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# The Nature and Need of Educational Research

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THE MOVEMENT FOR FEDERAL SUPPORT of scientific research began last summer with the physical sciences, but hearings before a subcommittee[1] in October made it clear that the social sciences had their own contribution to make to national welfare and security. The proposals currently being considered in the Senate and being discussed in scientific periodicals are of immediate concern to education as one of the social sciences. It is the purpose here to give a preliminary sketch of changes in the form of support for educational research which are needed if this field of research is to contribute appropriately to the progress of our society.

The main problems of education cannot be solved with research facilities which are now available. Under existing conditions our central problems are not even likely to be attacked—certainly, not for a long time. Most of our research is being conducted by individuals, working usually alone and employing toilsome procedures. The approaches to educational problems are commonly made under conditions suited to historical work and individual deliberation. The effect of these conditions on the kind of work done is to make impracticable the gathering of extensive field or laboratory data; to preclude the study of long-time growth problems; to exclude large-scale geographical problems; and to minimize the cultivation of those technical insights and sharp penetrations which come from group discussion and cooperative endeavor. The effect of these conditions on the quantity of work done is to obtain from each individual research worker only a small part of his maximum potential contribution, because of his having to do so much of the work personally, and to obtain in the aggregate a quantity of educational research far below the level necessary for the proper functioning of our society.

In terms of sheer number, the large proportion of our studies are being made by graduate students writing Masters' and Doctors' theses. And, because of the professional character of their training, they are likely to be urged to select problems having a connection with their field of practical work, rather than choose problems which are designed to test hypotheses and round out theory. Professors interested in research work diligently, possibly with some relief in their teaching schedule, but usually singlehandedly, and often with nothing whatever in the way of funds for the purpose. There are a number of research bureaus connected with higher education institutions, and these set an example which affords hope for the future; but they are far too few in number and have too small a budget. The U. S. Office of Education has been concerned largely with reports, summaries, and survey data. State and city research bureaus are concerned with immediate and usually local problems.

The great majority of persons who are interested in relatively fundamental research labor under a pattern of research support which evolved during an earlier period. We are now attempting to carry on under this pattern while living in a world of quick change, rapid progress, and tremendous competition. Our universities, largely because of financial necessity, are continuing a tradition of individual scholarship and personal research which is delightful to those who enjoy the stimulation of participating in it, but which is entirely inadequate to serve the needs of modern social progress. Our Western World is moving rapidly ahead—with our aid if we can keep up, with our hindrance if we cannot. We have recently been talking much about going forward in the postwar world; but if we are to have in this emerging period an education which is in any way distinctive, we will of necessity have an expanded and quickened research program commensurate to the task of serving an ever more intricate and kaleidoscopic civilization. There are increasingly great and increasingly acute problems which depend for solution on educational research.

There is still largely prevalent the notion that research in the social sciences is bibliographical work.

[1] See *Science legislation; analytical summary of testimony.* Appendix to Report from the Subcommittee on War Mobilization to the Committee on Military Affairs, U. S. Senate. (Subcommittee Monograph No. 5.) Washington, D. C.: U. S. Government Printing Office, 1945. See also *Hearings on science legislation* (S. 1297 and related bills), which are now available in published form in five volumes.

The library is a basic provision, needed in all sciences. But in those centers where social science research is being carried on vigorously it is an active, aggressive enterprise, taking its orientation not from books but from social life as it is being lived. It deals with human beings currently in action. It seeks data on the problems faced by individuals who are still looking forward. It is concerned with yesterday as a means of understanding today, and with today as a means of determining tomorrow. Students must turn to the written record to know what has previously been proposed and tried; but scientists must turn to the laboratory and to the field for fresh data and final tests.

There is on the part of many physical scientists the feeling that social problems are solely matters of conflict between interests which are to be "solved" by methods of rhetoric or the show of force. Social problems typically do involve competing sets of values, and their scientific study is likely to include certain elements which are absent from physical science. We cannot hope, and we do not wish, to make social science in all respects the same as physical science. But we can hope, and we do desire, to make social science as effective in its sphere as physical science is among physical things. We may point out that social science has been successful, in the same way that physical science has, in noting and quantifying many regularities in the realm of human living and in showing their dependence on measured amounts of conditioning factors.

On the applied side, the social and physical sciences are again similar. The physical scientist has not succeeded in eradicating friction, but he does hope to avoid explosions. So with the social scientist. In the physical field there are no fewer opportunities than in the social for the opposition of strong forces or for their dangerous disproportion. In both fields we seek forms of operation which will regularize and channel the expression of forces so that desired results may be obtained, with increasing efficiency and ever more certainty. The industrial physicist or chemist can tell management what certain amounts of certain substances will do under certain operating conditions. So the social scientist can explain to deliberative bodies the steps necessary to produce certain desired results; and knowing these things, the group can think more clearly and decide more wisely than if the issues are hazy, the elements unanalyzed, and the whole matter left to the relative effectiveness of competing vocal cords.

