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PREFACE

On the occasion of its 1965 annual meeting, the National Research Council turned its attention, in a plenary session held March 15-16, to the effects of current trends on the support of research. Papers were presented by: Harvey Brooks, Gordon McKay Professor of Applied Physics, Harvard University; Colin M. MacLeod, Deputy Director, U. S. Office of Science and Technology; Carl E. Barnes, formerly Vice President for Research, Minnesota Mining and Manufacturing Company; and Leland J. Haworth, Director, National Science Foundation. Because of the interest expressed in this symposium, the papers have been brought together and, with generous assistance from their authors, are presented in the following pages.

EFFECTS OF CURRENT TRENDS ON THE SUPPORT OF RESEARCH

Harvey Brooks, Gordon McKay Professor of Applied Physics
Harvard University

In these remarks I intend to comment on some recent trends in federal support of research and development, and then to go on to discuss three problem areas concerned with federal research support which are currently receiving a good deal of attention.

In any global view of research and development one must fall back on budgetary statistics, because these provide the only common measure for such diverse activities. The more one plays with such statistics, however, the more one is struck with the fact that the national research and development enterprise is an organic whole, and that any attempt to categorize the components of such a rapidly changing effort is imperfect and often misleading. It turns out, for example, that the largest expenditures in a given category tend to lie at the interface between it and neighboring categories—e.g., basic and applied research, or academic research and research institutes such as Brookhaven or Kitt Peak. Small changes in definition, or even in interpretation, may radically alter apparent expenditures in a given category, and even apparent rates of growth.

Another problem in fiscal statistics is inflation—not so much monetary inflation as increases in the cost of a man-year of research effort, estimated to average about 5-7 per cent a year. It is probable that this increase in cost is offset or even overcompensated by increasing scientific productivity of research effort, but we have no way of measuring the latter, and so opinions on this subject tend to be violent and unsubstantiated by objective evidence. In the present discussion of trends I have ignored any value judgment on the outputs and considered only the fiscal inputs. Some of the trends are listed below:

1. Total federal expenditures for research and development have leveled off at about $15 billion a year.

2. The leveling off is due mainly to a decline in expenditures for development, which in turn is due to approaching completion of development on two major strategic weapons systems, Minuteman and Polaris, and to a conscious policy of holding space expenditures at a level near $5.5 billion a year. Research since fiscal year 1965 has risen from 30 per cent to 35 per cent of the total effort, while basic research has risen from 12 per cent to 14 per cent.

3. Academic research—what the National Science Foundation classifies as "research in universities proper"—has grown somewhat faster than research and development as a whole—about 25 per cent a year to 1962, and about 10 per cent a year since. Much of the growth in academic research has been due to the biomedical program of the National Institutes of Health; the growth rate in other sectors is about 15 per cent a year.

4. In academic research there has been a steady trend toward greater diversity in sources of support. The Department of Defense accounted for 70 per cent of academic research in 1954 and only 27 per cent in fiscal year 1966. The National Institutes of Health's share has leveled at a little over 40 per cent, although the Department of Health, Education and Welfare accounts for 55 per cent of university support if other mechanisms such as fellowships, institutional-type programs, and construction are considered.

5. Defense, space, and atomic energy account for more than 90 per cent of current federal research and development. If one subtracts general-purpose and basic research and such programs as civilian nuclear power, 80-85 per cent might be a fairer estimate of federal expenditures that are highly specific to the space and defense field.

6. Of the fiscal 1966 research and development budget of $14.5 billion, nearly half is for federal space activities, chiefly Department of Defense and the National Aeronautics and Space Administration. Half the increase in research and development since 1959 is attributable to the National Aeronautics and Space Administration, and currently 36 per cent of all basic research expenditures are attributable to the scientific satellite program (excluding manned space programs). From fiscal 1964 to fiscal 1965, 60 per cent of the increase in basic research was due to space activities, but the increase from fiscal 1965 to fiscal 1966 is much more diverse, with 27 per cent attributable to the National Science Foundation in the President's budget.

7. No striking change in the pattern of performance of federal research and development has taken place in the last 10 years. The

most marked change is the relative drop in expenditures in government "in-house" laboratories, but this has been partly offset by increased use of federal research centers under contract. There has also been an increase in the proportion spent in universities. Considering all sources of funds, federal and non-federal, the university share of research activity (excluding development) has risen from 20 per cent in 1958 to 25 per cent estimated for the current year.

Within the framework of current trends described above, three problem areas have emerged to attract particular attention in the government. These are (1) the over-all size of research and development expenditures, (2) the competition between "big" and "little" science, and (3) the relationship between growing university aspirations and the declining growth rate of federal programs. Obviously these three areas are closely interrelated.

1. As a fraction of the gross national product, research and development activities are nearly three times what they were during the peak of effort toward the end of World War II, during the Manhattan Project. Technical manpower is doubling in 12 years, while federal expenditures have been doubling in about six years. It is obvious to ask whether there is any natural or logical limit to this trend. It is hard to see what it is, or should be, although clearly there must be some limit determined by the marginal utility of the expenditures to society. Leveling off of research and development in the past has proved to be temporary and has been broken through as a result of a new technological revolution. Today we do not foresee another technological revolution of the magnitude of past revolutions, but technological prediction has been myopic in the past and may prove to be so again. Few would have predicted the magnitude of the present space effort even as late as 1959. Nevertheless, there is increasing pressure to apply cost-benefit criteria to research and development, and this pressure is likely to increase.

2. Much of the recent growth in basic science has been due to "big science"—e.g., space science, high-energy physics, and oceanography, in about that order of importance. The significant concern about these fields is not so much the great equipment expenditure *per se* as the commitment they imply for operating and logistic costs. During the early 1960's, many programs were started with the expectation, implicit, or explicit, that research budgets would rise fast enough to take care of the increased operating costs of major new facilities, while allowing a comfortable margin for the expansion of general science. This expectation has not been realized, and there is fragmentary and somewhat inconclusive evidence that suggests that total funds available for general science have remained almost static for the last three years. This is hard to quantify because of uncertainty as to what is really "big" and what is "little" science. Clearly one characteristic of "big science" is the high entrance fee. In a very real sense, the scientific output is

proportional not to the financial input but to the input above some rather large threshold. In fields like high-energy physics and space science, austerity is highly inefficient and wasteful. Yet such fields are often at the most exciting forefronts of advancing knowledge. Not to support "big science" in order to protect "little science" would also be unwise. The compromise here is a delicate and complicated one, which will never be made to the satisfaction of all.

3. The discussion of "big science" versus "little science" is complicated by the fact that "little science" is done mainly in universities, while "big science" is done mainly outside of universities. If this were completely true, however, the problem would be simpler than it is. The effect of recent trends is that rapidly rising aspirations and expectations on the part of universities are coming into collision with the slowed growth of federal funds for general science, the kind on which most graduate students are trained. The rising aspirations of universities are manifested in many ways: in the projected growth in the graduate school population and Ph.D.'s granted, in the long-range planning of individual institutions, and in applications for National Science Foundation construction grants. Typically institutions plan to expand faculty by 50 per cent, graduate students by 30 per cent, and research associates by 50 per cent in the next five years. Many new institutions aspire to significant graduate programs and even plan to buy into the field by means of post-doctoral associates. In the past 15 years federal support of graduate education has grown naturally as a by-product of the research needs of the mission-oriented federal agencies. The growth of total federal research and development provided enough to take care of the research needs of graduate education without any explicit concern as to appropriate federal responsibility in this area. Now, the slowing of growth poses the question of federal responsibility more definitely than before. Such a responsibility is recognized to some extent in the mission of the National Science Foundation, which, however, accounts for only 13 per cent of the research support available to universities proper. Consistent with this, the President this year asked explicitly for an addition to the National Science Foundation basic research support budget, to maintain the minimum growth in the totality of academic research believed to be necessary to provide for the growth of graduate study and the expansion of faculty. As the effects of the birth-rate increase of the 1940's become more and more apparent in the graduate schools, however, question of federal support for advanced study and research is likely to come increasingly to the fore.

