reviewers were informed or naïve, judicious or prejudiced, apt or inept. There is no appeal, no court of last resort. Final decision is left, often without adequate safeguards, to the editor and his board.

This policy is wrong in two respects. It is unfair to authors, and it places undue responsibility on the editorial board. If the board decides to publish, it runs the risk of wasting money, time, and precious journal space, and of incurring ridicule, if the paper proves to be worthless. It therefore may well fail safe by rejecting the paper—but then it runs the risk of letting some important innovation die aborning.

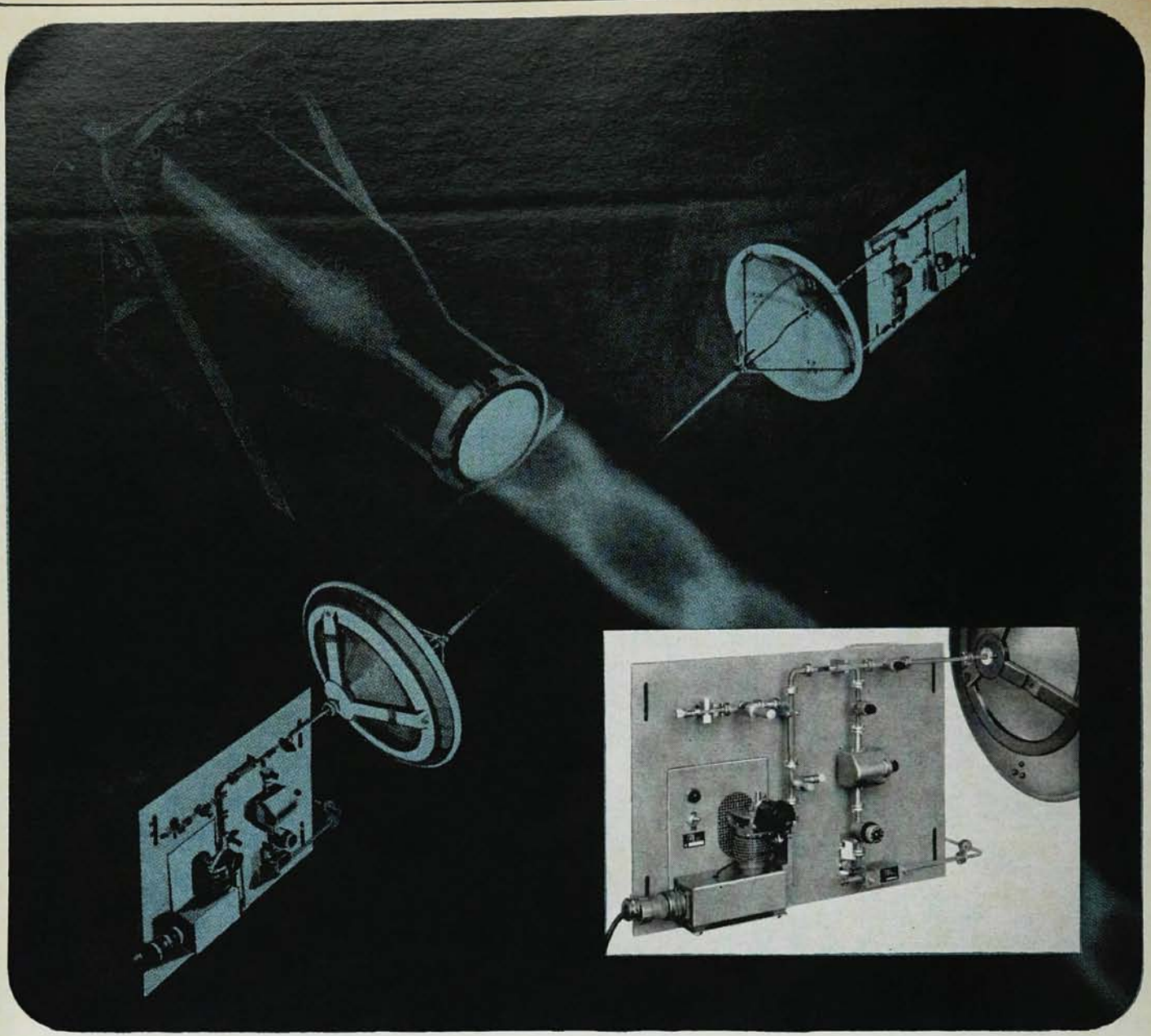
It is with this latter risk that I am concerned. Editorial boards have long given in too easily to the temptation to reject. There is need of a higher court. I support the idea of a journal selecting articles by the criteria proposed by Professor Sachs (PHYSICS TODAY, June 1966), provided that the editorial board hears not only the reviewers' criticisms, but also the author's rebuttal of those criticisms, before rejecting an article.

Dale M. Grimes
University of Michigan

High-energy physics—a comment

We have read with interest the article "High Energy Research" by George Tautfest (PHYSICS TODAY, July, page 69). This concise survey of recent developments in the field seems to us to require some clarification in the section dealing with wire chambers, and therefore we would like to make the following points:

1. The recovery time quoted for wire spark chambers, 200 microsec, is optimistic. As far as we know, no one has obtained such recovery times for a system of wire chambers operating in an experiment.



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LETTERS

(Continued)

2. The juxtaposition of "Lindenbaum-Yuan" with the detailed description given by Tautfest of a recent Brookhaven experiment as an example of "on-line experimental control" might suggest to the casual reader that the experiment was carried out by that group. This experiment was in fact carried out by ourselves together with a group from Carnegie Institute of Technology during the summer of 1965. Preliminary results were recently published (*Phys. Rev. Letters* 16, 855, 1966). The experimental system was the result of a two-year development program carried out by us in collaboration with the Instrumentation Division of Brookhaven Laboratory and had no connection with the Lindenbaum-Yuan development of large scintillator arrays.

3. Our experiment was the first to use the PDP-6 of the on-line data facility of Brookhaven. It is a sign of the rapid progress in this field that we were able to saturate this moderately powerful computer during parts of our run, when the event rate exceeded 150 events/pulse (50 events/sec).

4. Most of the material used by Taufest in his description of this experiment is not from reference 15 but from Fujii et al., *IEEE Trans. on Nucl. Sci. NS-12*, no. 4, 231 (1965).

A request for permission to use the figures, omitted through oversight, would have avoided this misunderstanding.

E. W. Anderson, E. Bleser, G.B. Collins, T. Fujii, N. C. Hien, J. Menes, E. Turkot
Brookhaven National Laboratory

A CORRECTION: The hydrogen maser measurements attributed in our August article to Norman Ramsey were indeed discussed by him in his talk. They were made, however, at the National Bureau of Standards, Boulder, Colo., by a joint effort of NBS and Varian personnel using Varian hydrogen masers. □

Arthur D. Ballato, Robert V. McKnight
US Army Electronics Command