The social scientist must collect raw data and then analyze those data in order to produce specific or generalized truths. He commonly needs access to groups of pupils or adults, usually under specified conditions—perhaps controlled, perhaps experimental, perhaps representing differing cultures; he needs access to tabulating and calculating machines for statistical analysis; he needs professionally trained assistants, working in something of a stable organization, to aid in carrying on the work of gathering, classifying, and analyzing large masses of data.

Let us look a little more closely at the sources of data and the kinds of problems for which they serve. Roughly, social science data lie in three directions. The first general source is the field study. This may embrace any group of persons, in this country or in other lands, representing modern or primitive cultures. Studies of how people live, grow, and implant their culture in each oncoming generation, under different physical conditions, economies, customs, and ideals, provide descriptive and comparative data of great value. They reveal the existence and relative importance of factors which, under more limited conditions of observation, are either ignored or assumed not to exist. Such studies deal usually with the large outlines of social and educational structure, give us many suggestions as to the variety of patterns which are workable, and prevent us from drawing false conclusions.

In these field studies we may be interested in general, over-all descriptions, or we may wish to single out certain factors for special study. We may make a quantitative survey for analytical, normative, and comparative purposes, or we may be concerned with the more immediate interaction between forces and consequences, leading to causal-comparative, ecological, and correlational studies. For example, one may desire an inventory of the learning problems faced by children from bilingual homes of specified socio-economic levels. One may seek to ascertain what factors discourage young people of high native ability from continuing through high school and college. One may ask: "To what extent and in what ways does education pay, granted equally favorable economic conditions?"

A second source of data is the experimental study Here we have typically the laboratory and the controlled classroom or experimental school. Data from experiments give results on principles of learning, or the efficiency of different methods of teaching, on the value of different kinds of incentives, and so on This is the type of study which, in the social sciences corresponds most closely to typical work in the physical sciences. It is not, in our field, any more scientific than other forms of research, nor is it necessarily more valuable; each type of work is essential for its own area of understanding. For certain social experiments it is necessary again to go into the field since problems of morale, of loyalty, of motivation call for large groups of persons under conditions gen

erally representing their ordinary walks of life but with experimental factors present.

The third large source of data in social research is the longitudinal study. This approach yields data on growth, on learning over extended periods, and on the long-time interaction of varying factors. Both normative interest and experimental attack are here represented, with another dimension—time—added in. The longitudinal approach is necessary to obtain natural case histories, such as those needed by the physician to know the normal course of a disease, those needed by the sociologist to relate antecedents and consequents in the unfolding of delinquent and criminal careers, and those needed by the educator to ascertain whether precocious and brilliant youngsters make good in later life. What are the case histories of mature personality maladjustments, what experiences lead up to them, and what are their early signs? How helpful are certain of our common forms of education in later life? Of those students trained in a specific vocation in high school, how many later work in that field? In addition to such natural histories we need results of long-time experimentally controlled factors. What is the effect of a learning experiment carried out, not for three or six months, but for twelve years of elementary and high school education? Can intelligence be improved, or is it constant for a given individual? To what extent can education for specific ends be made to carry over into adult habits and community affairs?

When one compares the work necessary in gathering data from these sources with the normal classroom-office-library routine of college professors, the difficulty of doing fundamental research becomes clear. There are but few instances in the social sciences, and especially in education, where professors are given the funds and other administrative concessions necessary to do anything adequate in the way of gathering and analyzing fresh data on problems of some magnitude. Individuals with scholarly inclinations do find worthwhile problems of the type and size they can attack singlehandedly; but all such problems combined and worked on indefinitely will still not make up the range of essential research, for the most fundamental problems cannot be attacked by individuals working alone. The painstaking development of insights and the thoughtful spinning of new theories are necessary and can be done in part by individuals; but such work is most prolific and helpful when it grows out of, and is supplemented by, a large amount of empirical study to guide thinking and to try out the generalizations being put forward. Science does not progress without imagination and venturesome suggestion, but neither does it make solid gains without gathering data to test each plausible proposal. Rationalization precedes empiricism at constant peril.

The means for bringing the routine of research-minded professors more nearly into line with the accelerated production rate demanded by modern life is to be found principally in terms of adequate funds. Colleges and universities cannot provide the money necessary to capitalize on the existing research capacities of their faculties without either increasing their income two- or threefold or greatly curtailing their teaching. To embark on the latter course at a time when our civilization is calling for more people with more education would be unreasonable. Universities must still honor their first obligation to teaching. But meanwhile society has a crying need for productive work in the social sciences which cannot be satisfied by existing patterns of research support.

