

### 3. LONG RANGE PLANNING

#### LONG RANGE PLANNING AT THE NATIONAL SCIENCE FOUNDATION

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I wish to discuss long range planning and particularly concentrate on the NSF procedures. You will see that I am talking primarily about planning for basic research. To put this in context, it is useful to realize that the NSF annual budget is roughly one billion dollars, the total Federal support for R&D is \$30 billion annually and there is another \$30 billion from industry. Therefore, most of what I say concerns a relatively small part of the long range planning for the total U.S. R&D enterprise. You will also see that I'm going to put primary emphasis on the process that has been termed "bottom up" planning – that is, giving primary consideration to the needs of science as perceived by the scientific community. I will emphasize that aspect but remind you that there are some policy makers in the Government and in the scientific community who clearly have strong interest in the "top down" part of long range planning.

With regard to basic research, the main objective of a long range plan is to develop procedures that would provide to the scientific community the resources it perceives are needed to advance science. With that point of view, a major emphasis will be on identifying those areas of science that are emerging, are growing, and in which exciting new ideas are being formulated. All of you will recognize that a harder bureaucratic problem is to identify those areas of science that appear to be at a standstill and therefore may have lessened resources provided to them. This is often politically difficult.

A key problem in the U.S. scientific community is providing the major pieces of equipment, large instrumentation, or what in high energy physics might be called small instrumentation. These include, for example: large nuclear magnetic resonance machines; large mass spectrometers; and electron microscopes. We try to put major emphasis on identifying the needs of the community and we try to plan in some rational way for providing these major resources over the years.

In considering the planning tools used at the National Science Foundation, I am going to describe the process as if there were no external influences. For the time being I am going to be talking as if the White House and the Congress did not exist. Thus, I will be considering only a relatively small part of the entire process. I will return to the roles of the White House and the Congress later on.

The Annual S&T Report and the Five-Year Outlook Report that have been mandated by Congress are relatively new. Those documents should become a cornerstone of any long range planning for science in the U.S. The planning at the National Science Foundation has not yet been dramatically impacted by those new documents. However, it is anticipated that in the next planning cycle they will be a major force. There are three major internally produced planning tools that we use. One is the *Status of Science Review*. The major part of this document is a description provided by the various program officers of how their subdisciplines of science are progressing. We ask them their perception of what the perceived needs are for the next few years; what exciting new ideas and new opportunities are being developed, or whether, on the contrary, the field is pretty stable. They call upon their own expertise since they are, or were recently, practicing scientists. Another main source of their information is the proposals that they receive from the scientific community. They also have discipline advisory committees made up of scientists external to the Foundation. This group will be primarily academic scientists, because they are our largest clientele, but also will include industrial scientists and some Government scientists where they bring particular expertise.

The *Status of Science Review*, in addition to describing what is perceived to be happening in the various disciplines and subdisciplines, will also contain statistical information of the success ratios of proposals in various programs. There is data on the size of the proposals – How much money is being asked for an investigator. There is also some discussion of science in the other Federal agencies because any long range plan should take cognizance of what the other agencies are doing and planning to do. There is also a discussion of the status of American science in these disciplines relative to foreign science. For example, we try to assess how laser chemistry is doing in the U.S. as compared with other countries, for example. The *Status of Science Review* volume is rather large, as you might expect, and after having produced it a few years ago, we now annually make modifications and updates because

generally science does not change rapidly. That document is produced by the NSF staff. When I use the term *Status of Science Review*, it is really the status of science as perceived at the NSF by the in-house staff.

A second planning tool that we use is also constructed discipline by discipline. We ask the staff for a statement of priorities from the disciplines on the basis of three budget projections over the next five years. For example, we ask the program officers in the materials science discipline to tell us what they would support over the next five years if their budget were held constant; secondly, what if there were to be a small increase in actual effort, and thirdly what if the budget were to have a reasonable increase over the next five years. Let me emphasize again that at this stage we are looking at single disciplines – physics, materials science, sociology, economics, etc. The point of this exercise is to determine what is important in the particular discipline. If the budget remained level or increased only slightly how would the discipline react to the support of individuals. As an example, we became concerned a few years ago that in attempting to spread money to as many talented individuals as possible we were cutting the level of individual support too much. We were starving the community to an extent that no investigator really had the resources to do a decent job. Thus, one question that we asked of program managers is: under the budget projections would you increase the number of people you are supporting by cutting budgets or would you reduce the number of people you are supporting and support those with a larger sum of money. We do not have “block grants” for general support to the university. Therefore, for most of the investigators, these project grants amount to their entire research support. They do not have “general funds” as in the Japanese university system.

