National Planning for Medical Research

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Let me say at once that I do not advocate the synoptic planning attempted by the systems analysts, or the balanced growth which is frequently taken as a prime desideratum, but, instead, recommend that those who plan a national medical research enterprise exercise skillful opportunism as they stimulate the growth of the system by relatively disjointed increments.

At first approach such planning seems simple. A small nation with limited resources of funds, facilities, and manpower need merely decide which is the most important biomedical problem in its part of the world and then direct those resources to solution of that problem. An economically well-developed nation, with substantially greater resources, might consider simply giving the entire system free rein in the expectation that its scientists will attack those problems which are important and approachable experimentally. Later, in retrospect, one might assess what had actually been accomplished. But neither approach is really acceptable. All the considerations which have been raised with respect to the allocation of some fraction of a nation's total resources to the biomedical research enterprise are equally appropriate when one attempts, in turn, to fractionate that enterprise. Accordingly, the problems posed by biomedical research in the smaller or less-developed nation *are* more simply managed than are those of the more complex nations. One cannot but feel that control of schistosomiasis or of frank malnutrition, for example, where these are endemic, is of overriding importance. Surely these much more properly command the attention of those concerned with the public health in such areas than do the more universal problems of heart disease, cancer, or genetic disorders.

For those responsible for decisions under such circumstances, I have but one counsel. Every research enterprise flourishes best when the group which is so engaged attains some meaningful, critical mass. Hence, a nation with one or two medical schools should seriously consider the possibility of developing only a limited number of research groups, each addressed to a problem of maximal concern to that nation and each large enough and so equipped and financed as to afford some prospect of success. Such success will not only have immediate relevance to the public health of the area but will effect a marked enhancement of morale and create an intellectual and political climate of richer opportunity for subsequent endeavors.

Only a handful of major clinical triumphs, such as the eradication of pellagra, penicillin therapy for syphilis, general antibiotic therapy, treatment of arthritis with steroids, and the recent accomplishments of vascular surgery, have, in the United States, served as catalysts which have opened the public purse for support of biomedical research. Those nations which, of necessity, can at present expect to mount only relatively more modest biomedical research enterprises may find it best not to engage competitively in those aspects of medical research which are under intensive investigation elsewhere. I do not mean to imply that individual scientists in smaller nations cannot successfully compete, for example, in molecular biology. Nor do I suggest that the scientists of the emerging nations must mark time for decades as they retrace from its beginnings the long evolution of medical research. Quite the contrary. The scientist born in one of the emerging nations but trained in one of the older laboratories and with access to current literature need suffer no handicap save the limitations of his own talent and of the resources which his society places at his disposal. Nevertheless, unless he can be joined by a sufficient group of competent colleagues, I believe he will best serve his own ends and those of his nation by addressing himself to a problem of unusual significance in his own locale.

Internal and External Pressures

The 2nd NIH International Symposium on Biomedical Research has emphasized the concept that, for science generally, two significant sets of pressures determine the allocation of resources: pressures which arise from within the scientific community and those which arise from without (*1*). This concept is equally applicable to the allocation of resources within the biomedical enterprise. The pressures from without are easily identifiable. They include the general aspiration to free man of cancer, of heart disease, of infection, of malnutrition, of fears in the night; society expects, and quite rightly, that much of the total research effort shall be directly devoted to these ends. They include the expectation that the biomedical community will operate an educational system which will produce physicians in sufficient numbers to provide adequate care for all members of society. They include the expectation that those engaged in research will reproduce their kind in numbers sufficient to assure an adequate continuing supply of individuals who will pursue medical science. And it is gratifying to recognize that they include a growing expectation that man will intensify not only his exploration of the universe in which he finds himself but his exploration and understanding of himself.

The internal pressures, generated by the research community itself, are less widely experienced but, unless modified, more likely to give direction to

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the conduct of research. For example, if left to its own devices, a substantial segment of the biomedical community is likely to eschew the immediate problems of disease. Some may enjoy the esthetics of enzyme kinetics, while ignoring metabolic disease; others may explore viral genetics, while ignoring the consequences of viral infection. Or, some wisp of the Zeitgeist may lead many to examine the mechanisms of carcinogenesis while none seek insights into the bases for schizophrenia. In sum, the scientific community continues to press for the vitality and expansion of the relevant scientific disciplines and for biological research at its most fundamental levels, preferring to defer direct attack upon overt disease until, in its view, the stage has been adequately set. In general, I share this approach.

