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BIOLOGICAL RESEARCH STRATEGY AND PUBLICATION POLICY[1]

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THE difficulties in the field of biological publication, which we are to discuss at this conference, are only partly of a technical and administrative nature and therefore can only partly be overcome by technical and organizational measures. The roots of the problem lie much deeper and concern the whole future of biology.

As publication is merely crystallized research, publication ills are largely an outcome of defective research, and bad research, in turn, may be blamed on bad training for research. The publication problem thus appears as just one aspect of a much broader problem; namely, that of teaching and research in biology in general.

If the volume of publication threatens to surpass what is technically and economically manageable, we must decide on how to meet the threat without hampering scientific progress. Our capacity to process, finance, store and utilize scientific literature is certainly not unlimited. Yet, in the past we have often behaved as if it were. We now realize that we are approaching the critical limit at an ominous pace, and we want to be prepared.

Logically, there are several ways of keeping the volume of publication within reasonable bounds. We might reduce the volume of research production, or we might continue to produce at full capacity but publish only part of it; above all, we might increase the efficiency of both production and publication; that is, the yield, per unit of time, of scientifically useful results, and the yield, per printed page, of scientifically useful statements. In deciding just what course to follow in practice, the long-range interests of biological science as a whole should prevail. However, since biology lacks unity of purpose and has never

[1] Address before the Conference on Publication Problems in Biology, held at the Cleveland meeting of the American Association for the Advancement of Science on September 11, 1944.

developed a deliberate and consistent research policy, we simply do not possess valid criteria by which to judge what would, and what would not, serve the interests of biology as a whole.[2] Our opinions on the subject are diverse and often conflicting, and by their very diversity reveal the part which fashions, local traditions, personal predilection and expedieney have played in their molding.

Opinion is too casual and unstable a basis for any science worth its name to rest upon, and if we are really concerned about the future of biological research, the thing to do is to provide it with a firm foundation of principles defining the purpose of research and the methods that have proved most successful in attaining it. The time has come for changing from a drifting to a charted course, not only in the field of publication, but in biological research. This study of the publication problem might, therefore, profitably be made the occasion for initiating a much broader enterprise: a concerted reexamination, reevaluation and restatement of the goals, ways and means of biological research. I shall try to illustrate in the following very sketchy outline how the field of publication would benefit from such a more comprehensive action.

It has become perfectly plain that with the increase of workers flocking into research, the cherished doctrine of freedom for random movements will have to be gradually abandoned. This is not the place to go into a detailed discussion of these matters. All I mean to indicate is that, while the shelling of every new walnut promises to reveal some new configuration on the inside, we shall have to consider how long it is scientifically proficient and economically feasible to continue to shell walnuts in the hope that something worth while will come of it.

There are some basic fallacies current among biological workers. They are: (a) that every as yet unrecorded item is worth recording; (b) that every recorded item is worth reporting; (c) and that every fact worth reporting is worth publishing in print.

There are those who contend—I am sure, quite sincerely—that in science anything at all that has not yet been done is worth doing, and that one fact is as good as any other fact. I respect their viewpoint but can not share it, because it seems to me to be based on an utter misconception of the aim and function of research. It is a perversion of the teachings of the history and philosophy of science to claim that the aim of research is fact finding. The aim of research is knowledge, and as knowledge is not a mere collection of data, research can not consist merely of a compilation of facts. Information does not constitute knowledge. It merely furnishes the food on which knowledge grows. Like food, data must be digested and assimilated before they can be incorporated in the body of knowledge. Like food, they must lose their identity in the process of being assimilated. To expect that a hodge-podge of miscellaneous information is going to sort and order itself automatically into knowledge, is like the illusion of the medieval magician who expected a mixture of chemicals in a retort to organize itself into a homunculus.

Now, if facts are but the food on which knowledge grows, is it not about time that we concern ourselves a little with the dietetics of knowledge to find out which foods are healthy, how they are to be prepared and what constitutes a balanced diet? Certainly, as bulk is no criterion of nutritive value, so the volume of data being piled up can be no measure of the progress of science. The primary aim of research must not be just more facts, but more facts of strategic value.

By strategic value I mean that property of an observation or experiment that leads to the clarification or solution of a problem, to deeper insight into a phenomenon, to the linking of previously unrelated facts and ideas, or simply to the birth of a new problem; at any rate, leads to some end other than the bewildered question, "So, what?"

