

tion, is compiling a roster of nearly 2000 persons who can serve as expert advisors on toxicology or related subjects. (Since the Society of Toxicology numbers only 385 members, our file obviously will include advisors whose interests are tangential, as well as those whose interests are central, to toxicology.)

With assistance from industry, universities, professional societies and government, we hope gradually to develop an inventory of information resources indexed in depth to provide referrals to references, documents, evaluated data, and information. Additionally, the National Referral Center for Science and Technology of the Library of Congress, under an agreement with NLM, will publish a directory of general toxicology information services before the end of 1969.

The Library plans ultimately to maintain a file with the capability of supplying up-to-date information for access by data line telephone connection. Qualified users may then readily obtain information on a host of products and compounds with known or potentially toxic effects. The realization of this plan depends on our ability to

create and maintain a file satisfactorily describing the capabilities and services of specialized information sources. Its realization depends also on the utilization of codes, standards, and criteria acceptable to participants in the toxicology information network. The dream of a truly responsive data and information resource would otherwise be impossible of realization.

A rational system of information handling requires common acceptance of forms, terms, and units of measure. Unless those who use services and those who provide them can understand each other, the system will be grossly ineffectual.

As planners responsible for national toxicologic information activities, we are sensitive to the influence which the structure and contents of our files and the nature of our computer programs will have upon establishment of standards of terminology, codes, practice, and information transfer. Any sizable, potentially useful resource must, by its very nature, influence the ways and means by which prospective users and contributors will interact with these activities.

## Literature and the Creative Process—Help or Hindrance?\*

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### Introduction

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We begin with a user's view of our topic. I asked our user not to be objective; I think we have enough of objectivity. It is healthy occasionally to have some good, old-fashioned, strongly-held personal opinions about things, and a user certainly has a right to a good, old-fashioned, strongly-held personal opinion about whether information service available to him is any good.

I asked our user not to conduct a study among his colleagues and to report percentages about things, but to give us his personal views, as biased as he wishes to make them.

Our user who starts this panel is Dr. Erwin Klingsberg, a synthetic organic chemist and Research Fellow, American Cyanamid. Following him are: a chemist-librarian, Mrs. Mary Jane Bloemeke, University of Pittsburgh; an information processor, Dr. Eugene Garfield, President of ISI; and a computer system designer, Dr. R. L. Wigington, R & D director of Chemical Abstracts Service. No panelist represents his employer; each speaks as an individual member of a profession.

### Printed and Other Impediments to Creation

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As I organized my thoughts on creativity, I realized that they seemed to be centering on impediments to creation—hence, the title of the talk, and also perhaps the first lesson in creativity: If a problem seems baffling, turn it upside down. What are some of these impediments?

The first impediment I would like to mention is the librarian. Why do I say that the librarian is an impediment to be eliminated? Because the chemist, within his own field, must know the library better than she does, so that he is not dependent on her help. The chemist must have the sources in his field at his fingertips, including, for example, the indexing and organizational characteristics of the major sources like *Chemical Abstracts*, Beilstein, or encyclopedias, such as the Elsevier Encyclopedia. Depending upon the particular problem at hand, one or another of these sources may come up with the answer very much quicker, and we must know more or less automatically how to go about making the search in the most expeditious way.

After the chemist has mastered the sources completely and can find answers to his questions in minimum time, the next stage is somewhat harder to define. It represents what we might call the creative use of the literature.

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It is one thing to look for some specific piece of information; either you find it or it is not available. It is another thing to go to the literature without really knowing what you are looking for. But when you find it, you recognize it. In this case, you are looking not so much for a fact as for an idea. How, for example, do you search the literature for ways to synthesize some novel chemical system from an undefined starting material? Collateral questions here are likely to be very important. There might be the question of availability of materials or particular types of apparatus or technical help in a laboratory. Or, having obtained a really unusual compound, how do you prove its structure? How do you find out what it's useful for, either from the chemical or from the application standpoint? The starting point for a conventional literature search is nonexistent, because our assumption is that the compound is radically new.

Without going into detail, I'd like to emphasize the great subjective difference between these two types of searches. In one, you know exactly what you want, you go after it, either you find it or you don't; this is a job which can be delegated. The other type of search is quite different. You find yourself in the library, not really knowing what you are looking for, and you browse around, almost for lack of something better to do. If you are lucky, you come up with exactly what you wanted, only before you found it perhaps you had no idea it existed. This job obviously cannot be delegated to anyone else. It may not happen very often, but when it does happen, you're likely to recognize the subjective quality of the experience, and it is likely to play an important part at turning points in your research. To summon or to invite this kind of creative experience, we must appeal to intuition and analogy in a way that varies from one field to another, but I emphasize that the prerequisite is mastery of literature techniques. The chemist must keep as close to the literature as possible, without reliance on intermediaries or retrieval services. He may want to make use of such aids, but he should not be dependent on them.

