The Choice of Research Projects

RESEARCHERS, whether professionally affiliated with academic institutions, non-profit organizations, governmental agencies or industrial concerns, all aim for success in their work. In addition to satisfying curiosity and learning something new, professional recognition, position, promotions, salary increases, bonuses, are all dependent upon success. Some are conscious of this goal throughout every stage of a research project and modify their activities in keeping with it. Others hope for success, of course, but pay little attention to it, devoting their energies instead to the immediate problem which is of interest to them. Probably no one undertakes a research project without the anticipation of a successful conclusion. Most researchers, however, rather than having one absorbing problem or question which excites their curiosity have a multitude of them. The judicious choice of direction in which to pour one's concentration and energy is, therefore, of great importance.

Sometimes there is no choice. Circumstances may dictate the research project on which one will work. The institution of affiliation may determine the research that the individual will do. Sometimes it is a professor or a research director who determines the research which will be done. In these cases there is no choice, except, of course, the choice of institution or professor or research director, if this choice is open to the individual. Nevertheless, someone must decide what research will be done and the thrill and satisfaction of success or the frustration and disappointment of failure will be his.

The factors to be considered in choosing a research project include (1) personal (or personnel, in the case of research directors), (2) physical, and (3) circumstantial. Of these, the factors most easily evaluated are the physical factors. What I am here calling circumstantial factors are the most difficult to assess. The personal factors are intermediate between the other categories because there are some recognized standards or measures which can be used.

Among the personal factors, probably the most important one is interest. As used here, the word is not intended to mean sophisticated curiosity leading to casual interest, but a real and compelling or motivating interest in the field. If interest is not intense, work could not possibly be inspired or at least enthusiastic and reach some goal. It must be remembered that some aspects of research work are routine and repetitious. If the other aspects are sufficiently exciting, however, this excitement can carry over and sustain enthusiasm during the routine aspects of research work. But because of this factor, i.e., because a fair number of the clock hours involved in research can be routine, it is important that one be intensely interested in the work, otherwise progress will not be made. Creative work comes from actively interested minds. This important factor—interest—is, unfortunately, difficult to be sure of.

Another personal factor is knowledge of the field of research. It is assumed that a certain level of intelligence will be required in order to gain the knowledge necessary for good work in a field. There is some objective evidence available for the evaluation of this factor. Academic courses successfully completed, academic degrees, experience, and reading, all can be, to some degree, evaluated and provide an indication of the extent of knowledge of the field. Thorough knowledge of the area makes it possible to have some new ideas about the field—ideas that have not been expressed before. This could lead to very fruitful projects. On the other hand, gaining a good knowledge of a field can expose areas of ignorance which might have been thought to have investigative possibilities, but which further knowledge shows to be areas already investigated.

A third personal factor to consider is aptitude. It is unfortunate for one to be intensely interested in some field but to have no aptitude whatever for it. If an individual has no aptitude at all for mathematics, for example, clearly theoretical physics or astronomy or genetics are not suitable fields for that individual. Likewise, if an individual has no feeling whatever for physics, then work in physiology is not a good choice. Or, if an individual has no mechanical aptitude, experimental research would not be as wise a choice as the more theoretical areas, assuming other aspects are satisfied. In this area, also, there are available so-called objective methods for evaluation. I personally do not believe that one can put too much reliance on the various tests which are available to measure aptitude because predictions made on the bases of these tests have not always proved to be accurate. In other words, to predict the future possibilities for one particular individual based on a score is a dangerous thing to do because the tests are not that reliable. They can be useful pieces of evidence to consider, however.

The fourth factor of a personal nature one must consider is resources. (There are other resources that are not personal and which will be considered below.) That one must have the bodily ability, e.g., manual dexterity, necessary to perform the operations required by the research work is hardly worthy of mention, and no more will be said of this. For the independent investigator, financial factors are, of course, important. The availability of fiscal support will determine if a project can be done at all. At times this factor can decide where a research project can be done. This requirement of the independent researcher may be currently made possible through generous grants, fellowships, scholarships and contracts. Some of these points are of concern only to independent workers. Research directors would not be concerned with these, but would have to adapt the others to their personnel.

It should be pointed out that in the case of the individual researcher, the personal factors mentioned here need not all be present to the same degree. Even if they are equally present, this does not preclude successful work in a field. That is to say, it may be possible to overcome some shortcomings in resources with an abundance of interest or knowledge. It is not uncommon for considerable creativity to be elicited in such circumstances.

Physical factors are of concern to both independent investigators and research directors, of course. They are proper laboratory and library facilities including adequate space and environment (e.g. air conditioning where necessary, as in electronic laboratories), furniture, equipment, apparatus, supplies, journals and references. As for the independent worker, personal finances or grants can solve many problems in this area. The institution or company often solves this problem for independent researchers as well as directors of research since relevant equipment, apparatus, supplies, books and laboratory facilities would be provided for appropriate research. Association with a particular professor or director of research solves most problems of this sort for the independent worker. In addition there is the fringe benefit of stimulation and professional recognition which accrues to the individual by virtue of association with established and recognized researchers. It should be pointed out that some research cannot be undertaken because equipment necessary for the solution simply does not exist. In such cases instrumentation work is necessary, and if the project is promising, is decidedly valuable.