The crux of the problem, therefore, is to make research workers more strategy-conscious. How? Most of us are vaguely conscious of an unwritten code of scientific strategy, which has been passed down through the medium of example and personal contact from teacher to student. But now that scientific research is assuming mass-production dimensions, this mode of transmission is breaking down. More and more students leave our classrooms for so-called independent research, with barely the faintest notion of what science is all about and of how best to promote it. While they are learning tactical tricks, they rarely come to know those rules of scientific strategy which could give aimfulness to their future research.

Now, if the patriarchal system of instruction, which could give a student perspective, is forced out of operation by sheer pressure of numbers, then we shall have to do what all communities and tribes had to do when they outgrew the patriarchal state in which unwritten convention could pass for law: they had to codify the law, and so we may likewise have to formulate and codify the rules of scientific research. Unless we do, we can not expect an uninitiated generation of to-morrow to observe these rules; for they will not know them.

Yes, I am envisaging a written code of scientific research to serve as a manual of scientific strategy and as a standard frame of reference for the rating of scientific products, for the guidance of research workers, editors and administrators alike. However, in contrast to a code of law, with its powers of

[2] P. Weiss, SCIENCE, 95: 32, 1942.

enforcement, the implementation of this scientific code must be left to the conscience of the research man himself. Conformance must be a matter of his sense of responsibility and judgment, which we, as teachers, must strive to develop, and there must be no coercion. Some will continue to waste their time and somebody's funds on petty tasks, but this will still be much less costly to science than would be any attempt to prescribe certain ways of research and proscribe others. On the other hand, unless we do exercise and teach self-discipline, restraints may be forced upon us from the outside by agencies of much less vision and competence. And this we want to avoid.

The suspicion that I am here advocating a subtle scheme to deprive the scientist of his freedom of decision may be allayed by reiterating that, quite to the contrary, I want us to provide him with a more rational basis on which to make intelligent and responsible decisions. Even now freedom of choice in research is restricted by such factors as pressure of opinion or authority, accidents of training and circumstances, subsidizing policies of institutions and foundations, practical needs, and others. These are extraneous influences. How, then, could any one object to letting biology develop a directive from its own inner resources, namely, the envisaged articles of strategy of scientific research? The quality of both research and publication could only gain from such an educational campaign, which would reduce the aimlessness of thousands of sorcerer's apprentices now busily engaged in swelling the flood of literature. So much for long-range policy.

Of immediate benefit would be the incorporation in our educational program of some instruction to graduate students on how to organize their material for publication. We teach our students to use only clean instruments and glassware, but we do not always insist on similar cleanliness of their mental and verbal tools. We expect them to be meticulous in their observations, manipulations and measurements, but we often let them get away with a muddled presentation of their results. Would it not be wise to develop their sense of proportions and, for instance, call their attention to the fact that they only hurt themselves and irritate the reader by such common practices as camouflaging important results by setting them down in an underbrush of irrelevant trifles?

We must also help the student to steer clear of two extreme and opposite attitudes which he is prone to adopt. On the one hand, there are those who feel that the main thing is to do the work and that publishing it is a side issue. On the other side of the picture, there is the understandable tendency of some to recite for the reader all the little incidents of their research, which have no scientific interest. Adding to this the tendency of "padding" for the mere sake of attaining impressive volume, it can be seen that a lot of improvement might come from proper education at the research end, long before a manuscript reaches the editor.

I have been talking of the student, but the student of to-day is the research worker of to-morrow. If there is nothing we can do about the past, we can at least provide for the future.

Cleanliness of the mental tools and mental operations in the description of scientific results would go a long way in saving publication space. A concrete step in this direction could be made by insisting on more uniform and consistent terminology. If one compares the care with which terminology is treated in physics, and nomenclature in taxonomy, with the terminological carelessness in some other biological fields, the contrast is appalling. Three main improvements could be made with little effort.

(a) The creation of new terms or symbols, even if only for temporary use, to designate complex phenomena or situations, which otherwise would have to be circumscribed at each mention by long-winded phrases, should be encouraged. Authors should acquire the habit of giving a vocabulary of their main terms in the first part of their paper, and then sticking to it.

(b) The creation of new terms for phenomena for which there is already a good old term should be discouraged.

(c) Use of the same term in different meanings by different authors is a common source of controversy, leads to polemics, and should be eliminated.

The time seems ripe for various biological disciplines to attempt some terminological house cleaning, and it might be profitable to encourage the setting up of commissions in each field to attempt a standardization of terms in that field, possibly to be incorporated in sectional dictionaries to which reference could be made whenever the terms are being used in publications.

