Models for Science Planning
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by the airport operator. Not until widespread pressure grew for remedial action was meaningful federal action undertaken to deal with the problem.

16. "The Transportation System of Our Nation," Message from the President of the United States, U.S. House of Representatives, Document No. 384, 87th Congress, Second Session, April 5, 1962.
17. See particularly Charles L. Dearing and Wilfred Owen, *National Transportation Policy* (Washington, D.C.: The Brookings Institution, 1949); *Issues Involved in a Unified and Coordinated Federal Program for Transportation*, A Report to the President from the Secretary of Commerce (Washington, D.C.: U.S. Government Printing Office, December 1, 1949); *Revision of Federal Transportation Policy*, Report of Presidential Advisory Committee on Transport Policy and Organization (Washington, D.C.: U.S. Government Printing Office, 1955); *Federal Transportation Policy and Program* (Washington, D.C.: U.S. Government Printing Office, March 1960); *National Transportation Policy*, Preliminary Draft of a Report to the Senate Committee on Interstate and Foreign Commerce, 87th Congress, First Session (Washington, D.C.: U.S. Government Printing Office, 1961).
18. For a discussion of the fear of centralized power in the administration of federal transportation policies, see George C. Wilson, "Congress Fears Creation of Czar in Transportation Department Legislation," *Aviation Week & Space Technology*, Vol. 84, No. 15 (April 11, 1966).
19. Whether such criteria could be applied successfully to transportation planning is still an open question. Certainly the application of such techniques as cost-benefit analysis to the field has been extremely limited. Even where it has been employed extensively, such as in highway planning, the emphasis has been put on *micro* applications. For a review of some of the enormous problems involved in applying such techniques, see particularly Robert H. Stroup and Louis A. Vargha, "Reflections on Concepts for Impact Research," *Highway Research Board Bulletin 311*, "Impact and Implications of Highway Improvement," (National Academy of Sciences, National Research Council, 1962); Alan K. Campbell and Jesse Burkhead, "Public Policy for Urban America," in Harvey S. Perloff and Lowdon Wingo, Jr. (eds.), *Issues in Urban Economics* (Baltimore: The Johns Hopkins Press for Resources for the Future, Inc., 1968), pp. 603-606; Floyd I. Thiel, "Social Effects of Modern Highway Transportation," *Highway Research Board Bulletin 327*, "Indirect Effects of Highway Improvement" (National Academy of Sciences, National Research Council, 1962); Sidney Goldstein, "Non-User Benefits from Highways," *Highway Research Record Number 20*, "Highway Financing" (National Academy of Sciences, National Research Council, 1963); E. J. Mishan, *The Costs of Economic Growth* (London: Staples Press, 1967); Tillo E. Kuhn, "Economic Concepts of Highway Planning," *Highway Research Bulletin 306* (National Academy of Sciences, National Research Council, 1961), p. 81; and John R. Meyer, "Transportation in the Program Budget," in David Novick (ed.), *Program Budgeting* (Washington, D.C.: U.S. Government Printing Office, 1964).

MODELS FOR SCIENCE PLANNING

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A CHARACTERISTIC OF SCIENTIFIC RESEARCH is that the intellectual issues with which it is concerned are seldom uniquely related to the purposes or ultimate use of the research. This is especially true for basic research, but it is to some extent true for all research which is not directly developmental, i.e., aimed directly at the creation of a product, system, or process largely envisioned in advance. This lack of one-to-one correspondence between research activity and its end product makes the problem of planning peculiarly difficult. The research enterprise is more like an organism than like a collection of objects. The removal of one part may degrade the functioning of the whole organism and not just the particular function ostensibly served by the part removed. On the other hand, some parts are more essential to the functioning of the organism than others, and what may seem least important from the point of view of its own unique function may be the most important from the viewpoint of the organism as a whole.

