Goals and Trends of Research in Geology and Geography

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IN A TIME SUCH AS THE PRESENT, when conditions are unsettled, when the tremendous impact of recent scientific discoveries has turned attention as never before to science and to scientific research, and when all aspects of science are beginning to be of interest to the politicians, it seems pertinent to take stock of our situation as scientists and to inquire: What are the goals toward which we are working? What are some of the present trends in scientific activity? How may the attainment of our goals most effectively be promoted?

Before discussing goals, let us consider briefly the motives which inspire an individual to scientific endeavors. These may be personal gain or livelihood, desire for acclaim from his fellows, desire to contribute to the welfare of humanity, or just plain curiosity—an intellectual urge to understand. Most people probably work under a mixture of all of these motives, but for many of the world's greatest scientists we would probably find, if we could know the truth, that the last-named motive was dominant.

Industry presumably engages in research for the sake of gain or to improve its competitive position. Governments have long engaged in research as a service in developing and conserving the natural resources of the state or nation, and the public health.

Recently a powerful incentive leading to research by national governments has been *fear*—or should we, perhaps, say national security?—fear that some potentially hostile government may be first in the development of deadly weapons of destruction.

The fear motive leads to an aspect of research that may have most serious consequences both to scientists and to science in general—secret research. This is neither the time nor the place to discuss secret research further than to voice a plea that if research must be done in secret, let it be done exclusively by governmental agencies and by scientists who well know the implications of secret research; but, by all means, let it be kept out of the universities. Only the gravest national dangers of a war emergency would seem to justify introducing into the universities the state of mind implied by such research. This applies as well to commercially-sponsored research done in universities, except in instances when not even the briefest period of secrecy and exclusive possession of the results of the researches is stipulated.

Adapted from the address of the retiring vice-president of the Section on Geology and Geography (E), AAAS, delivered on December 26, 1947, in Chicago, Illinois.

GOALS OF RESEARCH

Whatever may be the motive which impels an individual to engage in scientific research, or a business organization or a government to support it, the primary goal to be attained would seem to be a better understanding of nature and of man and of how to apply that knowledge for the benefit of mankind.

Fundamental research—pure science—aimed at the acquisition of knowledge and understanding, must necessarily precede the application of that knowledge to "practical" ends—applied science, but both are necessary if the final goal is to be reached. It seems to me that practical application is the ultimate long-range goal of all research, but, as already pointed out, it is not, and it should not be, either the goal or the impelling motive of all researchers. Besides, an ultimate practical goal is not necessarily soonest reached by a direct approach. The shortest route may lie through researches having no immediately or even remotely visible practical applications.

Let us turn now to a consideration of goals in our own sciences.

The Goal in Geology

In Geology we already have amassed a large body of information and have effectively solved many of the problems which puzzled earlier workers. But many of the most fundamental geological problems still remain unsolved and, for some large categories of fact and distribution, we have scarcely made a beginning in the collection and assemblage of data. One example is the problem of the interpretation of ancient sediments in terms of the environment of deposition. This problem is at present held up by lack of detailed knowledge of the kinds and distribution of materials being deposited on the sea bottom of today.

Many of our unsolved problems are related either directly or indirectly to another major problem—the mobility of the outer shell of the earth. We may mention, specifically, the origin of compressional mountains of the Alpine type, isostasy, continental drift, regional metamorphism, the origin of geosynclines, and many others. Among immediately practical economic problems, we have not yet solved that of the origin of oil, nor are we entirely clear as to the mode of formation of certain types of ore deposits.

For the solution of many of these problems the mere collection of facts is not enough. Facts are infinitely numerous, and any attempt to obtain "all the data" on a problem is likely to result in such an unwieldy mass of mostly irrelevant information that the solution may be hindered rather than furthered.

To be efficient, the gathering of data must be selective, and the best guide to its collection is probably the method of multiple working hypotheses proposed many years ago by Chamberlin (*1*). The available information is first studied and analyzed; several working hypotheses are formulated; from each of these are deduced the consequences which should follow if that hypothesis were correct; further data are then sought and the old data re-examined for evidence which would either bear out or refute the hypothesis under scrutiny.

When search for data is thus guided, there will be a minimum of blind collection of irrelevant facts and a minimum cluttering of the literature with undigested factual information.

The formulation of correct hypotheses may have to await the coming of the right man endowed with some measure of genius. It is doubtful if it can be hurried by any amount of research planning or expenditure. It may even have to await the development of new ideas in other lines of science, such, for example, as the discovery of radioactivity and its heating effect, which may prove to be the key which will unlock some otherwise insoluble mysteries.

