Research Planning and Research Policy Scientists and Administrators

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> . . . The proceeding upon somewhat conceived in writing doth for the most part facilitate despatch: for though it should be wholly rejected, yet that negative is more pregnant of direction than an indefinite, as ashes are more generative than dust.
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> —FRANCIS BACON, "Of Despatch"

ONE of the central dilemmas of research is reconciliation of the intellectual freedom required for effective exploration of the unknown with the selection and direction of effort implicit in the functioning of any organization with defined functions or limited resources. The concept of "research planning" is one aspect of this dilemma. This paper explores some aspects of the problem, some ways in which untoward consequences of the dilemma can be minimized, the meanings assigned to research planning by different groups, the kinds of planning appropriately done by these groups, and the interrelationships among various kinds of research planning. It also points out some of the issues that arise when the planning function appropriate to one group is undertaken by another, or when one group fails to recognize the nature of and necessity for planning by another group. Since these questions are complicated, the emphasis is on the interrelationship among different kinds of planning, rather than on the content of planning at any one level.[1] Finally, while the observations on which this discussion is based relate primarily to government, some of the problems seem common to the planning of research in industry and universities.

Consequences of Limited Concepts of Research Planning. One may say that a concept of research planning that encompasses decisions made by people or groups quite remote from a laboratory is nothing but an exercise in semantics, and that even if the idea is sound it has no particular use. However, there are important tangible and unfortunate consequences of any limited concept of research planning, whether it be a concept of research planning limited to the framing and execution of a set of experiments or a concept of research planning as the determination of general relative emphasis upon areas of investigation and the allocation of men, materials, and facilities. Some of these consequences can be profitably outlined before the kinds of research planning that take place in the laboratory and elsewhere are discussed.

[1] See D. C. Marquis, "Research Planning at the Frontiers of Science," *The American Psychologist,* Oct. 1948, p. 431, for a discussion of research planning in terms of three levels —experimental design, program design, and policy design.

If a scientist does not understand and accept the fact that he cannot rationally expect to be a free agent in any absolute sense, the result is likely to be resentment that can impair both his work and that of people whose job it is to plan for a more productive sum total of the work of all investigators.[2]

Investigators sometimes incline to lump together all decisions affecting their work but not made by them as "administration," and to view "administration" either as irrelevant or as an impediment to their work. This is not, of course, an attitude confined to people engaged in performing experiments. The director of a laboratory or a set of laboratories, for example, may deplore many "administrative" decisions made by those who have some degree of authority to plan his actions. These "administrative" decisions may be made by such groups as a board of trustees, a board of directors, or the Bureau of the Budget in the federal government.

Scientists whose work involves a great deal of administration sometimes seem reluctant to admit that they are "administrators" and tend to view themselves as "scientists" even when little of their time is spent on the substantive conduct or guidance of specific research projects. This may be because "administrators" and "administration" are held in relatively low esteem by substantial parts of the scientific community. Moreover, people with a heavy intellectual and emotional investment in the direct conduct of research are understandably reluctant to admit that they are leaving the field in which they have been specifically trained. Alan Gregg, among others, has deplored the fact that many good "research" men become "administrators": "The extraordinary feature of medical research in America in our times is the frequency with which demonstrated ability in research is rewarded by being extinguished. It would be consoling to think

[2] Some people who read this article in draft commented that, since scientists, like other people, generally recognize and accept forces beyond their control which affect their work, there is not much point in explaining what these forces are and how they operate. In the author's experience, however, the idea of scientific freedom is sometimes carelessly invoked to justify the position that any limitation upon scientists' freedom to determine what they will work on and what resources will be devoted to the work is wrong in principle.

that only in the earlier stages of institutional development is it necessary to sacrifice research to administration, and that once the preliminary planning has been done, men selected and trained to do research will find a tradition and circumstances facilitating their natural tastes and aptitudes. But the evidence about us hardly justifies such an evasive hope" (*1*).

