Preliminary Communications in Chemistry

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A study of the two journals entirely devoted to the publication of preliminary accounts of chemical research shows that these journals are not fulfilling their declared aim. Rather than publishing preliminary reports of research of outstanding specialist importance or great general interest, the majority of their contributions appear to be short definitive papers of average interest. Less than 30% of the papers are followed within 2 to 3 years by a full report.

Deeply entrenched in the norms of the scientific community is the notion that reports of original scientific research should not appear as a primary publication more than once. The reasons for this are twofold. Firstly, the quantity of published scientific information has been growing exponentially since the 17th Century, and it is becoming increasingly difficult for the individual researcher to master the literature even in a narrow area. This exponential trend cannot continue indefinitely and any effort to contain the flood of authorship and to ease the problems of bibliographic retrieval must include curbs on the multiple publication of identical research findings. Secondly, the practice of multiple publication throws the reward system of science into complete confusion. Science is a uniquely successful enterprise as a consequence of the adherence of the scientific community to a number of ethical principles, one of which is the principle of "communism,"[1] the dissemination of original research results to the scientific community. These "gifts" of information are carefully scrutinized by the community before being accepted, but once accepted and published in the primary literature, the information becomes enshrined as a publication and due recognition is accorded to the author. This is Hagstrom's information-recognition exchange model[2] of the reward system of science, a system in which the "recognition" is its own reward; eminence follows naturally upon a long and distinguished publication record.

But we are aware of the fact that scientists do try to increase the over-all numbers of their publications since appointments and promotions are often decided on criteria which include a crude paper count; in American and, to a lesser extent, British Universities, the maxim "publish or perish" has a very real meaning. Ambitious scientists, therefore, strive to maximize their publication numbers usually in a more subtle manner than outright multiple publications of the same material. There is no doubt that multiple publication has occurred in the past,[3] but it is one of the more serious offences against the scientific community—ranking almost with faking one's results—and is not extensively practiced. A given piece of research work may be split up and published as several articles where one would do, and publication of research findings as preprints or as conference proceedings in addition to primary publication are ways of increasing paper numbers.

There is one way, however, in which the scientist may present the same material in two primary journals; if he considers his work to be of outstanding specialist importance or of great general interest he may publish a preliminary report in a journal which carries a "Letters to the Editor" section or which is entirely devoted to the rapid publication of short important items. In this way, the results are rapidly disseminated to the scientific community, but they are usually accepted for publication on the understanding that a full report, incorporating the work described in the letter, will, eventually, be submitted for publication. Multiple publication of this sort is not only allowed, it is encouraged. *Nature*, in an editorial indictment of the malpractice of multiple publication suggested that for preliminary reports

> "there are good reasons for complaint if the fuller version of the discoverer's (preliminary) report does not eventually materialise"

The journals which specialize in the publication of short communications make constant efforts to stem the tide of authorship with which they are besieged and stern warnings are issued in editorials[4,5] and in the "instructions to authors" against the practice of using the journals primarily as instruments for multiple publication, for the establishment of priority or for reporting the results of "dead-end" research.

A STUDY OF THE LETTERS IN *CHEMICAL COMMUNICATIONS AND TETRAHEDRON LETTERS*

In the guide to the 1970 Science Citation Index (SCI), it is claimed that the Index may be used to find the follow-up papers of preliminary reports and to assess the importance of any paper to the scientific community. Using the SCI, we have carried out a study of the preliminary reports published by authors working in British laboratories in the 1968 volumes of *Chemical Communications* and *Tetrahedron Letters*, the two journals in chemistry which specialize in the rapid publication of short reports of outstanding importance. The sample consisted of all the reports published in *Tetrahedron Letters* (123) and 50% of those in *Chemical Communications* (265) (Table I).

Table I

Journal	Total Papers	British Papers	Sample
Chemical Communications	1393	531	265
Tetrahedron Letters	1547	123	123

The *Chemical Communications* sample was selected by listing in page order all of the British papers and choosing every alternate one. Short reports of crystallographic studies were eliminated from the sample since it is common practice not to follow up these reports.