In this article I intend to discuss four models of the research enterprise which might serve as partial bases or rationales for planning the support and development of science. My purpose will not be to suggest that one model is more valid than another, but rather to suggest that each has some validity and a claim to serve as a partial basis for allocation of public resources to science. Each implies different mechanisms and criteria of choice, and the scientific system as a whole should consist of a mixture or blend of different allocation mechanisms in which the criteria appropriate to one of the four models are dominant. The models used represent an elaboration of views first put forward in my paper, "Can Science Be Planned?" [1]

Underlying Rationale for Public Support of Research

There are four main reasons for the support of scientific research: (1) cultural, (2) economic, (3) social, and (4) educational.[2] Opinions differ as to the proper measure of federal responsibility for each of these purposes. For example, in the United States, government responsibility for the support of the arts and other cultural activities has been accepted only recently, although there is a long tradition of tax exemption for cultural and educational gifts and institutions, which constitutes an indirect form of government subsidy. After World War II the federal government did assume responsibility for the support of basic science, but primarily for reasons of national security rather than culture. Similarly, the suport of research as part of the underlying basis for economic growth is very recent. Serious federal concern with the relationship between publicly supported science and technology and the structure and growth of the economy dates only from the early 1960's. Agriculture represents an outstanding exception, in that federal responsibility for the health and technological advancement of the agricultural sector of the economy has a long history. Agricultural research was not merely a service to a segment of the economy, but formed part of a comprehensive national policy for the agricultural sector as a whole. By contrast, the National Bureau of Standards grew up to serve the needs of industry, but never constituted part of a comprehensive policy for the development of the industrial sector. Similarly, in higher education, federal responsibility was arrived at largely by accident, as a by-product of the support of research for other national purposes, first in defense, later in atomic energy, and most recently health. In recent years the federal government has become responsible for graduate education and associated research in the natural sciences and engineering *de facto* but not *de jure*, and even today there is debate as to the extent and nature of federal responsibility.[3] Of the four purposes for support of research only the social is fully accepted as a governmental responsibility because the goals themselves are recognized as a federal responsibility, for example in defense, space, atomic energy, and public health.

In the economic sphere there has grown up a gradual recognition that research whose benefits are not uniquely appropriable by any private interest may be more efficiently funded from tax sources. This is especially true of basic research. Knowledge which can be used for many purposes and by many different private groups is best developed by sharing the cost among all the potential beneficiaries. The basis for this is best provided by the tax laws. If the knowledge were developed by private competitive corporations, the tendency would be for it to be kept secret in order to maximize the competitive advantage resulting from it. This would result in wasteful duplication of effort, and would be highly inefficient. In the opinion of some observers this inefficiency already exists in some industries, such as the pharmaceutical industry where a great deal of research effort goes into the development of minor variations on a competitor's product in order to protect a patent position. In technology the patent system does provide a mechanism by which new knowledge can be made public while permitting the inventor to appropriate the benefits. In science there would be insufficient incentive to patent new knowledge, because its commercial usefulness is unknown at the time of discovery, even if the patenting of laws of nature were legally possible. As a result of such arguments, government support of basic research was at least partly justified during the 1960's because of its long-term economic benefits.

Recently the goal of economic growth without reference to the quality or direction of growth has come under severe attack.[4] Growth for its own sake is being replaced by the concept of "directed growth"—the channeling of economic growth in ways which enhance the "quality of life" and preserve the "amenities" including especially the natural environment. Making the complex choices necessary for directed growth requires much deeper and more comprehensive scientific understanding than does technological innovation by itself. Thus the concept of directed growth provides an enhanced rationale for the public support of research, and may even call for the transfer of certain types of research from the private to the public sector.[5]

The educational rationale for the support of research is the easiest to quantify, once a federal responsibility is accepted, although such quantification may be deceptive because of the omission of considerations of the quality and adaptability of both the inputs and the outputs of the system. However, there are two rather contrasting approaches which may be characterized as supply-oriented and demand-oriented. In the supply-oriented approach, which has been implicit in most American thinking until recently, the capacity of each level of the educational system is determined by the size of the cohort of students desiring to enter it and having the capability to meet its requirements. This was the basis, for example, of the White House scientific manpower report of 1962.[6] On the basis of past experience it was believed that the supply of skilled manpower would tend to create its own demand, that the capacity of the educational system, private industry, and government would expand to absorb the output of the educational system. In the 1950's, indeed, new government programs had always made their appearance to absorb this output, and there had been, in fact, a considerable transfer of scientists and engineers into research and development activities.