In order to progress most surely and most rapidly to his goal—the solution of his geological problems—the geologist should, it seems to me, avoid premature wanderings into other related fields. He should give first and most serious attention to the *geological* evidence, giving most weight to that which is simplest and most obvious. He should frame his working hypotheses to harmonize with that evidence, and he should enter the domain of the other sciences only as he follows out trails suggested by these working hypotheses. His excursions into other fields will then be directed and purposeful and, in all probability, fruitful.

The above procedure demands careful analysis of existing data and constructive imagination in formulating hypotheses and in deducing their consequences. No amount of footwork can substitute for headwork in analyzing the data and in formulating and testing working hypotheses. This calls for a different order of geological talent than that needed for mapping and the collection of factual information. The footwork can be hired and can be done by any reasonably well-trained geologist. The headwork may *possibly* be hired, but it cannot be commanded or produced at will by executive decree; and it is not likely to be developed in committees.

The Goal in Geography

For many years the geographers of the United States have seemed to be at a loss as to just what Geography is. Many years ago, a survey of geographical literature revealed a surprising number of papers bearing titles such as "The Scope of Geography"; "What Is Geography?"; and "The Circumference of Geography." A goodly number of such titles still appear. In a subject such as Geography, covering a wide range of interests and in which no general agreement has been reached as to proper content, it is natural that the goal should be ill defined.

Geography, as now understood, includes purely physical aspects — Geomorphology, Physiography, Meteorology, and Climatology—as well as Regional, Commercial, or Industrial, Human, and Political Geography.

Can we find some tie which binds together all of these widely diverse subjects and name a goal toward which all of them should consciously or subconsciously be aimed? If such a goal could be defined and generally accepted, it might give a direction and purpose to geographical studies which they seem not to have had in the past.

Tentatively, we propose such a goal: "The ultimate goal of Geography is an understanding of the natural and the human environments to the end that the natural environment may best be utilized for the benefit of humanity."

Such a goal emphasizes our view that, for scientific studies of all kinds, the final goal is reached only when their results are applied for the highest good of humanity. As "pure science," the basic scientific studies are vital, and in satisfying intellectual curiosity they may have a high function and provide a powerful motive, but neither the gathering of data nor understanding nor the satisfaction of intellectual curiosity seem to me to constitute final goals.

With a goal such as that proposed above, the various branches of Geography fit into place. The physical branches define the environment; the more "human" branches deal with past and present human reactions to and upon the environment; but the attainment of the final goal of the science demands that, after a particular environment has been defined, we learn how, in the light of the latest developments in other sciences, we may make the best possible adaptation to it. This is not, in any sense, to urge that all geographical studies should, by themselves, have a practical slant. It is, rather, to define the long-range goal of the science.

By way of illustration, let us imagine a study of the methods used by the Algerian natives to conserve their scanty water supply and to make it contribute to the growth of the best possible crop of dates. Such a study, by itself, would be interesting and even fascinating. It might be followed by other studies of how men in other lands or in other ages have solved the problems posed by a similar climate. But to approach as closely as possible to the goal which we have here set up, it would need to go further or to be supplemented by other studies pointing out how, not only considering various devices which men have invented and used in the past, but also in the light of the latest developments in modern science and in social organization, men might best utilize such a topographic and climatic environment as that of the parts of Algeria under consideration.

The attainment of our goal will be difficult and definitely is not a job for a novice, but the goal would give point and direction to the studies of even a beginner working on some specific and limited phase of geographic endeavor.

Trends

Having considered briefly the goals of geological and geographical research, it is in order to examine some of the trends which have been developing in recent years, in an attempt to determine whether they are likely to lead most directly and economically to the desired goals. These trends are of two sorts, one being related to the work itself and the other to its financing.

Among trends of the first sort, the following may be listed:

(1) Toward what may be called "research by committee," involving elaborate planning and organization and elaborate and all-inclusive fact-gathering.

(2) Toward crowding of scientific meetings and overburdening of scientific literature as a result of the prevailing practice of soliciting papers for scientific programs.

(3) Away from thought, critical analysis, and the formulation of working hypotheses.

(4) Toward continually lessening discussion, at meetings and in journals, of work already done and of hypotheses proposed.

During the past few years the idea that a baffling problem could be solved, if only a large and sufficiently active committee would go to work on it, seems to have pervaded the domain of scientific research. Without intending criticism of those who have worked so diligently on such committees, one is led to wonder if more might not have been accomplished if the committee members had spent an equal amount of time on their own researches.

The long and comprehensive outlines of proposed research projects typically turned out by a research committee are generally too vague to be of much aid to a graduate student and are not likely to appeal to the independent-minded scientist. To attack properly the problems as outlined by the committees would seemingly require a large, highly organized, and closely supervised research organization.

