COMMUNICATION IN SCIENCE:

DOCUMENTATION AND AUTOMATION

A Ciba Foundation Volume

Edited by
ANTHONY DE REUCK
and
JULIE KNIGHT

J. & A. CHURCHILL LTD.
104 GLOUCESTER PLACE
LONDON W.1 1967

COMMUNICATION IN SCIENCE:
DOCUMENTATION AND AUTOMATION

COMMUNICATION IN SCIENCE:

DOCUMENTATION AND AUTOMATION

A Ciba Foundation Volume

Edited by
ANTHONY DE REUCK
and
JULIE KNIGHT

J. & A. CHURCHILL LTD.
104 GLOUCESTER PLACE
LONDON W.1 1967

First published 1967
Containing 17 illustrations
Standard Book Number 7000 1313 X

Contents

CONTENTS

Membership

Symposium on Communication in Science: Documentation and Automation, held 22nd–24th November, 1966

Sir Ewart Jones (Chairman)	Dyson Perrins Laboratory, Oxford
B. W. Adkinson	Office of Science Information Service, National Science Foundation, Washington, D.C.
R. Brée	Center for Information and Documentation, European Atomic Energy Community, Brussels
C. W. Cleverdon	College of Aeronautics Library, Cranfield, Bedford
H. Coblans	Aslib, London
M. Cremer	Institut für Dokumentationswesen, Frankfurt
M. M. Cummings	National Library of Medicine, Department of Health, Education and Welfare, Bethesda, Maryland
W. D. Garvey	Johns Hopkins University Center for Research in Scientific Communication, Baltimore, Maryland
H. T. Hookway	Office for Scientific and Technical Information, London
P. J. Judge	Section for Scientific and Technical Information, Organisation for Economic Co-operation and Development, Paris
M. M. Kessler	The Libraries, Massachusetts Institute of Technology, Cambridge, Massachusetts
W. T. Knox	McGraw-Hill Inc., New York
H. Menzel	Department of Sociology, Graduate School of Arts and Science, New York University
A. Merta	Centre for Inventions and Scientific Information, Czechoslovak Academy of Sciences, Prague
J. G. Miller	Mental Health Research Institute, University of Michigan, Ann Arbor, Michigan
J. R. U. Page	European Space Research Organisation, Paris
D. J. de S. Price	Department of History of Science and Medicine, Yale University, New Haven, Connecticut
B. Rexed	Science Advisory Council, Statsradsberedningen, Stockholm
J. R. Smith	Institution of Electrical Engineers, London
F. A. Sviridov	International Federation for Documentation, The Hague, and Academy of Sciences of the U.S.S.R., Moscow
F. A. Tate	Chemical Abstracts Service, Ohio State University, Columbus, Ohio
H. W. Thompson	St. John's College, Oxford
Lord Todd	University Chemical Laboratory, Cambridge
J. Wyart	Laboratoire de Minérologie-Cristallographie, Faculté des Sciences, Paris

The Ciba Foundation

The Ciba Foundation was opened in 1949 to promote international co-operation in medical and chemical research among scientists from all parts of the world. Its house at 41 Portland Place, London, has become a meeting place well known to workers in many fields of science. Every year the Foundation organizes from six to ten three-day symposia and three or four one-day study groups, all of which are published in book form. Many other informal meetings also take place in the house, organized either by the Foundation or by other scientific groups needing a place to meet. In addition, bedrooms are available for visiting scientists, whether or not they are attending a meeting in the building.

The Ciba Foundation owes its existence to the generosity of CIBA Ltd, Basle, who, realizing the disruption of scientific communication caused by the war and by problems of distance, decided to set up a philanthropic institution whose aim would be to overcome such barriers. London was chosen as its site for reasons dictated by the special advantages of English charitable trust law, as well as those of language and geography.

The Foundation's many activities are controlled by a small group of distinguished trustees. Within the general framework of biological science, interpreted in its broadest sense, these activities are well summed up by the Ciba Foundation's motto, *Consocient Gentes* —let the nations come together.