In recent years there has been a tremendous spurt of Ph.D. production in science and engineering, particularly the latter. The annual output of engineering Ph.D.'s has been increasing at the rate of 12-15 per cent and has barely begun to level off. While this has been happening, there have been few dramatic new thrusts in governmental programs for "national missions" such as new weapons systems or the space program. Despite this, there has as yet been no serious unemployment or even underemployment among science and engineering Ph.D.'s, although the slackening of the boom seller's market of the 1950's and early 1960's has created apprehensions bordering on panic which considerably outrun reality. Moreover, the longer-term outlook is rather pessimistic as suggested by recent studies.[7]

In the demand-oriented approach, an attempt is made to anticipate the occupational requirements of society and the need for scientists and engineers to attack present or anticipated social or industrial problems. It is then suggested that the capacity of the educational system be geared to meet this anticipated demand. Such an approach has become more fashionable recently as signs of the saturation of the market for Ph.D.'s have begun to appear. The weakness of this approach is simply that in the past we have been very poor at foreseeing new demands more than a few years ahead. The educational system responds very sluggishly to the directional signals of society, and usually the situation which generated a given response has greatly changed by the time the educational output has been appreciably affected. This is simply because of the length of the educational pipeline, and the fact that the universities in effect have to make long-term commitments in terms of facilities and faculty appointments even to meet relatively short-term needs. This inhibits both expansion to accommodate new needs and contraction in some areas to meet a reduced relative demand. This problem is aggravated by the fact that society has been changing its mind more and more rapidly of late, and the academic system has to be responsive not only to real needs but also to alleged needs as currently perceived by public opinion or the political system.

The Four Models of the Research System

The four models may be summarized as follows:

1. Science as a quasi-autonomous, self-structuring activity which ultimately benefits society but must be left to develop according to its own internal dynamics if the social benefits are to be realized most efficiently. This will be called the Polanyi-Price model.
2. Science as a technical overhead on applied research and development aimed at specific social objectives or missions. This will be known as the Weinberg model. (Ben David)
3. Science as a social overhead investment in which research and education in the broadest sense of the development of institutional and individual capacity to the maximum potential are inseparable objectives.
4. Science as a tertiary industry or consumption good, representing the nonmaterial, nonproduction aspect of the goals and activities of an increasingly affluent society. This will be known as the Toulmin model.

These four models only partially correspond to the four purposes of scientific activity discussed earlier. Thus the Polanyi-Price model corresponds most closely to the cultural purpose, but also serves the economic, social, and educational purposes. The Weinberg model emphasizes the economic and social goals. The social overhead model corresponds most closely with the educational purpose, but is also important for economic and social goals. The Toulmin model is again, primarily cultural, but has strong social overtones in connection with the "quality of life." The four purposes serve primarily as a qualitative rationale for the public support of science, while the four models are planning models which have a potential for serving as indicators for more quantitative planning, and for providing a means by which the output of the scientific system might be measured or assessed.

The Quasi-Autonomous Model

This is the model dearest to the hearts of scientists, since it corresponds most closely to the scientific ideology. It was rather clearly foreshadowed in Bacon's writings, and it emphasizes the cumulative and community character of scientific activity.[8] The underlying concept is succinctly summarized by Bernard Barber in the following sentence:

> However much pure science may eventually be applied to some other social purpose than the construction of conceptual schemes for their own sake, its autonomy in whatever run of time is required for the latter purpose is the essential condition for any long run applied effects it may have.[9]

The Polanyi and Price views of this model are somewhat different. In the Polyani view science is a delicate plant to be carefully nurtured by society and protected from the rough winds of political interference and social direction. Too much eagerness to harvest the fruits of science would kill or distort it and make the process of scientific discovery costly and inefficient. On the other hand, Derek Price[10] tends to regard science as a rather vigorous weed with which society would find it difficult to interfere even if it wished to. He bases this view largely on the empirical observation that the national output of scientific papers seems to follow rather closely a nation's GNP. Thus the U.S., with 30 per cent of the world's GNP, accounts for about 30 per cent of the papers abstracted in *Chemical Abstracts*. This relationship exists despite wide variations among nations in their policies towards science, their support mechanisms, and their commitment to applied research and technology.

