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VETERANS ADMINISTRATION

COMMUNICATIONS: A SCIENTIST'S RESPONSIBILITY

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The author of this guest article, the second in a series of such articles, is Dr. Robert O. Becker, Chief of the Orthopedic Section, VA Medical Hospital, Syracuse, New York. Dr. Becker was the 1964 winner of the Dr. William S. Middleton Award, the VA's highest honor for medical research. The award was made to him for his outstanding research on the identification of electrical control systems in living organisms including man.

The circumstances surrounding the founding of the Royal Society and its publications furnish an excellent starting point. The intellectual ferment in England during the early part of the 17th century led to the loose aggregation of "diverse worthy persons inquisitive into Natural Philosophy and other parts of human learning" known, in Robert Boyle's term, as the "invisible college" whose meetings and communications were largely secret. By 1660 circumstances had changed sufficiently to enable the meetings to be held openly with official sanction of the Crown, and in 1663 Charles II issued a royal warrant founding the society. The mace presented to the society by the King was inscribed "The Royal Society for the improving of Natural Knowledge by Experiments." By this time the society had developed extensive open correspondence within its own membership and with continental scientists and on Monday, March 6, 1665, the first number of volume one of the Philosophical Transactions was formally published, "giving an account of the present undertakings, studies and labours of the ingenious in many considerable parts of the world." The scientific journal then, as it was first conceived, was primarily a medium of exchange of meaningful ideas and data concerning ourselves and the world we live in. From the formal inception 300 years ago scientific communications have grown in a reasonable orderly fashion to meet the needs of the scientific community until, not unlike the world's population, it has proliferated uncontrolled and at almost an exponential rate since the close of World War II. At present, new journals are being founded almost daily and the scientist desiring to keep abreast in his field has either almost given up or has begun to rely upon various abstracting services most of which are generally unsatisfactory. Attempting to keep informed in other fields or even related fields is an impossible task as the tendency is to produce new journals even more narrow in scope.

Are there really that many new ideas? Are the data reported in all of these publications meaningful? I think not. It seems to me that the original reason for writing a scientific paper, that of communicating meaningful ideas and facts, has been largely superseded by other more "worldly" considerations. The major route to academic advancement is via publications and all too frequently the scientist is judged on the length, not worth, of his bibliography. The "value judgment" on a research project is often based upon the publication of papers bearing the imprint: "This work was supported (in part) by Grant # _____ from the _____ agency." Without publications, one soon finds himself lacking both academic advancement and support for research activities. This has led unavoidably to publication of large numbers of skillfully written but pedestrian type papers consisting of little more than restatement of known facts, and to horrendous examples of multiple authorship, in which it is anyone's guess as to who did the work.

The steps necessary to correct his situation are not easy and involve basically three parties: the scientist himself, his peers, i.e., his academic institution and granting agencies, and the journals themselves with their various editorial and review boards.

The scientist has a responsibility to the community of science to report his findings, but above that he has the ethical responsibility to report only meaningful findings that add to the body of knowledge. The findings, if worth of publication, should be reported once and the practice of extracting two, three or more papers from the same work is not defensible.

There are some among us who utilize their positions, prestige or ability to procure funds, to append their names to published works of others. The ethical question involved is obvious. There are others, utilizing laboratory space and research funds, who are merely "playing the research game" and whose motives for being engaged in research are questionable. Unfortunately these people frequently have considerable literary talent and produce large numbers of published papers, as well as annual research reports of sterling quality, neither of which have much value other than literary. The policies followed by the other two parties involved in this situation (the scientist's peer groups and the journal editorial boards) tend to encourage this type of individual and frequently to discriminate against the individual who, while he may be better motivated, may be less articulate or lacking in the necessary "connections" (see example in Greenberg, D.S. "What Happens When a Do-It-Yourself Scientist Looks to Washington for Support"; Science, 146, 621–623, 1964).

This, of course, is quite impractical unless the other two parties concerned change their policies, which have been in large part responsible for creating the present situation.

The academic institutions should realize that their prime mission is not research but teaching. Of course research people have a place in the academic community; the promising student must be stimulated to follow a research career if he has the talent and motivation, and the average student who will be graduated as a practicing physician or engineer is better taught if he is made aware of the current thinking at the frontier of his specialty. However, the fact remains, that someone must teach the facts and concepts constituting the recognized body of knowledge. This prime function of the academic institution is best performed by th ose whose first love is teaching. The present policies either force these people into "research" where they contribute little (except a plethora of piddling papers) or discriminate against them in academic advancement. The use of the bibliography as the determining criteria of the worth of a teacher by the academic institution should be discarded and equal consideration given to an assessment of his pedagogical abilities.