Preface

THE suggestion for an international conference on the development of automation techniques in the handling of scientific information arose at a dinner arranged by the Ciba Foundation for the Scientific Publications Council in December 1964. The Council asked the Deputy Director of the Foundation, who was also Honorary Secretary of the Scientific Publications Council, whether the Ciba Foundation might devote one of its small international symposia to this topic. Another member of the Council, Dr. Herbert Coblans, was invited to draft a programme and membership list. In the following months of preparation, further advice was forthcoming from almost every member of the symposium, but the Foundation is particularly grateful both to Dr. Coblans and to Dr. Martin Cummings, Director of the National Library of Medicine in Bethesda, for the assistance given to the Deputy Director in planning this symposium.

It is also a pleasure to acknowledge the debt of the Ciba Foundation to Sir Ewart Jones for his sympathetic chairmanship of the meeting and his congenial guidance of these discussions.

The needs for a national information policy and for government support for the services storing, retrieving, and supplying scientific and technical information are now widely recognized. The possibilities in machine-aided information retrieval systems are unlikely to be realized without large expenditure, and call for efforts on a national or supranational scale. The international character of science could well enable great economies of scale and effort to be effected by an international division of labour between disciplines on the one hand, and by collaboration between national information centres on the other hand. It is hoped that this review of the ideas and experiences of those concerned with policy and with large-scale information systems may give not only an insight into current practice but also some promising pointers for future action.

CHAIRMAN'S INTRODUCTION

PROFESSOR SIR EWART JONES

As an amateur in this subject, although very much concerned at the consumer's end, my introductory remarks to this symposium must be brief; but I will attempt something of a general nature.

What we understand by communication in science has become vastly complicated today and in considering it many factors must be taken into account. Clearly, the kind of material to be communicated is of prime importance and this, as we know, has increased—not merely in depth but in breadth—with the growth of science. Then there is the mode of communication—the mechanisms and the whole variety of media employed. Scientific communication was originally by word of mouth and by letter and as it happens, these modes of communication are still extremely important today, accelerated by telephone and air-mail. The amount of oral communication that now takes place in private meetings, in small private gatherings and in vast international conferences is very considerable. But the main mechanism for some three hundred years has been the printed word. In its most urgent form we now have preliminary communications of various types, for which there is a growing demand. In the Chemical Society we decided to begin such communications in 1957 and in a few years they have grown to some seven hundred a year. Dr. Cahn, in his *Survey of Chemical Publications* (1965), found that about 10 per cent of the publications in chemistry today are in this form of preliminary, urgent communication. The situation in other subjects is not dissimilar and of course we have to take account of the still less formal preprint system that has grown up so rapidly in some other areas. These are problems of which we have to be aware.

Full publication takes place, as we know, in formal papers in a range of journals, and has not changed much over a hundred years. The process of setting up in type has perhaps improved to some extent and we now have off-set printing from the original manuscripts which is advantageous from the point of view of authenticity but very wasteful in terms of paper and library space. Finally there are the secondary publications: abstract journals, title journals, annual reports, reviews and the "*Advances in...*"

and "*Progress in*. . ." types of publication. It is with this proliferating mass of literature, primary and secondary, that we are concerned.

I think we must assume that the present rate of growth will be maintained for some time yet, and may even increase, although probably none of us believes that it will be sustained indefinitely. We have to explore and find ways of improving our ability to cope with the present flood and with an even greater volume in the years ahead.

The participants in this process of publication must be considered: the producer and the user and the middle-man—the documentalist. The producer—and here I can speak from personal experience—is mainly concerned with communicating with his own kind. He is not much interested in other people's concern with his publication, and the user is all too often unaware of what is available to help him in the way of literature aids of one kind or another. The education of both parties is something that we must all have very much in mind.

We face these problems at a time when a vast number of technological aids are available and are continuing to be developed. Our task is to discover how best to take advantage of them. However, and this is a point that we must emphasize, we shall have to be prepared to depart appreciably from our traditional attitudes, as both producers and users. It may be that the situation in the last two or three years has been such that one can already discern changes in attitudes, and the time may be ripe for more rapid progress.