The Polanyi-Price view of science bears a close resemblance to the theory of *laissez-faire* economics. Science, like the economy, is viewed as a highly decentralized system of rational decision makers, each striving to enhance his own reputation and recognition by contributing to the common pool of scientific knowledge and theory. This decentralized system is viewed as if it were guided by an "invisible hand" which optimizes the progress of the conceptual structure of science much as the self-interested behavior of entrepreneurs was supposed to optimize economic productivity.

In terms of this model, the problem is how to measure the output of the system in a quantitative way. One way is by counting scientific papers, a method heavily exploited by Derek Price. For this to be meaningful, it is necessary that each paper be certified in some way as a valid contribution to science. This certification is analogous to purchase by a customer in the economic analogy, and is provided by the refereeing system, at least in theory. Even the counting of referred papers is not very satisfactory, since there tends to be hierarchy of journals, and eventually an author may find one that will accept his contribution. There is thus an insufficient quality factor attached to individual papers, and similarly no account taken of differential productivity among individuals and groups. A refinement of paper counting is to use citation indices to estimate the impact of, and thus the relative scientific value of, individual papers, with frequently cited papers being given a higher weight.

Still, these are all relative measures, and leave us with the question: How can we measure the rate of advance of science as a whole, especially in view of the fact that the system is constantly changing in the sense that what is significant today may be much less significant tomorrow? For example, how does one calibrate the progress of science against the fact that in each subfield discovery becomes more and more difficult and expensive as the subfield matures and the main conceptual framework is established? Price has recently shown that rapidly advancing subfields can be identified by an unusual predominance of very contemporaneous citations in the literature.[11]

Another possible basis for judging the output under this model may be found in international comparisons. Nobel prizes per capita might be one such measure, for example. This type of measure has been used in a quite convincing way, showing historical trends, by Joseph Ben David.[12] Other comparative measures include invited speakers at international meetings,[13] tabulations of informed opinion,[14] or critical reviews of papers, such as *Math Reviews*, national or international awards and honors.

As in macroeconomics, these science "indicators" have the advantage of a common unit of measure. The value system used is that which is internal to science itself.

The Technical Overhead Model

In this scheme, popularized by A. M. Weinberg,[15] society is viewed as allocating resources primarily to technology in the broad sense of developing or improving society's capability for achieving certain predetermined nonscientific goals. The allocations are made to agencies, private or public, which are responsible for certain social missions or services, and these in turn devote a certain proportion of the resources available to them to the support of general-purpose science more or less related to their mission. This model has the virtue of being a fairly accurate description of the pluralistic philosophy for the support of science which has evolved in the United States during the years after the war. This scheme may be described by saying that each level of management is expected to set aside a certain proportion of the funds available to it for general-purpose research, that is, for research having objectives beyond an immediate time-bound developmental goal.

This model emphasizes much more explicitly than the first model the interaction between science and technology and between technology and society. Science is thus evaluated as a means to other ends, and the problem becomes how to measure its contribution to these ends in a quantitative fashion. The "overhead" metaphor is analogous to overhead functions in a business where it is difficult to attribute the contribution of a particular function such as central staff to a particular product line or business. The problem of quantification is to establish a legitimate percentage of technical overhead. In industrial research it is customary to look at this in terms of percentage of gross sales, or possibly of total value added. In part this is the result of the competition between firms for technical talent, since the opportunity to engage in research of their own choice may be thought of as part of the compensation of scientists and engineers, to some extent exchangeable for salary. Thus it may be that the percentage of sales devoted to general-purpose research will be in some measure a function of the ratio of demand to supply of technical talent. This figure varies greatly from industry to industry, but tends to be fairly constant among different size firms within a single industry classification.[16] It might be considerably closer to constant between industries if all the research connected with a particular product line were taken into account, regardless of whether it is conducted by the manufacturer himself or by another supplier of inputs, as in the case of synthetic fibers supplied to the clothing industry, or building materials supplied to the construction industry. One might imagine the possibility of developing input-output tables which would measure the research associated with the output of a given industry not only in terms of its own research expenditures but also in terms of the prorated research expenditures of its suppliers. In such cases, however, the general-purpose research would have to be assigned as an overhead, distributed according to the value added for each product or service component of a firm's output.