THE EFFECT OF CURRENT SUPPORT ON THE BIOMEDICAL SCIENCES

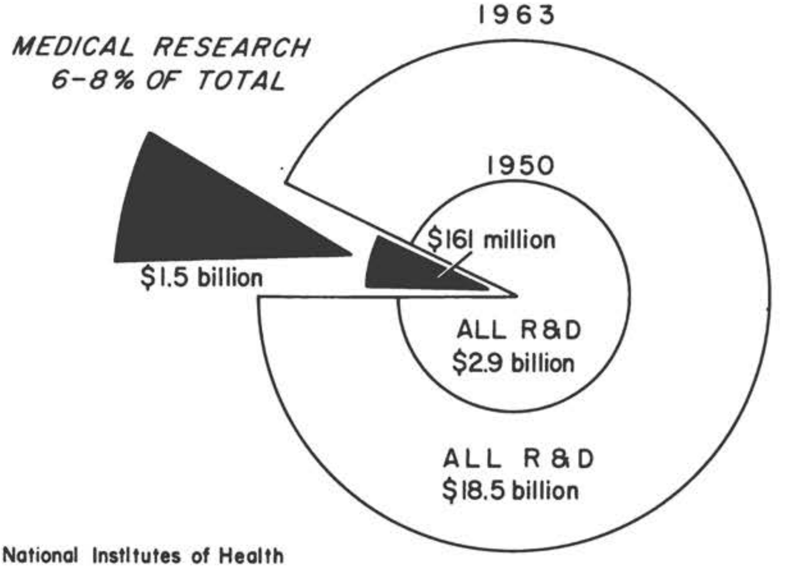
Colin M. MacLeod, Deputy Director
U. S. Office of Science and Technology

Following a period of unprecedented growth and development in the biomedical sciences, it is of interest to survey the effects of current support on medical research, education, and services. It is evident that programs in support of biomedical sciences exert a major influence in all three of these areas because of their magnitude and their profound effects on research and the training of investigators and physicians.

The Growth of Medical Research in the United States

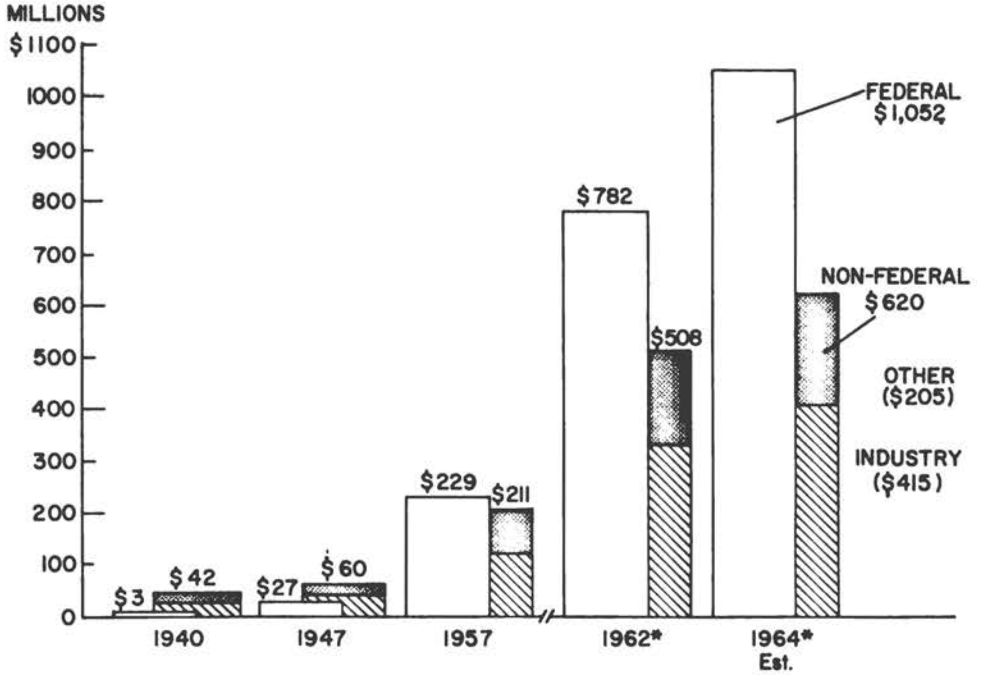
There has been very rapid growth of medical research and research training since World War II. Less familiar is the fact that the growth of medical research has been roughly proportional to that of all research and development. As shown in Figure 1, medical research expenditures increased from about $160 million in 1950 to $1.5 billion in 1963, while total research and development increased from $2.9 billion to $18.5 billion. Thus the "explosion" has been science-wide. Medical research has kept pace, ranging between 6 and 8 per cent of all research and development since 1960. Here the term research does not include research training, construction, or other auxiliary activities.

Figure 2 shows the sources of all medical research funds spent in this country since 1940. Non-federal expenditures exceeded federal until the late 1950's. Estimated federal expenditures for 1964 were well



Source: National Institutes of Health

Figure 1. Medical research as a proportion of all research and development



*Non-Federal data not strictly comparable with those for prior years, since coverage has been improved.
Source: National Institutes of Health

Figure 2. Sources of medical research funds, 1940-1964

in excess of $1 billion and one and three-quarters times the non-federal figure. The bulk of federally financed research is conducted in schools and other nonprofit, non-federal institutions. It is noteworthy that private support has continued to rise despite the taxpayer's support of medical research through government, suggesting that public and private efforts are mutually stimulating.

Figure 3 indicates the distribution of support from present sources, with particular reference to the National Institutes of Health. The non-federal sector comprises industry, private foundations, health agencies, other private sources, and state and local governments. Paramount among non-federal sources is the industrial component, representing largely the expenditures of pharmaceutical firms in their own laboratories. On the federal side, the National Institutes of Health (NIH) predominates, contributing $650 million, or nearly 40 per cent of the nation's medical research support. Other federal sources in 1964 were Department of Defense, $86 million; Department of Health, Education and Welfare (other than NIH), $100 million; Atomic Energy Commission, $78 million; National Aeronautics and Space Administration, $43 million; Veteran's Administration, $33 million; Department of Agriculture, $28 million; and National Science Foundation, $25 million.