But, if administration is viewed as research planning on a broad scale—as the development of scientific strategy, as the evolution of a consistent philosophy of research, and as the difficult task of bringing a sound philosophy to bear upon the conduct of research—there may be no net loss to research when a scientist turns administrator. Is not the resolution of such problems a task worthy of people who are scientifically trained, and a task that some scientists are uniquely equipped to perform? Is it not possible that in deploring the loss of scientists to administration one can lose sight of the facts that administration is more than a business management, logistics, and semiclerical functions and that science therefore has a tremendous stake in proper administration?

One may be skeptical of the productivity of research on the scale that now prevails in this country, but the situation exists. If the maximum return on the investment is to be secured, there are vitally important aspects of research planning that should be undertaken by minds of the highest caliber, sensitivity, and purpose.

The kind of limited concept of research planning that is potentially most dangerous is one which views the function as concentrated in groups outside the laboratory. If "administrators"—a department head, a laboratory director, an industrial executive, the Congress—fail to realize that there is an area and kind of planning that must be left to those actually engaged in research, the consequence is sterility of research. In the extreme, a concept of planning that overemphasizes the elements of planning that go on outside the laboratory leads to "master plans" that seem to be characteristic of authoritarian states.

The preceding observations on the kinds of planning decisions that take place in and out of the laboratory seem fairly obvious. Then what accounts for the undoubted fact that differences of attitude and tensions sometimes mark the relationship between persons engaged in different aspects of the planning of research? A full answer to this question would require a fairly extensive treatise on the philosophy of research, theories of group identification, and the theory and practice of administration. But some general sources of the problem can be briefly sketched. One of the sources of the problem is the tendency for persons at each level in a bureaucracy to consider themselves and those in their hierarchical class—laboratory chiefs, office of the secretary, bureau chiefs, deans—to be the level at which true wisdom and balanced perspective are concentrated.[3] That level is also often considered the one at which the really important decisions are made. Actions taken higher on the ladder are sometimes considered essentially unproductive maneuvering that can reach merely "administrative" or "political" ends. They often appear to be taken by persons whose actions are misguided or irrelevant and whose mission seems to be to impede, curb, and delay. Those actions taken lower on the ladder are often regarded as the commendable efforts of zealots upon whom the productivity of the organization depends, but who simply cannot or will not understand the full range of influences that must be taken into account in making the broader decisions.

When these attitudes develop, as they sometimes do, they are generally accompanied by the attribution of somewhat stereotyped personal characteristics to those in other hierarchical classes. Those above are often regarded as somewhat deficient in specialized training and viewed with a mild sort of tolerance. Those below are often regarded as highly but narrowly trained, and viewed somewhat paternalistically.

Another source of the difficulty is a tendency at times to make an unrealistic and grossly oversimplified distinction between "scientists" and "administrators." There are people who do research at the bench and nothing else. They are clearly scientists and not administrators. But a large proportion of scientists engaged directly in research are responsible in varying ways and degrees for the work of others, and the discharge of these responsibilities constitute administration. On the other hand, there are people whose work is confined solely to such tasks as "preparing budgets" and "processing personnel actions." They are clearly administrators and not scientists. But many scientists in university and industrial as well as governmental laboratories have administrative duties which they must perform. They are administrators as well as scientists. When scientists or administrators overlook the fact that many people perform mixed functions, it becomes easier to sort people into "us" and "them" groups.

A strong "us" vs. "them" atmosphere in a laboratory can impede the communication and working relationships that are essential to effective planning of research. For example, if administrators view scientists as a group apart, they may be led to view administration as an end in itself and forget that their role is to facilitate research. The large group of people who have both scientific and administrative duties may deplore and slight their administrative work because of the low esteem in which these activities are often held. Scientists may fail to appreciate the relevance and importance of decisions which affect them but which they do not make and may therefore resent the fact that those decisions are made.

For all these reasons, an understanding and acceptance of the fact that research planning necessarily occurs on a number of levels is important to the prosecution of research.

There remains, however, a more significant and a more difficult question. What kind of planning is ap-

[3] This may help explain why "freedom of research" is sometimes invoked most often by persons who are quite authoritarian in their relationships with those over whose work they have some degree of control.

propriate at what levels? The remainder of this paper centers around the total research planning process from the laboratory to Congress.