The effect of liberal amounts of money on the rate of research production has been abundantly demonstrated during the recent war.[2] The newspapers have released accounts of many new inventions. Most outstanding is the atomic bomb which, according to newspaper reports, cost some $3,500,000,000. In the biochemical field, a substitute for quinine is a striking example of what can be done under urgency and financial adequacy. According to reports,[3] $5,000,000 was set aside for this work, with the result that we now have several drugs *superior* to quinine in the treatment of malaria. In both of these instances, and in numerous others, the desired results were achieved because good research men were brought together, were given a liberal budget, and were told to produce. Results were demanded, paid for, and delivered.

One may reasonably ask: "Why not spend comparable sums in an effort to avoid any occasion for using the atomic bomb?" That is certainly the hope of all people. The only answer is that our society has not yet come to think in terms of social science. We have confidence in the physical and biological sciences to the extent of the expenditures reported; social science has had no such amounts given to it by which to produce spectacular results. We would not expect to get impressive returns in the physical sciences on a niggardly outlay; neither can we expect to do so in the social sciences. It is becoming literally true that society can buy almost any result it wishes; it cannot, however, do so without paying a commensurate

[2] It has been suggested by M. L. Tainter in "An industrial view of research trends" (*Science,* 1946, **103,** 95–99) that research productivity *per man hour* of research work during the war was not, on the average, increased. That scarcely has bearing on the fundamental point that by employing more persons more research will be undertaken and completed, and that by grouping workers and employing assistants more basic problems will be undertaken. The research program *per calendar year* was amazingly stepped up during the war.

[3] Associated Press dispatch by Frank Carey, 3 January 1946. See also "Wartime research in malaria," by the Board for the Coordination of Malarial Studies (*Science,* 1946, **103,** 8–9).

price. These comments do not mean that research comes in small packets, already on a shelf, to be taken down when wanted, but they do mean that there can be great faith in the creative genius of man when his working conditions are good and when the need is urgent. We have the men; we have the need. The question is whether we shall supply the essential working conditions.

One may ask what amount of money would be called for in order to carry on the research in the social sciences needed by our society and by the world. The only correct answer is that the amount depends on the results wanted and the speed with which they are wanted. It is possible, however, to give something of a picture of the structure of research provisions which would seem, on a priori grounds, to be desirable; and it may be reasonable to make certain suggestions as to amounts that could be used to advantage under present conditions. In doing so, the present writer can give only his own ideas, and those concern only educational research; other social fields must produce their estimates of needs.

It seems clear that allowances must be thought of in terms of different levels of work, called for by different types of problems or by different degrees of intensity with which it seems desirable to pursue different problems. Normally there would be a small number of very large—or very important—problems which would demand large expenditures. These can be estimated only roughly. At any time there might be from one to half a dozen projects going forward, each of which would involve from $75,000 to $500,000 or more, though not necessarily this much each year.[4] These would probably be nationwide undertakings, though not all of the survey type. There are large experiments to be undertaken, with large numbers of persons, which would be costly but are essential to social progress and control.

On a more limited level, research centers of the types which have proved so valuable in contributing to educational research in recent years should be established in some 50 to 100 places throughout the country. One need but mention the Institute of Educational Research, with its several divisions, at Teachers College, Columbia University, the Institute of Human Relations at Yale University, the Child Welfare Research Station at the State University of Iowa, and similar centers at the Universities of California, Chicago, and elsewhere, to suggest the basic importance of this organized form of research for producing vital results. It is possible also that national organizations which exist solely for the purpose of fostering research, such as the American Educational Research Association or the Society for Research in Child Development, should have the disbursal of some research funds under their control. The amount needed by each such center or agency might range from $10,000 to $50,000 or more per year, again depending on the kind of problems attacked and the rate of work. Centers of the types referred to, organized with some permanence, are in a position to work on long-range problems which the individual research worker cannot or will not undertake.

In addition to such research through organized staffs, the rich potentialities of individual work should be freed from the limitations which now restrict it to problems which are convenient for singlehanded attack. Professors or other qualified research workers who have problems of significance should be given aid in the amounts that their work may demand for its effective prosecution. They may need funds for travel to distant sources of data; they may need tabulating and calculating machines and skilled operators; they may need the services of research mathematicians or of consulting statisticians to design experiments or to work out new mathematical procedures appropriate to their problems; or they may need the full-time cooperation of other social scientists throughout the study. Universities may have to be reimbursed if occasionally a faculty member takes off full time for the prosecution of research. These personally directed projects will call for, let us say, from $1,000 to $15,000 per year in each case. How many persons could use this aid in a year? We do not know in advance of trial, but if pressed for an answer, we should note that there are approximately 900 colleges, teachers colleges, and universities in the country. At least one faculty member in every third institution would have a research bent and sufficient perspective to be ready to attack, to the advantage of our society, several problems in succession.