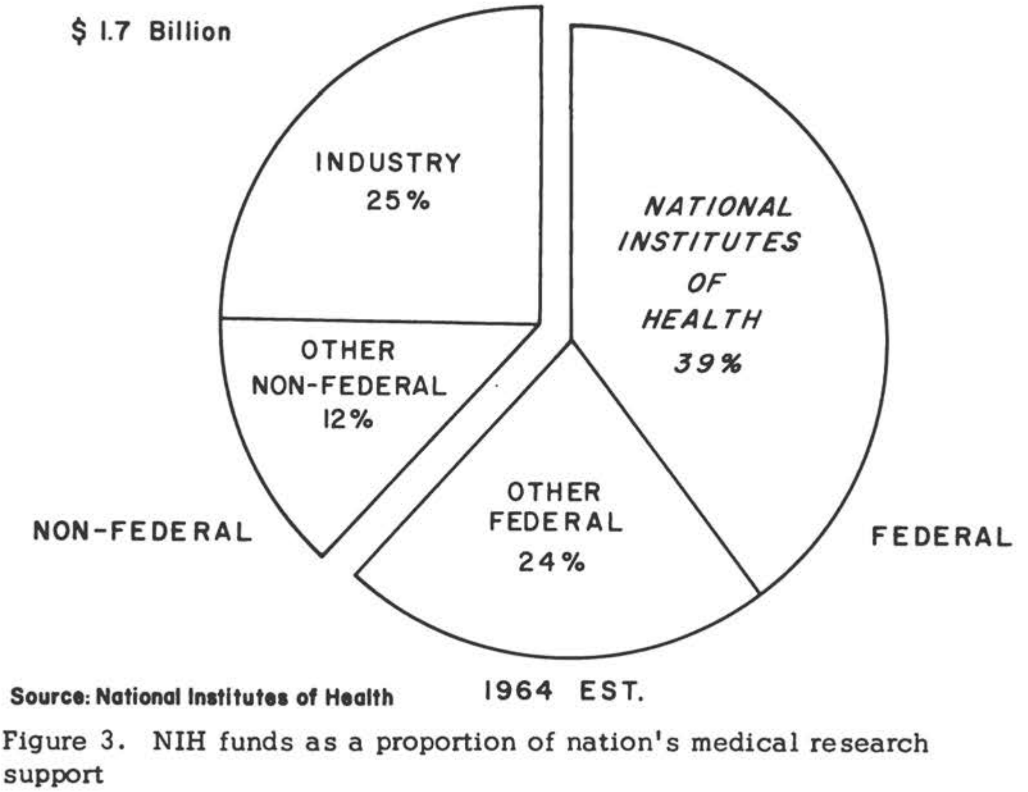


Figure 3. NIH funds as a proportion of nation's medical research support

The Role of NIH

NIH, as the principal supporter of medical research in the country, has conceived its responsibility to include the provision of resources for a sustained national effort—manpower, facilities, and special resources such as animal production and computer centers. Figure 4 represents the expansion of NIH appropriations from about $50 million for 1950 to $1,059 million for 1965. The growth has been predominantely extra-mural—grants for research, training, and construction—and has resulted primarily from Acts of Congress, which have tended to broaden program authority. NIH has met an increasing share of the cost of research in educational institutions, and is now the largest single contributor, providing one third of all federal research support.[1] Today the distribution of NIH research-grant funds is about equal as between medical schools and all other types of institutions. The extent of NIH support of non-medical institutions is a measure of NIH interest in the basic sciences.

Assessment by non-federal as well as federal bodies has played a very important part in the use of NIH as an instrument for meeting national research needs. Projections of total national expenditures for medical research have been derived by NIH from various studies, not

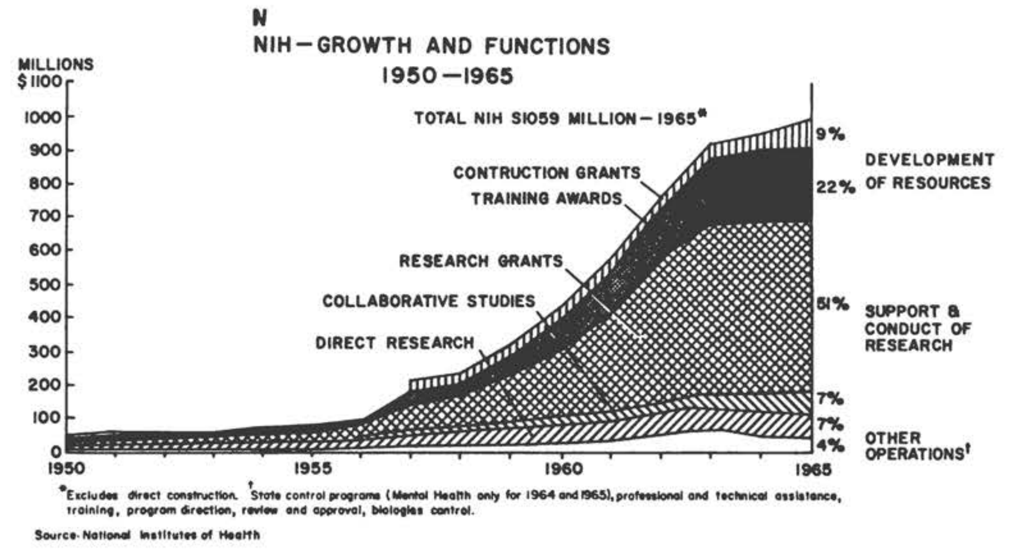


Figure 4. NIH—growth and functions, 1950-1965

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1. Exclusive of university-managed, federal-contract research centers, such as Los Alamos, Argonne, and the Jet-Propulsion Laboratory.

as goals or targets but rather as working estimates. The figure $3 billion, which would call for a doubling of the nation's expenditure for medical and health-related research by 1970, has been utilized by NIH as a basis for planning.

NIH programs will continue to play a significant part in the future of medical research. They will have the capacity to influence the course of the biomedical sciences, the functions of educational institutions, the careers of scientists, the process of medical education, and the character of health services. The content, policies, and direction of NIH programs are therefore paramount factors in shaping the national health-research effort. It may be well, however, to reiterate that significant contributions to medical research also come from other agencies and that these too have shown considerable growth.

Scope and Diversity of Medical Research

It is appropriate to call attention to a point made recently by Dr. Jerome B. Wiesner in testimony before the Committee on Interstate and Foreign Commerce of the House of Representatives. In response to concern about the size of the NIH activity, Dr. Wiesner emphasized the range of this activity and the diversity of health problems covered. A few salient examples are abstracted here from his data.

Of a total appropriation of $147 million for 1963, the National Heart Institute allocated only $6 million for the grant support of studies on hypertension, $5 million for heart failure, and $4.5 million for congenital heart disease. These important elements of the nation's major disease category had to take their place among a dozen research-budget items. Similarly, the Dental Institute, out of a total budget of $21 million, allocated less than $2 million for awards in the field of dental caries and periodontal disease, probably the nation's most prevalent health problems. The Arthritis and Metabolic Diseases Institute, from a total budget of $103 million, awarded grants for research on arthritis in the amount of $6 million and for diabetes, $4 million. Grantees of the National Institute of Allergy and Infectious Diseases studying virus respiratory infections, including influenza and the common cold, received only $2 million out of an appropriated $66 million. It should be pointed out that the budget in all these instances provided also for basic studies bearing to a greater or lesser extent on the specific diseases cited.

These examples warrant several observations. First, it is apparent that federal expenditure for research on specific diseases is quite low relative to the sums budgeted for broad disease categories. Second, the federal expenditure for research on a given disease is usually in the same order of magnitude as that of the corresponding voluntary health agency, emphasizing the continued importance of the voluntary agencies in research support. Third, general support programs such as those of the Institutes of General Medical Sciences and Child Health and Human Development bear a considerable share of the responsibility for gaining new knowledge applicable to specific health problems. With respect to the sums budgeted for specific diseases, the following quotation from Dr. Wiesner's testimony is significant:

> I have seen research and development budget breakdowns of this kind through the years for aircraft or radar or atomic weapons. I am used to seeing items of $200 or $300 or $400 million for very critical hardware, such as a missile or an aircraft; and even a total of $10 or $20 or $30 million for a vital vacuum tube area is not at all unusual. So when you find a sum of money much smaller than that for research of a critical component in the human body, you get a different perspective when you see the total summed up for a single item.

Impact of Research Support

What are the effects of support at current levels, following as it does a very rapid growth period? More specifically, what are the effects upon the advancement of the biomedical sciences and the attack on disease, upon educational institutions in the advancement of scholarship, and potentially, upon the character and quality of medical services?

Programs emerging at mid-century, when federal and voluntary-agency support were almost entirely mission-oriented, failed to provide an adequate base of fundamental investigation and resource development. Basic science disciplines such as genetics, embryology, and physiology were poorly covered in programs to attack disease categories. In NIH support operations, a better balance was gradually achieved among the biomedical sciences. This effort resulted in relative expansion of the attack on less awesome but nevertheless prevalent and devastating diseases—arthritis as compared with arteriosclerosis, dental caries as compared with cancer.