It is the obligation of those charged with the responsibility for what is euphemistically called "planning for science" to be aware of both types of pressure, to admit that each is a valid criterion for decision making, and to recognize that neither set of pressures, alone, constitutes a sufficient basis for national decisions.

The Extreme Views

To be sure, each extreme view has had its exponents. At one extreme are statements such as that by Michael Polanyi (*2*), who argues, "No committee of scientists, however distinguished, could forecast the further progress of science except for the routine extension of the existing system. The pursuit of science can be organized, therefore, in no other manner than by granting complete independence to all mature scientists. The function of public authority is not to plan research but only to provide opportunities for its pursuit. To do less is to neglect the progress of science. To do more is to cultivate mediocrity and waste public money." The adherents of views such as this are numerous, and history documents their claims. Indeed, in only a handful of instances has organized society recognized a major problem and directed to it the scientists who found an appropriate solution.

In this country, for example, our Public Health Service recognized the threat posed by pellagra in our Southeast and dispatched Joseph Goldberger to investigate the problem. His triumph is now history, but it is rather ironic that, having prejudged the nature of the problem, the Public Health Service dispatched a bacteriologist to address himself to what proved to be a nutritional problem. And if this tale has any moral it is that the triumph reflected the genius of the investigator rather than the wisdom of those charged with allocating the then meager resources of the U.S. Public Health Service. How many instances of societal planning of successful major advances in the elucidation of human biology or in the understanding, prevention, or treatment of disease can one add to such a list? The development of Atabrine, understanding of the etiology of retrolental fibroplasia, the development of antiviral vaccines, and control of insect-borne diseases are among the relatively few such major, planned accomplishments. The development of new drugs by the laboratories of the pharmaceutical industry, an arm of organized society, must also be included.

On the other side of the ledger—that of the unplanned accomplishments which we owe entirely to the imagination and initiative of individual investigators—is virtually every other major advance in man's understanding of himself and of the disorders to which he is subject. Surely this history indicates that the criteria for research support which arise from within the scientific community are generally valid. In fairness, however, let it be said that large-scale public support of research and the opportunity to "plan" are recent phenomena, and this judgment must be held in abeyance.

Nevertheless, many concur with Hogben (*3*), who said, "To get the fullest opportunities for doing the kind of work which is worthwhile to themselves, scientific workers must participate in their responsibilities as citizens. Among other things, this includes refraining from the arrogant pretense that their own preferences are sufficient justification for the support which they need. This pretense, put forward as the plea that science should be encouraged for its own sake, is a survival of Platonism. Science thrives by its applications. To justify it as an end in itself is a policy of defeat."

Such statements engender much controversy—and properly so. Patently, modern society supports the laboratory of a scientist not so that he may amuse himself but, rather, in the hope that his activities will, in some measure, make possible realization of one of society's own expectations. To be sure, these expectations include, broadly, the advancement of knowledge, but this ranks well below the hope that the scientist's findings can soon be translated into some practical end. Accordingly, in this country we have attempted to manage a national enterprise which provides opportunity both for the scientific giants whose research, freely undertaken, results in "quantum jumps" in our understanding and for those scientists who seek to exploit such understanding in the common interest.

In our own time it has become apparent that planned science—here I use the term *planning* rather broadly—*is* feasible. There have been no planned breakthroughs, nor are there likely to be any. But there can be and there has been planned exploitation of such breakthroughs. Not even Fleming planned his astute observations, but the subsequent effort required to produce penicillin and to determine its structure was most effectively planned. Society did not plan Enders' observations of viral propagation in animal tissue in culture, but society did plan the large program which supported the development of effective antiviral vaccines. Society did not plan the observations which led to the strong suspicion that elevated concentration of serum lipid is related to the development of atherosclerosis and myocardial infarction, but society can and does plan the effort necessary to validate that conclusion and to develop means for alleviating this disorder. Watson and Crick were free scientists, engaged in a problem of their own choosing, but society could and did plan to support the broad-scale effort required to amplify their hypotheses, in the hope of bringing understanding of those phenomena which underlie genetic disorders of man, viral infectivity, and perhaps cancer.

But the administrators of science must not plan the *doing* of science. They can but plan *opportunities* for the doing of science and hope that talented, competent investigators will avail themselves of such opportunities. Effective planners may not do less and should not do more.