A related approach might be to divide all the activities of society into broad functions such as transportation, shelter, communications, recreation, protection, health, etc., and allocate to each of these functions those parts of the total national research and development effort which can be fairly uniquely attributed to a given function. For general-purpose scientific research, the ultimate applicability of which is too uncertain to be ascribed to particular functions, the total of such research would then be aggregated and distributed as an overhead to all functions based on a percentage of the directly allocated R and D for each function. Such an exercise would involve many arbitrary and subjective decisions and value judgments. Nevertheless, it could be quite useful in arriving at a general picture of how the development of science serves the society and how the allocation of technical resources to social goals is changing with time.

The analysis of R and D expenditures, like marginal analysis in economics, assumes full employment of resources and full and instant interchangeability of technical resources with respect to various national goals. In fact, such interchangeability is only true over relatively long time periods. Idling technical and scientific resources does not benefit society if the resources are not or can not be redeployed to serve ends which society prefers. If we exclude development expenditures, only about one per cent of the GNP goes into scientific research. Thus it is difficult to argue that there is a scarce resource problem with respect to the money spent on research. If there is a scarcity problem, it must relate to the availability of skilled and educated manpower either as between different areas of science or as between scientific research and other societal activities demanding similar levels of education or skill. Thus, by itself, the argument that we ought to stop the space program in order to devote the resources to poverty or the cities only has validity to the extent that the *real* resources used in space research could help immediately in the other areas. Merely creating unemployed brainpower will not automatically result in its application to the problems that society is newly interested in. Even if the brainpower were in fact transferred from space to the poverty program, there is an implicit assumption that the same amount of effort applied in the new area would produce comparable results. This is not true. In science and technology results do not bear any simple one-to-one relation to expenditures or effort, and a large deployment of targeted effort can even be of negative value if the proper foundations of understanding and insight for a targeted program are lacking.

Another approach to the overhead measurement of research is the sort of analysis carried out in the National Academy of Sciences report on chemistry.[17] In this report an attempt was made to identify basic research contributions to specific advances in industrial chemistry by counting the basic research publications referenced in the associated patents or reports. This is hard to make quantitative because there is no consistent way of weighting the importance of each reference. But it does have considerable value. Similar objections apply to other studies such as Project Hindsight, which attempted to identify the technical "events" leading to the development of selected military weapons systems.[18] In the absence of a better procedure, each event was treated as of equal weight, and the events used were essentially precipitating events, with relatively little attempt made to trace them back to their origins and no account taken of the way in which the background of scientific knowledge contributed to the choices of direction in the applied research and development process. A somewhat more sophisticated approach was used in the so-called TRACES report prepared by a group at the Illinois Institute of Technology for NSF,[19] but here again events tended to get lost in the dim fog of technological history.

All of these analyses are attempts to treat science primarily as a part of the overall process of technological innovation, and the overhead concept is simply a device to assign that portion of scientific activity which is not clearly attributable to a particular social or economic goal.

The difficulty with the technical overhead concept as a model for planning is that, while it may be related to direct research costs, there are not clear criteria on what percentage of direct costs should be devoted to general-purpose research. Evaluating the payoff from such research is somewhat similar to evaluating the payoff from management in an institution. The costs are visible, but the benefits are intangible. Merely to demonstrate anecdotally that basic research pays off, and has paid off in the past, does not say much about the marginal return of additional basic research. Even in terms of internal scientific criteria the payoff from research is statistical.