In addition, two new institutes and a division were established at NIH in 1963 with a view to extending non-categorical support. The National Institute of General Medical Sciences, assuming former operating functions of the Division of Research Grants, undertook to provide broad support of basic investigation in the health sciences; and the National Institute of Child Health and Human Development began a program designed to study life processes as a continuum from conception to death.

Recent creation of a Division of Research Facilities and Resources has supplemented basic science support with programs of research construction, general support to institutions, and the establishment of clinical and special resource centers. It is encouraging that the Congress has generously sustained this basic support of the medical sciences without the emotional appeal of categorical disease.

It seems clear that there is now better balance within biomedical research, as between disciplines and types of support, than ever before obtained. On the other hand, there is still great imbalance among medical research, education, and services.

Effects of Current Support on Science and Health

From the historical view, biomedical research is a new venture and we are still proceeding in painstaking steps, frequently in what at first blush seem unrelated developments. Nevertheless, in recent years, solid results have come from investigations in the basic sciences, whose importance is crucial in understanding fundamental life processes as well as disease.

Some examples may be drawn from the harvest of recent developments. In addition to knowledge of the chemical basis of genetics, we are gaining new insights into the mechanisms of viral entrance into and action within the cell, particularly the antigenic and disease-producing effects. We are now deriving some understanding of the interplay between virus and cell, permitting fresh approaches to viral diseases and the viral causation of cancer. Evidence of a relationship between viruses and cancer continues to mount. A short time ago, certain types of adenoviruses were implicated in the introduction of experimental cancer in hamsters; another type of virus is known to cause leukemia in mice in 30 days. We have advanced another step with the discovery of virus particles in the blood of leukemic animals and of human leukemia patients, but not in that of normal individuals. This is the kind of progress that hints at the possibility of a vaccine or chemical capable of preventing or suppressing such viruses, if indeed they prove cancer-causing in man.

Research activity into the nature and activity of viruses is very extensive. Virologists are giving special attention to the role of viruses in neurologic and heart disease. They are seeking, for example, to isolate viruses capable of lingering in the nervous system over months or years and slowly causing the progressive neurologic damage characteristic of multiple sclerosis, amyotrophic lateral sclerosis, chronic epidemic encephalitis, and certain forms of childhood epilepsy.

Viruses believed responsible for approximately 60 per cent of serious respiratory illnesses in children have been identified in recent years. Altogether, more than 100 different types of viruses have been isolated, identified, and classified, and at least half of these are known to cause mild or severe respiratory disease. An important discovery has been that a single virus, the respiratory syncytial virus, is responsible for a major portion of severe respiratory disease in young children and a significant amount of minor illness in adults. Supporting virus research is a broad program to develop reliable standards of comparison and diagnostic reagents for screening and identifying viruses.

Sustained research efforts against viral infections in recent years have produced new and improved vaccines against major diseases. During 1961 and the first six months of 1962, it was possible—with the overcoming of certain troublesome problems in the production and standardization of the live-attenuated polio vaccines—to move forward to the licensing and production of oral vaccines for all three types of poliomyelitis. In March 1963, two vaccines to combat measles were licensed for commercial use. In West Africa, cooperative studies—undertaken by the National Institutes of Health, the Agency for International Development, and the Ministry of Health of Upper Volta—have demonstrated that African children respond in the same manner as do American children to the live measles vaccine. Other studies in West Africa have shown that combinations of live measles, smallpox, and yellow fever vaccines can be successfully inoculated into susceptible infants, and that live measles vaccine is fully effective for infants over eight or nine months of age.

The attack on disease has nowhere shown more spectacular results than in the development of new and improved surgical technology. The recent substantial inroads into congenital heart disease have been made possible by striking strides in cardiovascular surgery, as well as in development and refinement of heart-lung machines, hypothermia, and other life-support techniques. Twenty years ago, surgeons could correct only two of the 15 most common congenital heart defects and improve two others. Today, 12 of these defects can be completely corrected and the other three improved. Surgery is having profound effects on handling occlusive artery disease and stroke, especially in limited, highly localized lesions in medium-size arteries in conditions not caused by disturbances in lipid metabolism. In many cases, it is no longer necessary to remove sections of the arteries; the artery is opened, the occlusion removed, and a dacron patch placed on the artery. A recent advance that may further increase the chances of desperately ill infants with congenital defects causing severe tissue-oxygen deficiencies is hyperbaric oxygenation. Corrective surgery is carried out in a compression chamber containing pure oxygen under a pressure of several atmospheres. This enables enough oxygen to enter the blood to sustain the infants during surgical procedures that they might not otherwise be able to withstand.

No discussion of modern surgery can avoid mention of the work being done on organ transplant from one person to another, and on the development of methods of suppressing the body's natural immune response to such transplantation. In kidney transplants, for example, modest success against the immune response has been achieved with the use of X-rays, cortisone-like compounds, and certain drugs used in anti-cancer therapy. In the meantime, artificial kidneys have been developed as a means of sustaining patients with renal insufficiency for long periods of useful life.

Medical research, it seems obvious, has now reached a stage in which its complex problems will require for solution highly sophisticated engineering and instrumentation that permits more refined measurements and more sensitive discrimination in biological systems. The parallel to the physical sciences is apparent and, as in these disciplines, biomedical investigations will need the methods and large-scale organization for measurements, discrimination, analysis, and synthesis of biological data.

Effects of Support on Educational Institutions

Let us now consider the impact of rapid expansion of research support upon educational institutions, especially medical schools. The effects have been substantial and, on the whole, salutary. The magnitude and composition of the expansion may be indicated by a few figures.

NIH research grants to educational institutions, for example, have increased fifteenfold since 1955—from $27 million to an estimated $400 million. This support has by no means been confined to the "haves." It is characterized by (1) an ever-widening participation to the point where NIH now provides research support for nearly 400 educational institutions in every state of the Union; (2) a steady rise in the number of educational institutions with substantial NIH program involvement;[2] and (3) a broadening of the base of excellence, bringing a diminution in the concentration of funds among the top 25 institutions (50 per cent of total funds in 1948, 42 per cent in 1963).

The composition of the "top-25" group of institutions has changed considerably. These are now characterized by integrated research, training, and demonstration programs cutting across feudal departmental boundaries and drawing upon broader and interrelated forms of support—grants for graduate education and post-doctoral training, construction of facilities, provision of specialized resources, and general support of research and research training to be allocated locally rather than exclusively on the banks of the Potomac.

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2. About 75 institutions fell in the $1 million-or-over class last year: 23 in the East, 20 in the South, 17 in the North Central States, 12 on the Pacific Coast, and 3 in the Mountain States.

These far-reaching effects have extended into all parts of the educational institutions—graduate schools, professional schools, schools of agriculture and veterinary medicine, and, to a lesser extent, undergraduate colleges of arts and sciences.

It is clear, however, that the medical schools of the United States, which receive about half of NIH extramural support, have been the chief beneficiaries of the national decision to expand support for medical and health-related research. Between 1957 and 1963, for example, medical-school operating expenditures nearly tripled, rising from $240 million to an estimated $600 million. During this same period, sponsored programs (research and training) rose from less than 40 per cent to nearly 60 per cent of the total. As a consequence, medical schools have become increasingly dependent upon the financial support provided by federal programs, especially those of the National Institutes of Health. But financial comparisons fail to reflect adequately the far-reaching transformation—the qualitative "spin-off" that has occured in medical education.

The impact of expanded research support upon the quality of the educational process must be appraised in the perspective of decades—not by year-to-year gains. It will be reflected most accurately in the improved quality of our intellectual resources in the decades ahead. Short of such a comparison, we can turn to interim measures, which are indicative though they do not evaluate the final product. For example, the number of full-time faculty in medical schools of the United States has nearly quadrupled since 1951, rising from 4,000 to 14,500. This remarkable growth of full-time faculty has been linked part and parcel with the transformation from medical school to medical center.