More serious consideration should be given to the state of digestion at which research results or theories are deemed ready for presentation. This is one of the knottiest problems because it involves so many considerations other than the sheer interest of science; for instance, competition, priority, baiting of funds, institutional publication pressure, etc. This problem reaches over into that of the technique of publication, and the solution may lie in giving factual data which deserve quick diffusion a different treatment from other materials in which the advantage of maturation would outweigh the disadvantage of delay.

One could envisage a bulletin service through which raw research data would be communicated to only those research workers known to be engaged in related work or specifically requesting them. Routed through

a central clearing agency and reproduced mimeographically or by other cheap processes, such bulletins would not only speed the dissemination of information, but would make information available that otherwise would have to remain unreported. Some specialized and highly active branches of biology have already adopted such a scheme unofficially, as it were. There is no reason why it should not be made universal and be given bibliographic recognition. Lack of editorial screening is counterbalanced by the author's awareness of the fact that his information will reach the most critical judges, namely, his peers in the same field.

Relieved thus of the congestion caused by the growing tendency of authors to present their work in installments, publication in print would return to its original function of reporting work which has been brought to some sort of conclusion. Printed publication would be reserved for results and thoughts worthy of more general circulation and of permanent preservation and would no longer dignify the ephemeral.

Let us now turn to some further aspects of the publication problem which would be materially affected by whatever policy biological research would choose to follow.

(1) Biological disciplines have gradually and imperceptibly changed their content and methods so that many of the historical designations which now delimit fields have lost their former meanings. Other fields have not changed in content, but in emphasis. While there has been a tendency to accommodate new trends by establishing new journals, there has been less evidence of re-orientation among the existing ones to adapt themselves to the changes which have occurred. No satisfactory solution of this problem will be reached unless biologists get together and rearrange and reallocate their various disciplines. This could, of course, come only from free, wise and cooperative planning.

(2) Many existing journals are distinguished by their history. It would be deplorable if attempts at standardization and uniformity were driven to the point where individual organs of publication would lose their personal character. However, with all due reverence for historical tradition, certain incongruities of the past are in need of correction. Most serious among these are the duplication of effort and the overlap of domain between journals covering nearly identical fields in a spirit of rivalry. We shall not be able to change the weaknesses of human nature, but we do not have to accord them a prominent place in the determination of our scientific policies.

(3) Institutional administrators, government bureaus, etc., will have to be persuaded that the number of printed pages or articles is no valid measure of a man's productivity and usefulness. That some waste of print can be ascribed to institutional publication pressure is generally recognized. What is not usually pointed out in this connection is that we can not expect administrators to give up the convenience of counter and yardstick as long as many of our scientific societies apply this criterion in ruling on admission to membership. The question of what other criteria to substitute for volume of publication involves the fundamental problem of how to appraise research efforts and assess research achievements, and this again can not be answered otherwise than in terms of certain agreed upon standards of the goals, ways and means of biological research.

(4) Universal adoption of the policy of making publication an integral part of a research project, so that at least part of the publication costs would naturally have to be carried by the institutional, foundational or private funds supporting the research might cure some of the ills. Psychologically, it would make for greater care in publication. Economically, it would discourage expansiveness. Publication is as inseparable and as legitimate a part of research as is the developing of an exposed plate in photography. The objection that funds would thus be deflected that otherwise might be used for productive research is not really serious, for anybody who takes the trouble to look into the concrete figures will soon convince himself that publication costs, by and large, amount to only an insignificant fraction of the total cost of a research project.

In conclusion, our present system of publication in biology, while perhaps capable of temporizing a little longer with the aid of technical improvements, subsidies, stronger editorial control and similar expedients, is admittedly unprepared and unsuited to serve the needs of a science which grows as rapidly and vigorously as biology does. Our collective responsibility for the future of biology forces us to take notice. To take appropriate action will require much wisdom and experience, but above all, vision. It is my plea that whenever such action is taken, the publication problem be dealt with not separately, but as part of the larger problem of biological research, of which it is a natural branch. I feel that education for research, planning of research, prosecution of research, financing of research and publication of research, should each be viewed from the perspective of the whole complex. To be able to do this, we need a more explicit and consistent research policy than we now possess, and it might be a timely undertaking to formulate and codify the unwritten rules of scientific—more particularly, biological—research strategy, for the good of research workers, teachers, students, administrators, legislators, publishers, editors, donors and all others whose actions or inaction may affect the future of our science.