Social Overhead Model

This model is quite similar in principle to Weinberg's technical overhead model, except that the research is regarded as underlying all the purposes of society. A social overhead investment is an investment in the sense that it underlies the productivity and social effectiveness of a society but does not contribute uniquely to any one aspect of it. In this model the support of science is very closely tied to the support of education. In a sense scientific research is merely a sophisticated extension of education, a means of creating capacities in individuals and institutions which can be used for a wide variety of purposes. "Every society," says Daniel Bell, [20] "now lives by innovation and growth, and it is theoretical knowledge that has become the matrix of innovation." However, this matrix cannot be considered only as codified information. It is a living organism embodied in the long-term scientific experience of individuals and the organizational memories of institutions. It can be maintained only by constant self-renewal, and by the continual acquisition of new insight and understanding by those who have the talent and training to advance the frontiers. In this model, education and the most advanced theoretical research are part of a continuum in the development of human resources, which pyramids in the frontiers of basic research.

Thus research, and especially academic research, is continuous with education. An advanced society must somehow get away from the notion that learning in the classroom is a social investment in education while research is not. In fact, learning from nature is just another form of education, open to a smaller fraction of people with the particular talent for it, but education nonetheless. The problem of how much resources should be devoted to education as compared with research is essentially a distributional problem, quite analogous in many ways to problems of income distribution. In the 1920's the United States found that it was dangerous to the economy as a whole if too much personal income ended in the hands of the wealthy, because this tended to generate investment funds in excess of those needed to create productive facilities to meet consumption demand that could be generated with the existing incomes of the people. One could conceive of a similar maldistribution in science, e.g., too much "big science" in relation to the "little science" which is a source of new ideas for big science experiments, too many research scientists in relation to the pool of technical people needed to carry on lower-level but vital technical activities in the economy and society, an overproduction of scientific ideas in relation to what can be exploited technologically.

An illustration of the distribution problem lies in the following statistics. While 2.1 per cent of the U.S. population is enrolled in higher education as compared with only 0.32 per cent in Britain, Britain has more Nobel prizes per capita and generates three times as many abstracts in *Chemical Abstracts* in relation to the higher education population as the U.S. Similarly, in the U.S. the number of scientists and engineers engaged in R and D is 11 per cent of the enrollment in higher education, while in Britain it is 35 per cent. In terms of the real costs of research, Britain spends 3.1 times as much and Germany 4.1 times as much as the U.S. on academic research per student enrolled in higher education. These statistics demonstrate dramatically the different choices made by these European countries. Britain and Germany have clearly concentrated much more on the elite end of the spectrum than has the U.S. We are not able to say much at present on how these differences in distribution may relate to social performance. I bring them out merely to demonstrate that in evaluating research as a social overhead investment, its distribution within the total research-education continuum is an important parameter to be considered, and not just aggregate amount.

One might indeed draw a close analogy between the investment in public roads and the investment in research-education. Both are social overhead investments. For roads, the national investment has to be properly allocated between interstate highways, secondary roads, minor country roads, and city streets. For a particular economy and population, there must be in theory an optimum mix among these from the standpoint of the overall objectives of the transportation system. For research-education, there is a parallel in the relation between the distribution of population and economic activity and the occupational structure of the labor force and the sophistication of industry. In practice we lack a reasonable theory for the optimum mix of either roads or research-education. The analysis does imply, however, that there should be some relationship between the national investments at various levels of education including research. As in the road system analogy also, the performance of the system is dependent on the intersections between the various levels. In the research-education model the mobility of scientists and engineers, and their ability to navigate the intersections in the system, are important sources of social productivity. A high-level basic science which is isolated in the universities is like an interstate highway with no exits.

How do we measure the productivity of social overhead investment? In our particular case, since the investment is in human resources, we cannot ignore the ultimate disposition of these resources. If the research-education system were completely closed, i.e., if it only reproduced itself and there was no flow of knowledge and people into the general economy, the investment problem would be meaningless. Clearly there has to be some feed into the larger system of society, but what should be the excess ouptut, and how should it be measured? I am afraid I can only suggest questions and not answers in this area.