Now let us suppose we have perfected ourselves in the use of the literature. In this great task of gaining access to man's accumulated chemical knowledge to benefit our own creative efforts, we have eliminated all impediments between us and the literature. What about impediments in the literature?

Recently, while preparing a literature review, I came across a paper describing some results, followed two years later by another paper by the same author republishing the same results, with something extra to be sure, without citing the original publication. Examples like this can be multiplied indefinitely. There are chemists who consistently misrepresent the published record to claim more credit for their work than they deserve. There are chemists who ignore the work of their contemporaries, or cite it only to take issue on trivial points; the result is a gross distortion of the record. There are chemists who submit papers to journals in such chaotic condition that the referee, if at all conscientious and charitable, is faced with a Herculean task. I emphasize that I'm speaking of chemists of repute, who have made a name for themselves and, willy-nilly, are setting an example for others, particularly, of course, the academic people among them, who are training others.

Another example. A very well-known professor of chemistry reviews a new book in highly unfavorable—in fact, deliberately insulting—terms. A published correspondence ensues, from a number of different contributors, pointing out crass blunders in the review that destroy its case against the book. The reviewer himself joins in this correspondence, so there is no doubt that he is aware of his mistakes, which he makes no attempt to deny, although, not being a gentleman, he does not apologize. One might think that this is an embarrassing episode that he prefers to forget. Not at all. A year or two later he is publishing papers in which he cites his review, but not the correspondence that set the record straight.

To me, this is shameful. We scientists, by and large, have a pretty good opinion of ourselves. Yet, for a scientist, what sin could be worse than one against the scientific record?

I would like to cite passages from a 1963 editorial by Philip Abelson, editor of *Science*: "The scientific community has been curiously flabby in reacting to evolutionary trends which challenge the vitality of science." His reference is to the enormous growth of the literature. "Instead of tackling these communication problems, we have ignored them; and we have retrogressed, for we have allowed our standards to deteriorate." Abelson points out how scientists are permitted, and even encouraged, to give the same lecture over and over again at different meetings, or to publish the same paper over and over again in different journals. He wrote, "I have noticed instances in which basically the same article has appeared more than five times. . . . If editorial policies were tightened, the amount of material appearing could be cut to a quarter of the present volume, with no essential loss. This tougher approach might well take the form of a stern attitude toward repeated publication of the same material."

Imagine what a burden would be lifted from us if this could be accomplished, what a relief and economy it would be to our search, retrieval, and abstracting services, to everybody who uses the literature. Is there any hope at all? Is there any possible way of discouraging these malpractices? Certainly, the individual author can't really point the finger in his own articles and reviews. He might offend his friends, who won't invite him to the next symposium. It is not really a question for polemics. It is not a question of right and wrong between contending scientists, but rather a question between the individual and the scientific community with its obligation to uphold standards, to be less "flabby," as Abelson put it.

I would like to put forth one modest, limited, suggestion. If it is tried and works, perhaps it can be expanded. As we know, the journals publish errata, usually at the end of each volume, in which the contributors have the opportunity to correct typographical or other mistakes in the articles they have published. As far as I know, all journals limit the errata to this one function—correcting one's own mistakes. Now, it seems to me that it would be sensible to expand the function of errata by permitting others to correct mistakes. It could begin in a very restricted way, to the case where the reader observes that a writer has neglected to mention his own previous publication of the same material. Why not publish the omitted citations in errata? This would take up very little space and be completely noncontroversial. It would accomplish,

at once, something very desirable: setting the record straight. It would encourage respect for the record, and discourage sloppy handling of references. The referees would be greatly helped by having all pertinent references in the manuscript, where they can catch and reject multiple publication. The expanded errata would constitute a negligible burden on the journal. I don't see any possible way in which this practice could be abused. There is a certain resemblance, of course, to the opposition proceedings which are a feature of the patent systems in a number of countries.

I now tread on dangerous ground in naming another impediment to creativity—theory. Obviously, I am talking as an experimentalist. To do experimental work, we must know the relevant facts and theories. Yet, theory can be dangerous. It may be confused with fact, or twisted to conform with fact. It may inhibit really original experiments. Or, it may encourage experiments which at best can result only in a trivial confirmation of theoretical ideas. A former president of the ACS, Professor Noyes, in an epigram that I have cherished, said that the most useless member of society is a poor theoretician.