METHOD

It seems safe to assume that a piece of research reported in a letter should be completed and published within 2 to 3 years, a timescale used in a similar study commissioned by *Physical Review Letters* in 1964.[6] Each letter in the sample was looked up in the SCI for 1968, 1969, and 1970, and any publication by any of the authors which cited the letter was noted as a possible follow-up paper unless the citing article was a letter, review, or technical note. The number of times the article was cited was recorded to give some measure of its short term impact. The possible follow up papers were then compared with the letters to determine whether or not they were genuine, fuller reports of the work. The only criterion used to decide this was that the information from the letter, or a corrected version of it, must be included in the full paper. If the full paper was an extension of the work in the communication but did not include it, then it was not considered to be a true follow up. In the study, it was assumed that an author will follow up his preliminary report within 2 to 3 years and that he will cite his preliminary report in it.

SHORT TERM IMPACT

The assumption that the number of citations a paper receives is a measure of its quality was first used in 1927 to evaluate scientific journals.[7] A careful discussion of the value of citation counts as a measure of quality can be found in Cole and Cole,[8] who found that the average yearly number of citations in the SCI to the life's work of the 1955–65 Nobel Laureates was 58. Of the other 250,000 scientists listed in the 1961 edition of the SCI, only 1.1% received 58 citations or more in a year, and the average number was 5, Garfield[9] has argued that the citation method measures the short term impact of a paper rather than its importance in the long term but this does not matter in this study; it is only to be expected that the impact of the preliminary communications should be limited in time but that the impact should be greater than that of an "ordinary" paper. Reports in the letters are supposed to contain information of immediate relevance to others working in the field which they will seize upon, use, and, hopefully, cite in their own papers.

Although an author may be frequently cited because his work is incorrect or for reasons which are unrelated to scientific merit, it is still reasonable to suppose that letters which have been influential will be cited frequently. The citation habits of authors have been pilloried as being haphazard and unrepresentative,[10,11] yet, although it cannot be guaranteed that all relevant works are cited by the authors who draw upon them for guidance and inspiration, at least those cited are not irrelevant since editors insist that all references are mentioned in the text. Table II lists the citation numbers and the average number of citations received by cited articles in our sample in 1969 and 1970.

Table II

	Chem.Comm.		*Tet. Letters*	
	1969	1970	1969	1970
No. of citations	713	520	255	182
No. of papers cited	222	208	96	81
Citations/cited preliminary paper	3.2	2.5	2.7	2.2

Martyn and Gilchrist,[12] in an evaluation of the major British scientific journals, found that the citations/cited paper for the 1963/64 letters journals and their parent publications were 2.5 *J. Chem. Soc.*, 3.17 *Proc. Chem. Soc.*,[13] 2.6 *Tetrahedron*, and 3.5 *Tet. Letters*. The average number of citations/cited item may be taken as a guide to the usage of the material in a journal which was found to be citation worthy. The slightly higher figures found by Martyn and Gilchrist for the two letters journals may be attributed to the fact that theirs was a complete census whereas our study concentrated on authors working in British laboratories. An alternative explanation is that the usefulness of the material published in the chemistry letters journals is diminishing. The fact that we did not exclude self-citations, only serves to reinforce this alternative view. It is clear that if the citation pattern of a paper reflects its usefulness, then the preliminary reports in chemistry show little of the urgency and general interest which their authors claim for them.

Table III

No. of Times Cited	% of All Existing Papers Cited in Any Year[14]	*Chem.Comm.* 1969 %	*Chem.Comm.* 1970 %	*Tet.Letters* 1969 %	*Tet.Letters* 1970 %
0	35	16	21	22	34
1	49	20	25	25	29
2	9	19	18	23	16
3	3	11	11	11	8
> 3	4	34	25	19	13

In Table III the citation frequencies of the letters in the two journals for 1969 and 1970 is compared with the frequency with which any existing paper can expect to be cited in any given year. It is seen that 96% of all existing papers can expect to be cited three times or less in any year. The corresponding percentages of the sampled papers receiving three citations or less in 1969 and 1970 are 66% and 75% for *Chemical Communications* and 81% and 87% for *Tetrahedron Letters*. Unfortunately, no data are available showing the frequency with which any paper can expect to be cited one and two years after publication but it is only to be expected that such data would only reinforce the view that the preliminary papers are less important than their urgent publication would suggest.

