

was done with the best intentions, but it was still done.

It is noteworthy that Princeton University recently elected two members of the graduating class to their board of trustees. Before this the average age of the board exceeded 60 years. Perhaps we can learn from their experience. Should the councilors be a more heterogeneous, more youthful and broader based group? Should it include one or more recent graduates? Should it include people from industry?

Improved representation might help us out of our present dilemma. An amendment changing the nature of the council might well be in order.

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Suggestion: the minireview

The need for an improved system of reviews has been stressed in recent articles, particularly in the physics literature. The suggestions are equally applicable to chemistry.

The primary need is for timely substantive reviews—in which topics are analyzed exhaustively. These should indicate what data and concepts are still valid and why others should be discarded. The synthesis of "best values" for molecular properties and parameters of systems, that is, the most reliable values the reviewer can derive from the data at hand, and recommendations for future work should be included where applicable.

Such reviews are research and information filters, not reporting. They are new source documents that will replace, for most readers, the research reports that are analyzed. Conyers Herring's phrase "Distill or Drown" (PHYSICS TODAY, September 1968, page 27) is apt.

Much of the discussion to date has been aimed at the improvement of the invited review, the large-scale review that is the staple of the review journals and the *Progress in X* series. Examples of large substantive reviews are: L. J. Kieffer and Gordon H. Dunn (*Rev. Mod. Phys.* 38, 1, 1966) on electron-impact-ionization cross sections and H. S. Johnston (*NBS-Nat. Std. Ref. Data Series No. 20*, 1968) on the kinetics of oxygen reactions.

Unfortunately, most large reviews are straight reporting. The time and

effort required for the preparation of a large critical review usually are greater than the author is willing to devote to the subject. In contrast this detailed coverage is feasible in finite time and space if the topic is limited in scope.

I suggest that the review literature can be expanded rapidly by encouraging the *contribution* of small-scale reviews on restricted topics. The suggested incentive is a special publication medium.

For these small reviews the research community is the pool of talent. The reviews would be evaluations of topics that a research scientist masters in the course of planning or writing up his own work. At its very simplest the proposal is: Capitalize on the evaluation effort that goes into a first-rate research paper.

This does not mean that the typical discussions in papers are suitable. They rarely are scholarly. They are often partisan justifications for publication. Even the best are viewed in this light by the jaundiced reader.

The short evaluation is not easy to prepare. It must meet the goals listed earlier. It is important that the review be definitive and present well documented conclusions and recommendations. This program will not appeal to all scientists, but, for those who are attracted, the time required for preparation will be reasonable because they will be drawing on experience gained in their own work.

If the scope is kept small, five pages of text should be a generous limit. An example of a review having the scope envisaged is the discussion by Eugene S. Domalski and George T. Armstrong of the heat of formation of tetrafluoromethane in section 10 of their paper (*J. Res. Nat. Bur. Stds.* 71A, 105, 1967).

These short evaluations should be given high visibility. They should be published in special sections in the journals that normally cover the pertinent field.

This proposal has several implications:

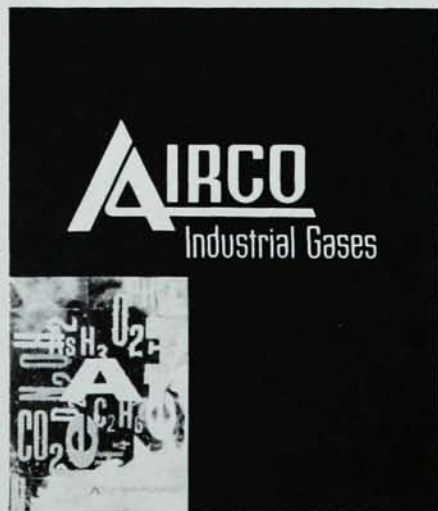
- Prepublication refereeing must be thorough, because a "best value" (a vital part of these papers) is often taken as gospel by the nonspecialist. If the evaluation stands alone it will get close scrutiny by the specialist.