It is important that these projections not be made by the NSF staff alone; we require these priority statements be developed in conjunction with discipline advisory committees. Thus, we require that the priorities as perceived by the NSF discipline program officers either are agreed upon by the appropriate advisory committee or that the advisory committee states what it believes the priorities should be. This is like asking the disciplines: what if your budget were this? or What if your budget were that? The process should have, and must have, heavy input from the scientific community.

The third document that we ask for internally in the Foundation is based upon our organizational structure. The Foundation is organized into several directorates, each headed by an Assistant Director of the Foundation, that have various groups of disciplines in their purview. For example, there is a directorate for Mathematical and Physical Sciences that consists of physics, materials science, chemistry, mathematics, and computer science. Each Assistant Director looks at the priorities enunciated by each of the disciplines and gives his evaluation of the relative priorities. What emphasis should be put on computer research compared with chemistry, for example, under certain budgetary situations. Long range planning gets very difficult in making these tradeoffs among disciplines. Those are the three main documents that we actually produce inside the Foundation for the planning process.

I should comment about the organizational structure of the Foundation. The Foundation is unique in Government agencies in the United States in that it has a part-time group of people who serve as a Board of Directors in actually running the National Science Foundation. This National Science Board is made up of twenty-four scientists, engineers, and research administrators, appointed by the President and confirmed by the Senate, who spend roughly two days a month formulating policy for the National Science Foundation. Legally they and the Director are the Science Foundation. The Director of the Foundation and the National Science Board make the policy, make the long range planning decisions, and, therefore, make the budgetary decisions. As we are dealing with a group of people who are part-time policy makers, the NSF staff try to provide them with as much information as possible or, more to the point, we try to design the type of policy we think is needed because they do not have the time to do all the background work. Part of the job of my office is to provide the Board with as much relevant data and analytical staff work in as succinct a form as possible.

In addition to the Assistant Directors making trade-offs among relatively closely allied disciplines, the Science Board and the Director have to make the decisions as to how to trade off social sciences vs physical sciences or biological sciences. A major problem in long range planning is making and stating the decision that over the next so many years one discipline is going to get less every year while another is going to get more. You can imagine that the scientific community, particularly in the declining discipline might get more than a little upset over statements like that. It is not facetious to say that one of the problems with long range planning revolves around how much “heat”, “flack”, or “abuse” that the Director of the Foundation is willing to take in stating his decision to reduce or hold constant the funding for a discipline over the next few years. This is not a trivial matter in the practical application of long range planning in the United States. Perhaps it is less of a problem in Japan with its relatively strong bureaucracy.

As I said at the beginning I was going to be talking as if there were no external influences in long

range planning. Obviously that is not the case. We have concentrated so far on what we do internally in the Foundation to arrive at a projection of resources required by the scientific community.

There are two main external influences on our long range planning and budget process. One is the White House. The White House has two important organizations in this regard. One is the Office of Science and Technology Policy, headed by the Science Advisor to the President, Frank Press. There are overarching policy considerations on a national basis that will come from Frank Press' office. These perceived national needs are transmitted to the Federal agencies; but the readiness of the science to respond is another matter.

The other relevant part of the White House is the Office of Management and Budget which, as the name implies, is the President's Budget Office. This office determines in large part the size and shape of the budget which goes to Congress. We would not be able to start a large project without a clear understanding and approval by the Office of Management and Budget. The newest example of such a project is the program for Ocean Margin Drilling. This is a ten-year program expected to total about a billion dollars. The plan is to drill some rather deep holes in the ocean margins. It involves technology to drill in 13,000 feet of water with 25,000 feet penetration beneath the ocean floor. This large, managerially complicated project has been under discussion for four or five years in OSTP, OMB, the Foundation, and in the scientific community.

