SCIENCE

Friday, May 14, 1920

CONTENTS

MSS. intended for publication and books, etc., intended for review should be sent to The Editor of Science, Garrison-on-Hudson, N. Y.

THE STIMULATION OF RESEARCH AFTER THE WAR[1]

At the time when I received from Dr. Cook the notice of my assignment to this topic, the phrase "after the war" seemed to be of rather indefinite and at least possibly remote significance. There was a chance at least that anything I might say would have time to be forgotten before its timeliness would be put to the test.

To-day we are face to face with the problem of stimulating research in this new epoch, which the political and social cataclysms of the past four years have ushered in. I am not one of those who are inclined to minimize the significance of the period through which we have just passed in its relations especially to the advance of knowledge. It is a reproach to biological science that we are not able to predict evolutionary trends, but it is perhaps on the whole a hopeful sign that we frequently differ so widely in our judgment of the significance of current events, and of the world problems which the great conflict involved.

It is for us, who conceive biology as in any true sense the science of life processes and activities in plants and animals alike from the lowest to the highest, to look to our fundamental conceptions and take thought of the responsibilities which our scientific pretensions involve. In my opinion we may find in the final assessment of responsibilities for the world war that a pseudo-scientific dogmatism, and the promulgation in popular form of superficial and wholly misleading views of such evolutionary concepts as the struggle for existence and the survival of the fittest, have had a share, both in the production of the false national and racial ambitions which lead up

[1] Read before Section G, American Association for the Advancement of Science, at the Baltimore meeting in the symposium on "Research after the War."

to the war, and the savage bitterness with which it was fought by its instigators.

It certainly behooves us as evolutionists to endeavor to clear up in a fashion not yet, in my opinion, adequately accomplished, the relation of Darwin's great concepts to such struggles, not that I assume that any one has the illuminating word now ready to be spoken. It is sufficiently obvious that a vast amount of further study of the problems and relations involved in the evolution of races, states, societies and civilization, as well as human individuals, is necessary before the concepts of struggle, progress, survival, etc., will attain a clearness which will finally prevent their use as the shibboleths of barbarism and savagery. As scientists we must all agree that in increased devotion to research and in the growth of that passion for understanding the living organism, its environment, its origin and its possibilities, our safety for the future lies. No ready made or lightly thought out theories will suffice. The danger from lightly conceived and lightly held political evolutionary theories promulgated by visionary and ill-trained statesmen and politicians, was never more real. The misuse of scientific half truths, misleading phrases and superficial analyses, was never more threatening than just now, when the central empires are endeavoring to regain their poise after their debauch of mad ambition. It is for scientists in the future to set an example of discriminating judgment and careful analysis of evidence of which they have not hitherto been capable.

The practical issues of the day we may say in a sense are still in the hands of men rather than of scientists and will be met and their problems solved instinctively and in accord with moral aspirations rather than by the application of established principles and concepts as to the nature and possibilities of further development of human societies and civilizations. The great men, the leaders, are so by virtue of an instinctive rather than analyzed feeling as to what is possible and achievable in the given conditions. The pragmatist with his worship of the man in the street may feel sure that this will always be the case, but in this assertion he loses whatever of truth there is in his philosophy and becomes the plain and familiar dogmatist of the past.

It is for us to see that a continually increasing number of those who are great leaders by virtue of their instinctive grasp of the significance of human movements and world situations, are also able to avail themselves of an increasing mass of analyzed and tested data bearing on the problems of life and evolutionary progress.

As a physiologist I may note that the stimulation of research does not involve the production of the fundamental motive power back of the advance of knowledge. Physiological stimuli liberate energies accumulated in the organism they guide and regulate activity but furnish no appreciable energy for its maintenance. They initiate reactions but do not cause them. The familiar illustrations of their nature and relations to organic activities are the pull on the trigger or the engineer's hand on the throttle. If there is no research mechanism well stocked with mental energy stimulation can do nothing. It may even weaken and destroy if the energies for normal reaction are not available.