During this symposium we shall learn of hopes and aspirations, of progress made and of limitations and difficulties being encountered and overcome in exploring the usefulness of automatic information-transfer systems. Underlying it all, one is aware of the practical and economic difficulties in the communications process affecting learned societies, governments and industries. As science grows, we surely must be prepared to see increasing resources devoted to communications of all kinds if our knowledge is to be fully effective. It is generally said that much scientific effort is being wasted through duplication and ignorance of what has been done. Should we spend rather less time and money on creating and more on disseminating and digesting? And if so, how can more scientists and technologists be persuaded to engage in such activities on a full professional, rather than on an amateur basis? This is a problem that we must all face.

The Ciba Foundation has been most assiduous in searching out a unique collection of talented individuals greatly experienced in this field; their contributions to this symposium, which together will embrace most

facets of the subject, and the discussions they engender, will focus attention on this vexing problem and—hopefully—stimulate fresh and more vigorous action on all fronts.

REFERENCE

Cahn, R. S. (1965). *Survey of Chemical Publications*. London: The Chemical Society.

INTRODUCTION: THE PROBLEM STATED

LORD TODD

The University Chemical Laboratory, Cambridge

WHEN I was asked to take part in this symposium I was rather disturbed, for I am a mere tyro in the field of documentation and information exchange in science. True, I was Chairman of the Commission on Documentation set up by the International Union of Pure and Applied Chemistry immediately after the war, but its primary purpose was the re-establishment of the great German chemical encyclopaediae *Beilstein* and *Gmelin*, so that it taught me little of the detail of the subject—although it did indicate to me its complexity. Beyond that I have had a little experience as editor of a scientific journal and considerable experience as a reader, but that is about all. As Chairman of the Advisory Council on Scientific Policy I came in contact with the problem of setting up the National Lending Library for Science and Technology and of developing a policy for information services in this country. But I suspect that it was my rash action in devoting part of my presidential address to the International Congress of Pure and Applied Chemistry in London in 1963 to the urgent need for work on information exchange in chemistry that is responsible for my being here, faced with the need to state the problem we are to discuss to a group of people who know vastly more about it than I do. I hope, however, that you will bear with me if I try to set out a few reflections on the problems which appear to me as a consumer to be important.

I suppose the origin of our concern with the subject is the growth of the scientific literature. This is hardly a new phenomenon; I remember much head-shaking about it in my own subject of organic chemistry 30 years ago, but it has admittedly become much more acute since the last war through the fantastic rise in the number of people pursuing research of one kind or another and the consequent spate of publications in an ever-increasing number of journals. Of course, the present rate of increase cannot continue indefinitely, but it may go a bit further yet before the curve flattens and something must therefore be done about it. Thinking of this sym-

posium the other day, I had a look at the array of current chemical journals received by me personally. There were 24 journals given over to publication of scientific papers each appearing at least once a month (and a few of them even more frequently), in all of which papers relevant to my interests as an organic chemist appear regularly, as well as the usual group of assorted general scientific periodicals of more casual interest. Of course, I cannot read them all—and still less can I cover the others which are taken by our library. (To tell the truth, I cannot really keep pace even with the two abstract journals which cover my interests.) At a rough estimate if I spent all my waking hours reading (including week-ends) it would take me about a year and a half to read one year's journals in my own subject. We are all familiar with this fact even if at times we are reluctant to admit that we do not keep pace with anything more than a small fraction of the literature in our subject.

You will notice that I am beginning to get down to a discussion of the problems as seen in my own particular field of science. This is, I think, a reasonable way to begin, for I am basically an organic chemist and organic chemistry is a pretty highly developed science from the standpoint of documentation. Moreover, my research interests lie partly in the borderlands between organic chemistry, biology and medicine, so that I have information needs which cut across accepted scientific boundaries. Certainly I am only too conscious of the problems set by the bulk of current literature to which I have referred.

This problem—that of keeping abreast of advancing knowledge in one's subject—is only one of three basic problems we have to face, the others being the retrieval of information already existing, and the conveyance of relevant information from one science to the practitioners of another. But I will deal with it first. The main vehicles for the transmission of new information are the scientific journals, the abstract journals and patents, and verbal communication either by private contact or by symposia and conferences. Of these, journals are perhaps the most important, for information passed by word of mouth is usually fragmentary and abstracts are necessarily abbreviated, variable in quality and as a rule late in appearance.

