by the referees, then there exists the choice of either incorporation of the referees' comments or of actually doing a major revision with the inclusion of any material which has been published subsequent to the initial submission of the review article. If the latter course is pursued, it should be approved by the editor prior to initiation since the author may find himself in the position of actually submitting the manuscript as though for the first time with all of his previous problems to encounter once again. After the revised manuscript has been returned to the editor it is set in type and shortly thereafter the author will receive the galley proof. This proof should be checked very carefully for correct content, for typesetting errors, and for errors in the structural formulae, and the corrections or changes previously noted by the author on his copy of the original manuscript should now be entered on the galley proof.

There are some problems inherent in the publication of a critical review, particularly if the author is currently involved in research in this area. For example, there is now a complete literature search in existence for other investigators currently in the area as well as anyone contemplating future research in this area. Some of the most rewarding and interesting research originates from an obscure reference which other investigators are unaware of. Also, a critical review usually emphasizes or points out the most promising areas of future research or certain areas which have been neglected by previous investigators. This poses the possibility of someone else publishing certain portions of the author's projected research before he can complete it. However, if this does happen, the author can take consolation in the fact that he must have written a good review article and that his and the editor's initial assumption that the area should be reviewed has been justified.

In summary, I should like to reiterate some of the steps involved, the questions which must be answered, and the problems which may face a potential author of a critical review.

1. Determination of need for review in this area and the scope of the review.
2. Capability of the potential author for writing a critical review.
3. The need for obtaining a commitment of publication prior to writing the review.
4. Evaluation of individual sections on the basis of the journal in which the review will be published.
5. The procedure for and necessity of a thorough and complete literature search, including an assessment of the resources available in the potential author's library.
6. Factors involved in the evaluation of previously published articles.
7. The basis on which previously published material should be republished in the review article.
8. How to determine if structures and tables should be included or excluded.
9. The need for establishing a procedure for writing the first rough draft.
10. The steps involved in the conversion of the first rough draft into the final copy.
11. What to do about the referees' comments and the problems associated with the galley proof.
12. Reprint requests.

These are the major steps and problems which a potential author of a critical review article should consider very carefully before committing himself to the tremendous task which is inherent in the writing of a critical review article. If the challenge is accepted, I am sure the author will find this endeavor to be a most rewarding experience.

Critical Reviews: The Editor's Point of View*

HAROLD HART
Department of Chemistry, Michigan State University, East Lansing, Michigan

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Rapid expansion of the chemical literature has increased the need for well-written reviews of all types, particularly of critical reviews. Such reviews extract from the morass of verbiage those contributions which are most significant and focus attention on major problems and ignore trivia. They not only summarize the field at the time of writing, but suggest new directions in research which may be most profitable. In the selection of a subject, a case is presented for timeliness, broad appeal, and unconventional organization as factors which may enhance a review's critical value and its impact on science. Authors preferably should be established investigators familiar with the nuances of the subject, or bright young investigators initiating research in the topic being reviewed. Influences which an editor can exert are discussed.

One cannot arouse much argument with the question—Do we need critical reviews? Every research chemist will argue that we do (provided, of course, that sharp criticism does not strike too close to home). Most chemists probably also agree pretty well on what constitutes a critical review; or at least they will easily recognize that whereas some reviews are primarily compilations, others present material with a distinctly new twist, thus stimulating new thought and research. Most reviews, of course, fall somewhere between these extremes which, incidentally, are not necessarily mutually exclusive; comprehensive reviews can also be critical.

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The main problem for the editor is to seek out and publish those reviews which will be most valuable to the chemical community. In general, the constructively critical review will be more valuable than the review which is merely compilative. The difficulty is, of course, that it is easier to compile, collect, and present information in an orderly fashion than it is to digest it and present it in a stimulating and critical way.

Having assumed the editorship of *Chemical Reviews* less than a year and a half ago, I cannot profess to bring the voice of editorial experience to this Symposium on Critical Reviews. However, the acceptance of that editorship indicates the considerable importance which I attach to the role played by critical and comprehensive reviews in the advancement of chemical research. Perhaps in this article I can indicate some of the ways we are trying to improve *Chemical Reviews*, as they do, I believe, have general significance for the problem of critical reviews; what, why, when, and how.