In a recent article, the authors were investigating a new heterocyclic system which was capable of substitution in two different positions, alpha and beta. They did molecular orbital calculations: the localization energy calculations showed that it should go alpha, but the charge density calculations showed that it should go beta. Well, the experiments went beta, so it was fairly easy to show why the localization energy calculations could not be expected to give reliable results and why we should really trust the beta prediction of the charge density calculation. But what they did not point out, although it was perfectly clear at a glance on reading the paper, was that these compounds which gave beta substitution were blocked in the alpha position. This little matter was not mentioned. Then of course, the paper gets written up in the Annual Reports as representing a confirmation of something or other, and this is what we know as the knowledge explosion.

Of course, students have to be trained to do experiments, to identify products; they have to be trained to do molecular orbital calculations, and the results have to be published, I suppose. However, I have an uncomfortable feeling that students trained in this way are likely to have an exaggerated respect for theory instead of what they should have, an exaggerated respect for experiment and for original ideas.

It is easy, in the course of our training, to gain an exaggerated impression of the definitive character and completeness of the literature on a subject. In the laboratory, we see case after case where experimental results just don't look reasonable in terms of accepted ideas, even in such basic matters as the activities of substituents in the benzene ring, orientation effects, and the like. There is just an enormous amount that we don't know, and it's a common mistake to take the familiar broad generalizations, useful as they are, for gospel. Justice Holmes used to say that it was the chief function of man to make generalizations, but that no generalization is worth a damn. It's in the exceptions that advances are made. Of course, we have to know where to look. A spectacular example, certainly familiar to all of us, was the recent

discovery of the noble gas compounds. Here was a classical case of theory masquerading as fact. Everybody knew that the closed shells made compound formation impossible, until this idea turned out to be wrong. There must be thousands of experimentalists who were perfectly capable of the experimental work leading to this astonishing discovery, and I suppose 90% of them would have rejected the suggestion to do the experiment, as an absurd waste of time.

The function of theory is certainly not to set up impediments to creation, not to inhibit the experimenter, but rather to stimulate and liberate him. One way theory fulfills this function is to provide a set of symbols which are useful for creative manipulation.

In my own field of organic chemistry, I never fail to be impressed by the fruitfulness, the creative possibilities, of the very simplest symbolic manipulations, hardly rising to the dignity of theory. I'll try to give some examples.

In an organic system, we can replace an atom of carbon by the neighboring atom, nitrogen, to obtain an isostere. If one carbon atom of benzene is replaced by nitrogen, the result is pyridine, the isostere of benzene. As one might expect, the two systems are in many respects very similar in their chemistry, in other respects different. Now if a chemist working in the pyridine field has a problem, or needs an idea, he naturally—almost automatically—turns to the benzene literature for help. But do you suppose that a chemist working in the benzene field is likely to turn to the pyridine literature for guidance, or for ideas? This is much less likely to happen, although logically there is no difference between the two substitutions. We could discuss the reasons, but I think it is quite clear that the main reason is *habit*, which is the antithesis of creativity. A chemist who stands habit on its head reveals interesting signs of creativity. And this again shows the great importance of the utmost familiarity with the literature, because if a question like this comes up, it makes all the difference in the world whether it takes fifteen minutes or the whole morning to find some kind of answer. In practice, of course, if it is going to take all morning, then the question just never surfaces.

Returning to the benzene ring, we derive naphthalene from it just by doubling it up. This is a very simple symbolic manipulation which has a much less pretentious name—doodling. Now naphthalene is in many ways more interesting than benzene; it includes all of benzene chemistry, and a lot more besides. Benzene has only one monosubstitution product; naphthalene has two. Naphthalene has bond alternation; benzene does not. The possibilities of isomerism among simple derivatives are far greater in naphthalene than benzene. And naphthalene has been around for a long time; as a matter of fact, it was isolated in the 1820's, even before benzene. An enormous number of derivatives are known, and many have great commercial importance. And yet there are yawning gaps in our knowledge that suggest some kind of failure of imagination. Take the matter of ionization constants and the effect of substituents; I hardly need emphasize the importance of this subject, or its venerability, in the development of our fundamental ideas. Generations of chemists have been weaned on the acidity of benzoic acid, *o*-, *m*-, and *p*-chlorobenzoic acid, and the like. What about the naphthalene series, with its much greater structural

richness? If you start looking, you'll be surprised how little has been done.