Physiologically speaking the regulative stimuli, those wonderful activities of the enzymes and hormones which can accelerate or retard, direct and coordinate reactions so as to produce the complex and wonderfully adaptive phenomena of organic growth and behavior are those most interesting in present-day biological research and furnish the analogies on which the widespread demand for better control and coordination of scientific research is based. Why is it not our highest function as scientists to so regulate control and coordinate research that each problem shall receive its fit proportion of attention so that now when the world seems to need above all food supplies, a speedy physical rehabilitation to repair the wounds of war and a special set of political and social maxims for the use of nations in the transition from autocratic to more democratic governmental forms the whole energies of the world of science, political, social and biological, can be turned to producing these desiderata. This would be efficiency in the German sense and a reasonable regard for such demands is necessary and desirable. There are, however, it seems to me, some more fundamental viewpoints that under stress of immediate physical need may be overlooked. And first among these is the fact that as noted stimulation, initiation, regulation, coordination, do not furnish motive power, imply indeed an exhaustion of energy rather than its increase. The withdrawal of men from the active prosecution of their own investigations in order that they may spend time on commissions, boards and other executive agencies for controlling and directing the research of others is doubtless a necessary evil but is in danger of being regarded as a useful end in itself. The activity of such agencies in securing funds and thus contributing to the motive power back of research is quite another matter but even there it is deplorable when a man of first-class talent withdraws from his own work and devotes his energies to obtaining financial support for a group who thus become in a sense his subordinates. If he makes efforts to direct and coordinate in detail the activities of such a group with their diverse capacities and widely separated lines of activity, his influence may even be positively harmful. The importance and advantage of cooperation in research have been very adequately and effectively presented from many quarters. The socialistic trend is obvious here as in so many phases of modern thought and action and it is at least worth while to consider what may be said from other viewpoints. Of spontaneous cooperation individually initiated there can not be too much, but if it becomes the fashion to work only in groups and on problems in which group interest can be aroused in my opinion we shall be disregarding many obvious teachings of experience. You may gather from this that I am not hopeful that research can be socialized in any very significant degree. It seems to me that this is especially true of those higher efforts of the human mind when it actually breaks over age old barriers or enters on wholly new and hitherto unsuspected fields for thought and action. Routine solutions of definite and simple problems can be achieved by the factory and piece work system but the highest achievements of the mind are always individual and seem frequently to mock all attempts to relate them to the environment or the period of their occurrence. In my opinion the distinction between routine research on the problems which are already clearly stated and for which methods of attack are obvious from data available and the studies which really open up new fields of hitherto unperceived interest and importance or solve problems long given up as ridiculous, is more significant than that between so-called pure and applied science. This distinction, it seems to me, has been overworked at least in its relations to the development of research. On the other hand, whether or no we conceive ourselves as either practically or theoretically able, by taking thought, to influence the course of events, it may tend to clearness of thought about what is actually going on in these times of turmoil and excitement if we recognize more fully that there are these two types of research activity, each with its own clearly marked prerequisites. The war experience of the nation has shown plainly enough that when the money and incentive are at hand staffs of experts can be organized and laboratories equipped on short notice which can solve a vast number of important problems relating to the chemistry of dyes, high explosives, gas warfare, aeroplane engines, etc., with a high degree of promptness and efficiency. I am sure too that we should be mistaken if we expect from such efforts only new applications of already known theoretic principles. We are perhaps quite as likely to arrive at theoretically significant new conceptions of matter and energy in the study of the vastly practical problems of static disturbances in wireless telegraphy (which problems by the way the newspapers recently announced had been solved by work in the laboratories of a great commercial corporation) as in the study of the wave theory of electricity as such with no practical problem in mind.

A vast amount of useful and theoretically highly important work is being turned out yearly by investigators who either have definite problems assigned to them by others or who see a problem so clearly that they can at once present it to a board or committee or executive agency in charge of funds, and immediately win financial and other support for its study. We can not have too much of this sort of work and most of the research agencies now under consideration, such as the National Research Council, the various scientific departments of the national government, the research departments of the agricultural experiment stations, the great research institutions and commercial laboratories, are all well calculated to foster and develop work on problems whose possibility of solution is fairly evident or whose significance is already so fully understood that their study is suggested even though they seem for the time insoluble.