FUNCTIONS OF CRITICAL REVIEWS

Rapid expansion of the chemical literature has increased rather than decreased the need for well-written reviews of all types, and especially for critical reviews. One need not belabor the literature growth, particularly before this Division of the Society, but as a graphic illustration one may cite the increase in number of columns in *Chemical Abstracts* from 7706 in 1946 to 17,536 in 1956 to 41,400 in 1966![1] In 1967, *CA* published nearly 244,000 abstracts or, put another way, a chemical article, patent, book, etc. which needed abstracting was completed approximately every other minute, day and night. One might well ask why, with so many papers to read, one should solicit additional articles, in the form of reviews. Yet it is precisely because of this profusion of literature that additional reviews are essential.

It is perfectly clear that all research which is published is not equally significant. Indeed, many factors built into our academic, industrial, and government research organizations in this country and abroad guarantee that much insignificant work will reach and be published in our research journals. This may be a harsh statement, but I believe it is true and a consequence of the system. That advancement in major academic institutions usually depends in good measure on high research productivity cannot be denied. In the best universities, quality as well as quantity is insisted upon. But in many universities, particularly those which might be described as "aspiring," it is assumed that if a faculty member produces some research he is alive, alert to developments in his field, and therefore a desirable faculty member. Indeed, this is frequently true and I do not mean in any way to belittle academic research. But such research often lacks a major directing force with regard to scientific progress; its driving force is frequently directed toward academic survival. Put another way, the goal of doing research may be to maintain or improve the academic qualifications of the investigator rather than to accomplish a specific research objective. This lack of a major goal or orientation with respect to the discipline itself can lead to research, and papers, which lack major significance and therefore clutter our research journals with fragmented and sometimes trivial research.[2]

The same motivating forces are not as openly apparent in the chemical industry, but they are there nonetheless. Only a fraction of patents are financially profitable. The belief prevails that this fraction does not vary markedly with the number of patents produced within a given time interval. Therefore the pressure is on the research department to produce more patents per unit time, with the hope that the net number of profitable patents will increase. The result once again can lead to a cluttering of the literature with sound but relatively insignificant research, when considered in terms of long-range progress of the science.

I am not suggesting that the publication of such research be curtailed; if it meets the tests of authenticity and quality, it should reach the journal or patent page. For although an expert can read a particular manuscript and frequently give a valid judgment with respect to the probability that its impact on the future development of the science will be high, medium, or low, he may on occasion miss the target by a wide margin. Sometimes the significance of a piece of research remains dormant, only to be appreciated years later.

This is where the writer of a critical review can provide a major service to science. His review, if critical, will extract from the morass of verbiage those contributions which are most significant. It will focus attention on major problems and ignore trivia. The reviewer will sift and sort much material for the overburdened reader and present it to him in a condensed, new, and stimulating form. His review will not only provide a summary of the state of affairs with regard to a particular research field at a particular point in time, but hopefully will also pinpoint the problems which still need attention and, after spinning the reader around several times, will set him off in the "right" direction.

One can cite many examples of reviews, even books, which have been a spur to new research, which have been provocative and challenging; unfortunately, one can cite all too many more which have not. Two important ways in which an editor can help increase the percentage of the former are in the selection of subjects and authors.

SELECTION OF A REVIEW SUBJECT

The fact that a subject has never been reviewed, or that it has not recently been reviewed is, in my opinion, a poor justification for embarking on the project. Yet it is surprising how frequently this is the opening gambit in the letter from a prospective author. True, one does not want to needlessly duplicate effort, and in *Chemical Reviews* we prefer to publish articles on those subjects which have not been *thoroughly and critically* reviewed within the previous five years. But some reviews are neither thorough nor critical, and there is always room for an excellent article that will correct these deficiencies, provided the subject matter itself justifies the effort.

A review should be written for the audience that will read it *at the time it is published.* Some reviews bear the passage of time well and retain their freshness and vitality; but unfortunately, like the people who write them, most reviews age and become dated. A few classic reviews are referred to 10 and 20 years after their publication,

but on the whole a critical review will have its greatest impact within the first few months or years after it is published. For this reason, I would consider *timeliness* an important criterion in the selection of a review subject. If there is active research in the field, the criticism and evaluation offered by the reviewer will be seriously considered by the many persons who, at the moment, are exploring and expanding the subject. In short, there will be a wide and receptive audience for the review. Thus the opportunity for the reviewer to influence the course which the science may take is considerable. Occasionally a subject may be revitalized by a stimulating review but more frequently it is the vitality of the subject which prompts the writing of a review.