Consider the halogen derivatives: Fluorine chemistry has been growing at a great rate, and not long ago perfluoronaphthalene was synthesized for the first time. Almost immediately, work began to appear on replacement reactions of the fluorine atoms. But the corresponding chlorine compound, first reported in 1876 and easy to prepare, was ignored, although it is a low-priced commercial chemical. Nobody thought of investigating the replacement reactions of the chlorine atoms, despite the fact that the fully chlorinated benzene was well-known to be quite reactive. But when the fluorine compound of naphthalene was synthesized, it was investigated at once, although its limited availability probably made it awkward to work with. Things like this astonish me. In this case, a partial explanation may be that perchloronaphthalene—though, as I say, a low-priced chemical—is not in the Eastman catalog. I'm quite serious; this catalog and others like it represent a very important literature resource.

It seems clear that most of us waste most of our creative gifts. Why do I say this? Conclusive evidence, I think, is provided by the historical record of human creativity. We see enormous variation. We see long periods of mediocrity or somnolence, when nothing worth remembering seems to come into existence, punctuated by exceptional bursts of creativity, which are often short-lived, and which go down in history as golden ages. The great prototype of the golden age is, of course, Greece in the classical period. The extreme irregularity of this pattern has given rise to endless speculation among historians and other scholars. Obviously, we can't go into this speculation, but one conclusion seems inescapable. The human potential, the genetic endowment if you will, obviously cannot vary nearly as much as actual achievement has varied, from time to time and place to place. All ages, including the one we are living in, must have similar possibilities. All ages are somehow trying to be golden ages—but very few succeed. Of course, the individual cannot make a golden age for himself. Each of us is the product of our times. Yet all around us, people waste the possibilities right at hand.

Many chemical companies attempt to foster creativity among their scientists by encouraging them to work on their own ideas, without any accountability, during a certain proportion of their time. It seems that in most cases the allotment of time goes unused. Why? In all probability, because of fear. Fear of making a mistake. Fear of looking ridiculous. Fear of annoying the supervisor by taking time away, or seeming to take time away, from the principal project, the assigned project. All these fears prevent us from making the best and freest use of our talents.

If we wish to create, perhaps we must begin by casting out fear, that great ally of the unknown and worst, perhaps, of all Impediments to Creation.

### Response of a Chemist-Librarian

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I feel that proper literature support can be a tremendous help in all phases of research. I am willing to accept

the judgment, however, that one place where a librarian is not needed is during the act of creativity.

Librarians need not be the impediment Dr. Klingsberg fears. I think we can be, perhaps, a little more helpful than he is willing to allow us to be in pointing to potentially valuable browsing sources. A librarian can be a source of information about new publications that are in the library—things that you may not know about—without causing too much trouble. When you ask us questions of this sort, you will find that we are willing to point out the sources and leave you strictly alone.

The librarian can come into the picture best probably during the critical appraisal. I don't know much about creativity, but it certainly seems to me, as Dr. Klingsberg has pointed out, that the worst thing about it is the barriers which must come down. First the new ideas appear, and after that they are critically evaluated. The librarian can be of help in placing the new idea in the historical development of the science, and perhaps in pointing to potential uses. Don't fear the librarian; fear the literature, if you must, but not the librarian.

I think most of us can name one or two chemists who are completely past the idea of producing anything new. They spend their entire time gathering background information, and nothing else comes out of it. That's the thing to be afraid of.

The development of new information systems makes this difficulty even more of a threat to the chemist. It is very easy to be preoccupied now with the business of designing an elaborate classification scheme—with shuffling punched cards. It's fun to write profiles, and it's nice to analyze search results; but this can be a tremendously time-consuming process. I think the creative chemist has got to be aware of this threat and he has got to put it in the proper perspective. At the University of Pittsburgh, all graduate students are warned repeatedly about preoccupation with literature. New information techniques are good in themselves, but only if used to supplement the other really important creative aspects of the research man.

The same new systems that become so engrossing can provide extremely powerful new tools for creative chemists; this is where the librarian comes into the picture. It is necessary for the librarian to know what new systems are developing, and who among her patrons might possibly use them, to provide the initial introduction. After that, she should get out of the way and leave you to the important business of research.

### Response of an Information Processor

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An ideal information system is one that eventually shows a profit. But, contrary to the notion of some scientists, most entrepreneurs do not consider profit alone as the criterion of an ideal system. If we were solely profit-motivated, we could find countless better ways to make money faster, with less toil and much greater return. There must be psychic satisfaction, and this derives mainly from providing a useful service for scientists who want it so badly they are willing to pay for it year after year.