The journal situation has, of course, been getting more and more complex and unsatisfactory. The tendency to assess ability by the weight of printed paper issued by a scientist has encouraged a vast and not always necessary amount of publication. Attempts have, of course, been made to keep it down by the referee system, but this has had only limited success since referees (who are usually enthusiastic followers of the kind of work

written up in the papers sent to them) tend to lean over backwards to avoid rejecting papers out of hand. As a result we have seen the slow decline of the general journal within a discipline—even the *Journal of the Chemical Society* has now succumbed and is appearing in several sections—and the appearance of a great mass of specialist journals covering limited areas. Beyond that there has been a rapid growth in the "Communications" type of journal, in which short papers are published without much in the way of experimental detail, and the experiment of the Information Exchange Groups in which preprints of work done within a given field are circulated among active workers. And the results are not yet satisfactory.

I suppose we should ask ourselves what is really important in a journal—what information do we really want from it? Here I find a considerable difficulty. Thirty-five years ago when I was a young and active research worker in organic chemistry there were in fact only about four or five journals which consistently published important material; odd important papers appeared elsewhere, it is true, but they could be picked up in the abstracts and if one browsed in the four or five main journals as they appeared one could keep pretty well *au fait* with what was going on—and it was possible to do so. But I got a lot more than a mere awareness of what was going on from this browsing. I repeatedly came across things in papers bearing no apparent relation to my own research activity which were either applicable in my field or sparked off new trains of thought—and this to my mind is the crux of the problem of journals. How am I to get this invaluable stimulus when faced with the mass of printed paper that arrives on my desk today? One might say that I could get this from the list of titles of papers, choosing to look only at those that might be relevant. But this is not possible if the information is in papers dealing with research areas with which I am not directly concerned. Even where it is in such areas, the titles given to papers by scientists are frequently bad and sometimes even misleading. The same, I regret to say, applies to the summaries which are attached to most papers today.

I confess that I cannot myself see how this problem is to be overcome by any kind of automation. It may be, of course, that I lack imagination, but I should be extremely interested to hear of any way in which automation could help. Speaking of organic chemistry I would say that for the purposes I have mentioned the experimental detail could be omitted from the published papers provided it could be made readily available on request from a central store. It is the description and discussion of the work that matters. This, written in a condensed form but including all really *new* features, would be what one would publish in the journal; in cases where lengthy

argument was a necessary feature from the author's standpoint the full-length discussion could be filed at the central repository to be on call, like the experimental detail. Papers in published journals would then be rather like the present "Communications" type of paper and the volume of publication could be reduced to something one could cope with reasonably. Papers dealing with routine matter—for example, those in which a range of compounds is made but no novel methods are employed—could be recorded in journals by a properly designed title and a line or two of print. Some system of this type will have to be adopted, for I hold that the scientific journal must continue as a major information source.

What I have said about a possible journal format brings me on to the subject of information storage and retrieval, since without a proper system to deal with that, the suggestions I have made would be worthless. There is no answer here but a computerized system under which retrieval of information of any desired type is made simple and automatic. This I am sure can be done. Of course, the labour involved in setting up such a system even for chemistry alone is enormous, but developments in this direction are well under way in several countries—notably under the auspices of *Chemical Abstracts*. Given the necessary expenditure of time and money I have no doubt that this will be possible. There are, however, a number of points about this aspect of our problem to which I would like to draw attention.

Documentation has long been a subject of great interest to organic chemists partly because the science is highly systematic and partly because its industrial importance has helped to stimulate the provision of ready access to existing knowledge. We have in *Beilsteins Handbuch der Organischen Chemie* a remarkable and perhaps unique example of comprehensive documentation in science. *Beilstein's* aim is to record every known organic compound with its method of preparation, properties, derivatives and reactions, with literature references; moreover in its preparation *all* the relevant literature is critically reviewed so that the reliability of the information is indicated and, for example, the best method of preparation described. The preparation of *Beilstein* involves enormous labour but its success has been such that up to the coverage date marked on the latest volume you can be pretty sure that everything known about a compound is there or can be traced through the references given. The trouble is that the growth of the literature is defeating *Beilstein*—the Supplement presently being issued and only partly done covers only the literature to 1949 and the whole work already runs to 95 volumes, so that the labour of finding things in it is slowly mounting. *Beilstein*, of course, employs a specially

