

How to Improve Scientific Communication*

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The theme of my talk has been announced as the future of scientific communications. I have taken some liberty with that theme, choosing to stress the impediments in the way of future developments rather than the developments themselves.

More words are being written and spoken about communications problems these days than about any other subject—except perhaps space and the Cold War. Hosts of communications “engineers” have sprung up, and I sometimes think that it is their verbosity and the indirectness of their approach—more than what they say—that establishes the fact that the problem exists.

Science in general, and chemistry in particular, is deeply concerned about the tremendously accelerating growth of research activity, and the demands consequently being made for more efficient systems for recording this research.

I propose in this talk to explore with you some of the ways in which these developments are affecting the ACS publication program, present and future; to suggest some solutions, and to pinpoint some of the obstacles in the way of their accomplishment. I shall present nothing really new, but sometimes rearrangement of recognized factors in new ways illuminates the problem, and suggests hitherto unenvisioned avenues of solution.

The point I will strive to make is this: the communication process among scientists could be advanced 25 years this very day if we could take advantage of the things we already know. We are hampered in progress not by lack of know-how but rather by an unwillingness on the part of the users of journals—both authors and readers—to permit established concepts to be changed.

Now I make haste to point out that I am not talking about information retrieval. There is a technology for this field that is being worked out and each new day brings new advances. I am talking instead about the primary literature—that vital area of communications which has been given the least study and attention. The irony of the situation (and this is a partial aside) is the fact that the retrieval problem never will be solved until the input problem—the problem of the primary literature—is solved.

The purpose of communication, at least for the scientist, is to permit one person to get into another's mind quickly and easily. It is the sobering fact that anything we interpose between the two minds, even the written or

spoken word, acts as a distorting impediment to communication. Yet these imperfect instruments could be improved greatly in efficiency if it were not for certain psychological attitudes and habits which users insist upon, some of which we intend to examine here.

What the Author Can Do.—In the first place, the scientist simply will not take the time, or has not learned how, to write in a simple expository style. We believe it is not that he can't; we think he simply will not be bothered. There have been enough out-and-out scientists who wrote superbly well to give the lie to the allegation that bad writing is an inherent characteristic of the scientist. Think of Faraday, Tyndall, Osler, Bragg, Huxley, Holmes, Albert Noyes, Sr. These men were scientists, but they had enough pride in their writing to express themselves lucidly and with style.

The scientist, we believe, has taken refuge in the easy out that his work is in the laboratory, and that the actual writing of papers is an onerous chore. This attitude just won't scan. Research results aren't worth a hoot if they are not communicated to others.

The writing of a good research report is admittedly a difficult task. It is not much more difficult for the average scientist than it is for the professional writer. Writing well is the hard task. It requires painstaking work. But it is worth the effort since it saves the time of every other person along the line who has contact with the paper and it will assure maximum readership.

The editor will assay the worth of a well-written report more quickly and without exchange of delaying correspondence. Referees will be influenced positively. Technical editing time will be reduced. Composition will move faster. And perhaps more importantly of all the reader will get your ideas faster and more accurately. What is more, he will soon come to remember favorably the author of easily read papers.

In this same connection the author, especially the established scientist, must relinquish some of his *prima donna* attitudes about his copy. Editors of the ACS basic journals are literate individuals; they are not inclined to change copy just for the sake of change. Editorial changes are made to clarify, to shorten, or to intensify meaning. This is where the average scientist differs from the professional writer. The “pro writer” is not so quick to bridle at changes in his copy. He doesn't like them, but he understands the need for them. The consequence of the quibbling which ensues is delay—needless delay—in publication.

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Much worse than resistance to editorial change, however, is the insistence of too many authors on rewriting their papers when these reach them in galley form. Now this is inexcusable. It means that the author originally prepared his paper in sloppy form. And now he wants to recover. This delays publication, and the cumulative effect is the expenditure of an astonishing number of dollars. It delays publication not only of the paper in question but also of every other author's paper which must await resetting in type of the revised paper.

There is an easy solution to the problem. It is one which at least one of our editors is considering. That is *not* to send galleys to authors any more or, short of this, to ignore authors' changes except where these are changes of fact.

Some authors are accountable for another kind of communications problem arising from the practice of fragmenting one unified research report into several parts for submission as independent articles. This practice is sometimes justifiable, but not always. And how about the author who produces several variations on the original theme, with the only major change being in the title, and then submits these to several different journals? And what say you to the author who rushes a preliminary communication into print and never follows through with the complete results?