Once we specify that an ideal system must provide psychic gratification, we arrive at another aspect of the information scientist's entrepreneurial dream. He wants to provide a profitable service with the absolute minimum of physical effort. That is why computers seem attractive.

There is also a peculiar attraction in large scope. Since this happens to be what users want, the dream of a world brain gets intertwined with the other ideals. All of us tend to be empire builders unless we are checked by society! Ideally, for secondary services, as long as journals survive, it would be nice if all of the information needed for a service like *Chemical Abstracts* or *Index Medicus* could be automatically derivable from the journals. It is anything but possible now, and probably will be less so as the journals compress information further. So, the secondary services will have to expand selectively as the primary journals compress.

It is the ideal of every manufacturer that everyone who can use his product will recognize that fact instantly and buy his product the day it hits the street. That is never the case. Everything in life must be sold. Survival is a form of persuasion. In the information business, education is a long-range process; ideally, it ought to be done for us in the universities, but most schools don't even scratch the surface. Ideally, every scientist would be an avid consumer of information products. As things stand, we are only on the threshold of the information consumption revolution that is yet to come.

Ideally, every chemical compound reported in the literature would have a correct molecular formula, a Wiswesser Line Notation, a Dyson or IUPAC notation, a correct name according to CA, a correct structural diagram, a connectivity table, all activities known or potential, and so on. What would be left for us to do is merely to rearrange the information. Under ideal circumstances, therefore, our reason for being would be seriously questioned. In a paper I gave about three years ago entitled "The World Brain—Memex," I stated that an ideal system for searching the literature would contain ideal word indexes, ideal citation indexes, etc. But life is not an ideal state, and our conception of the ideal life is constantly changing.

Now, let me turn to Dr. Klingsberg's paper, a paper that certainly justifies this particular session. People like controversy, so I've tried very hard to find those points on which we disagree; it wasn't easy.

I don't agree that the scientist must know as much as his librarian. No more so than the synthetic chemist must know as much as the head of the analytical lab, or vice versa.

Dr. Klingsberg and I agree that there are basically two kinds of information retrieval problems, information recovery and information discovery. Distinctions between these two are frequently confused or missing. While I agree that the chemist can and must work more creatively with the literature, he can and must delegate many routine tasks to others. Many research directors forget this.

Incidentally, to get some idea of how creative Dr. Klingsberg is, I looked him up in the *Science Citation Index* and the proof is very clear—he is a very creative individual. His work is consistently cited year after year; even a paper published in 1951 is still cited very heavily. His work has been cited in every country of the world

in which chemical research is done, with the one exception of Russia. He must know of references to his work in the Russian literature, but we were not able to find any.

One can always find researchers who ignore the work of their contemporaries or who do not cite the literature properly, but my experience shows that, statistically, citations are pretty reliable indicators.

Ideally, all editors would adopt Dr. Klingsberg's suggestions about errata in citations. I couldn't agree more that at times the situation in refereeing appears to be horrible. We should require every scientist who submits a paper to include an approved literature search by a literature searcher or provide evidence of his own search.

The reason I suspected Dr. Klingsberg did not know *SCI* is his statement about reviewers "a year or two later publishing papers in which he cites his review, but not the correspondence that set the record straight." He is absolutely right, people do this. But the indexes available to the referee reveal such frauds rather easily. I don't think Dr. Klingsberg attended our Divisional Symposium on Errors in which this whole question was thoroughly reviewed, in particular by Dr. Addelston. I'm not so sure either that Dr. Klingsberg or Dr. Abelson are correct about the redundancy which exists in the literature. It is perhaps the nature of the communication process that redundancy is necessary. Hans Selye has given cogent reasons why papers should be published in more than one journal. Like the patent system, however, we need better ways to identify the repetition.

Now further about errata: Many journals do, in fact, publish exactly the kind of errata containing omitted citations. I fully agree with Dr. Klingsberg's proposal, but I should like to point out that this practice alone would not insure that the errata would be found unless chemists are taught to use citation indexes, in which correction notes can be identified.

Let me cite a rather classic example: A completely duplicated piece of research was acknowledged by two coauthors to have repeated work reported five years earlier in *Analytical Chemistry*. Since 1962, the duplicating paper has been cited over 25 times by more than that many authors, since there are co-authors involved, even though a full correction note was published in the *Journal of Biological Chemistry*. And none of these 25 authors knew about the correction note. I know this, because I wrote personally to each one to point this out and to verify the statement. I then published the example.

In conclusion, I heartily agree that we have nothing to fear but fear itself.