Planning in the Laboratory. Investigators quite often confine the meaning of "planning" to the degree and kind of establishment of hypotheses, organization of work, and assignment of tasks that they feel essential to their work. If a person defines planning in this way, he must arrive at the conclusion that "planning" by anyone else for him can constitute nothing but ignorant interference. And, given this definition, he is right.

What determines the degree of freedom that an investigator should have to plan and carry out his work?

The general mission of the laboratory is significant. To the extent that this mission is restricted, some limits can properly be placed upon the kind of work which the investigator undertakes. For example, if the task of the laboratory is to develop devices or processes, the staff can properly be limited to this area and the planning of individuals must be within these limits.

In organizations where basic research is an important or sole mission of the laboratory, the competence of the investigator generally plays a relatively more important role as the mission of the laboratory as such becomes broader.

Individual investigators—subject to the qualification noted below—must be free to select the problems which they wish to work on, how they approach the problem, how they design experiments, and must be free to shift from one area of research to another. These decisions certainly constitute the most direct, precise, and tangible form of research planning. This freedom of the mature investigator is necessary in basic research to provide the environment within which his intellect and ingenuity can operate most effectively. Such freedom may seem superficially to lead to anarchy. In fact, however, the "free" decisions of the investigator are closely determined by the nature of his endeavor and by his training even when he is not subject to the direction of another person. Even when the investigator follows a pure hunch, his hunch arises out of his store of accumulated facts, principles, and relationships in his field. This knowledge is supplemented by an indefinable process that fruitfully synthesizes in his mind what appear to be unrelated elements of a problem. For this reason, freedom of the able investigator does not lead to miscellaneous puttering, but to an intellectual effort that is generally highly disciplined. The nature of science itself imposes elements of planning.[4]

The varying capability of individuals sets limits on freedom. Full freedom is not an immutable natural right of all investigators. There is at times a tendency to identify "the freedom of science" with the right of any investigator to complete independence. Freedom is predicated upon the exercise of mature judgment by trained minds. The freedom of individuals to plan their work is, therefore, not a "yes" or "no" matter but a matter of degree. The right of an investigator to freedom in his work, i.e., to plan his work and to receive the resources to carry it out, is earned by achievement. Junior investigators must learn techniques and develop competence by working in collaboration with persons who have demonstrated their ability before extensive freedom of action and resources can be profitably put at their disposal.[5]

This question is faced by industrial as well as governmental laboratories. "It is the general experience of those laboratories in which fundamental research is carried out that the individual scientist almost always decides what specific problems he will undertake. A corollary to this is that if the scientist is not capable of deciding for himself what he should do, the director will see that he is given work which is supervised more closely" (*2*).

For all these reasons, the work of most investigators is planned to a lesser or greater degree by other scientists who guide or direct them by reason of status, intellect, or personality. It is therefore somewhat unrealistic to talk of "scientists" as if they constituted a homogeneous group and to speak of planning in a laboratory as if each scientist has, or should have, an equal degree of freedom to plan his work.

Planning by the Laboratory Chief. Just as work is often planned for junior investigators, the plans made by senior investigators are made within a framework set by decisions which they influence but do not control. This set of decisions can be considered planning just as the design and execution of a specific experiment require planning.

One set of decisions affecting investigators must be made by laboratory directors in governmental and industrial laboratories, and their counterparts in universities. These decisions relate essentially to the deployment of resources—manpower, space, facilities, supplies—among competing staff demands. The decisions set limits within which the plans of project leaders and individual investigators must be laid.

The extent to which and the detail with which specific tasks are assigned to investigators or teams of investigators by laboratory chiefs varies widely among laboratories. In general, the greater the element of applied research and development, the greater is the extent to which specific tasks should be assigned to investigators. The more fundamental the work of a laboratory, the greater should be the degree of freedom to select work and to deviate from any predetermined plan.