Planning a Research Enterprise

It becomes apparent that, in attempting to plan a national biomedical research enterprise, one must view the enterprise while simultaneously considering each of a series of seemingly independent parameters. Among these are the various diseases which ravage mankind, perhaps the organ systems of which man is built (liver, kidney, brain, and so on), the continuing vitality of each of the related scientific disciplines, and the integrity of the academic institutions in which much of the research is to be performed. One must weigh the relative importance of research done on man himself and research performed on animals or model systems; of research in the laboratory and research in the field; of research in areas clearly identifiable as "biomedical" and research, essential to an understanding of life, in tangentially related disciplines; of the support of research and the support of training for the future conduct of research; of the support of research and research training and the support of education in clinical medicine; and of hosts of seemingly lesser parameters. Each of these parameters is relevant to each decision concerning the planning and funding of individual research programs.

At this point one might visualize the development of a matrix in which each parameter has a weighted value and is brought to bear on each decision; this would be an idealized version of the approach of the operations or systems analyst. Successful development of such a matrix would seem to suffice for the total planning operation, and all one would then need to know would be the total appropriation to be made available by the state in any one year; all other decisions would then be automatic. This is an exaggerated version of what Charles V. Kidd has termed "allocation in multiple dimensions." In the exaggerated form here presented it is rather horrendous to contemplate and, no matter how conscientiously or painstakingly developed, is guaranteed to yield many decisions which time will prove to have been incorrect.

In a limited sense, however, the principle does have merit. Those encharged with planning responsibility must indeed be aware of the various criteria which are meaningful in the decision process. They must assure society that none of the meaningful parameters have been neglected, although they cannot possibly guarantee that a perfect balance among them all has been assured. Indeed, such balance is not even necessarily desirable.

Happily, in the real world, matters can proceed more easily and more successfully than the novice in planning might have thought. No nation has actually engaged in such detailed allocative planning. In most instances planning has been done, rather, in a single dimension. Resources have usually been allocated by disease or by discipline, or, in nations with university grants systems, have simply been apportioned among universities and other appropriate institutions. But for our purposes it is important to note that the other dimensions do exist, whether they are planned for or no. Each research project which is supported, or for which support has been denied, has relevance in virtually every possible planning dimension. And, in annual retrospective examination, it is imperative that the operation of the system be examined in as many dimensions as possible, so that, if necessary, corrective action may be taken. One can hope in this way to assure that certain broad priorities are operative. Probably highest among these is the assurance that, at all times, a future generation of investigators is being trained and that their number bears some reasonable relationship to the desired future magnitude of the national research enterprise. Second priority might be given the assurance that all the disciplines currently meaningful on the biomedical scene are given sufficient support to assure a vigorous national effort. Third priority might relate to the vitality of academic institutions and of individual laboratories. In fourth place might be the distribution of resources by disease categories, ranked in the order of the severity of such disorders in a given community. The fact that it is this fourth priority which is frequently given most obvious expression relates to political considerations rather than to the internal logic of the system.

It will be evident that in a nation confronted with a planning problem of this magnitude there *already* is a system in being which can be retrospectively examined and corrected. Indeed, much of what is called planning is essentially remedial in that it seeks to rectify apparent errors rather than move toward planned objectives. Planning proceeds from an existing base, and each proposed increment to the existing system can be considered rather readily from the multidimensional standpoint.

Allocations and Adjudications

These thoughts, lead, then, to consideration of the actual process whereby one establishes allocations within a budget and then adjudicates the competing claims of individuals or institutions within some category of that budget. Patently, this cannot be done in an information vacuum. The establishing of allocations is the more complex task, as it demands a weighing of the values of the internal and external pressures. These pressures certainly vary among nations, and in any one nation they must vary from time to time. In any case, they can only be designated as weak, strong, or paramount. Thereafter one requires real data descriptive of opportunities: knowledge of the number of competent investigators interested in a given area, of the physical facilities, of the number of students in training, and of the cost of doing business in a typical research group; and, most importantly, an assessment of the "state of the art" in each subfield of research endeavor—that is, an informed guess concerning when the time is right, conceptually and technologically, to increase significantly the level of effort in a given research area. Evaluation of this information and appraisal of the scientific field should permit tailoring of the demands of the scientific community to the interests of society. They yield a crude determination of the relative magnitudes of support to be given, for example, to fellowship programs, arthritis or dental research, genetics or pathology, clinical or basic research.