Along with timeliness goes *broad appeal.* If the subject of a review is too narrow, its limited appeal will restrict the impact of the review, no matter how brilliantly or critically it is written. If a choice is available, therefore, the editor should give priority to articles which will affect a large community of scientific readers.

Controversiality may be another factor in selecting a subject for review. Undoubtedly a controversial subject (for example, at this writing—the question of nonclassical carbonium ions in organic chemistry) will attract readers. Whether more important consequences than just wide readership will ensue from the writing of such a review is open to debate and will depend on the treatment of the subject. The few reviews on this particular subject, though soundly written, do not seem to have sent researchers forth into new and more fruitful research gardens. In my opinion, timeliness and broad appeal are far more important criteria than is controversiality.

Unconventional organization, or the *new view*, is a most valuable criterion for selecting a review subject. If a field can be subdivided in a new and different way; if it can be viewed in terms of principles rather than compound or reaction type, if the subject cuts across established disciplines, then it is more likely that the review will be critical and stimulate new thinking than if it is organized along conventional lines. I would like to cite a specific recent review which superbly illustrates what I mean. I hope the author will not be embarrassed by my choice of his article; since it appeared in *Chemical Reviews* before I became its editor and since I do not know its author personally, the choice is made without prejudice and simply because the article illustrates so well the point I wish to make. The review is entitled "Peri-Interactions in Naphthalene Derivatives."[3] Because of his subject, the author was forced to cut across conventional disciplines in writing his review. He discusses both physical and chemical evidence for the phenomena reviewed, and as a result includes such diverse subjects as x-ray structure determination, molecular spectroscopy, dipole moments, dissociation constants, as well as a great variety of chemical reactions and such phenomena as tautomerism, resonance, chemical kinetics, molecular rearrangements, and hydrogen bonding. Because of his choice of subject (as well as his care in writing), the author's article has been stimulating to a wide audience.

Finally, although the subject itself may be fairly conventional, it may be viewed from many vantage points and thus provide new perspectives to the reader. In 1969, *Chemical Reviews* will initiate a new program in which certain issues, from time to time, will be devoted to a group of reviews centered around a single theme. The theme itself may be fairly conventional as, for example, "chemical kinetics," but various aspects of it will be reviewed by authors who are experts, highly qualified to be both critical and prophetic. Perhaps by focusing light on a single diamond from many angles, its various facets will become apparent and will stimulate the beholder.

SELECTION OF A REVIEW AUTHOR

It is not always possible for an editor to choose the author he wants for a review article, but when this is possible, the choice can be important and influence the quality of the article. Authors for articles in *Chemical Reviews* are usually chosen by one of two ways. Frequently, a chemist will write the editor saying that he wants to prepare a review on such-and-such a subject, and he submits an outline for approval. If the subject has the desirable features set forth in the previous section of this article and if the author seems qualified, the proposal is approved. The author in this instance possesses at least one important attribute—a spontaneous will to write the article! If the qualifications of the author and the outline fall outside the competency of the editor, the outline is submitted to expert referees for comment. Perhaps 50 to 75% of unsolicited proposals are approved, and the editor exercises some control, probably more on subject matter than on quality of the author. If the author is a well-known contributor to the field, there is no problem. If he is relatively unknown, some judgment can be made from his present position (an indication of his scientific, if not writing and critical ability), his educational background, and his use of language in the proposal (lucid and well-organized, or muddy and disjointed). Admittedly these criteria are a bit vague and it is true that the editor does have to function, at times, on intuition.

Alternatively, authors are invited to contribute an article on a particular subject, and here of course the editor exercises considerable control over both subject and author. If possible, it is best to have an author who has been an active contributor to the subject being reviewed. Even though this may result in some bias in a review on a controversial subject, I think that intelligent bias is to be preferred over ignorant or dull objectivity. An author will appreciate the nuances and subtleties of a subject if he has done research in that field himself. His article will, in general, be more critical and sophisticated than that of a stranger to the field. Such experts may be too busy to write a review, but they are my first choice.