For several reasons, these principles are easier to state than to apply. Distinctions between basic and applied research are useful conceptually, but hard to make in practice. There is sometimes a tendency for

[4] Polanyi has stated this proposition elegantly in a chapter, "The Example of Science" in his book, *The Logic of Liberty.*

[5] This, like other fairly broad assertions that the reader may notice, is probably true as a generalization, but not as an invariable rule. Part of the job of a laboratory director is to select younger men who can be given a free hand to work on unorthodox leads. The story of Banting, Best, and insulin is a case in point.

investigators engaged in "applied" studies to claim that they are engaged in "fundamental" research because of the vague stigma that some scientific circles attach to applied work, and because they wish to secure for themselves the freer hand that traditionally goes with fundamental research.

In this connection, if competent investigators must be left free to determine what they work on and how they proceed, how is it possible to determine in advance—to plan—the general areas within which a laboratory will operate without decreasing the quality of research?

The answer lies in large part in selecting for work in a special field men with the training, experience, and desire to work in that field. As a general rule, even those investigators who shift their general field of inquiry do not do so abruptly. This makes possible the deliberate initial direction of the nature of the work of a new laboratory without directing the work of individual scientists.

Careful selection of personnel cannot only permit a laboratory director to set a laboratory program while leaving individuals quite free, but can even reconcile a substantial degree of coordination, direction, and "team" research with individual freedom. "A large measure of freedom and individual initiative can be preserved, if the coordination be wisely directed by selecting for the team those whose interests and aptitudes have already led them toward the problem which is to be investigated. They can then be left to explore, as they will, their own sector, aided and guided by the related findings of others in the group, encouraged by the assurance that their discoveries will quickly become a part of a greater accomplishment than they could attain unaided. It is thus possible to direct research for a desired end, without losing those intimate satisfactions which come to the investigator who follows his curiosity until he acquires understanding" (*3*).

The power of most laboratory directors to shape the programs of their laboratories is probably more easily exercised while the laboratory is being staffed than at any time thereafter. After the laboratory is staffed, the director must rely primarily not upon his authority to hire—a task that is in general performed with relative ease and in an atmosphere where freedom of choice is available to all concerned—but upon his power to stimulate, challenge, guide, and coordinate. Turnover may help him, but restaffing of a laboratory as a means of research direction is likely to be a long-drawn-out process. This does not mean that an established laboratory is inflexible, but it does mean that the extent of a program change and the speed of a change are limited by very practical factors.

The fact that the laboratory chief sets limits upon the resources available to investigators is in no real sense "a denial of freedom of research" or "administrative interference with research." While it may be in the interest of productive research to leave the minds of competent investigators free, each investigator can hardly exert a valid claim to unlimited financial support because his vocation is scientific research. The difficult problem, of course, is to make the judgments of the laboratory chief as sound as possible. One of the most crucial problems faced by a laboratory chief is to determine the rate at which the area of freedom given to an investigator—and the manpower, supplies, equipment, and space made available to him—should expand.

With the rapid postwar increase in both the number of large laboratories and the total scientific effort of the nation, a shortage of persons with the training and experience required to make these judgments has developed. Many laboratories are just beginning to realize that it may be necessary to plan more consciously the development of executive talent for research.

The laboratory director can, if he has adequate training and intellect, make sound judgments only if he is able to assess not only the current or potential significance of work in progress or proposed, but also the personalities, competence, and potentialities of the scientific staff. Unless his communications with the scientific staff are open, the director will make arbitrary and poor decisions, or decisions will go by default.

Conant has pointed out in another context the kinds of decisions faced by the laboratory director. ". . . many scholars have continued to dig assiduously but unprofitably in exhausted mines. All of which is inevitable and trivial except when loyalties and traditions urge men to claim either that digging is a worth-while activity in and for itself, or that the yield from an exhausted vein is full of gold. It is at this point that the argument between 'science for science's sake' and social utility begins. It may soon degenerate into an argument for the continuation of a particular line of intellectual activity merely because this has once been a fruitful direction of adventure. The argument soon becomes an emotional defense by those who love the field in question and who endeavor to support their loyalty by an appeal to general principles of the sacredness of all knowledge" (*4*).

It takes two to make an argument, and the argument in Dr. Conant's case is usually between the individual investigator and someone with authority to determine the resources available to him—a laboratory chief in government or industry or a department head or a faculty committee in a university. These decisions are the heart of "research planning" by the laboratory chief. They are hard to make. If they can be judged right or wrong, the judgment can be made only in retrospect, only as a batting average and only in the form of another set of interim judgments.