The first of the circumstantial factors that one must consider is the relationship of a proposed research project with the whole of science. In other words, the question to be raised here is, does the project lead somewhere, i.e., does it have ramifications with other areas of science, or is it a circumscribed project dealing only in the backyard of the immediate vicinity ? To use an illustration from biology, the question is, is the project something like the study of an aspect of one species with no significance to other species rather than something like the mechanism that determines species ? The latter has far-reaching possibilities, the former is limiting by its very nature. Frequently, researchers tend to choose a project because the ingredients necessary are readily available. This is not the proper approach. One ought first to ask the question, and then search for the appropriate materials that would help most in getting at the answer.

New equipment, procedures or techniques frequently open up new areas for fruitful research projects. In each area of science, important discoveries of great significance are occasionally made, advancing knowledge in that area by giant steps. Between these periods of rapid progress are

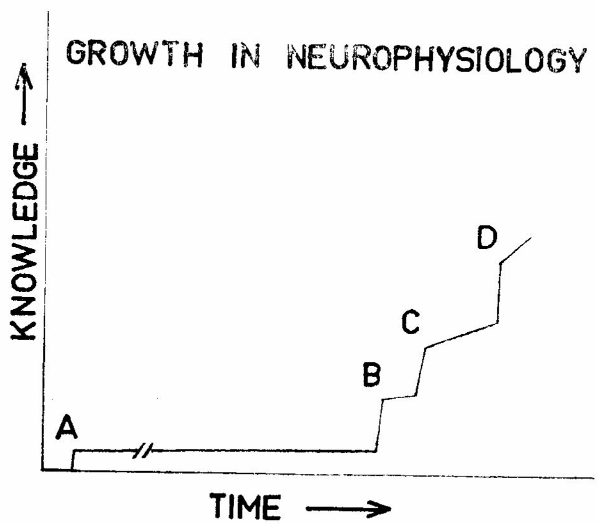

Figure 1. Growth of Knowledge in Neurophysiology

A—description by Aristotle of contact of horse with *Torpedo* (322 B.C.).

B—experiments by Galvani and Volta on animal electricity (1791 A.D.).

C—development of galvanometer (1820) and its use in neurophysiological experiments by Matteucci (1840).

D—use of cathode ray oscilloscope in neurophysiology by Erlanger and Gasser (1922).

plateaus or periods of relatively slow progress during which many details are filled in. See Figure 1 for an illustration from the field of neurophysiology. Here can be seen the giant step made by Aristotle (A) with a plateau following for a long period of time during which no significant observations were made. Then at B are represented the experimental observations of Galvani and Volta, again followed by a flat period of virtually no increase in knowledge in this area. Following the discovery of the galvanometer and the observations of Matteucci (C), there is no plateau but rather a continued increase in knowledge, although at a reduced rate. In other words, knowledge was advancing in neurophysiology but not as rapidly. The next giant step was made by Erlanger and Gasser (D) and knowledge has been increasing ever since.

To see new possibilities in already existing equipment, or to devise new techniques or apparatus or equipment does, indeed, require imagination. Sometimes one can be surrounded by fruitful possibilities which remain obscure for lack of imagination to make use of them.

Another important factor that is not infrequently ignored is the factor of timing. If one has an idea for a research project in a virgin, new field, one has a much better chance for success. Successful completion of such a project obviously necessitates that most of the investigators who follow have to refer to the beginning of the area of research. Such a project, then, becomes the foundation on which subsequent research will be built. At far as success is concerned, this type of project involves the greatest risk, but it also promises the greatest returns if it is successful.

Next to that, one is wise to choose projects in an area of science that is on the rise; that is, getting more emphasis rather than declining in importance. In all areas of science, I suppose one could say that any aspect of that science dealing with problems associated with space would be such a field. It still is on the incline, there still is a lot of emphasis on it, a large number of scientists in this area, good fiscal support, and it still has not reached a plateau. On the rising phase of such a field would be an advantageous time to start a project, second only to a project which originates a new area of investigation. Whether one starts on the rising or declining phase of interest in the field is another factor to consider. The earlier one starts on either the rising or declining phase, the greater the chance for success. Of course, fields of declining emphasis are least desirable because they are already getting less and less emphasis and therefore the chances for success are severely limited and, in any case, can only be attenuated by the decreasing emphasis placed upon the field.

One last factor must be mentioned: the knowledge of when to abandon a research project, i.e., when one knows that the work is going nowhere and one ought to drop it and start on something else. There may be virtue in work, but it will not necessarily make one a successful scientist. Caution must be used, though, that a research project is not abandoned too soon. Much of research work is unwittingly directed toward failure. Frequently, however, some of these failures result in important observations which can lead to significant new research projects.

After considering these factors, the choice of a research project can be made with reasonable confidence. This must then be followed by equally careful performance of the usual research procedures, i.e. the literature is reviewed, analyzed and studied, a hypothesis is formulated, experiments are designed and carried out, good notes are taken, the results of the experiments are evaluated and analyzed, the conclusions are drawn, the whole is written and published. If these factors have been considered in choosing an area of research and if the hypothesis is well thought out and the research procedures used are sound, a successful conclusion should result. It should mean the experiments or observations in the research will not be just a series of isolated experiments or observations but ones leading toward a goal which is significant and from which an individual will derive satisfaction, a sense of accomplishment and the exhilaration which comes from a research project well done.

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