designed system for indexing its contents and a standard form of nomenclature. There are other compendia of less importance (although they are still valuable) but they tend to use different systems and even in journals the systems of nomenclature are by no means uniform. It seems to me that in the future the *Beilstein* type of information source will be replaced by the computer store, but the mess that chemistry is in through the use of a variety of indexing and nomenclature systems sounds a warning to us. I feel it is desperately important that all organizations working in this field should get together and ensure that one common system is employed so that all chemical information is readily interchangeable and accessible in all countries. This is no place for national systems and if we do not act in unison from the start we shall be in dire trouble quite soon.

By the same token it is now more necessary than ever to see that not only in the different branches of chemistry, but in all the sciences, we should use the same symbols and names for the same things and should see to it that we employ words with the same meaning. If we do not, cross-communication between the sciences may become more difficult than ever.

Given the development of a central repository of information and the type of journal I have suggested, abstract journals as we know them would no longer be necessary. Their place could be taken by lists of titles of papers (some efforts along such lines have already been made). But there is one important proviso. Somehow or other authors will have to be taught how to give titles to their papers which really mean something and are relevant to the contents. Otherwise it will be very difficult to know what information one wants to retrieve, in some cases at least. Here again the need for worldwide co-ordination of procedures is evident.

The problems to be faced in establishing a satisfactory information retrieval system are vast and no doubt we shall hear a good deal about them during this symposium. But I would stress again that this is not a matter to be dealt with nationally; if ever a project called for international action it is surely this one.

In addition to what I have mentioned, it will, I believe, be necessary to develop review journals intensively if we are to have a really effective information system. It is necessary to have a means of getting a general idea of the overall situation in those areas of one's own science in which one does not actively work or consistently read, as well as in neighbouring relevant sciences. Here the review journal is indispensable. But the review must be both accurate and readable—qualities not always found in existing reviews. We need to take far more trouble over the production of high-quality reviews. And reviews covering the same kind of subject from differ-

ent angles are necessary, since one of their major functions will be to ensure cross-fertilization between various branches of a science or indeed between various sciences. I have myself always depended heavily on review journals for knowledge in biology and medicine and in many cases have had to extract the information I wanted with some difficulty, whereas the same material reviewed with a different slant would have been much easier to cope with. Admittedly it is difficult to get information conveyed between one science and another, but this is usually because the practitioners of different sciences don't quite know how to express what they are looking for in comprehensible terms or in a way that will register with each other. Yet such interchange is vital to development in the borderlands between established disciplines and it is in such borderlands that the growing points of science are frequently found. I believe that only the properly written review journal, coupled with personal contacts, can solve this problem.

My general picture of the future in chemistry at least is one in which all information coming from research will go into a computer store with that already known so that it can be accessible on demand. Coupled with this we shall have (*a*) journals containing the condensed contents of each paper—perhaps like the current "Communications" journals but with the very important (and indeed vital) difference that nothing will be published unless on submission it is accompanied by the fully documented experimental evidence ready to go into the information store, (*b*) a comprehensive list of titles which will be regularly circulated, and (*c*) a highly developed review system to provide cross-fertilization. I do not claim that there is anything novel in this. Indeed some people might say that there is too much of the printed word in it for the brave new world of computers. But I do not believe this is so. We must surely make the maximum use of computers and associated automation, but if we carry it to the point where the scientist no longer browses in the literature without first of all formulating questions then I believe we shall do harm to science. Inspiration and genius are a necessity in science and they are matters of the human brain; they are in my view too wayward and unpredictable to be mechanized.