Such considerations are particularly germane to those components of the system which are properly called "small science"—science in which the individual professor or senior investigator and his coterie of junior colleagues are the meaningful productive and budgetary unit. Whether he works in a government-operated establishment or in a university where his work is supported by a national research grants program is inconsequential. When the funds available are less than those requested by the scientific community (and this should always be the case, else excessive funds have been provided), competing requests can be evaluated only on the basis of intrinsic scientific merit—that is, the competence of the investigator and the imagination, soundness, and feasibility of his proposal. The evaluation can be made only by a jury of his peers, drawn from a national panel of experts. To be sure, they may share his enthusiasm for his discipline but they are not rivals, on his local scene, for prestige, salary, space, or influence. It is the lack of this evaluative process which is the cardinal weakness of a university grants system and of other purely bureaucratic administrative devices. Conversely, it is the operation of this evaluation system which is the best guarantee that society will get its money's worth.

Proposals for "big science" are rare in biomedical research. They must be examined closely both for their intrinsic value and for the harm they could do the rest of the system through imposing a drain on manpower, facilities, or funds. By and large they are foreign to the university biomedical community, and, if they are desirable at all, their operation is a proper function of government or of a contractor-agent.

The greatest advantage of incremental planning is the fact that such planning makes it possible to seize previously unforeseen opportunity. And it is here that the quasi-mathematical approach to total planning fails most seriously, since it does not take into account the manner in which science itself grows. Let us consider this in some detail.

Balanced and Unbalanced Growth

There is a great temptation for those engaged in planning to attempt to project systems of "balanced growth." Indeed, "balanced growth" has been the acknowledged objective of most of those who plan a nation's economy, its weapons systems, and its support of science generally as well as its support of biomedical research. Although planners frequently recognize that they cannot realize this ideal, this so-called balanced system is the proximate objective of their development programs. As noted by Hirschman and Lindblom (*4*), the basis for this ideal is a "faith in the existence of basic harmonies similar to the Greek belief that the truly beautiful will possess moral excellence as well." It seems opportune, therefore, to direct to your attention a recent series of papers which have taken striking exception to the concept of planning balanced growth of a large enterprise and have advocated in its stead a process which has been called "disjointed incrementalism."

Because the analogies are pertinent to the problems here considered, it seems appropriate to summarize the views of various members of the group who advocate this process. For example, Hirschman (*5*), an economist, has offered as the basic defense of *un*balanced growth the concept that an economy's resources should not be considered as rigidly fixed in amount. He argues that more resources or factors of production will come into play if development is marked by sectoral imbalances, since these will arouse private entrepreneurs or public authorities to action. In the present context, there are many analogies. For example, the existence of a large pool of investigators who lack facilities for their activities constitutes a pressure which, ultimately, will result in the construction of new and more adequate facilities. The appearance of large numbers of young men and women desirous of training in biomedical research results in pressure which leads to the development of fellowship and training programs. Recognition that a temperate bacteriophage can disappear into the genome of the host bacterium, be reproduced with that genome for many generations, and then reappear in vast numbers under adverse circumstances prompts many investigators interested in the nature of the viral origin of cancer to take a new tack in their explorations. As Hirschman has said, to the extent that the imbalance is self-correcting through a variety of mechanisms, unbalanced growth may propel the economy forward jerkily but also more quickly than by planned, balanced expansion.

Klein and Meckling (*6*), students of development policies for weapons systems, allege that a given development is both less costly and more speedy when marked by duplication, confusion, and lack of communication among people working along parallel lines. They argue against early attempts at integrating subsystems into a well-articulated, harmonious general system. They advocate, instead, the full exploitation of fruitful ideas regardless of their fit to some preconceived pattern of specifications. The principal basis for this attitude is the very fact of uncertainty. They note that the final configuration to be developed is, in any case, unknown, and that knowledge increases as some of the subsystems become articulate. Knowledge about the nature of any one subsystem increases the number of clues concerning the desirable features of another, just as it is easier to fit in a piece of a jigsaw puzzle when some of the surrounding pieces are already in place. What is important is to develop the pieces; one can adjust them to each other later. This view argues for maximum support of the current enthusiasm for molecular biology even though its immediate clinical application seems remote, and for vigorous follow-up of clues to the possible viral pathogenesis of cancer even though the major psychoses remain enigmas and relatively few biologists seem to be immediately concerned with their elucidation. Similarly, it argues for full support of all the competent scientists in our midst, even though this results in overcrowding of their laboratories.