The direction of education and basic research is clearly influenced by, but not dominated by, social needs. Not only does much of support for education and research come from mission-oriented agencies and from private donors with special interests, but new students are constantly entering the system carrying with them some of the new values, priorities, and interests of the external society. This is dramatically brought home to us today by great student interest in environmental and social problems. It is clear that the flux of people through the education-research system is the major form of coupling between this system and society, and provides the feedback by which society influences the system. The process, however, is exceedingly complex and very little understood or studied.

As an example, during World War II there was a tremendous reshuffling of people in all levels of society. Rural Negroes moved into Northern factories. Many people received technical education and practical experience in the armed forces. Scientists became immersed in military problems and operations and developed or learned new technologies. Many academic people found themselves with command or entrepreneurial responsibilities which their training and habits would not have led them into otherwise. After the war the G.I. Bill offered unprecedented opportunities for technical education to upward mobile people who would have otherwise flooded the job market with their existing skills. It is obvious that the impact of this mixing up of society was profound and probably very beneficial to our postwar economic and social development, especially in developing our strength in science and engineering. In fact the output of engineers reached its peak in about 1950 and has declined since. The postwar mobility leads one to speculate whether there may be ways of creating such mobility as a matter of deliberate policy without the psychology of war. Furthermore, such mobility ought to be measured as one of the indicators of system performance.

The Tertiary Industry Model

This model I have ascribed to Stephen Toulmin,[21] who suggests that the advance of science at the frontiers is not just useful for other social purposes but is itself part of the purpose of a society. As a society becomes more able to satisfy its material wants, it looks for new goals and aspirations to remain viable as a social organism. Science is a candidate for such a goal because it is both open-ended and also public—open-ended in the sense that each advance opens up new vistas of challenge and new questions to be answered; public in the sense that the results of science are valid for all men who can understand and appreciate them. It may also be said that the provision of meaningful work for people is a social goal of equal importance with the production of material goods and services. If so, the generation of new scientific knowledge is one of the most meaningful and rewarding forms of human work, engaging the fullest capabilities of the individual, while possessing an aspect of participation in a common enterprise which is integrated into a worldwide community. Science is the single human enterprise most independent of time, place, and human divisions.

But how does one quantify an allocation of public resources to science on the basis of this model? Even more than literature, art, and music, science is an elite activity, tending to become more and more remote from public understanding.[22] We still support the arts, at least in the United States, primarily by a market mechanism, with only indirect public subsidy through tax exemption. We rely on inequality of income and its correlation with educated taste to allocate resources for art and literature in a way which satisfies our educated view of what is right. But high-brow culture today is barely viable economically, and is in serious trouble, and science has long passed the stage where it could rely largely on private resources. The individual packages are too big, and the need for continuity of support is too important. Our society has no mechanism other than taxes for assembling resources in sufficiently large packages on a sufficiently continuous basis for a viable basic science. Since the total cost of science is less than one per cent of the GNP, it is not cost by itself which is limiting. Indeed, just the annual growth of the economy is about six times the amount spent on research.

The tertiary industry or consumption good model for science is the most difficult to quantify and manage. The system of selection of scientific projects by peers, as practiced most completely in NSF and NIH, works fairly well within science if the field of comparison is sufficiently small, i.e., if all the projects compared lie within an intellectual framework which is possessed in common by those making the judgments. But fields like molecular biology and high-energy physics are largely incommensurable in scientific terms, and if we are dealing with science as a consumption good, scientific criteria really form the only legitimate basis for comparison.

On the other hand, in the long run it may be that the drift of student interests within the graduate education system may provide an adequate guide. The relative positions of scientific fields both in student interest and in the scientific community at large do shift with time and can be measured by such criteria as "proposal pressure." What makes one nervous is that we really do not know how much feedback there is in the system. For example, to what extent are recent declines in interest in physics due to cut-backs in federal funds, and to what extent are they reflections of the changing intellectual and social climate?