The modern medical center not only includes the education of medical students but extends far beyond this traditional central purpose. For example, medical school faculties last year were responsible for teaching some 70,000 students, 32,000 of whom were medical students, 18,000 were internes and residents, 10,000 were graduate students and post-doctoral fellows in the sciences, and another 10,000 were students in the other health professions.

Concomitant with this expansion in teaching responsibilities, faculty participation in research has also increased steadily. As a consequence, the quality of medical and graduate education has continued to improve through the interplay between research and teaching. Moreover, the expansion of research activity has had a profound effect upon the character of the communities and the geographical areas in which they exist. Such activity constitutes the training ground for tomorrow's generation of scientific and professonal talent, and affects in a most pervasive way the aspirations and values of children and youths in local communities and throughout broad regions.

The number of full-time faculty members receiving some part of their salaries from federal funds—from research or training grants—has risen steadily and is 44 per cent of all full-time faculty members in 1964. On the one hand, this development reflects the policy of the federal government to pay for the full cost of research. On the other hand, it should be recognized that such support is linked to discrete research projects and training programs, and hence the educational institutions are becoming increasingly dependent upon these programs as a means of financing faculty salaries.

In summary, the impact of expanded research support on the schools has been far-reaching. It has substantially extended the scope of activities and opportunities in educational institutions in the interest of advancing the nation's health through new knowledge. But is has also raised many questions about the role of educational institutions in accelerating the application of knowledge, in improving the quality and standards of health services, and in serving as a center and focus for lifetime education of its graduates and other health personnel.

The Future of Biomedical Science

The rapid growth and current magnitude of support in the biomedical sciences will surely lead to pronounced effects within the coming decade. These sciences are in the process of maturation. They are emerging into a position comparable to that of the physical sciences after World War II. Within the next two decades, it seems probable that the biosciences will dominate research activity as the phenomena of life and disease are studied with ever greater precision and sophistication. This would appear to be a natural result of the merging of interests and approaches as physics, engineering, mathematics, and the social sciences become increasingly applicable to biomedical problems.

Will further expansion pose the problem of deciding the best institutional form for biomedical science of the future? In other words, should expansion take place within the existing framework of educational instituions or should a major development of independent research institutions be undertaken? There is every indication that the schools, augmented in number and facilities, will be able to expand their research capacity. This will demand, however, a major evolution of government policy and programs in support of the educational function, bringing the schools into better balance.

Probable Effects of Research Support on Medical Service

An important change may be due in medical services as a result of the extending influence of biomedical progress. As the advancement of research has enhanced the quality of medical education—as academic and scientific medicine improve as a result of research support—a worrisome differential is developing between the quality of medicine in centers of excellence and that at community level. Growing popular awareness of this differential will generate increasing demands for access to superior medical services. Thus, through creation of a substantial gradient between academic and community medicine, pressure for a more effective flow between these two areas will become overwhelming. Some needs already felt are those for more effective medical communication and application of research findings, and legislative proposals are being considered at present for bringing about a more direct linkage between academic medicine and community practice.

Broadly, then, the effects of current research support on the biomedical sciences are felt above all in the schools and research institutions, which are afflicted with growing pains. Balance among scientific disciplines is approaching, but balance of functions within the schools awaits active governmental action based on a frank policy of educational support. Research itself is advancing in scope and depth, with an accelerating flow of practical results that will progressively affect the national health picture. Through enlargement of support of research, the character and quality of medical education has been transformed: a firm scientific base has emerged, influencing favorably the academic environment and the education provided. A final effect, and one that will derive from advances in education and available knowledge as well as from public demand, will be a generally higher level of medical practice, and thus of national health.

BREAKING THE INNOVATION BARRIER

Carl E. Barnes, formerly Vice President for Research
Minnesota Mining and Manufacturing Company

Within our broad topic, "The Effects of Current Trends on the Support of Research," I shall talk on one aspect: innovation. Innovation is a vital link between research and its impact on the economy, and should it lag, it might be incorrectly concluded by some that research itself is less productive than they had been led to believe.

I shall be speaking as an industrial research chemist, inventor, and research administrator. There has been much theorizing on this subject by economists and social scientists. My hope is that the views of a physical scientist may also be helpful.

In the last few years, millions of words have been written and billions of dollars have been spent on research—the greater portion of these dollars having come from governmental sources. Many of the words have been in the form of suggestions as to how research can be made more effective in meeting the needs of the future. And the dollars have been spent in various ways. In the broadest sense, they have been spent to enrich our knowledge of the universe, and some of them have been specifically designated for this purpose. Many of them were spent to support what is broadly known as defense, but I do not plan to discuss this area; it has its own set of problems. But underlying it all, there are so-called "practical" objectives. These may be classified broadly into two types: (1) the development of highly sophisticated, labor-saving devices, and (2) the development of new products or processes. As long as these two types of research are equally productive, we have a kind of "balanced aquarium" situation, with no serious sociological problems—other than, perhaps, the retraining of certain segments of the labor force to enable them to become re-employed in the new jobs that have been created in this process of technological change.

But should, for example, progress in automation forge ahead of new product innovation, we may find ourselves in a difficult situation. For the "balanced aquarium" condition depends upon the availability of new jobs created from the new businesses that resulted from significant new product innovations. These new jobs must take the place of those that were eliminated as the result of our research-sired automation. If this happy balanced situation does not exist, then we are in the perhaps embarrassing position of having spent government funds for expensive research and development programs that have, as the net result, put many people out of work!

As an indication of the seriousness of the situation, Dr. Wolfbein of the Department of Labor has stated that we need right now new jobs at the rate of 300,000 a month to fill the combined need of those made jobless by automation and new people coming of employment age.[3]

This points up the importance of making sure that the second major objective of our over-all research effort is successful. Making sure, that is, that we are doing more than simply discovering potential new products; that we are also bringing about the innovation of these products and creating the new businesses and new jobs in the ratio needed to keep things in balance.

Are we actually accomplishing this, or is innovation lagging in this country? There seems to be a general feeling that it is lagging. While hard facts to support this are difficult to come by, there have been complaints by business executives during the past two years to the effect that increases in the number of marketable new products have not kept pace with rising research expenditures. One drug executive complained that, although research spending in his industry had increased three-fold from 1955 to 1962, the increase in the number of new drugs put on the market advanced only slightly more than 20 per cent. This same trend has continued. Just last week the New York Times reported that, despite record-breaking research expenditures in 1964, the U.S. pharmaceutical industry introduced fewer new products than in 1963.

Another indication is that, while in the past decade the total annual spending in industry has risen from about $3 billion to about $7 billion, the rate of filing of U.S. patents has increased at the sluggish rate of only about 5 per cent per year, which, when compared to growth in

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3. Seymour L. Wolfbein, Director, Office of Manpower, Automation, and Training, U.S. Department of Labor, at the Hearings before a Senate Subcommittee on Small Business, June 20, 1964, Part 3, page 232.

technical manpower, is actually a decreasing rate on a per capita basis. The superimposing of the tremendous government research and development expenditures on the industrial program has had no noticeable effect on the rate of patenting—unless you wish to argue that the rate would have fallen off drastically without the government program.

Perhaps the very magnitude of the federal research and development budget itself is the best indication of the seriousness with which the situation is regarded. As you know it now totals \$15.5 billion a year, of which \$6 or 7 billion is spent for non-defense research and development. These figures are doubling every four years. While there is no really clear-cut statement as to the necessity for spending these huge sums(we got into this business, you recall, as the result of the first Russian Sputnik), the fact is most of the reasons advanced do center around the need to bolster our economy by making the "fullest use of our technical resources—in the form of brains."

Understandably the first reaction to this is an attempt to devise ways and means of stimulating creativity, to encourage the development of new ideas, to train more people as scientists, and, in general, to support research more fully. All this we are doing and have been doing for a while. Yet it seems that the need for new products and new jobs continues.