DISCUSSION

Tate: I would endorse Lord Todd's comments, although I feel that in chemistry many of the types of help for which he asks are well on the way to coming into existence. Those of us working with abstract and indexing publications realize that we need to provide a substantial increase in flexibility and access to the information, but this is coming. We are solving

the problems caused by chemical nomenclature in the past and are now able to deal in terms which are the common parlance of the chemist, namely a straightforward structural representation. There is considerable doubt about whether the handling of material in abstracts and indexes should always take the same form in the years ahead, but there will certainly be some printed form by which browsing can be accommodated, and more quickly than at present.

Jones: Is there any move in the United States in the chemical literature towards the kind of preliminary communication referred to by Lord Todd, which also gives one the opportunity to browse? This will obviously come from your *Chemical Titles* and other activities but so far, apart from Communications to the Editor in one or two journals, I have not seen much evidence of interest in this form of communication.

Tate: There has been considerable discussion on this method of handling information. A couple of experiments are in progress in the American Chemical Society; one is a service consisting of brief preliminary announcements backed by greater detail in a store which can be called upon on request. However, we find little immediate willingness on the part of the chemical public to accept this form of abridgement of their reports.

Lord Todd mentioned the value of good review articles. The Society is starting a new review journal next year which will include commissioned reports. They will be relatively brief and on timely topics and will be circulated to the entire membership of the Society immediately. This will, I think, lead to the abridgement of papers that Lord Todd advocated, although there is the question of whether this form of restriction will be accepted by the chemical public.

Todd: I am a great admirer of all you have been doing at the Chemical Abstracts Service. As to acceptance of this innovation of brief announcements by the chemical public, surely over the years chemists have been going in more and more for these abbreviated types of communication; why should they not accept them as the general means of publication? After all, the coming into existence of brief communications *without* experimental data, to which I do object strongly, depended simply on the fact that the time-lag in publishing scientific papers was becoming so great that people resorted to this kind of communication. Why should the chemical public object to a system in which, by using this method of publication only when they are ready to publish, they can lodge all their experimental data and at the same time have very rapid publication of small condensed papers? If the chemical public does not like it now, it will have to like it pretty soon; I see no other way out of the problem.

Tate: I agree with you in principle, but chemists are not the most adaptable of people in their handling of their literature. Our experience is that ideas of this kind come about only with great birth pains; therefore, the Society is not yet willing to impose this scheme on its membership.

Jones: Chemists are not exceptional in this lack of adaptability; it is an attribute of scientists in general.

Adkinson: Lack of good reviews of the literature is a cry that I hear from many different scientific disciplines. Looking at it from the information side, I wonder if the scientist may regard the preparation of reviews as something that fails to give him the kudos that he obtains from other kinds of work—whether the lack of good reviews is not due to the fact that scientists themselves do not place the writing of reviews at a sufficiently high level of importance?

Todd: I would say that this is a basic problem. In chemistry some review journals have kept up a high standard but there has been a great proliferation of review journals in recent years, and, as you imply, when a request for a review arrives one tends to look for the least busy person in the department to write it. Thus reviews often come to be written by people who may not be really expert or even critical in the particular field. As you say, the production of reviews has not been given by the scientists themselves the sort of kudos that it should have.

Adkinson: Is this a problem for the people in information work or a problem for the scientific disciplines?

Price: May I put that question another way: do you write any of the reviews that you would like to read?

Todd: It is quite some time since I have written any reviews, and you might regard me as something of a back-slider! But among my colleagues are several who are perfectly competent to write good reviews.

Knox: The implication is that they do not do so?

Todd: They tend not to, although there is no reason on the grounds of time or ability why they should not write reviews. They feel that it is a lot of trouble, it is not well paid and nobody pays much attention to review-writing.

Miller: The usual view of the nature of science is an individualistic view of the creation of a theory and the creation of experimental evidence to support some point of view, and the rewards in science, as Dr. Adkinson suggests, are for this creative process of the individual rather than for the interactive process of the whole system of science as a fabric. The acceptance of the theoretician by the experimentalist and of the experimentalist

by the theoretician has become fundamental and central to science, but the acceptance of a third role, that of the reviewer or documentalist, as an essential role to deal with this vast communication problem, has not been comparable—in the same way that the paediatrician who examines a newborn baby does not get quite the credit that the parents do! Would it be possible to give the same forms of recognition—by prizes, by membership of the Royal Society and the National Academy of Sciences, and so on —that is already given to those who create the theories originally, to those types of minds that are most effective in the critical and integrative role, recognizing that we are now in a situation where the fabric of science must be a three-way co-operation? When one looks at the degree of preparation of the average writer of reviews in terms of previous training, salary level, title and appointment level, and of his degree of recognition amongst his colleagues, there is a clear difference between him and the theoretician and experimentalist. Unless this fact of life is altered you will not get the quality of reviews you are asking for.