Each of these deplorable habits adds to the proliferation of the literature, increases the cost of literature production, takes pages which might be devoted to more important information and slows the appearance of other articles.

The author, of course, is not solely at fault. Reif in his splendid article in *Science*, "The Competitive World of the Pure Scientist," has documented the vicious pressures placed upon the scientist by university as well as industrial research administrators to publish or perish.

If chemists can be made to feel that this is an untenable state of affairs, the Society can exert legitimate pressure to correct it. However, the chemist so often falls into the trap set by his dean or research director, and comes himself to believe that his scientific worth is measured by quantity of publication, by number of papers, number of pages—or even weight!

It should be a sobering reflection to such victims of false scientific standards that Albert Einstein, prior to announcement of the Special Theory of Relativity in 1905, had published but a handful of papers. I believe I am correct in saying that even at the time of his death Einstein had authored far fewer papers than the average assistant professor. It is obvious that Nobel Prizes are awarded not for number of publications but for quality of research. On the other hand, I know of one chemist who could boast that his name is on many hundreds of publications, and yet this gentleman, while eminent and respected, has probably never been considered by the Nobel Committee.

And while we are criticizing the author—and criticize him we must—we pause to inquire why he has a horror of the condensation of his article—a step intended to save money, speed publication, increase readability—and why he balks at deposition of data material in a repository. Let us face it, extended data is not best-selling material, and some is of interest to fewer than 50 readers. Why publish 80,000 page impressions to satisfy the ego of a

man working in some esoteric field, however important to the half dozen scientists who share his interest?

What the Meeting Chairman Can Do.—Now that we have set out a list of complaints against authors, we wish to unloose an arrow or two at the reader, the editor, and others associated with the publications program at any distance, however great.

Let us pause for a moment to reflect upon the scientific meeting—that breeder of literary files. Twice each year, soon to be thrice, we hold a national scientific meeting. In each division of the Society some unhappy soul is chosen as a symposium chairman. It is his lot to put together a program of related papers, and put it together he does, whether or not there is anything new to report. "Harry," he says to a colleague over lunch, "I've got to have a paper from you for the September meeting. Put something together on steroids. They're hot stuff right now . . . attracting a lot of interest." Poor Harry who is two lunches and dozen martinis in debt to his companion has little choice. He throws something together on steroids. It is the same something he threw together for last year's meeting, but a friend is a friend. He doesn't throw it together too well, however. It is January now, and the meeting is in September and, goodness knows, he might luck into something new by then. So he writes a vague abstract which includes his fondest hope. Then he sits back to await developments which never develop. At the last moment he hies himself to Washington with some sketchy notes and several used slides.

Meanwhile, the Editor of *Medicinal Chemistry*, who has spotted the intriguing title in the Abstracts of Papers, puts his double X on the article for publication. Woe betide him! When he hears or sees the report, it turns out to be close to a first class turkey. Yet the author, who by this time has expended sufficient energy that he wants to be rewarded, argues that the paper should be published when the editor politely rejects it. Or, as more commonly happens, he stalls for time to write up what he so glibly gave from notes. By the time the editor finally bludgeons the manuscript away from the author it hasn't been worth the trouble. What is the moral? The ACS journals could cut receipts virtually in half if it were required that each paper given at a national meeting be submitted to the symposium chairman before the meeting in a form ready for publication.

What the Reader Can Do.—Now what about the reader? He insists upon a rigidity in presentation of published material that would put the ordinary perfectionist to shame. Articles for him must be prepared according to a traditional, stylized format and method of presentation, and God help the publisher who strays away from it. For example, among other things, he insists upon an abstract plus a summary although both contain virtually identical material.

Will inexpensive IBM type with unjustified margins printed on less than parchment paper do? It will not. Such a fast, inexpensive, practical, efficient method of printing is not sufficiently conventional; it lacks class and snob appeal. Mind you, the meaning of the paper is not less clear. Remember that cold type usually cuts costs virtually in half. Recall that the process is faster. Still in all, it just won't do.

More disturbing than what readers insist upon is what readers do not insist upon. For example, they tolerate

uninformative titles. They do not insist that the plethora of accolades, recognition of grants insisted upon by government agencies, thank-you notes, and other non-essentials be minimized, with resultant saving of space, readers' time—and money.