There is no doubt that more research is needed. I am convinced that one of the causes of the present falling off in the productivity or industrial research was the rapid depletion, some years back, of the vast storehouse of fundamental scientific knowledge that had accumulated over the past century. In the chemical industry, to cite an example from a field with which I am familiar, there was a sort of "golden age" of innovation in the period before World War II. It was during this period, for example, that such important plastics as the polyacrylates, polystyrene, and polyethylene were introduced and a whole new plastics industry came into being. The packaging industry was revolutionized with products ranging from squeeze bottles to plastic films. DDT heralded an important expansion of the pesticide industry, and with sulfa drugs and penicillin the pharmaceutical industry experienced rapid expansion. The era of wholly man-made fibres was ushered in by the advent of nylon. The photographic industry began a period of spectacular growth with the introduction of Kodachrome, the first really practical color film. There were, of course, corresponding "new-industry-making" developments in other fields too—such as the long-playing record, fluorescent lighting, television, and electronics generally.

Some of these products were not discovered in this country and many of them were academically old or based on well-established information. A good example is polystyrene, which had been known for nearly a hundred years. But the incentive to innovate polystyrene and a host of other products came during this period. Was this rush to innovate just chance, or were there significant reasons?

Partly, without doubt, it was due to the fact that industrial research had "come of age." Top management in many of our industrial concerns had just come to appreciate fully the potential of research as a source of new products. The success of one company in this activity spurred others to follow in their footsteps. All this brought with it a tremendous incentive on the part of industrial research investigators to delve into the academic secrets of the past and ferret out those that might have commercial potential.

This period of industrial research may be likened to an earlier period in our history when our forests were cut down indiscriminately and the land nearly denuded—all to support a thriving lumber business. One of the things that has been wrong with the productivity of industrial research during the past few years is that the reservoir had already been drained nearly dry, and we were in sore need of something akin to a "reforestation" program to replenish the storehouse of fundamental knowledge, which is the "mother liquor" from which new industrial products and processes are developed.

This we are doing, and properly so. I suppose that, at the rate we are going, it won't be long before we pile up as much fundamental knowledge as was collected in the past 100 years at the unsubsidized rate of expenditure—if we haven't done so already. We might ask: Why hasn't all this research resulted in a flood of new products?

One reason is that it has been done over too short a period of time. Another, and more important, reason may be that we have not been as selective. During the previous 100 years, let's say, research funds were hard to come by; hence only the most promising avenues could be explored. Others had to be passed by. It has been said that the only justification needed to obtain research funds <u>today</u> is to have an idea that has not been investigated before. The mere fact that "it hasn't been done" is reason enough to spend money on it. While I doubt that the situation is quite this bad, it may well be true that we are not as selective today as we used to be when funds were much more limited. It is possible that this weeding out of the least attractive ideas of the earlier research period improved the general quality and significance of the work. The comment has been made that we are compiling a tremendous mass of detail, much of which has no special significance.[4]

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4. <u>Fortune</u> magazine of September 1964 quotes Hans Bethe of Cornell University: "...the huge sums of money spent for physical research over the past thirty years (has) yielded nothing comparable to the theory of relativity or the quantum theory—only a lot of detail—important but nothing you cannot summarize in one or two sentences." He pointed out, of course, that it is difficult to know ahead of time which of these details are going to prove important.

At any rate, we have done a lot of research during this recent period and we have collected a lot of facts. Some of this knowledge is of such a nature that it leads us to a better understanding of our universe, and some of it is what we call "practical"—meaning, I guess, that it seems to be of more immediate value to humanity.

Not long ago, President Johnson stated that our national space program has already produced more than 3,000 new products and processes of private commercial potential.[5]

And so it appears that "practical" ideas have come, but the commercial new products have not. Again we must ask why. What was it that we had during the "golden era" that we do not now have? Perhaps a word or two is needed here to define better the term, "new products." Many will tell us that the flow of new products has continued unabated—that there are just as many today as there ever were. This is undoubtedly true if we mean new types of toothpaste, hairsprays, or new varieties of breakfast cereals. But if we define a new product as something unlike anything that has preceded it—like the zipper, synthetic dyes, aluminum, penicillin, synthetic fibres, television, ball-point pens, scotch tape, plastics—products that created whole new industries—then, I think, we may find that there has been a slackening in pace.

In speaking about the need to learn more about the relationship of research and development to economic growth, Dr. Hornig has said: "We simply don't understand it in quantitative terms. We need to have the answers to such questions as: How do we stimulate the growth of the economy? What, if anything, should the Federal government do along this line, and how should it do it?"[6] During the course of my talk I hope I can make some helpful suggestions relative to these problems.

Dr. Hollomon has come a little closer to an answer, I think. He said recently before a Senate subcommittee: "More often than not our economy is limited not by our physical understanding of the universe, but by our inability to introduce new devices and new techniques into use in the economy." And again: "Several studies have indicated that perhaps the stimulation of the entrepreneur, the one who can take the risks, and who understands the process of technology, is more important to the development of our economy than science itself."

Perhaps the best over-all summary was that of Vice President Humphrey, who made these observations when chairman of the subcommittee at which Dr. Hollomon spoke:[7] Mr. Humphrey said: "Our

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5. National Rocket Club dinner, March 1963.

6. Chemical and Engineering News, Oct. 19, 1964, p. 92.

7. Quotations from the opening statement by Mr. Humphrey, "Hearings before the Select Committee on Small Business, Subcommittee on Retailing, Distribution, and Marketing Practices", U.S. Senate, May 20, 1963.

national well-being is becoming more dependent upon human resources than upon natural resources." And again: "The strength of our economy and our economic growth depend increasingly upon and are limited primarily by our technical capability." And—of particular significance, I think—he said: "The growth of our economy will depend more and more on technical solutions to the problems of our industrial life. We should therefore find ways to encourage technical entrepreneurship; the responsibility falls on the Federal Government to assure that proper conditions exist in which technical entrepreneurship can flourish."

This is the obvious answer to the problem: Innovation is the bottleneck—not research. I am not suggesting that we cut down on research. But I am suggesting that if we do not find an answer to innovation, Congress may cut down on it for us because research is not "paying off."

What is wrong with innovation? It appears that it would be worthwhile to look into this matter carefully. One difficulty may be that the managements of many of our industrial concerns do not appreciate the commercial potential of the new developments coming out of their own research laboratories. We haven't in the past had many company managements with sufficient technical competence to do this, and scientific discoveries are getting more and more complicated. The situation is improving, however. Statistics[8] show that from 1950 to 1963 the number of companies whose top executives have a technical background has risen from 20 per cent to 36 per cent. This should make it easier.

Yet, paradoxically, in the "golden era" of industrial chemistry to which I referred earlier, managements with only a poor grasp of scientific and technical matters nevertheless did a most effective job of innovation. Why? It was clearly a matter of business climate. Obviously business climate is a more important factor than a technically trained management!

It did not take a technical background when you had, for example, products like Scotch Tape, or a plastic as clear as glass but unbreakable, or a chemical sweeter than sugar but non-fattening, and each of these well protected by patents. In those days our courts had not yet developed a tradition of not upholding patents. There is one court that hasn't sustained a single patent that it has reviewed in the last seven years! There must have been some good patents among all those reviewed in this period.

In those days, too, you could do more or less what you wanted to do with your own property, the patent. You could license it or not, as you chose. If you did decide to license (as most companies did), you could control the price at which the competitor sold his product. Naughty as it seems by today's standards, it did leave the newly created competitor with but one advantage he could hope for: improved quality. And this

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8. Editors of the Scientific American, "U.S. Industry: Under New Management," New York; Scientific American, 1964.

was to the benefit of the public. There were, of course, many other differences in those days, but I think these will serve to illustrate.