Research Planning as an Executive Function—Governmental and University. Another set of decisions affecting research must be made by the secretary of a federal department, the head of an industrial enterprise, or the president of an educational institution. Executives in these positions must weigh the

deployment of resources available to them, but their interests must center around the emphasis to be given to the research function as compared with other functions for which they are responsible, and the broad areas of emphasis of the research program.

Stated generally, "each step upward in the administrative hierarchy . . . should involve decreased concern with the concrete problems of research and increased concern with the more abstract problems of choice of facilities and of research programs" (*5*).

The way in which these problems of emphasis and choice are resolved constitutes a broad research plan, since the decisions fix the total scope of the research to be undertaken. In most federal agencies, the budget is the point at which the judgments are made final and effective.

But the problem is faced by universities, foundations, and industrial concerns as well as governmental agencies. For example, one of the broad choices to be made in guiding the general course of a university is to determine the relative emphasis to be placed upon the extension of knowledge, the conservation of knowledge, and the diffusion of knowledge. A determination of the emphasis to be placed upon extension of knowledge through research involves a deliberate choice among alternative courses of action. Specifically, the conditions under which a university will accept or reject gifts, the policies it will follow with respect to the volume of research grants or contracts that it will accept, determination of departmental budgets, establishment of general guides as to the kind of research that the institution will undertake, and selection of faculty constitute research planning on a relatively broad scale.[6]

Research Planning in Relation to Public Policy. The general planning decisions of individuals or groups responsible for specific research programs in industry, universities, and the government are in turn affected by forces which they may influence but which they do not control. At this stage, the idea of "planning"—already progressively attenuated through successive levels of decision making—virtually disappears. One is here in the area of the climate of public opinion toward science, general public understanding of science, the general place of science in our society, the schemes of values of large groups of people, and the broad political and economic forces that affect the volume and nature of research.

"Planning" in this area, if planning exists, is selection of national goals in the broadest sense. The planning process, if it exists, is the evolution of public opinion, and opinion in the scientific community, on general issues affecting science. One of these issues, indeed, is the extent and manner in which science can or should be "planned" nationally.[7] Another is the balance between secrecy and free communication, or between freedom of thought and behavior dangerous to the national security, that is best calculated to advance science and the national welfare. A third is the broad division of effort between applied research and development and fundamental research. A fourth is the proportion of the federal budget and of the national income properly devoted to research.

Attitudes toward and actions affecting these and other equally important issues affecting science as reflected in actions can be called "national science policy." In this sense, national science policy is as complicated and elusive as national policy in any other area of major concern to the country as a whole—foreign policy, agricultural policy, or management-labor policy. The issues seem amenable to discussion, clarification, and action not as a global whole but as separate but related areas.

At this level, the scientist reenters the planning scene as a citizen (*6*).

The Links Between Different Levels of Research Planning and Kinds of Research Planners. In this complex system for arriving at different kinds of decisions relevant to science, ranging from the design of specific experiments to quite general issues of public policy, there seem to be two particularly important conditions that must be fulfilled if the system is to work reasonably well. The first is maintenance of adequate communication between groups with different research planning functions. The second is the formulation of specific kinds of decisions at the level where competence and authority to make the decision are concentrated.

With respect to communication, the path must be open for the transmittal of the implications of research findings to the points where general decisions are made. To be specific, findings with respect to new weapons must be interpreted to those who lay strategic plans, and who determine procurement policy for the armed forces. In less applied fields, the potentialities of alternative areas of investigation must be communicated effectively to the points where budgetary decisions are made. Perhaps more important than communication of technical information is the existence of an understanding of the power of the experimental approach and of the conditions under which science will flourish or languish. Unless a substantial block of administrators has this understanding, the transmission of specific information is to a large degree meaningless. Indeed, decisions at higher administrative and political levels do not for the most part deal with the implications of specific findings—the early history of the atomic bomb project to the contrary notwithstanding—but with the general scale, the general areas, and the general conditions of scien-

[6] The establishment of a Committee on Institutional Grant Policy by the American Council on Education is an interesting example of the research planning process on this broad scale. The Committee's *Preliminary Report* (American Council on Education, Washington, D. C., Feb. 14, 1953) proposes a number of concrete policies which, if accepted, will constitute a form of research planning by universities.