- Since in most cases critical evaluation involves subjective judgments, one can expect that many of these reviews will arouse strong differences

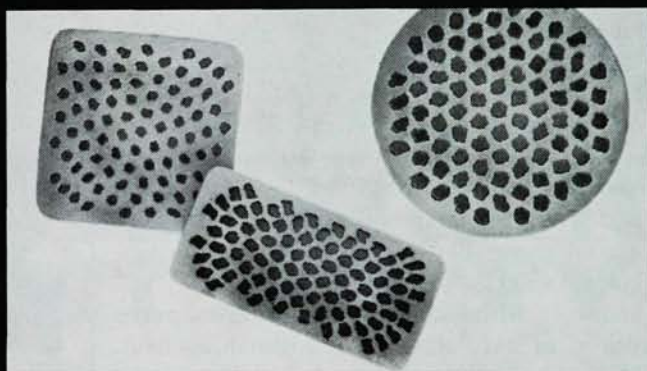


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of opinion. These comments should be published in the same section. If they come from a referee, they should be published (under his name) along with the review.

• Finally, particularly for data evaluations, a highly organized, reasonably standard format can be imposed as an aid to the reader. The highly organized format is more common in reports than in articles. An example that illustrates its utility is the series of rate evaluations by D. L. Baulch, D. D. Drysdale and A. C. Lloyd (*High Temperature Reaction Rate Data. No. 1*, Department of Physical Chemistry, University of Leeds, 1968).

This plan has several significant things in its favor. It has a broad base for input: the research community. It supplements but does not interrupt research. It offers a suitable medium for publications of auxiliary output. It appeals to self-interest. It even may strengthen the discipline of evaluation and attract more scientists to it. Potentially it could produce evaluated data on a scale far beyond that of organized review programs. An information explosion in data evaluation would be welcome.

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Contributions to cosmology

I have read with considerable interest the article by Donald D. Clayton on "The Origin of the Elements" in the May 1969 *PHYSICS TODAY*. Since the author goes into considerable detail in reviewing the subject, including a list of 63 references, I think it is timely to emphasize some of the earlier highlights that the author seems to have overlooked, or forgotten, in an otherwise fascinating condensed account. I refer to the contributions of the very gifted and imaginative George Gamow, and his collaborators of many years, Ralph Alpher and Robert Herman.

To be sure the author does refer to Gamow's 1928 paper dealing with the quantum penetration of Coulomb barriers, and although few people would quarrel with the importance of this contribution, it is a matter seriously to be questioned as to whether or not this was Gamow's sole contribution to cosmology.

With the exception of Alpher's 1948

George Washington University thesis for which Gamow was the faculty advisor, the Gamow, Alpher, Herman papers are to be found in well known journals such as *The Physical Review*, *Reviews of Modern Physics*, *Nature*, etc. In view of the continuing and indeed growing interest in cosmology, both from the observational and the theoretical points of view, it is important that one maintain a full and accurate record of the more important contributions to the field. I should like to refer the reader, for a proper perspective, to such recent articles as those by William A. Fowler "The Empirical Foundations of Nucleosynthesis" in the Pergamon Press (1968) volume *The Origin and Distribution of the Elements* (L. H. Ahrens, ed.), and W. A. Fowler, W. E. Stephens, *Am. J. Phys.* 36, 289 (1968).

I also note the author's reference to the discovery in 1965 by Arno A. Penzias and Robert W. Wilson that the universe appears to be filled with a photon gas at a temperature of approximately 3 K. I should like to point out that Clayton also seems to have overlooked that Alpher and Herman predicted a 5-K cosmic background black-body radiation in their 1948 *Nature* and 1949 *Physical Review* papers, which were part of a continuing study of the origin of the elements in the context of an expanding universe. This was indeed a rather remarkably accurate prediction as has been borne out by the observations of nearly 20 years later.

Those of us who were both physically and intellectually close (for some years my office was adjacent to Herman's, the separating wall being made of some acoustically transparent material) to the Gamow-Alpher-Herman collaboration vividly recall the marvelous excitement of their frequent sessions. Gamow's insight and imagination have left their mark on modern



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