Lindblom (*7*), who has been concerned with general aspects of policy making, takes as his point of departure a denial of the general validity of an assumption which is implicit in most of the literature on policy making—that there exists sufficient agreement to provide adequate criteria for choosing among possible alternative policies. This assumption is often questioned in contemporary social science, yet many of the most common prescriptions for rational problem-solving follow only if it is true.

Conventional descriptions of rational decision-making include the following steps: (i) clarification of the objective or values; (ii) survey of alternative means of reaching objectives; (iii) identification of consequences, including the side effects or by-products of each alternative means; and (iv) evaluation of each set of consequences in the light of the objective. However, Lindblom notes that such synoptic attempts at problem solving are not possible when, for example, clarification of objective founders on social conflict, when required information is not available or is available only at prohibitive costs, or when the problem is simply too complex for man's finite intellectual capacities. Most importantly, it does not logically follow, Lindblom argues, that when synoptic decision-making is extremely difficult it should nevertheless be pursued as far as possible. Hence he suggests that, in many circumstances, substantial departures from comprehensive understanding are not only inevitable but desirable. I cite his thesis in detail because the analogy to me seems so close.

Working Principles

I have summarized the case for what may be called "semi-planning." What are the working principles of this approach? A few major notions are worthy of consideration. (i) An element of laissez-faire, with its attendant duplication and gaps, may well be desirable rather than abominable. (ii) Orderliness, balance, and detailed planning may be more satisfying to the planners than to the society they serve; some matters probably ought to be left to what has been called "a wise and salutary neglect." (iii) It is unwise to specify detailed objectives in advance when the means of obtaining them are virtually unknown. (iv) A rational problem-solver wants what he can get and does not try to get what he wants except after identifying what he wants by examination of what he can get. (v) Arrangements must be established whereby decision-makers are made aware of, and can react promptly to, emerging problems. (vi) Long-range planning is a valuable exercise, but long-range plans for a research enterprise which is the sum of many smaller research programs are of dubious validity.

These principles, taken in part from Hirschman and Lindblom (*4*), approximate a real world which is almost invariably characterized by unbalanced, not balanced, growth. It is the above-scale salary offered to the new appointee which is the surest guarantee of an increase in the scale. It is the existence and success of the National Science Foundation which provides the platform on which stand those who argue for establishment of a National Humanities Foundation. Instances of the principle that imbalance results in pressure for a correcting growth are commonplace. And these same principles seem entirely germane to the planning of a national biomedical endeavor which is as inherently sporadic and random as is the natural growth of science itself. Indeed, the hallmark of the competent investigator is that he seeks constantly to identify the most important problem which can be attacked with the technology currently available and limits his goals accordingly. But his attention is continually given also to developments within his own and related disciplines. He is quick to apply new information, new techniques, new apparatus. In short, he brings to research his imagination, his knowledge, and his technical know-how, and he combines these with what may best be described as a "skillful opportunism."

In my view, those responsible for the management of a national enterprise which is the sum of such individuals must do likewise. They must continually assess the major parameters of the enterprise for which they have responsibility, continuing the attack on the major public-health problems, insuring the vitality of the classic scientific disciplines and recognizing the emergence of new ones, insuring the training of new investigators and practitioners, and safeguarding the health of the medical schools and universities. The total system may then be nourished and made to grow, but by disjointed increments. For example, given a 10- or 20-percent increase in total funds, one should almost never expand support, across the board, of all existing programs by this 10 or 20 percent. Instead, one should take advantage of significant, albeit unplanned and unexpected, new knowledge of human biology or pathology, of the work of new investigators as it appears, of new approaches, new drugs, new apparatus, new facilities, new architecture, and newly awakened public interest, always utilizing the skillful opportunism characteristic of the individual investigator.

Goals may be set only in the broadest terms of ultimate objectives—for example, a general homotransplantation, effective cancer chemotherapy, a rational management of viral infections, genetic transformation as therapy for hereditary disorders or the prevention of atherosclerosis. And one can, in a general way, plan *for* the tasks ahead by providing the necessary physical plant, stimulating activity in biomedical engineering, and providing a sufficient number of specialized facilities such as animal colonies, hyperbaric chambers, and libraries. It is highly doubtful that the planner can wisely do more; he will fail in his responsibilities if he does less. And he must ever be mindful that the planning of science must be left to the working scientist.

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