### Response of a Systems Designer

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My comment on Dr. Klingsberg's paper combines four points of view: first, as a representative of a technical discipline other than chemistry, particularly computer science and electrical engineering; second, as a systems designer involved in the design and implementation of information systems with a close tie to the technical literature; third, as a manager, faced with my own personal information inundation, from which I draw some analogies

that may be useful in the area under discussion; and fourth, my own personal opinions.

Dr. Klingsberg's paper is as much a commentary on the behavior of people as it is a discussion of the literature. But, of course, that is highly relevant to the topic. Our literature, the way it is produced, and the way it is used, is the major record of how scientists produce and use information. The behavior of individuals and groups of scientists has major impact on the process. I find that I am in general agreement also with the six or seven hindrances to creativity that were listed in the paper. I'd like to expand on three of them and add a couple of other candidates for inclusion on the list.

The hindrance of language translation, as we mentioned, is very real and obvious for all monolingual Americans. But I suggest that we generalize this point to include the problem of transfer of meaning and understanding across boundaries between disciplines and between special interest groups within disciplines, even when all the people involved happen to speak the same natural language. The problem is well illustrated by the cadre of people working in the general information science and service fields. Our origins lie in perhaps the most diverse collection of disciplines of any of the currently popular interdisciplinary scientific endeavors. I find this exciting and interesting, but this characteristic leads to frequent and serious difficulties in striving for a common understanding of what are the components of an information system, what is important about each, which way is up on the scale of values, and most seriously, what action should be taken in establishing a system, or in operating it once it exists. Thus, one of the most serious problems we designers of information systems face is that of transfer of understanding from the needs, or sometimes dreams, of the user ultimately served, to the information specialist who specializes in information accessing and processing, to the designers and implementers who make the systems come into existence, to the processors who actually operate the systems, and finally back to the user for the economic and social implications of his wishes. Without this complete cycle of understanding, no design can be truly successful and responsive to the needs for which it originated.

Dr. Klingsberg referred to the misuse of publication as a hindrance. He implies a degree of mismanagement of the generation and selection of the primary literature. Some comments on the reviewing and refereeing process have been made; I agree with and add to them in this respect: As a sometime reviewer and referee for papers in the journals and conferences in the field of computer science and engineering, I take the responsibility very seriously. Unfortunately, I have noted recently that a number of the papers that I have recommended not be published have in fact appeared in the literature without repair to the weak areas that I pointed out. Either I was wrong about those articles, and outvoted by the other referees, or we aren't taking the referee system seriously enough.

The hindrance of ill-founded theories religiously believed is indeed serious, but couple that with the seemingly inherent negativism usually exhibited by experts in almost every field, and the result can be devastating to creativity. All of us are subject to the disease of negativism. Of the next 10 times you hear a new idea advanced, see

how many times your first instinct is to try to figure out what is wrong with it. It is not fair to include in this test any instance of hearing your own pet ideas expressed to you. The opposite extreme, gullibility, also is not desirable, but neither in scientific meetings nor in publications is gullibility as serious a problem as negativism.

Two other hindrances to creativity, to add to the list, are, first, poor writing and unimaginative presentation of information, and, second, the phantom of completeness and panic. The first is relatively straightforward. Scientists and engineers are not noted for their clarity of expression, be it oral or written. A few years back, two colleagues and I co-authored an invited article for a periodical. We labored long and hard to produce a draft of 9000 words that seemed to cover the subject well. Then an editor was sent out to work with us or, I should say, to work us over. When he was done, the article was down to 5000 words, it covered the subject much better, it was carefully tied in with the illustrations, and we wound up with a lesson in the art of communication. Few scientific papers can receive that kind of attention from the recipient journal. It would be far too expensive. However, improving the training of scientists and engineers for better written and oral communication is certainly in order, and more vigorous refereeing of style as well as content would also be in order. The author himself should seek out competent assistance of this type.

Another literature-related hindrance to creativity is chasing the phantom of completeness, that feeling that one must have personally read and understood everything that applies to a scientific topic. This feeling is often followed by panic when you finally realize that you can't do it. Certainly the volume of material to be known is huge, has been growing, and will continue to grow. There are some very real problems in organizing that information for use, selecting it for special interests, and delivering it where needed in the amount that the individual who wants it can use it. Many of us are working to solve those problems, but the topic today is the creativity of the individual and how that is aided or hindered by literature. Each must learn to limit his information appetite to what he can digest, and have confidence that information is being organized so that he can find it when he knows what he wants, so that he can get on with his research or development or design activity from which truly creative results can spring. I must observe, as a person with a foot in at least two scientific cultures, that chemists should really appreciate that their literature is much more disciplined and that access to it is better organized than is the case in other scientific fields which have less volume and a shorter history.