The weakness of the present system of science is its failure to communicate its values, standards, and accomplishments to the lay public in a way which really gives the larger community a sense of participation in the scientific enterprise. The psychological and intellectual involvement of the public tends to occur only in relation to costly "spectaculars" such as the Apollo program or heart transplants, and this public interest in turn tends to distort the values and goals of science itself, and to tempt its practitioners into illegitimate appeals to vulgarized conceptions of scientific goals.

Conclusion

The question raised by these four models is the degree to which they may be used as a basis for planning. Also how much centralized planning is possible or desirable? That some centralized planning is possible in terms of the technical overhead model is indicated by the fact that this is essentially the model used implicitly by large industry in planning its own research, including its corporate or general-purpose research. It is harder to see such a centralized mechanism operating successfully at the overall governmental level. Perhaps our attention should rather be directed at better understanding of the various intellectual market mechanisms which operate in the research system, and we should try to regulate these so that their fairness and objectivity are improved. Furthermore, by developing various indicators of the operation of the intellectual market it may be possible to develop tools by which the changing values and interests of society can influence the overall development of science with less direct intervention in individual scientific choices. The process might then become similar to the use of economic indicators in the regulation of the national economy while leaving most economic decisions to smaller, decentralized decision-making units.

Notes

1. Harvey Brooks, "Can Science Be Planned?" in *The Government of Science* (Cambridge: M.I.T. Press, 1968), chapter 3, pp. 54-80.
2. Harvey Brooks, "Future Needs for the Support of Basic Research," pp. 77-110, in *Basic Research and National Goals,* House Committee on Science and Astronautics, 89th Congress, First Session, March 1965, or pp. 126-175 in *The Government of Science, op. cit.*
3. National Science Foundation, "Toward a Public Policy for Graduate Education in the Sciences," National Science Board, 1969, NSB 69-1.
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PLANNING TO HEAL THE NATION

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HOW CAN THE IMPERATIVES of health be used as a major factor in planning the future of urban America?

Planning suggests change. Phenomena which we apprehend in our universe reflect dynamism to various degrees. That is, all things created have a beginning out of the elements of things which began previously; they exist in a state of two-dimensional tension; they project various manifest forms; they yield their elements to reproduction; they transform their appearance; and they surrender their vitality as their component resources return to the amorphous phases of some new creations.

Thus do we transfer to our young our visions, our personal values, our physical resources, and our struggles for a better future. We spend our lives relating these fruits of our being to the benefit of future generations. Thus do we recognize that change is the process that characterizes life. Where there is no change, there is death. These realizations are the essence of the imperatives of health.

We may analyze factors related to our life in terms which involve input, output, feedback. We take in proper nourishment, express appropriate behavior, and receive learning. In health we speak of the building-up process and the tearing-down process. This involves our metabolism or rate of biological activity within which we project physical factors about our life.

Our input, interacting with our being, produces our output. Our perception of this phenomena can be considered as our feedback. Our being includes all that we know of our nature and all that we do not know of our potential.

Our feedback is conditioned by the precision with which we select and consume our input, and by the understanding that we have of our being as well as our observation of consequent acts, sensations, fears, challenges, and other effects.

We recognize that our output, considered in terms of the *physical* by-products of our lives, can be modified only by manipulating our input of food, air, medicine, minerals, etc. But the mysteries of our inner physical elements have to a great extent escaped our mastery. What we have learned about ourselves has taken place in the course of modifying our output through changing our input.

Modified input and the results of learning process reach our consciousness as feedback. Thus we transform physical phenomena into a mental reality. Our learning and relearning is a consequence of perceiving our output, modifying our input, and reexamining our output.

This process can also be applied to learning. Yet how often we reject or ignore this knowledge. As an example, we establish corporations in perpetuity. This negates their need to yield their substance to new more vital organisms of their own kind. We instruct our young people to worship temporal events, personages, and propensities, as absolute values irrespective of our knowledge that such tem-