[7] For expression of sharply divergent views on this complicated and highly controversial question see: Michael Polanyi, "The Planning of Science," *Pol. Quart.*, **16**, No. 4, 324–5 (1945); R. B. Goldschmidt, "Research and Politics," *Science*, **109**, 219 (1949); J. D. Bernal, *The Social Function of Science*. New York: Macmillan (1939).

tific endeavor. This certainly appears true with respect to the attitudes and level of understanding of the general public, which have a powerful and pervasive influence on science.

With respect to formulation of decisions at the appropriate level, a shrewd observer has said, "There is more inadequacy in government because of the inability of officials to operate on their proper levels than from any other single cause" (*7*). This is as true in the area of research as it is in other fields.

Trouble is ahead when an individual attempts to make decisions that people "above" or "below" him are more competent to make. Specifically, if the head of an agency makes a habit of dictating specific lines of experimentation, he is likely to destroy the effectiveness of the laboratory. This is true even though his judgment in some scientific areas may be superior to that of the staff. In other words, he is, or should be, restricted in the kind of research planning that he can productively engage in. Much of the fear of scientists engaged directly in research is based not upon the exercise of research planning functions by administrators, but upon apprehension that they will not confine themselves to appropriately general decisions. This apprehension is not always unfounded.

Whereas the making of detailed decisions by those who should confine themselves to general decisions is pernicious, an attempt by those appropriately concerned with detailed decisions to make general decisions is bound to be frustrating and futile. For example, the head of a laboratory cannot properly or profitably take upon himself the responsibility for indicating how much of the total federal budget should be allocated to medical research, even though he may have firm convictions on the matter. He can indicate what he believes to be a proper level of operation for the programs he heads, but he cannot assess the full array of factors that should influence the decisions of the secretary of his department, the Bureau of the Budget, and Congress.

It would, of course, be quite useful if one could formulate a set of criteria indicating precisely the kinds of decisions that people at a given level should and should not make. However, any attempt at precision in this area is almost certain to be a meaningless exercise because the nature of organizations, the content of research, and personalities vary so widely that what may be most productive in one situation may be quite disruptive in another.

One generalization that does seem sound, however, is that persons engaged in research planning should, in making decisions, or in refraining from making decisions, bear in mind not only the substantive merits of a question but also the level at which they act. If this consciousness of role were invariably present, common sense, reasonable sensitivity, and knowledge of the organization would substitute admirably for a set of precise guides.

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News and Notes

Mechanism of Enzyme Action

THE Symposium on Mechanism of Enzyme Action, sponsored by the McCollum-Pratt Institute, was held at the Johns Hopkins University, Baltimore, June 16–19, 1953. This symposium, concerned with the fundamental problems related to the mechanism of enzyme action, was a logical and necessary extension of the previous symposium on Copper Metabolism, held in 1950, and the two symposia on Phosphorus Metabolism, held in 1951 and 1952. The symposium was organized, under the skillful guidance of W. D. McElroy and his associates in the McCollum-Pratt Institute, to follow a gradual and smooth progression from a consideration of electronic and ionic forces to group transfer mechanisms.

The first two days of the symposium were devoted to the more theoretical aspects of protein structure and the nature of the forces which bind protein molecules with each other and with smaller molecules. In the session on Protein Configuration and Biological Activity, J. G. Kirkwood discussed the forces between protein molecules in terms of a theory of matching constellations of charges. According to this concept, fluctuating distributions would, by induction effects, give rise to patterns specific for the interacting proteins. The nature of the essential groups for enzyme catalysts was considered by R. Herriott, who reviewed the evidence for the participation of specific functional groups in enzyme reactions, with particular emphasis on the appearance of such groups when inactive enzyme precursors, such as chymotrypsinogen, are converted to the active form. The first session was concluded by W. Kauzmann, who discussed the re-