As a manager, I learned long ago that I cannot keep up with all I should read, even that which comes forcibly to my attention by landing on the corner of my desk. There is a fundamental theorem from queuing theory which applies to this situation. In paraphrase, it is "for a transaction-oriented system containing statistical variations in arrival of work—the rate at which things drop onto your desk—as the load on the system approaches the capacity of the system, the queue of work approaches infinity, and so does the average time required for each item to wait to be processed." As managers must learn,

so must the scientist or engineer learn the strategy of maximizing the value of what he can and does do, rather than becoming frustrated in trying to minimize what he is not able to do. Further, he must learn to depend on others to do at least some of the work and not feel obligated to do it all himself. I believe an opinion roughly like that has been expressed earlier. Thus, I am suggesting that some of the hindrance to creativity suffered by the scientist or engineer is his own mismanagement of how he uses the literature of his field, and that this is of comparable importance to the deficiencies of the literature itself and of the ways that exist to use it.

As a computernik, I should be giving an exposition of how the new computer technology is going to solve all information problems in a blaze of glory. I think that access to data banks through interactive terminals, computer-controlled composition, and various computer aids to processing and evaluating the information that now constitutes our literature—these things in combination do provide a full bag of tricks and techniques that can be applied to our problems. I am thoroughly convinced that intellect and creativity can be enhanced with such systems. Further, I am certain that the economics of such techniques will improve so that they can eventually become widely available and not limited to experimental situations. However, the machine system cannot magnify only intellect and creativity; it can also magnify stupidity and waste resulting from human ineptness and carelessness. Thus, I am interested not only in the machine system aspects, but also in the associated human factors, and was most happy to conclude that Dr. Klingsberg's paper dealt mostly with the behavior of people, both as information producers and users. If that indeed is what he meant, I am very much in agreement with most of his views.

### Concluding Discussion

**Klingsberg:** With reference to Dr. Garfield's analogy between the library and the analytical lab, those, of course, are services to a synthetic organic chemist. But the analytical lab is a closed-end function, whereas the library is an open-end function. It is not really sound to say that because the chemist does not have to do his own analyses he does not, therefore, have to do his own searches. These functions are very different in their nature, in their utility, and in the possibility of producing unexpected results.

**Garfield:** Your comparison of the analytical lab and the library is also good. Two processes go on when a chemist uses the library. In a large number of searches he is trying to find information that he knows is there. That is information recovery, and it seems a waste of the time of a highly paid scientist to look for those things that he knows are there in a simple recovery process. If you are talking about discovery then we agree. I was trying to say that the distinction is not made enough; I've been in countless industrial and academic organizations where the position is taken that the chemist must do his own literature searches, without regard to whether he is recovering information or discovering it. There are still industrial organizations that do not have a reasonable library or a librarian or a staff of people to help the

scientist. I think we have a tremendous educational job in making that distinction.

**Wigington:** The use of letters as chains of comment, either for errata or expansion of the topics of papers, has been common in the electrical engineering literature for quite some time, and they constitute a very important string of intellectual comment and expansion of the original publication. There have been suggestions that the abstracting and indexing of that literature should also include the chains.

**Garfield:** A number of articles have been published which claim that the use of literature tends to inhibit creative research. A widely held notion among us is that if a man spends too much time in a library, he is apt to not repeat an experiment which was done erroneously. There is the old story that if he had not known about it, he'd have gone ahead and done it correctly this time. In fairly carefully studying the work of creative scientists, not only Nobel Prize winners but others, I have found that this is almost an absurd claim. A man who really is a creative scientist, who has demonstrated this by various publications and discoveries, usually is an avid consumer of literature. You always find exceptions cited. Recently somebody claimed that Einstein never used the literature. I don't believe it. With enough time, I think I could prove he did. But suppose that Einstein never read the journals. It seems to me absolutely absurd to use the man claimed to be one of the greatest scientists of all time as the typical representative of over a million scientists throughout the world...or the 1% who are the cream—10,000 truly creative men who are creating the literature—and to say that they are inhibited by the literature. Maybe I have encountered this view because at the time I'm talking with directors of research who are trying to avoid buying a searching tool and so have suddenly discovered literature isn't worth a damn.