We have come to look upon these practices as undesirable, and perhaps many of them were. One by one they have been prohibited. But I think we must now ask ourselves the questions: Has the pendulum swung too far? Is the public better off now? Where are all the new jobs that should have resulted from the huge research expenditures of the past decade? 3,000 potential new products do little to help.

Let's examine the situation today in an attempt to understand more fully why innovation is lagging. In all probability the solution is not to "go back to the good old days." At least I don't think this is a proper solution, for we have an entirely different set of conditions today.

One difference lies with the type of management personnel in many companies. Although they may have somewhat higher technical competence, the business experience of these people is considerably different from that of management, say, 20 years ago. These people have been dealing with the federal government to a considerable degree. Their decision-making has consisted largely of determining what kind of product is needed by the government agency with which they are dealing and then entering a bid for the business. Their planning consists of attempting to obtain a government research and development contract that will be supported by government funds, and that is aimed at developing a product that hopefully they can sell to the government after successfully negotiating a production contract. All very risky, what? I think it may take some really powerful incentives to get these modern "government-trained" managers to move on the innovation of new products for the civilian market. But let's continue with our profile of the modern manager.

Let's see what this "especially trained" manager must contend with if he should decide to make a try at innovation in the civilian market. Right off, he is confronted with a matter of price. In his familiar area of government negotiations, this was pretty much determined for him. But now he must make the decision. And this involves all sorts of unfamiliar questions. What will the customer pay? What are his costs? They are no longer just manufacturing costs. There are selling costs, and part of these are advertising costs. How much advertising is it going to take to develop a demand in the buying public? Will the public "go for" the product at all? How much they go for it will be an important factor in determining costs. What is his protection? His research people tell him he has a "strong" patent. But the courts in many parts of the country have not been upholding even "strong" patents. This means that if his product is successful he will undoubtedly have competition before he has had a chance to recover his development costs. And even if the patent is upheld he won't be free of this competition, for he will be compelled to license the patent anyway.

There are so many advantages to being on the other side of the fence that our manager may well reason that, if he must get into the civilian business, it would be far better to wait for another company to make the initial move and be the imitator himself. With lower costs, now that the innovator has paved the way and established public acceptance, our manager can cut prices and establish a position right away with hardly any risks. If a patent is involved he knows that he can challenge it with an excellent chance of winning, or, failing this, he can demand a license and get it. The royalty payments would certainly be far less than the innovation costs, so he would still have an advantage over the original producer. No, there are too many risks and too few advantages to being an innovator. It would be far wiser to wait for an opportunity to be an imitator. If new business is needed right away, our manager may well reason that the thing to do is to spend a lot of money on a lavish proposal for the next government production contract coming up for bid. This is far less risky, and besides it is in a familiar area anyway.

Now maybe I have over-simplified this a little in order to make the point. But I think it is not difficult to understand why companies are invading each other's fields these days instead of innovating new products from their own research laboratories. They don't feel too responsible for this research anyway, for half the cost of it was paid for by the government. Is this situation likely to be any better if the government were to pay for 75 per cent of the research costs, as has been proposed?[9] If we wish to give more tax incentive, perhaps it would better to direct this toward innovation rather than research.

For reasons I have just given, I doubt that more research is going to have any appreciable effect on the introduction of new products. It seems to me that there are two things that must be done if we are going to get innovation today. One is that we must take some of the risk out of innovation. No top executive wants to pile up losses or even have a record of decreased earnings during his period in office. The other is that we must put back some of the attractiveness of innovation that used to be there. Let's be honest. The main reason we had so much innovation during the pre-war period was that it was darned attractive and the risks weren't too high. There was a good chance that the new product would be a success; new markets would be developed, the company would grow, new jobs would be created, and the whole venture would be profitable. Let's be honest again. Today the odds are that it won't work, and the chief executive will be in for a lot of criticism for having been so foolish as to try.

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9. Richard F. Janssen, "Industrial Subsidies," Wall Street Journal, November 9, 1964

So first we must take out some of the risk; second we must make it attractive; and then, if we can add a third factor, we must devise a penalty for not innovating. Then I think we may get results.

Innovation necessarily involves risks. It is a venture into the unknown, and for many this is sufficient reason not to make the attempt. But, fortunately for society, we do have some entrepreneurial types to whom the unknown is a challenge, providing the promise of reward is considerably greater than the risk involved.

There may not be much we can do about the ordinary business risks—unless it be some sort of federal insurance policy—but we can certainly do something about the recently introduced additional risks. Most of these center around the patent system as it is now operated. For example, one important risk for an entrepreneur relates to the question: How strong is the patent? While this depends importantly on the originality of the invention, it also depends, unfortunately, on the attitudes of some of our courts. Surely this is a risk we can remove.

The most effective thing we can do about the risk factor is to subordinate it to the lure of the reward. If the hope of gain is less than the fear of the risk, we shall get little in the way of innovation—and I fear this is the present situation. But if the promise of reward is great enough, there will be those who will assume the risks. It has always been thus since the beginning of time.

Inasmuch as patents are the main instrument through which we can achieve the required level of incentive, we can logically turn now to considering ways of modernizing our patent system to meet current needs.

Our present patent system has gone essentially unchanged for more than 125 years, the basic law under which we are now operating having been established in 1836. There have been some minor revisions, of course, but no basic changes. During this long period we have changed from an agricultural nation to a highly industrialized one. The basic principles of our patent system are no longer as effective as they should be in our changed society, primarily because the incentive which they provide is directed toward the individual inventor. While there is still a goodly number of private, individual inventors, by and large the most significant inventions are those made by the "paid-to-invent" employees of organized research laboratories operated either by the government or by private industrial concerns.

There is no question but what the incentive is considerably reduced for these present-day inventors since, as a condition of employment, they have had to agree to assign the title of any inventions they may make and patents that may result from them to their employer. Although I will not agree that all the incentive is gone (for there still remains the recognition and public acclaim that come from being the author of a U.S. Patent, and, depending on the company, certain financial rewards). Nevertheless, I think most people would agree that the incentive is considerably reduced from what it was, say, 50 years ago when the inventor has full title to the patent.

It would seem, then, that any attempt to modernize our patent system should be aimed at increasing the incentive both for invention and innovation. Then there is something else which it would be desirable to accomplish. We should provide alternative routes for the innovation of a patent that for any reason has not been developed by its owners. By this I do not mean to imply that I believe big corporations deliberately "sit on" patents, as some would have you believe. Although it may have happened in some cases it is by no means general practice. There are cases, however, where the owners of a patent may lack the vision to see the potential of the new invention that they control. This can happen much more frequently than we realize, for only in those rare cases when somehow the invention gets into other hands and is then successfully innovated do such cases come to light.

There is a good example of this today in Xerox. No one would argue that the patent covering the process of xerography was not a valuable one to the public and owner alike. And yet the inventor took his process to 20 different companies[10] including two or three of our largest corporations, and all 20 of them concluded that this patent had no commercial value! Were it not for the fortunate fact that ownership of this patent resided in the hands of the inventor, the public might never have had the benefit of this useful invention. How many other potentially useful inventions has the public lost because control of the patent rested with someone other than the inventor?

Surely an objective of any modernization plan ought to be to provide a means of circumventing such a situation. A way must be found for the inventor, who had the imagination to make the invention in the first place, to have some effective voice in its innovation. He may well be the only individual with the vision to perceive its commercial potential.

A common proposal for circumventing a "Xerox type" situation centers around the payment of maintenance fees. I don't want to take the time to go into this matter in detail but I will simply say that in principle the patent would terminate if these maintenance fees were not paid. There are many who favor this plan.