The other major external influence is the Congress itself. Congress is the body in the United States that actually appropriates the money. In Congress we go before two committees each in the House of Representatives and in the Senate. One or more of these four Congressional subcommittees frequently tells us that our plan is interesting but that they have a different view of the world. This is a very real external force on our long range planning. We may feel strongly that for the best progress in science certain things should be done, but because of Congressional understanding of societal pressures or because of the interests of particular Congressman or Senators we may be directed to do something quite different. Programs are defined for us which you would be unable to find in any of our long range plans. It is a real external influence.

Let me end by giving you my impressions concerning the importance of a long range plan and how effective I think ours are. It is seldom in my experience at the Foundation that a given year's budget tracked very closely to a long range plan that was produced five years before. I think, however, the long range plans are valuable if done properly in conjunction with the scientific community. Such a plan positions the bureaucracy to respond to the needs of the community and prevents it from providing resources that are either not needed or are not useable. Even more to the point I think it keeps the bureaucracy from pushing unrealistically those socially useful projects to which science cannot respond. We had an example of such a case in the United States some years ago when it was decided that fusion power was the way to go. We produced Project Sherwood and spent an awful lot of money. And after the project had been going quite a while, it was realized that no one knew what the characteristics of a plasma were. I think sensible long range planning helps to prevent that. The bureaucracy can make mistakes very easily but at least a long range plan gives a knowledge base that should make it easier to decide whether it is sensible to start a particular project.

How effective are our long range plans? I guess "not very" is an honest answer. Long range plans are beginning to have a usefulness in the U.S., quite apart from their substantive content. Congress is asking more and more about documentation of the steps taken to arrive at a particular budget. Detailed long range planning with documentation is indispensable for backing up annual budgets. Congress is even offering to help us do our long range planning. I think the usefulness of long range planning depends to some degree on its credibility in the scientific community. If the scientific community perceives that the bureaucracy is correctly enunciating the needs of science, we have outside backing and understanding for what we are doing.

The final point is that one test of a long range plan would be to see how allocations among disciplines or projects actually followed the plan. I think that if you look at the planning that we have been doing at the Foundation, you won't see dramatic changes that are traceable to a long range plan, but you would be able to see each year's budget embedded somewhere in the long range plan.

# LONG-RANGE PLANNING IN SCIENCE POLICY

Hiroshi INOSE

## 1. Introduction

Ultimate objective of long-range planning in science policy will be to create and maintain a right climate for the sound growth of scientific and technological capabilities. Specific approach for accomplishing the objective may be different depending upon the characteristics of sciences in terms of their scale, radicalness and maturity.

There are a number of factors that make long-range planning quite difficult. To mention a few, limitation in monetary and human resources, uncertainty in social and scientific environment, unpredictable policy changes, and difficulty in evaluation, are typical examples of these factors.

Under the prevailing circumstances, the most universal and perhaps the most effective method in long-range planning will be the establishment of an appropriate infrastructure upon which future scientific activities can be based. Deployment of such infrastructure requires increase in investment, enhancement of education and training, improvement of information availability, strengthening of government-university-industry collaboration and promotion of international exchange and cooperation. In addition to the infrastructure, long-term government support should be provided to highly-complex and large-scale research projects.

The present paper describes the author's personal views on the above issues but does not reflect to any extent, the official policy of the Japanese Government.

## 2. Types of Sciences

Permitting some over-simplification, sciences may be categorized into three types in accordance with their characteristics.

Firstly, there are big sciences which are highly complex and very large in scale such as space program, nuclear fusion, high energy physics, information and transportation networks, etc. Because of their complexity and scale, long-range planning for big sciences can be undertaken only by the government or its equivalent. Although the objectives of big sciences are often well defined, enormous investment in terms of monetary and human resources is required over a long period of time, and the rate of return is very low during most of the period. Critical evaluation is crucial to adaptively adjusting long-range planning because no market forces will function for screening less prospective alternatives.

Secondly, there are sciences which may be called as radical sciences. These are generally premature but have enormous potentiality to drastically change not only the industrial structure but also the society as a whole. A typical example in the past was the solid-state physics that caused the invention of transistors. Presently, information science including pattern recognition and mechanical translation, and life science including genetic manipulation and photo-synthesis, are typical examples. Although these are areas of fundamental importance, the long-range planning tends to be very difficult because of uncertainties in scientific and societal environment. Since objectives of radical sciences are generally not very clear, very highly adaptive policy is required in long-range planning taking into account of industrial and societal implications. Successful accomplishment of adaptive changes depends solely upon the existence of sufficient infrastructure.

