Ethics of Scientific Publication

Rules for authors and editors may depend on something more than taste and convention.

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During the last few decades there have arisen several unrelated issues as to what one should do and should not do in generating, distributing, and consuming scientific information (*1*). These issues, or their appearance in more worrisome form, seem to be a consequence of the burgeoning "information explosion" and the increased involvement of government with science. I suggest that the time has now come to consider all such questions as a single complex situation in the ethics of scientific publication. I propose, moreover, that we need a searching inquiry into the basis on which such ethical decisions are founded. In all humility, I do not know if the judgments I propose are sound, but I know that, for entirely practical purposes, we should proceed no further without judgments. In particular, it must be found out whether the intuitive ethics of past generations are really valid or whether one must begin to replace them by a rational ethic based upon our new knowledge of the nature and function of scientific information (*2*).

Availability of the Literature

Perhaps the most basic question is that of the openness, democracy, and availability of the literature. The naive assumption is that there exists an immortal record of open publication where each man is entitled to present his findings and where each paper is subject to scrutiny by a jury of colleagues from all countries and, indeed, all times. It is known now that this assumption is basic to much of the motivation of scientists, and that the lure of such impersonal and objective immortality is a veritable spring of creativity.

Unfortunately, so much of this assumption is now suspect that we begin to feel it was nothing more than another of the several pious and prudish Victorian ideals deliberately promulgated in the late 19th century as a picture of what science would be like if it were perfect and pristine. There seems no evidence that this is now, or has ever been, much better than a fairy tale. In the first place, we now know with some precision that the greater part of the scientific literature includes material from which the juice has already been well squeezed by prior informal communication, by word of mouth, by preprinting, and by a host of other techniques that have been increasing mightily in the past decade (*3*). In a sense, we have been able to substitute the technologies of transportation, the telephone, and the newspaper for the scientific paper. The result of this has been a laudable increase in efficiency, without which many fields would be unworkable, but it has been at the expense of a certain amount of openness of the literature.

In the second place, the immortality of the record appears to be weaker than had been supposed. It can be shown, from network analysis of scientific literature, that only a small part of the literature has a usefulness that lasts after the intense localized research front has passed it by (*4*). Some papers, of course, are more immortal than others, and some are helped toward immortality by the widespread custom (accounting for 10 percent of all references) of authors' citing their own previous works. Certainly, some papers from the research front and others not belonging to it do become part of a long-term archive, but it seems clear that by far the greater number of published works do not. Part of the reason is that they do not merit it, but again the greater part of the reason seems to be that they were not designed for it. The greater number of papers are published for one's coeval peers rather than for the unseen audience of immortals.

Freedom to Publish

In the third place, there is a difficult question of whether one is free and entitled to present all findings. The starkest question here, but also the one that has been most fully discussed, is that of secrecy in scientific findings judged to be relevant to national security. The only comment I would make here is that the situation might be illuminated by being generalized. Historically, there has been a very interesting contrast between the literature ethics of basic science and those of technology. In basic science, the motivation is always for the most complete publication that will ensure the payoff, or recognition of the contribution of the individual scientist and his reward by eponymic fame, Nobel prizes, or similar honors, or at least by appreciation. In technological research and development, with profit or military ascendancy substituted so largely for honor, the effort is toward publication only as an epiphenomenon, not as an end product. If publication cannot be completely avoided because patents are needed, prestige is obtainable, or public money is to be accounted for, it takes place in an atmosphere that lacks the basic openness of science, though, in those technologies that are nearest to basic science, there is a reverent imitation, caused by the fact that scientists and technologists may well be the same people. Unfortunately, it is precisely this noncumulating, badly ordered technological information that we feel most highly motivated to structure, store, and retrieve for the good of society. It follows that technology might well attempt consciously to follow science in its attitude toward ethical matters.

Apart from this special sort of openness versus secrecy, there is another matter that needs consideration before we can judge about the freedom and right to publish. Even before the in-

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formation explosion caused us new troubles, it was clearly agreed in most fields and countries that freedom of publication was subject to the restraint of reputability. A paper had to run the gauntlet of editors and referees, and therefore it had to meet various professional standards. With widespread government support of science, some new dangers have entered here, and it is necessary to examine critically the ethical decisions involved in accepting or rejecting a paper for publication.

The rare instances of scientific dishonesty, gross incompetence, and action by the lunatic fringe are difficult, but they seem, for the most part, reasonably well patrolled. Questions of the significance of findings, of multiple publication (or the awarding of credit), and of the style of papers are not so easy, and they deserve more ample discussion and the deliberate evolution of standards. I suggest, as the most important principle, *that scientific publication be considered a privilege consequent upon the finding of something which people may need to read, rather than as a duty consequent upon the spending of time and money.* If a government or institution requires an accounting for funds spent, this should be considered primarily a fiscal document, and it should not be allowed to clutter up the dangerously overloaded scientific literature by back-door entry. Furthermore, it should be a rigorously applied principle that no paper be committed more than once to the published literature without very special pleading. There seems no reason why prior publication of the major part of a paper—for example, in a symposium or conference-proceedings volume—should not be grounds for disqualification before any other editorial board. A journal or an agency that willfully attempts to enhance its own reputation or interests by aiding and abetting such a cluttering of the science-information circuits is working to the detriment of all. It is a very dangerous folly in a situation where everyone wants to publish but only a worried few want to read.