Jones: The *Science, Government and Information Report* put this rather well when it compared this situation with the role of theoretical physics in physics today, and suggested that information and documentation should gradually assume this sort of role in all fields (*Science, Government and Information*. A Report of the President's Science Advisory Committee [1963]. Washington: United States Government Printing Office).

Todd: It is not inconceivable that the integrator of scientific work should get such recognition. After all, some people have achieved renown far more for their textbooks or monographs than for their original work.

Tate: However, in chemistry such authors have usually established their positions already through laboratory work, which in general does not allow much time for this kind of interpretive effort.

Knox: May I return to the question of whether this is the job of the scientific community itself or of the documentalists and information-systems experts. If the community of producer–users does not equip itself to do what it says it wants done, can others do this for them? I don't think so.

Todd: I agree that it will have to come from the scientific community.

Jones: There are indications that the scientific societies are becoming more aware of this responsibility. Dr. Tate referred to the new review journal being started by the American Chemical Society. We started *Quarterly Reviews* in the Chemical Society some years ago and are now expanding it. There is a gradual dawning of recognition; whether it is sufficient to take care of the problem is another matter. In chemistry it

depends on the scale of this new activity of the American Chemical Society, and the signs are very promising.

Price: There is a strong scientific tradition of relegating the packer-down of the research front into a corpus of knowledge, a sort of Instant Pabulum of knowledge, to a relatively inferior social and intellectual status: the Tyndalls of this world are despised of men! This tradition is still strong and there is remarkably little motivation to produce review articles. I would see very little possibility of social engineering producing the sort of status that would lead people to write review articles; even with the establishment of fairly prestigious review journals there has been remarkably little motivation for this.

Tate: In the American Chemical Society's project, a group within the society is commissioning individuals to write reviews with the endorsement of their organizations. Thus, these individuals will spend official working time on the reviews. But to cultivate recognition for this kind of effort is a relatively long-term project.

Kessler: This problem of lack of motivation and status in review-writing and related problems is a symptom of a social situation which goes back to the basis of our educational system. Students are brought up with little appreciation that the literature contains a summary statement of a science and is itself an important research tool. Until I came into this area of research it had not struck me that a science teacher assigns problems to students and indicates to them the tools available for the solution of these problems, and the students, depending on their maturity, solve them either by consulting textbooks or by experimenting in the laboratory, but the teacher never indicates to the student that a serious tool for solving problems exists in the literature. It is as if we had brought up a generation of students who have had no introduction to, say, laboratory methodology as a means of solving problems.

At the Massachusetts Institute of Technology we are experimenting with the idea of designing problems and exercises on various existing courses in which in order to solve a problem the student will have to go to the literature in the library, so that the technology of literature-engagement will be introduced to him as one of the tools available for the solution of problems. This area has been badly neglected. I understand that in the old days a gentleman-scientist would not go to the workshop and use a lathe; apparently a gentleman-scientist nowadays does not go to the library. But this is because we do not bring up our students to do this.

Thompson: This deficiency is not entirely universal. In chemistry now several books are available and widely used in which the problems are

actually taken from the literature. A problem is set out and references to other books and to the relevant literature are given, and nearly every problem requires the student to go to this literature.

As one who has taught chemistry for over 30 years at Oxford, I must also say that it has been my practice, and that of many of my colleagues, to advise undergraduates continuously about the important literature articles during their weekly work. I often found it useful to catch their initial interest by asking them to glance at some of the classical papers, Bohr's first short note on the spectrum of hydrogen, Langmuir's two famous articles on surface films, Einstein's little paper on the law of photochemical equivalence, and so on. Even though they could not fully grasp the subject, it showed them that great men published in simple ways, and perhaps encouraged them to read more of the literature. Part of the difficulty today has arisen because of the vast amount of literature, and students find it easier to read pocket versions and textbooks.