FOLLOW UP

In 1961, the American Institute of Physics commissioned a questionnaire study of the follow up habits of authors publishing in *Physical Review Letters*.[6] In this sur-

vey, 39% of the reports in the letters journal were followed up within 2 years, and 4% were printed in the proceedings of conferences. These results are shown in Table IV together with the results of our study.

Table IV

	Phys.Rev.Lett.	*Chem.Comm.*	*Tet.Lett.*
Sample size	377	265	123
No. followed up	147 (39%)	78 (29%)	27 (20%)
No. follow up papers published in parent journal	65 (44%)	59 (75%)	10 (36%)
No. of other journals in which follow up occurred	28	18	5

In our study, 49% of the letters in *Chemical Communications* and 53% of those in *Tetrahedron Letters* were not cited at all by any of the authors within 2 to 3 years.

It is interesting, in our study, to note the number of follow up papers which appeared in the parent journals. We also found that only 1.4% of the full reports of letters in *Chemical Communications* appeared in *Tetrahedron* whereas 44% of those in *Tetrahedron Letters* which were followed up appeared as full papers in the *Journal of the Chemical Society*. Acknowledging the fact that *Tetrahedron* publishes papers in organic chemistry only, the distribution still seems to be quite disproportionate.

There are many reasons why letters may not be followed by a full report. The author may be subsequently "scooped" by a rival; the work in the letters may be wrong; subsequent experiments may fail or the work may be put aside through lack of manpower or changed priorities. But the extraordinarily high percentage of letters which were not followed up seems quite unjustified. It would appear that the aims of the chemistry letters journals are not being fulfilled by the majority of the authors who take advantage of their services.

In our examination of the letters and possible full reports, a number of abuses became clear. In many cases, the letters journals were used as vehicles for the publication of short papers with no obvious follow up potential. Much of the work described in these papers tended to be outside the specialist field of the investigator; in an investigation of the conformation of a particular natural product, for example, some interesting by-products of a reaction, novel but unrelated to the problem at hand, would be published as a preliminary report. Physical data obtained by fashionable techniques would be separated out from the main investigation or interesting "effects" exhibited by the compounds when investigated by these techniques would warrant preliminary publications (long range coupling, solvent effects, Overhauser Effect in nuclear magnetic resonance, for example).

Some of the "preliminary" reports in *Tetrahedron Letters* were quite obviously not intended as such, judged by their excessive length and content. Acknowledgment of this fact was shown by some of the authors themselves who listed the papers as part of a continuing series. Acquisition of priority would seem to be the only motive for the publication of final papers in the letters journals. Conversely though, loss of priority seemed to be no barrier to preliminary publication. A number of letters were observed which *confirmed* observations reported by rivals in prior publications.

CONCLUSION

This study, using the Science Citation Index, of a sample of the authors of preliminary reports in chemistry indicates that the aims of the journals which publish them are not being fulfilled. Judgment of the value of these papers, based on citation data, reveals that the information which many of them contain is neither of outstanding specialist importance nor of general interest. Many of the papers are short definitive accounts of "dead-end" research with little or no follow-up potential.

There is no doubt that British editorial policy still regards the preliminary paper as a device for the rapid publication of important research results. It may be, however, that this type of publication will evolve in such a way that it will become the short version of a two-edition type of scientific journal. Many of the problems associated with the information explosion in chemistry could be solved by the adoption of such a system of publication—separately written short and long versions of papers, the latter version containing the full experimental data and details.

It has been suggested[15] that the long version might be produced on microfilm and deposited in selected libraries. The short version, containing the bare essentials of the research, would be rapidly published, and the need for the preliminary paper in its present form would disappear. Priority would be quickly established, short items would appear only once, and multiple publication would be quite unnecessary. Such an innovation would, undoubtedly, be strongly resisted not only because of the inconvenience which it would cause but also because of the loss of such a convenient method for paper production. But the answer to this is quite simple. Candidates for appointments and promotions should not be allowed to list preliminary papers in their *curriculum vitae*.

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