Thirdly, there are still another area which may be called mature sciences. These sciences generally have rather long history and through which a number of incremental progress has brought them up to a certain degree of maturity. Most of sciences with well defined discipline belong to this category. Because of their tradition, mature sciences may accomplish further incremental progress and through the accumulation of successive incremental progress, may contribute significantly to our society. Long-range planning for mature sciences is easier and perhaps less important because their objectives are clear and market forces are effective for evaluation.

In actuality, some big sciences are quite radical in their beginning, and become mature towards their end. And through their course of progress, radical sciences may turn to mature and mature sciences may turn to radical. The above categorization therefore may be viewed as a snap shot rather than a frozen picture.

## 3. Problems in Long-Range Planning

Although the difficulty in long-range planning depends upon the types of sciences as has been described, there are a number of common factors that affect or disturb the long-range planning.

Firstly, the obvious shortage in monetary and human resources which could be allocated for scientific studies, should be mentioned. Table 1 depicts the recent trends in research and development expenditures relative to the GNP. The relative expenditure declines in almost all major nations in recent years. Table 2 depicts the number of research and development staff per 1000 population. The number stays almost constant with respect to time. These two tables indicate that, in view of the slower or even negative GNP growth to be anticipated in coming years, no substantial increase in monetary and human resources for research and development could be expected, unless a radical change in science and technology policy is effected. Taking into account of the sustained recession and unemployment, the R&D investment of private sectors may continue to decrease, resulting heavier burden in the part of the government to maintain the R&D investment close to the present level. In view of these obvious limitations, it is highly important to optimally allocate the limited resources by avoiding duplications, by critically evaluating alternatives and by promoting interorganizational and international collaboration.

Secondly, it should be noted that, in the present age of discontinuity and uncertainty, societal and scientific environment is increasingly becoming unpredictable. Sciences in such circumstances, and in particular big sciences, are facing the threat of suddenly becoming obsolete in competition with other alternatives, or suddenly becoming unacceptable by the society. Such catastrophe may happen at any time, even at the very beginning of a project or long before the investment is sufficiently paid off. Conventional methods of forecasting either by extrapolation or through consensus may be used to some extent if mature sciences are under consideration. But the extrapolation approach cannot cope with discontinuous changes and the consensus approach generally provides nothing but a quantitative expression of common sense. To deal with radical sciences or big sciences with radical factors, the known methodology for forecasting is obviously too poor. Science policy should therefore be made as flexible and adaptive as possible, so that it can adaptively transfigure at any time in response to the changes in societal and scientific environment. Among other things, well developed infrastructure may provide basis for such transfiguration to be made smoothly and without delay.

Thirdly, it may be pointed out that government policies tend to be more unpredictable because of sudden administrative changes, unstable economic environment, kaleidoscopic international relations, etc. Sporadic changes in government funding often add uncertainties especially to the long-range planning of big science projects. Competition among government sectors sometimes makes it difficult to establish concerted and lasting policy and thereby fails to agree on priorities or to avoid duplications. Changes in regulation, procurement and taxation policies often disturb sustained innovative activities in the part of industries. As the results, research projects are increasingly becoming short-term rather than long-term, and industrial innovation tends to be more process-innovative rather than product-innovative.

Fourthly, it may be noted that evaluation of research projects are increasingly becoming difficult due to increasing diversification of research areas, widening gap between basic research and applied research, rapid change in society's system of values, etc. In the case of applied research and development in private industrial sectors, evaluation can be made in terms of such output indicators as private return, public return, product liability, etc. In the case of basic research, however, output indicators are much diversified and therefore are very difficult to formulate. Number of publications with sufficient citations, number of significant prizes from well-recognized institutions, number of patent registrations with good licensing status and other information may be used to some extent as common output indicators transcending academic disciplines and research areas. Setting unique standard for these output indicators, however, is quite difficult because of the different tradition and background of respective research areas. These output indicators are useful only for post-mortem evaluation because they are usually available after the completion of research projects. Perhaps the most important output indicator for basic research should be the rate of successful transfer of fundamental knowledge gained by basic research to applied research for contribution to the betterment of our society. However, this output indicator is very difficult to formulate because many basic research areas have no explicit connection to applied research areas and because existing connections between basic and applied research are falling off due to the lack of appropriate coordinating mechanism and of sufficient funding. Even such output indicators could be found, the change in our society's system of values makes the evaluation even more difficult. The technology assessment approach may be used to extract, to a certain extent, negative impacts and provide possible alternatives and countermeasures. The action options for reducing the negative impacts, however, generally reduce and often totally jeopardize the positive impacts of original scheme.