Granting agencies do need an objective yardstick to measure the value of a research project and there can be no argument with the concept that a successful project produces meaningful data which must be transmitted to the community of science. However, it does seem feasible to apply the criteria of quality as well as quantity of publication. Likewise, since all agencies require some annual accounting of progress made in a project, more attention should be paid to the content of such reports. The output of a project is firm data although frequently this may be negative data and non-publishable, or it may be difficult to obtain and insufficient in quantity to justify publication. The investigator should not be placed in the position of having to "make a paper" out of such material. The appropriate place for such material is in the annual report where it furnishes evidence of work being done and of an intent to fulfill the obligations of the research contract.

The research project proposal is basically (1) a question asked of nature and (2) a proposed plan of experimental activities designed to obtain an answer. The acceptance of grant funds on this basis constitutes a contract entered into by the scientist in which he promises to attempt to answer the question by scientific procedures. The research report then should be factual in content and consist of a recounting of what experimental procedures have been carried out and what answers have been obtained. Within recent years granting agencies have seemed to be "giving points" for not only neatness but for literary style rather than for factual content in both grant applications and annual reports.

It is my belief that the journals themselves through their editorial policies are primarily responsible for the increasing number of periodicals published in progressively narrowing fields. An established journal with a prestigious review and editorial board frequently tends to restrict its acceptance of papers to an "in" group consisting of established scientists and their disciples. While this is fine for the "in" group it is frequently not in the best interest of advancement in the field. "In" groups are notoriously intolerant of new ideas generated from without the group, so much so that the normal channel for communication and discussion of these ideas, the published page, is withheld. (The Politics of Science and Dr. Velikovsky. The American Behavioral Scientist, 7, 1963.) The general advancement of science can only come about through scientific evaluation of new ideas, not by automatic rejection of ideas inconsistent with the present dogma. The growth of such ideas and concepts is not halted, but is slowed until a sufficient number of people agree and become involved to found their own journal. Here they can happily talk to each other until they themselves are responsible for the next stage of binary fission. The ultimate absurdity, of course, is each man talking to himself or to one another at which point the whole machine comes to a halt.

The journals and their various policy boards have a tremendous responsibility to maintain clear channels of communication within relatively broad areas of scientific endeavor. The overriding need at the present time is not for more avenues of interdisciplinary communication. In this regard, despite the fantastic increase in the total number of periodicals there are at present only two interdisciplinary scientific journals of stature: Nature and Science. The responsibilities of the publishers, the editorial and the review boards are obvious.

With the expanded growth of science and scientific communication there has come an increased awareness on the part of the non-scientist that these affairs concern him a very vital fashion. Three hundred years ago Charles II recognized the value of science as an instrument of governmental policy and following the founding of the Royal Society, utilized it in an advisory capacity. World War II was a primarily scientific and technological war and has produced a penetrating analysis of the relationships between science and government (C.P. Snow, Science & Government, Harvard 1961). While this is another aspect of communications it is inappropriate for discussion here. However, in the present world situation the strength of a government is directly equitable to the attainments of its scientific community and consequently governments have been lavish with their support of science. While it is frequently forgotten, the ultimate source of the monetary support for practically all scientific endeavor today is public funds derived form taxes levied upon the citizens of the state. This fact, coupled with the increased interest that the non-scientist has in scientific affairs, places upon the investigator an obligation to inform the public of his actions. The widely held concept that scientific research is too complex and too sophisticated to communicate to the layman, has long been used as an excuse by the scientist for not making the effort to try to communicate. Those who did make the effort were labeled publicity seekers and sensationalists. While this may be the official opinion of much of organized science, the public news media do not at all recognize the validity of this concept. The scientist must realize that papers published in scientific periodicals are actually in the public domain and may be quoted in the lay press. It is obviously much more desirable to have correct quotations and interpretation printed than to have the untrained reporter make his own interpretations. The scientist responsible for a publication or a paper presented at an open scientific meeting should consider himself responsible for the interpretation of his findings to interested representatives of the lay press. This is no easy task and requires considerable patience and effort but, most assuredly, cooperation with these people is rewarded by the appearance of more accurate accounts in the popular press. An informed public is much more likely to continue to support scientific research and the problem of the separation of the scientific community from the population at large will be lessened (C.P. Snow, Two

Cultures and the Scientific Revolution,. Cambridge 1964).

In summary then, all of the communication aspects of the scientific community seem to be in need of repair. It will require the cooperation of all parties concerned: the scientists, the administrators, and the journals, to find a way out of the labyrinth. The scientist's assumption of his responsibilities regarding communication with his colleagues and with the public would appear to be the approximate point from which to begin.

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