It seems to me equally obvious, however, that these agencies do not provide at all adequately for the second type of problems, those which at present lie outside of and beyond the domain of clear thought at least on the part of the majority of intelligent people, and this again quite regardless of whether the problems seem to relate to practical matters or to have only a theoretic or philosophical interest. I think we must admit that many of the great advances in knowledge have been made by some one's breaking over these bounds of the average scientists thinking and experimenting and attacking some problem which had been quite unthought of or was regarded so unclearly as to be considered wholly visionary, impossible of attack or even ridiculous. To illustrate, I think we must admit now that the Wright brothers were more favorably situated for the solution of the problem of human flight in heavier than air machines than was Langley. Langley was in a great government supported institution with supposedly all the resources for the attack on the problem from the mathematical, physical and experimental mechanical side at his command. The Wrights had to develop financial and other support as they went along. The case illustrates perfectly the weakness likely to inhere in governmentally supported research. Langley in his position, could not afford repeated failures in experimenting on a problem which was still regarded as chimerical if not ridiculous by the great mass of intelligent people of his time. The Wrights, working on their own initiative, with everything to win by final success and little to lose by temporary failure, with no explanations to make to governing boards or scientific societies, were in a vastly more helpful and normal environment, it seems to me, for establishing a new point of departure in a new field of activity. At least the Wrights succeeded and Langley was unable to push further his partial success in an achievement which if it had been followed up might have won him the distinction which went to the vastly less well supported efforts of the Wrights. Langley in his position under the eye of the government could not feel himself able to support temporary failure or even partial success, though in reality the endeavor was worth prosecuting through a thousand failures.

Another instance is the historic one of Pasteur's discovery of the relation of microorganisms to fermentation and decay. No more fundamental and enlightening work has been done in the whole history of biological research. It gave the final quietus to the doctrine of the spontaneous generation of germs in decaying organic matter and laid the foundations for a whole series of discoveries in theoretic pathology as well as applications in medicine and the practical arts. Yet if we accept the current accounts of the attitude of Pasteur's colleagues and the general public to his earlier work in these lines, we can see that it would have been quite impossible for him to have gained support in advance for his researches on problems supposed to be settled, or quite insoluble.

Pasteur, like the Wrights, won his way to popular support, but it is certainly a question whether the work, brilliant though it is, which has so far come from the great institute founded in his honor equals in significance the work done by the great master.

It is the despair of organizers of research that work of the first rank such as that of Pasteur and Darwin shows so little dependence on facilities, equipment, etc., and it is always to be remembered that the problems on which they worked, and the results they achieved were not such as would have enabled them to win in advance either financial support or substantial recognition by the general public or their scientific colleagues. Pasteur could win his institute only by achieved results, not on an advance program for laying the foundations of a new science of bacteriology. Darwin could hardly have made the origin of species seem a promising and feasible field of research before he had the evidence of the efficiency of selection which made the whole subject of evolution a new and vital one. It is hardly conceivable that Darwin himself would have been able or willing to attempt to formulate in advance a project which would have covered the main field of his researches. He was working out into lines of thought and experimentation where clearness and feasibility became obvious after, and not before the event. In these days when in certain quarters it is assumed that every research must be outlined and made to appear reasonable in advance, it is worth while to remember that really new fields of study are very likely to look unpromising if not hopeless or ridiculous to the executive mind. If we require for every research project that it appear promising and workable within a so-called reasonable time, we put a premium on problems of the easy and less fundamental type. There is also a psychological factor here. The man who conceives vaguely at first a great new possibility in the advance of knowledge, is sometimes quite disinclined to talk about it merely because it seems so vague, hopeless, and perhaps even ridiculous. If we organize research to such a degree that it shall become the customary, if not the inevitable routine for every worker in an experiment station or research institute to feel that he can only work on problems which can be made to appear plausible and possible of solution in advance, we shall, as in so many socializing schemes, put a premium on mediocrity, and penalize real originality of the kind which has led in the past to many of the really great advances in knowledge.