Awarding Credit

It must be recognized that, whether we like it or not, new information-handling methods have a heavy impact on the intuitive ethic, such as it is, that governs the awarding of credit by deciding who is listed as first author in multiple authorship. The new methods have an impact, also, upon the general practice in citing previous works. Such things can now hardly be regarded as matters of taste or even of convention. For the present discussion, it is enough to note that, for all purposes other than those of constructing one's own lists for "publish-or-perish" deans, there is now at least ten times as much value in being listed first on the by-line as there is in being anywhere else on the rapidly growing list. Again, perhaps, one should propose that authorship is a privilege rather than a right. It is not a consequence of work done as a member of a team or project, it is a recognition of a distinctive contribution admitted by the editors and referees of the journal of publication. It is right and proper that editors take this duty seriously and refuse to allow publication in cases where members of a team are being given credit by virtue of their participation in the work reported rather than because of specific responsibility for the publishable contribution.

Citation

Similarly, now that citations to previous work have become a valuable tool for literature indexing, referees and editors should summarily reject bibliographies that are either insufficient or padded. Reference must be made to those immediately preceding papers that have been used as a basis for the new work; they may well be of interest to persons reading of the new use to which the earlier work has been put. Pious citation of works published long before and now part of the general education of those at the research front should be discouraged, except in very special cases. As a corollary, it might be noted that, even in the most general and popular writing, it is not good style to omit references. They should always be given, and they should, for purely practical purposes, be in the form of endnotes rather than footnotes or interpolations in the text.

In mentioning style, I must add that, contrary to superstition, the avoidance of the first person and the active voice of verbs is no guarantee of objectivity. The quicker one can destroy the early-20th-century myth of proper "scientific prose" and return to good English, the better it will be. Historians of science know only too well that the scientist is more frequently than not passionate, biased, illogical, resistant to proof and to change, and beset by other similar human failings. It is, today, better to let this show and be understood than to pretend that it is not there.

In general, in the interest of "freedom," the scientist has been able to get away with anything that leads to more research and more credit and publication. In the interest, now, of preventing an utter breakdown of the payoff of research and publication, it appears that one must institute, within science, a set of forces tending to oppose such free proliferation.

Retrieval

Thus far, I have mentioned only the ethics involved in generating and monitoring scientific information. A little more needs to be added about complementary processes in its distribution and consumption. It must not be assumed, naively, that all previously published information would be retrievable if only our libraries and indexing services were good enough. The best evidence now suggests that there is a sort of "uncertainty principle" built into indexing systems, such that every effort to retrieve more precisely the exact type of information required produces also a loss of documents that must be missed by the search. Correspondingly, if the class sought is broadened, this may lead to a smaller loss of significant sources but it will surely lead to an increase in the "background noise" of unwanted documents. The constants involved make it doubtful whether any sort of computerized indexing will ever be satisfactory for the control of general scientific literature except in very special cases (*5*).

Clearly, it is now more important than ever that we have further research in exploring all systems which might be partially workable, but we should not expect too much. At least two sorts of computer handling are, however, possible. One occurs with what one might call taxonomic or Aristotelian information, where there is a unique and well-ordered label that can be fixed quite unequivocably without ambiguity to any item. Thus, we can code nuclear moments, biological effects of pharmaceuticals, chemical properties, zoological and botanical taxonomy, and geographical data and always retrieve all relevant documents without background noise or loss of

relevant data. When this can be done, it should be done, and a print-out, for distribution, should be made at regular intervals. It has also been suggested that, in such cases, one might as well count publication in such an index as definitive and, in the absence of special cause, insist that an author should not also publish in the regular journals.

Invisible Colleges

Another computerizable procedure concerns the "invisible colleges" composed of the hundred or so really active and knowledgeable people in any particular part of the research front of science. It is eminently possible, by citation indexing and other means, to give such groups a custom-built information system to replace their own capriciously functioning system. Not only do such groups form the natural units of which science is composed but they are the main channels for the informal as well as the formal part of communication. We must realize soon that the existence of these groups is both natural and good. They are not vicious in their lack of democratic openness, and they sorely need help. It must be recognized that, just as a scientist should be given an opportunity to communicate by formal publication, he must also be given help in informal communication. One corollary of this is that universities and research institutions must encourage travel much more than they do now, allowing their own staff members to be away for a good part of their working time and offering hospitality to visitors from other institutions and organizations. This is not just good-neighborliness. Such activity is not merely a preliminary to the desired end of producing scientific information; in our present system, it has become another form of scientific publication. We can no longer pretend that such publication is not "open" and is, therefore, illicit and unethical. If such an informal communication or "preprint" be reckoned as proper, one can then suggest that only when a paper becomes archival need it be published in conventional journals. There seems little reason for the hollow pretense that open publication will make the paper an information source outside its "invisible college." Frequently, open publication of such little usefulness merely provides clutter that masks important archival material from individuals not at that particular piece of the research front.

Scholarship

Lastly, we must not be tricked by the information explosion into thinking that this is something totally unprecedented and unmanageable. The number of papers produced per scientist is still the same, approximately, as it was in the 17th century, and though there are more people involved, by many orders of magnitude, we have divided and ruled by the trick of specialization. Each specialty grows exponentially, so as to double every decade or so, just as it did in the days of Newton and of Franklin. We solve the problem of a mounting literature in each specialty by a curious expedient called scholarship. This is the art of packing down the accrued knowledge through more and more economical statement so that, eventually, it becomes part of the material that can be learned by the student before he arrives at the research front.

We have never found any substitute for scholarship, and we must not imagine that any method of juxtaposing relevant documents by computer could successfully synthesize new knowledge or make scholarship and expert knowledge otherwise unnecessary.

If we are to live with this information explosion, let us not be terrified into dropping all our standards of the nature and of the ethics of scholarship and of science.

References and Notes

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