Kessler: The situation is certainly better in chemistry than in physics. However, even the books you mention in which problems are taken from the literature are still in the nature of assigned reading. The student is told to read a particular paper, but he is not trained in the techniques of finding such a paper. Nevertheless it is a step towards an intelligent use of the literature.

Todd: In organic chemistry when we want a student to synthesize a particular compound we tell him first to go to the library and to decide from the literature what he considers the best method to use. Then we discuss the process with him and if necessary show him where he has gone wrong in his search in the literature.

Kessler: This is certainly the kind of thing I have in mind.

Tate: The problem of accepting the task of digesting and organizing the literature as part of science is contributed to, in large part, by the academic fraternity. In chemistry, for example, there is considerable emphasis on literature work, yet when the Chemical Abstracts Service recruits students, as it does from some 45 campuses, we have great difficulty in getting university staffs to recommend us to their students. We want the good students, not the inferior ones, but it is the general practice of the professorial staffs to direct their better students to laboratory work only. There is no recognition in the formal educational work in graduate departments of chemistry of the significant amount of information work done in industry and the large amounts of money spent so that resources are better developed. In almost any chemistry department in the United States there is a complete lack of awareness of this form of endeavour.

Price: Lord Todd mentioned the Information Exchange Groups of the National Institutes of Health, which constitute an interesting experiment that has now died. Unfortunately, it was my experimental animal, but I am not altogether unhappy because an autopsy is rather more convenient than vivisection for my purposes (Price, D. J. de S., and Beaver, D. de B. [1966]. Collaboration in an invisible college. *American Psychologist*, **21**, 1011–1018). It is interesting that the experiment failed and that it was partly for financial reasons; although it cost only about 125 dollars to supply each member for one year with memos, the rapid growth of the groups had meant that the National Institutes of Health were facing the ultimate possibility of an annual bill of 10–100 million dollars—a considerable sum. It also happened that from April 1965, one group, IEG 5 (immunopathology), began to suffer a rather violent reaction against this social mechanism from its professional association, feeling that it was contrary to all that scientists hold dear. This produced an infection which spread first to IEG 7 and finally to IEG 1, which had lasted for five years and gave in with considerable death spasms (see [1966]. *Science*, **154**, 843).

This demise leaves uncertain the future of the whole Information Exchange Group system. We are at an important stage of social engineering at which if anything is to happen it had better start being organized now, because the vacuum left by the collapse of these schemes will produce much more serious perturbations than if the schemes had never existed. There is a rather serious new situation here in the whole business of "invisible colleges" and informal communication.

COMMUNICATION IN A SCIENCE: THE SYSTEM AND ITS MODIFICATION

WILLIAM D. GARVEY
The Johns Hopkins University, Baltimore

AND

BELVER C. GRIFFITH
American Psychological Association

IN 1961, when we (the authors) began a study of scientific communication in the field of psychology, the "scientific information crisis" was generally understood to refer to an increasing quantity of scientific literature, especially in the archival journals. Coping with the crisis was usually assumed to be a matter of improving the publication, distribution and retrieval of this literature. We had been laboratory research scientists before our association with the Project on Scientific Information Exchange in Psychology and our previous experience suggested that the literature was only a portion of a system that encompassed many forms of information exchange. Our change in role from that of scientists within to researchers upon the system led to an initial impression that scientific communication was a completely unordered complex. The elements comprising the system of scientific communication in psychology often appeared to be competing with one another rather than fulfilling separate, special functions with respect to the whole. A scientist's behaviour relating to the use of the various elements apparently stemmed more often from "folklore" than from the objectivity characterizing his approach to his research.

Such an initial impression of the problem resulted in a series of exploratory studies, very general in nature, made in order to gain a clearer impression of what actually occurred in the communication process and of a context within which to ascertain the relative roles of various media used to exchange scientific information. Even in this early work signs of regularity of communication behaviour and co-ordination of activities within the system were evident despite the vastness of the system, the rapidity of its growth, and the heterogeneity of scientists' interests. These