**Klingsberg:** I encountered a slightly different form of this selfevidently ridiculous philosophy last year. While visiting a foreign university, I met a colleague and we fell into conversation about a third colleague. I was told that this man made a distinction, in his own work, between objective and subjective discovery. According to this view, it is one thing to discover something that is really new, but another thing to discover something which is new to the discoverer, even though it is in the literature. His feeling was that the important thing was whether it is new to the man who discovers it. This is, of course, ridiculous, and I hardly need add that the man who professed this philosophy was not very distinguished in his own contributions to the literature.

**Audience member:** One problem is that very few scientists are even fractionally as aware of the literature as is our main speaker. And they care even less. Especially are there many scientists unaware of the new services that have come into being since, say, 1908. They do not know about the kinds of questions they can now ask of the new techniques that they could not of the old.

**Audience member:** At my company, we did what a lot of people in this room did; we bought first class tickets on the current awareness express, and the last time I looked out the rear of the train, our librarian was way back on the track somewhere disappearing from view,



hollering "wait for me." For two days I've listened to all of the wonderful accomplishments and things that we are concerning ourselves with about alerting the scientist to what is new in the literature, but alerting is only half the job. The article itself that proves to be interesting by the alert has to be available. We are finding so many times that our chemists will say "don't bother with that one; it is Russian or Yugoslavian or something I can't translate," or else he asks for something that happens to be from the "Institute of Gas Technology of Belorussia" and is absolutely unavailable. The things that we find in *Referativnyi Zhurnal* are just curiosities, no more. I think that the current awareness services have gone way beyond the capability of the average library to supply. I think that ought to be added to the list of hindrances to creativity in the literature.

**Garfield:** We are going to index 350,000 papers this year, and if you can find one that we can't supply, I'll personally deliver it to you. It is our policy to list or index only papers we have actually available.

**Bloemeke:** I hope that *Access* is going to help us with that.

**O'Dette:** Of course, that's the purpose of *Access*. The function of the services that we produce at CAS is to direct attention to the primary literature. Gene Garfield has the same objective, and in addition, will physically provide the primary paper. We choose not to take this latter step for a variety of reasons, but to try, with the cooperation of a number of libraries, to say who has the paper. Now, whether the *Institute of Gas Technology of Belorussia Proceedings* are held by somebody that you can get at, I don't know. We will have to wait for the first issue of *Access* to find out, or check over our current *List of Periodicals Abstracted*.

**Garfield:** A very important point being brought up here is a basic notion that I try to inculcate in my students at the University of Pennsylvania. Very simply, it is that the better we disseminate information currently, the more we can rely on something that we tend to underestimate, and that is the collective memory of the people involved. It is really incredible, once a man has been alerted to the appropriate literature in his field, how much he will remember. That is why I say rediscovery or recovery is distinct from discovery. We can alert people to things which are not known to them which will pique their curiosity. Five years from now they have the problem of finding again that paper they remember that someone

talked about. So you really can't separate these two processes. If we do a good job of disseminating what's in a journal in the first place, then we will make it easier to bring the man and machine together later on. Over a period of 10 or more years there was a real breakdown in the process that was originally fairly successful—simply disseminating information through journals that everybody read.

The attitude of users now is that they've been looking at current awareness services for five, six, seven years. They are used to that method, but now their problem is, where do I find that paper that I recall from some time ago.

**Audience member:** I've been a little surprised at the absence of any introspective comment from people who admit they have been struck by a creative accomplishment once or twice in their life as to just exactly how interaction with the literature played any part in the creative process. Can anybody remember specific instances that would at least provide a base for speculation?

**Garfield:** Given more time, I could cite some classical examples of discussions. There is an interesting, well-known controversy about whether Mendel's work was ever cited in the literature. I have reported on this elsewhere, but William Bateson clearly states that Mendel's work was cited in 1881, twenty years prior to the so-called rediscovery of Mendel. You will find in the literature many examples of people openly acknowledging the debt that they have to the literature. The trouble is that we don't make a big thing about it. We just accept it. Every paper that is written presumably makes some such acknowledgment.

**Audience member:** I'm interested in it from the standpoint of the people that might have an opportunity, editors, for example, to encourage certain characteristics in selection of papers, in the style of papers, in abstracting and indexing methods, etc.

**Wigington:** I would like to return to the comment about the relationship with the libraries. Russ Rowlett in his paper yesterday morning said there were three major facets of literature: generating, storing, and accessing it. Many of us are sometimes authors and reviewers, but mostly we are interested in the accessing aspects. That has certainly been the center of attention. There does seem to be too little cooperation between various segments to look at the problem as an over-all system problem. As a systems engineer, this currently attracts my attention.