#### **4. Infrastructure Building as a Basis for Adaptive Long-Range Planning**

Under prevailing circumstances that include above-mentioned problems, long-range planning for scientific

studies should be made as flexible or adaptive as possible in response to unpredictable changes of various sorts. Adaptive planning can not be accomplished without strong and broad infrastructure which can work as a solid foundation for future scientific activities. The need for such infrastructure may be the most crucial for radical sciences. Big sciences where a firm and lasting government support is of prime importance, also need an appropriate infrastructure, by which radical factors can be incorporated to the maximum possible extent. Mature sciences for which market forces have been considered as the prime mover, may also need infrastructure by means of which incremental progress may be made more efficiently or through which can transfigure from mature to radical. It may be clear that, without an appropriate infrastructure, a long-range plan may turn out to be an empty paper plan or may end up as a total disaster.

Deployment of infrastructure for scientific studies requires diversified efforts including the following: increase in research investment, enhancement of education and training, improvement of information availability, strengthening of government-university-industry collaboration and promotion of international exchange and cooperation.

**(a) Increase in Research Investment**

Under the present economic situation in which recession and inflation coexist, the funding for basic research may continue to decrease. Research investment is far less than adequate especially in Japan. As has been shown in Table 1, the relative expenditure in research and development in Japan is 1.7%, which is the lowest figure among major nations. The Japanese Government pays only 28% of the total expenditure whereas other governments pay as much as 50% of the total. If we adjust for R&D expenditure for defense which is very low in Japan, the Japanese Government's share is 27% which is still much lower than other governments' shares that amount to 40%.

Taking into account of the fact that major portion of basic research has been conducted by universities and government laboratories, substantial increase in government funding should urgently be called for. Present investment for such national research centers as high energy physics, space sciences, molecular chemistry and plasma physics should be extended to cover more broadly such areas as information sciences, material sciences and life sciences which are key factors for future scientific activities.

**(b) Enhancement in Education and Training**

As has been shown in Table 2, the relative number of research staff in Japan already reached international level. However, this does not necessarily mean that Japan provides top-ranking education and training for scientists. As the result of post-war democratization of higher education, the number of universities and colleges increased dramatically. This brought about substantial bottom-up effect in higher education, but at the same time, resulted in diffusion of monetary and human resources. For the promotion of basic research, the present situation must be improved so that resources are concentrated to produce more scientists of international competence.

Equally important is the promotion of continuing education by which the knowledge of limited number of talented people is refreshed so that they can actively participate in research activities over their life time. This is especially important in the areas of radical sciences in which knowledge gained in the past becomes very quickly obsolete.