Close behind is the bright young author, just embarking on a career, who plans to initiate research in a field and is eager to review it thoroughly first. He actually may have already published a little in the area. He will bring a newer, fresher approach to the subject than an already established investigator, and though he may lack maturity, he will compensate for it with stimulating enthusiasm. Many of the best articles in *Chemical Reviews* and similar journals were written by such young authors, early in their careers.

Most reviews which have had an impact on their area of science were written by one or the other of these two types of author.

Finally, may I say on this aspect of the subject, one person serving as editor of a fairly general review journal, such as *Chemical Reviews*, cannot keep sufficiently atop all branches of chemistry to go it alone. Each year, I solicit from each member of the advisory board a list of subjects and authors; I exercise some discretion in extending invitations from these lists, but, in general, the suggestions are invaluable to the maintenance and improvement of the journal's quality.

REVIEW MEDIA

As a consequence of the general literature growth, there has also been a dramatic increase in the number and variety of review media. In addition to the leading review journals (such as *Chemical Reviews*, *Quarterly Reviews*, *Angewandte Chemie*, *Russian Chemical Reviews*, and the recently initiated *Accounts of Chemical Research*) and more specialized reviews (such as *Annual Review of Physical Chemistry* or *Annual Review of Biochemistry*, review issues of *Analytical Chemistry* and *Industrial and Engineering Chemistry*), we have seen the rise during the past decade of numerous hard-bound, monograph-like series which usually carry titles such as "*Advances in...*," "*Progress in...*," etc. Separate chapters are written by different authors, and the subject matter is more or less cohesive, depending on the particular series. Many of these are high quality books which contain critical reviews written and edited by experts, and their popularity attests to the useful role they play in the review literature.

Such series have become attractive to authors and editors for several reasons. That they are hard-bound may, in the author's mind, lend some feeling of permanence or immortality to his contribution, more perhaps than if the same chapter were to appear as an article in a review journal. The author also has the prestige of association—his article is included with those of his esteemed peers. Finally, the author receives some financial return for his effort, although this is usually far from commensurate with the time and energy expended.

Despite experience as both an author in and an editor of such monographs, I submit that, although they have provided a medium for critical reviews, they are not without some very serious drawbacks that can be avoided by the review journals. For one, they are extremely expensive. An article in *Chemical Reviews* or one of the other major review journals costs the subscriber only about one-fifth the cost of the same length article bound in an "*Advances in...*" series. The difference is partly due to printing costs, and partly to the requirement that commercial publishers make a profit; only a small fraction of the added cost is due to the editor's or author's royalties. Yet these royalties are an attraction to the author. I suggest that it is neither unreasonable nor economically impossible for review journals to present their authors with a substantial honorarium as a token of appreciation for their service to the profession. Several leading journals have already enacted or are considering such a policy. In this way authors can be attracted back to the review journals, and their articles will be made available to the rest of the chemical community at lower cost.

Review journals are usually subscribed to by most libraries where they will be used. Frequently the same libraries will miss one or another of the review monograph series—this is particularly true of smaller libraries—partly through oversight in ordering, and partly because of cost. There can be little doubt that an author's critical review will have a wider circulation and a greater chance for a permanent home in the world's libraries if it appears in a review journal rather than in an "*Advances in...*" series.

Finally, publication time can be very much shorter in review journals than in bound volumes. One does not have to wait for tardy authors, and journal issues appear at regular intervals. The time saved varies from 6 to 15 months, an important factor if the review is not to be dated or lose its impact before it appears in print.

For these reasons I believe there is much to be gained both by authors and by readers who preferentially patronize review journals as media for publishing critical reviews.

CONCLUSIONS

In summary, then, the need for critical reviews is greater now than ever before. Choice of subject, author, and publication medium may have an important influence on how effective a critical review will be in its impact on science and scientists. In selecting a subject, important criteria are timeliness, breadth of interest, and a new view or unconventional organization which will cut across usual disciplines. Authors preferably should be established investigators familiar with the nuances of the subject, or bright young investigators just initiating research in the topic being reviewed. Review journals can publish reviews faster, less expensively, and distribute them more widely than can bound monograph series.

LITERATURE CITED

(1) This comparison neglects changes in format, type and page size, and the practice of using author's abstracts.
(2) For a discussion of the distribution between "basic" and "undirected" research, see R. M. Lukes, *Science* **159**, 34 (1968).
(3) Balasubramaniyan, V., *Chemical Reviews* **66**, 567 (1966).