Research committees undoubtedly are useful when they point out gaps in existing knowledge which act as barriers to further advance. A committee may do better than an individual in judging what researches are *strategic* (*2*) in this sense, and its recommendation may carry more weight than that of an individual when backers are being sought to help finance the attack on these strategic barriers. But a research committee cannot be expected to supply that flash of understanding which, by recognizing the connecting links between various lines of evidence, not uncommonly leads to the final solution of a problem.

Not a few project outlines give the impression, by that very completeness already mentioned, that they are aimed at gathering "all of the data" in a rather broad field. As already explained, such would be a futile and wasteful attempt, because both data and interrelationships are infinite in number.

Entirely aside from what might result from projects formulated by research committees, the amount of uncorrelated, undigested, and unanalyzed factual material that is accumulating is enormous, and it continues to pile up at an ever-accelerating rate. A rough survey of publications of the *Bulletin of the Geological Society of America* and of the *Bulletin of the American Association of Petroleum Geologists* over the last two years revealed that 75% were factual papers with little or no analysis, 19% were analytical, theoretical, or methodological papers, and 6% contained mixtures of both.

Such an enormous and rapidly-growing accumulation of undigested published factual material poses a serious problem in indexing and abstracting which will soon require serious attention.

The trend toward the burdensome accumulation of data is undoubtedly furthered by the current practice of soliciting papers for the meetings of scientific societies, instead of depending on voluntary contributions. Would not the meetings be better and more stimulating if fewer papers were given, and those few by men who had something to say that they were definitely anxious to put before their fellows?

This leads directly to a consideration of the final trend listed above. Papers which seem to many to contain fallacies and nonsequiturs are given at the meetings and then, not infrequently, go through to publication without discussion. Perhaps, in the long run, the fallacies will be revealed, and probably it is true that well-informed workers in the field recognize them and quietly ignore them. But such a procedure is to be regretted, because the student and the less fully-informed worker in that or related fields is generally not in a position to recognize the fallacies and therefore gains false impressions which retard progress. Active criticism and discussion are stimulating and salutary and should help toward keeping the published literature less voluminous and more trustworthy. What if bitter personalities are injected occasionally into verbal discussions? These are quickly forgotten if editors keep them from appearing in print. Lack of discussion and criticism in meetings is partly due to the difficulty of effective discussion in the largely attended meetings now prevailing. Means of minimizing the difficulties might well occupy the serious attention of those in charge of meetings. Eliminating the solicitation of papers would help.

The Financing of Research

Methods of financing research have an important effect not only on the research itself but also on the freedom of the individual or the institution engaging in it. Momentous changes in the ways by which money is provided for research are due to the drying up of endowment incomes provided by individuals, by which research formerly was supported in the universities and private research organizations; to increased expenditures by governmental agencies; and to greatly expanded research programs sponsored by industry or by commercial associations such as the American Petroleum Institute.

Research of the type that has long been carried on by many Federal agencies, such as the Geological Survey, and by various States, seems to be an entirely appropriate function of government—especially when it is directed to projects of a broad, fundamental nature, not appropriately done by industrial organizations having limited objectives, or to projects for which necessary equipment is too expensive to be provided by institutions such as universities. No objection is likely to be raised to the expansion of such research to whatever extent may be sanctioned by Congress, provided the research is done in government laboratories by government personnel.

Likewise, the expansion of industrial research to any desired extent seems salutary. But on the other hand, a recent trend in the financing of university research seems fraught with peril.

Research in universities has been badly crippled because decrease in endowments came at a time when rising costs and greatly increased enrollments, coinciding with a decrease in the number of available instructors, resulted in burdensome teaching loads and in greatly decreased faculty time available for research. It is, therefore, perhaps only natural that a strong tendency has developed toward seeking governmental aid in the financing of university research. But governmental aid, particularly Federal aid, for universities is not without serious drawbacks and dangers. The elaboration of that aspect of the subject would be out of place here and will be attempted elsewhere. Suffice it to say that the trend toward the seeking of Federal support for research in universities is strong and that its desirability should be most carefully analyzed, for this trend carries with it the possible danger of governmental control and the lessening of freedom in the universities.

Summary

Summarizing in a few words the essence of the preceding analysis of goals and trends in research in Geology and Geography, we may say that the goal in Geology is a knowledge and understanding of the earth and of its history. In Geography it is a knowledge and understanding of the natural and human environments, to the end that they may best be utilized toward better living in the broadest sense.

Trends in research in these sciences that seem not altogether desirable are those toward overorganization of research and overdirection from others than the ones who are to do the work; toward indiscriminate fact-gathering; toward lessening of critical analysis of, and discussion of, papers given at meetings and of the published results; and toward premature publication and excessive volume of publications resulting from the prevailing practice of soliciting papers for scientific meetings.

Changes in methods of financing of research resulting from a great decrease in endowment incomes are noted, together with a strong tendency to look toward the Federal Government for the financing of university research, in spite of the obvious dangers to university freedom which such financing involves.

References

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