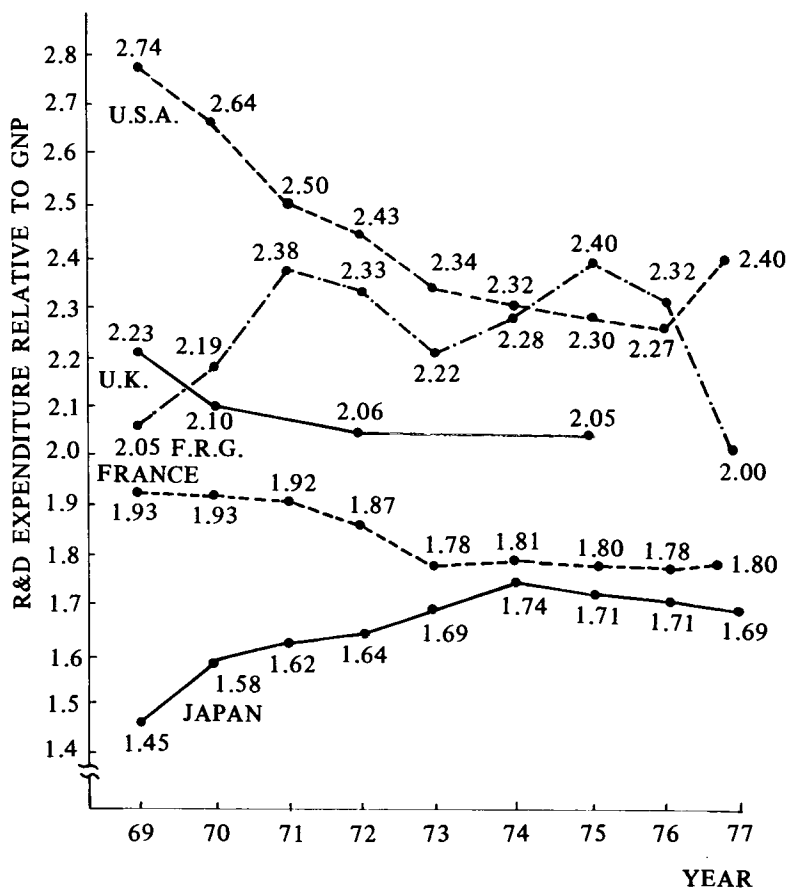
**(c) Improvement of Information Availability**

Availability of information for scientific studies is becoming increasingly crucial to the advancement of sciences. The needs for computerized information systems are stimulating the implementation of data bases for various scientific disciplines, and the deployment of computer communication networks. Automatization of library services including remote access to original documents will soon follow along with diversified use of new package-type media such as video tapes and disks.

Computerized information services in Japan have been quite behind to that in the United States. Scientific information provided by data base services, which are not very popular in Japan, is mostly those imported from abroad in the form of magnetic tapes. Packet-switched digital data network began commercial service only a few months ago. It is highly desirable that the Japanese Government's plan for the scientific information system, which connects data bases, university libraries, computer centers and user terminals on nation-wide basis by digital data network, be completed in earliest possible date.

**(d) Strengthening of Government-University-Industry Collaboration**

A large variety of different sectors that sometimes compete between each other and very often isolate themselves from others should be coordinated more closely and reasonably. Competition between pluralistically structured government sectors often confuses priority setting and budget appropriations. Direct government funding to the Japanese university research is almost exclusively through the Ministry of Education, Science and Culture. Research contract between national universities and private industries has many restrictions as compared to that in the United



COUNTRY	GOVERNMENT SHARE (%)	
	IN TOTAL R&D EXPENDITURE	EXCLUDING DEFENSE R&D
JAPAN (1977)	27.4	26.9
U.S.A. (1977)	50.5	35.1
F.R.G. (1977)	48.5	45.3
FRANCE (1977)	56.7	47.1
U.K. (1975)	51.7	35.0

Table 1. Research and Development Expenditure in Major Countries

States. No part-time appointment for national university professors and graduate students is possible, so that they can not participate in industrial research activities including high-technology ventures.

As compared with the university-industry collaboration, the government-industry collaboration may be better in Japan. Government sectors including the Ministry of International Trade and Industry, set up large-scale projects in collaboration with private industries on such topics as very large scale integration (VLSI), direct steel making by nuclear energy, pattern information processing, community cable information systems, medical information system, etc. Basic research activities in universities, however, are not always utilized in these projects.

**(e) Promotion of International Exchange and Cooperation**

Taking into account of the facts that each country has its own limitations in monetary and human resources for scientific research and that sciences are becoming increasingly large-scale and risky, the importance of international collaboration in scientific studies especially in the areas of big sciences and radical sciences should be emphasized. Bilateral and multilateral agreements for joint scientific studies should be promoted. International collaboration is not very familiar for Japan due to its mono-racial structure. Universities should therefore be internationalized by inviting substantial number of foreign professors and students. Industries should be encouraged to employ foreign scientists and to establish not only production plants but also research centers abroad. Government sectors should increase the number of officials with scientific and technological background and with international scope.

COUNTRY	RESEARCHERS PER 1000 POPULATION
JAPAN	2.4 (1978)
U.S.A.	2.6 (1977)
U.K.	1.4 (1975)
FRANCE	1.2 (1975)
F.R.G.	1.5 (1975)
U.S.S.R.	3.8 (1977)

Table 2. Number of Researchers per 1000 Population in Major Countries