A rough total can be obtained from the foregoing suggestions. The maximum called for in the outline would be about $9,000,000; the bare minimum, around $1,000,000. The minimum of *both* numbers and amounts, however, would be much less than a satisfactory figure, even for a minimum estimate. If these amounts seem large, they are small in comparison with the amounts spent for physical research, either by the Government during the war or by industry in any year. In fact, the maximum estimate is less than the average amount by which American industry *increased* its research expenditures each year over the period from 1920 to 1940.[5] A real question

[4] In the 1930's the U. S. Office of Education was allotted over $2,500,000 for five projects, and other undertakings were given from $500,000 to $1,000,000 each. See Carter V. Good, Educational progress during the past year (1937). *Sch. & Soc.*, 1938, **47**, 345–352.

[5] Obtained from Table I, p. 80, of Vannevar Bush. *Science, the endless frontier: a report to the President.* Washington, D. C.: U. S. Government Printing Office, 1945. Estimates of Federal support needed for physical research were set at $122,000,000 for a mature program (p. 33).

may be raised as to whether it is sound policy to operate a public enterprise as large as the school system, costing between $2,000,000,000 and $3,000,000,000 a year, with so small an expenditure for research to regulate and guide it. The maximum estimate suggested above is 0.5 per cent of this operating expenditure.

The lay person may inquire: "What will you study, aside from trying to improve methods of teaching spelling, reading, and so on? All of us got enough of these to get along in life somehow." This type of question has been asked countless times by practical people with respect to the physical and biological sciences during their history; it reflects simply an utter lack of understanding of science and its basic contributions. So long as nobody had ever thought of steel, iron workers wondered what physics or chemistry was good for. Before the atomic bomb, only physicists thought in terms of nuclear energy. Before medicine and public health measures had added 20 years to our lives, people thought that biological science was just a course to be taken in high school. Only persons of great vision know the future.

This is not the place for a listing of fundamental problems in education. Some have been suggested earlier. Reference to the *Encyclopedia of Educational Research* or a volume of the *Review of Educational Research* will make their nature and number apparent. It may be noted here simply that education is closely related to the total success of any civilization and of any world order. The fanaticism of the Japanese and the Nazis was the product of carefully planned education. What is taught in the schools eventually determines the character and organization of the world in which we live. In this country, the curriculum and the objectives of education are, according to a democratic pattern, largely determined locally, and usually by educators. A large responsibility rests on their shoulders, and they need the findings of extensive research to guide them in their choice of materials, methods, and objectives which will help bring into reality the sort of world that exists in the hopes of the common man everywhere. It is for the purpose of enabling the schools to fulfill their obligation to a society ever struggling toward cherished, but constantly expanding, goals that educational research seeks a new pattern of support. As research workers, we speak, not for ourselves, and not primarily for our work, but for a social world better adjusted to the aspirations and satisfactions of man. We speak from the conviction that genuine progress toward this goal will come not through physical force alone, but through the far-reaching power of psychological force channeled through the social institution of education.[6]

[6] The opportunity for all science to work together toward "a science of civilization" is presented by Ward Shepard in "Science for democracy" (*Science*, 1946, **103**, 65–68).

## *Scanning Science—*

The Editor of *Science*: Last Wednesday, May 6th, I witnessed a very remarkable experiment with Prof. Langley's aerodrome on the Potomac River; indeed, it seemed to me that the experiment was of such historical importance that it should be made public.

I am not at liberty to give an account of all the details, but the main facts I have Prof. Langley's consent for giving you, and they are as follows:

The aerodrome, or "flying machine" in question, was of steel, driven by a steam engine. It resembled an enormous bird, soaring in the air with extreme regularity in large curves, sweeping steadily upward in a spiral path, the spirals with a diameter of perhaps 100 yards, until it reached a height of about 100 feet in the air at the end of a course of about half a mile, when the steam gave out, the propellers which had moved it stopped, and then, to my further surprise, the whole, instead of tumbling down, settled as slowly and gracefully as it is possible for any bird to do, touched the water without any damage, and was immediately picked out and ready to be tried again.

A second trial was like the first, except that the machine went in a different direction, moving in one continuous gentle ascent as it swung around in circles, like a great soaring bird. At one time it seemed to be in danger as its course carried it over a neighboring wooded promontory, but apprehension was immediately allayed as it passed 25 or 30 feet above the tops of the highest trees there, and ascending still further its steam finally gave out again, and it settled into the waters of the river, not quite a quarter of a mile from the point at which it arose.

No one could have witnessed these experiments without being convinced that the practicability of mechanical flight had been demonstrated.

Yours very truly,
Alexander Graham Bell

*—22 May 1896*