It is, however, always to be remembered that there is probably a greater practical danger of our institutions of research becoming the refuges of incompetents and visionaries than that their methods will nip incipient genius in the bud. The illustrations I have used are, of course, extreme cases, and represent the exceptions rather than the rule as to the mass of scientific work now being done and which has been done in the past. It may well be said that the Darwins and Pasteurs will take care of themselves and that our plans and organizations should be for the average run of scientific workers. Still this objection overlooks the possibility that the case of the scientists, like that of other matters of heredity, can not be adequately analyzed on the basis of the simple assumption of "presence and absence"—in this case of genius. There are many grades of research ability. I have attempted to differentiate two classes of problems: first, those clearly conceived, and appearing more or less readily capable of solution; and second, those which, though obviously of vast importance if solved, are imperfectly conceived, or appear hopeless, or even fantastic. Still it is obvious enough that many if not most scientific problems lie somewhere between these extremes. Any problem which is worthy of serious effort will probably involve in its solution many lines of effort which were not foreseen at the beginning, and many important problems will seem, even to their projectors, too hopeless of solution to have any wide appeal, or to win adequate cooperative support, or even the approval of colleagues or superiors in attacking them.

In considering the whole problem of the stimulation of research we should recognize the limitations of controlled and directed effort, and learn if possible whether in our schemes provision can not also be made for that free and untrammeled environment where personal inclination and initiative are the major factors. Control and executive supervision become necessary in direct proportion as research is paid for directly as such. This is inevitable if government bureaus and research institutions are to be sure of some return for their money. It is the special advantage of the universities that in them research can in a sense be regarded as a utilization of by-products—not infrequently in modern industry a very important source of real profits. The member of a university faculty can give a return for his salary in the form of teaching—the relatively prosaic, but important work of passing on to the new generation the achieved results of the science, literature and arts of the past with all which that implies of stimulus and moral development. This is his modicum of contribution, but beyond this, the spirit of the university, the environment of young students, the seminar, the scientific conferences, the intercourse with colleagues in related but diversified fields—all these are stimulants to research of the highest efficiency, and constitute at once that free and untrammeled environment which incites to effort in purely ideal lines where no consideration save the intrinsic interest of the work in itself, and the desirability of the solution to be attained need intrude. The universities because of their functions in teaching, are the natural homes for research on problems whose appeal is to the desire of the human mind to understand and control its environment.

I need hardly stop to add that all universities as yet do not furnish in the highest degree possible this sort of environment. It is enough for us that there is no intrinsic reason why they should not all become such centers of stimulation and motive power in research. And for the warning of those who are too much given to reforming that which is already reasonably good, be it said that the tyranny of majorities and of professorial trade unions is quite as likely to meet with passive resistance and the undermining effects of indifference and superior interest in the real work of teaching and research, as the attempts at financial, social, intellectual and executive overlordship which have in the past been regarded as the most insidious foes of our much-prized and too frequently little understood academic freedom.

The further fundamental consideration which confronts us is that after all research is hard work and that the most important stimulus thereto is the force of example. After the exhibition of the past four years it is hardly necessary to emphasize that man is still very much of an animal. One of the oldest if not the primitive mental trait is imitation. We shall stimulate research in direct proportion as we plunge into it ourselves each on the problems that look large and appeal to him especially. With the socializing tendencies of the present day and the vast emphasis which is being laid on organization it may sound like serious heresy but I am willing to stand for the proposition that in peace times at least no one is justified in assuming executive work or work in the planning and direction of the research of others to the exclusion of his own research work. On those minded to do so I would urge first at least the need of research that the justification of their viewpoint be made more clear than it is at present. With all our present-day divergence of views we can perhaps agree that the advance of knowledge in the future depends most on the possibility of winning the brightest minds of the rising generation for research and for accomplishing this it seems to me the most important factor is that we convince our students by our own examples that research is really an absorbing and satisfying occupation that it is interesting in itself even independently of the immediately obvious value of the results obtained. Not by preaching research or organizing research or talking about the stimulation of research, but by showing a deep, insatiable curiosity about the things of nature and of life, we shall advance and win others to engage in the pursuit and practise of knowledge

R. A. Harper
Columbia University

JAMES M. MACOUN

James M. Macoun, chief of the Biological Division of the Geological Survey, Canada, died January 8, 1920, aged 58. He was well known as one of the best informed systematic botanists, not only throughout Canada but also in other countries, and was an expert on the fur-seal industry.