Readers do not encourage the scientific publisher to experiment with and introduce tentatively unconventional variations of format, contents, distribution, periodicity of issuance, and so on. They do not urge standardization of format to the point where all essential information is included and all non-essential information ignored.

What do I mean? Well, every author should concisely answer four questions right at the outset of his article. These four questions are required by a leading university to be satisfactorily answered on the first page of every doctorate dissertation in chemistry. In particular, if the author cannot contrive a convincing answer to the fourth question, he has no publishable paper. These questions are:

1. What was attempted?
2. What were the methods?
3. To what extent were the results successful?
4. What contributions actually new to the science of chemistry were made?

What else should the reader insist upon? He should demand a journal that lists every meeting in its field as soon as it is scheduled, along with the titles of papers and the names and addresses of authors. Such a comprehensive service could give researchers a six to eight months' lead in "current awareness" over the time it now takes to produce a finished article in a journal.

The reader should also insist upon an increase in the number of review articles published, for only by reader demand will this change come about. Review articles are the neglected partial salvation to the researcher's time problem.

The reader should insist upon an ACS journal which provides for the presentation of hypotheses—a forum where one may speculate upon what should happen, or could happen, or is likely to happen. Now we allow publication only of demonstrable data.

Finally, the receptor of information should begin to think about, and press for, some exposure to as yet highly unconventional forms of communication. If the main purpose of the exchange of information is to communicate ideas (not to get a promotion or a raise) then the scientist user of information should insist upon the use of T.V., radio, tapes, and any other type of idea transmission which may prove to be efficient. As a mental exercise, we are toying with the idea of a science satellite. With such a satellite broadcasting day and night a lot of papers could be presented in a very current fashion.

More Knowledge of the Scientist Needed.—Now what is the purpose of all this carping? Simply that as my staff and I attempt research into the communications problems of the scientist, we more and more keenly feel the lack of a psychologist's answer to the question: "What makes the scientist tick?"

It does us little good to determine the future growth of the literature, the emergence of new specialties, new ways in which to produce information better and faster, if we cannot have this knowledge to use. We are not

lacking in know-how on ways to produce a more readable journal, a more literate journal, a more efficient journal, a more inexpensive journal, a more specialized journal, a journal issued more frequently. All of us in the communications business know some of the answers to these matters.

What we do not yet know—and what we now are beginning to investigate more closely—are the social and psychological dynamics which prevent us from using the very great amount of knowledge we already have.

The Problem Restated.—I have directed certain criticisms against authors, editors and readers of scientific journals. Some of these criticisms apply equally to all, and in addition involve peripheral actors in the play, such as those responsible for programming scientific meeting. To summarize:

1. Authors should learn how to write acceptable English exposition. Science students should be given courses in technical writing. Those who have not had this advantage could profit greatly by reading, for example, "The Elements of Style," by William Strunk, Jr., and E. B. White.

2. Authors should be more self-critical about the scientific value of work on which they propose to write papers. "Bibliography-building," without sufficient regard to quality of research, and as a device for professional advancement, is unethical. Members should support the efforts of the Society to eliminate the excuse for such a practice.

3. Writers should cooperate with editors in such matters as writing to prescribed editorial format, gracefully accepting editorial changes and condensations, and exercising restraint in making galley proof changes.

4. Symposium chairmen should insist on having fully prepared papers ahead of meeting time.

5. Readers should not be hidebound by tradition, but should, like all concerned, accept and encourage experiments in innovations in the philosophy and mechanics of communication.

6. Readers should insist upon meaningful titles to papers, the elimination of extraneous material, and the publication of more review articles.

Certainly we will continue to seek new forms of media, ways to compress information, ways to speed transmission of information, ways to harness the input of original publication. But as we learn new things, let us hope that we shall not have to set these neatly upon a shelf to await the day we have learned how to induce the author and the reader to permit us to use them.

We do have a definite plan in mind to test the reaction of authors and readers to many of the ideas presented here. We hope to publish for a restricted time a very limited edition of an ACS journal. This edition will incorporate all the ideas discussed here over which we can exert control—inexpensive format, rewritten titles, our own design for the order of presentation of material, highly edited and condensed copy and the like. Then we intend to distribute copies of this experimental edition to selected subscribers who also will receive the regular version of the journal. This way we can test reaction to see if we can move these ideas from the discussion to the action stage.

We want to learn by doing and we will welcome your participation in this venture.