In the first place, the payment of maintenance fees would be a useful way of helping to make the Patent Office more self-sustaining. But, of course, there are many ways of accomplishing this if this is the primary objective. Maintenance fees have been recommended as a method of "weeding out the deadwood." Insignificant or inconsequential patents cost everyone money and it would certainly be desirable to eliminate

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10. Statement by Chester F. Carleson, the inventor, at a meeting to discuss possible changes in the U.S. Patent system, called by the U.S. Department of Commerce, March 1, 1965.

them. Here again, though, in the case of company-owned patents, we are faced with the problem of deciding whose judgment to follow—the owner's or the inventor's. Judging from the inventor's experience with the Xerox patents it would appear that the managements of 20 companies would have decided not to pay the maintenance fees. For reasons I shall discuss later, this would undoubtedly have resulted in the loss of this valuable process to the public.

The chief objection I have to the principle of maintenance fees is that it destroys the patent, that is, the patent becomes public property. In our country this defeats a major objective, which was to bring about innovation if there is any merit whatever to the patent. This point needs a little further discussion, since someone always points out that the system works well abroad and that we are, in fact, the only industrial country except Canada that does not have maintenance fees. We should remind ourselves that there is another way in which are are unique among nations: we are the only country with effective anti-trust laws. We do not believe in monopoly, whereas these other countries do. And this makes a world of difference in the way in which termination of the patent for non-payment of maintenance fees works.

In England, for example, if a major company allows a patent to terminate, how many other companies are there with appropriate experience and background who are also large enough to attempt the innovation? Possibly one or two. In this case, one of these companies can pick up the patent if it so chooses, knowing that for all practical purposes even though it is public property, they are without any effective competition. In other words, "public ownership" means, in effect, that the patent goes to perhaps the only other company in position to use it.

In our country, by way of contrast, because we have discouraged monopolies, we may easily have 10 or 20 companies any one of which could utilize the patent. None of them will, however, for there is too much potential competition. Throwing the patent open to the public is in fact the strongest possible deterrent to its use in our country.

There are many economists and social scientists who will take issue with what I just said. They will point to several examples of patents that have been innovated after being opened up to public use. Often-cited examples are an improved rayon-spinning process, government-owned fertilizer patents, and a textile-treating process developed by a government laboratory. An outstanding example is the case of transistors; public ownership of these patents resulted in a host of companies entering the field (many of them had rough going, too). By way of contrast, some will maintain that the monopoly created by the patent actually slows down commercialization, since others who might aspire to the business must wait until the patent expires.

On the other hand, successful inventors and corporate heads will insist that patents that are generally available will not be innovated. And they too can cite convincing examples, such as the many patents that emanated from various government laboratories and especially the situation that existed during the war and immediately afterward when many valuable seized German patents were offered to the public. Very few of these were ever used by anyone. And in one of the cases where an attempt was made—namely that of the Leica camera—the results were disastrous. This is a clear example of what most businessmen know without "burning their fingers."

And yet the fact remains that some publicly owned patents are utilized, and hence we have a situation that is not only confusing but leads to a continuance of the controversy. This seemingly anomalous situation may be explained in this way. Patents may be broadly classified into "original" or "basic" patents and "improvement" patents. It is the improvement patents by and large that confuse the issue.

If you are a manufacturer of rayon, let's say, and a patent on an improved process comes into public ownership, why wouldn't you use it? You are already in the rayon business and here is a better process that your competitors will undoubtedly use; if you don't, you will be at an obvious disadvantage. There is little speculation involved and it is a better process. Of course such a patent will be widely used. Who would want to be left as the sole high-cost producer?

Most of the examples of widely used, publicly owned patents will fall into this category. Even in the case of the transistor, which was a basic new discovery, it replaced the electron tube. Would you want to be the only electronics manufacturer who could not offer a transistorized version of your product?

When experienced inventors and corporate executives say that a patent will not be innovated if it passes into public ownership, they are not referring to these types of patents. They are talking about patents that cover totally new products in which the market potential is wholly unknown. Xerox again is a good example. I know of no cases in which patents of this type have been innovated as a result of passing into public ownership. Who would risk the capital?

Before coming to what I believe is a novel way to accomplish our objectives, while at the same time minimizing the ill-effects, let me summarize them. We should like to:

(1) create something in the nature of a penalty for failure to innovate;
(2) provide strong incentives for invention, in particular for the "paid-to-invent" employee of an organized research laboratory;
(3) provide maximum incentive for innovation, not only for the owner of the patent but also for an entrepreneur;
(4) establish alternative routes for the innovation of a patent so that it can have maximum opportunity to come into a favorable solution;
(5) and finally we must do all these things on as equitable a basis as possible.

If we can accomplish these goals, I believe we shall have taken a great step forward. For I believe there are a great many "Xeroxes" that have been imprisoned by our present system—inventions that, if released, could provide the new industries we so urgently need.

The change necessary to accomplish all this is a simple one. It would provide that after, say, five years, the title to the patent, if it has not been innovated, would revert to the inventor—that is, the "paid-to-invent" employee of an organized research laboratory. Private inventors would not be affected, of course. By this simple device we have accomplished all our objectives.

First, we have provided a penalty for not innovating, namely, loss of ownership of the patent.

Second, we have restored a great part of the incentive to invent. A would-be inventor now knows that his invention is likely to be used rather than "sit on the shelf"—a situation that has frustrated many an inventor and discouraged him from further efforts. He knows that, if his invention is not utilized, ownership of it will come to him and its future will be in his hands. If he cannot interest anyone in it he has only himself to blame. And the new invention is without question in the best possible hands for realizing any commercial utility it may have, for no one has more faith in it than the inventor.

Third, we have created a most powerful incentive for innovation by the original owners of the patent—the large corporation, shall we say. For the possibility now exists that the patent will fall into the hands of someone who will successfully innovate it—possibly a competitor. Such a situation would, of course, raise questions as to the wisdom and judgment of the management personnel.

No such embarrassing situation could exist in the case of the presently proposed maintenance-fee plan, for should management decide not to pay the continuing fees, the patent will simply terminate. Under these conditions it is unlikely to be used and no one will ever know whether management made a wise decision or not. Because of this, of course, it is not necessary to give the matter a great deal of thought.

Without doubt the proposed new plan would provide a strong incentive for the original owners to utilize the patent. It is certain that at regular intervals top management would take one last look at those patents nearing the end of the five-year period to make a final decision. As things stand now, most patents are not reviewed by top management at any time.

There is a modification of this plan, which I should mention here. Depending upon how much force you wish to impose in the direction of innovation, the maintenance-fee principle could be followed, with this difference: instead of terminating the patent at the end of the five-year period, the title would revert to the inventor if the proper fees were not paid.

Finally, we have now preserved the patent intact with its full value, rather than having destroyed it. The new owner is therefore in the best position to attempt innovation by license, outright sale, or by starting a new business after raising the required capital on the strength of the patent. We have, in other words, provided for several alternative routes by which the patent can be innovated.

There are, of course, many details to be worked out so that the arrangement will be fair to all parties. For example, in a situation in which a company holds a family of patents, all related to the same end-product (such as alternative methods of manufacture), it would be unfair to the company to force it to lose title to those patents they were not using provided they were in the process of developing the product via one of the patented routes. In such a case, the innovation of one member of a group of related patents should reserve the whole group for the company.

Another problem is the "conflict of interest" that would develop with employee inventors who still retain their jobs but, at the same time, are trying to exploit inventions that have now become their property.

Some businessmen will undoubtedly raise the objection that such a plan would be unfair, since it would deprive them of their property, which they have paid for in terms of research expenditures and a substantial salary for the inventor. In the granting of the patent rights to the businessman, however, there is an implied responsibility to utilize them for the public good, and he will lose these rights only if he does not fulfill this obligation. I suspect such a plan would prove to be a healthy medicine, just as were the anti-trust laws, to which business violently objected.

Doubtless many more such objections will come up, but these can be worked out on an equitable basis, and I do not think we should drop consideration of this plan unless insurmountable objections arise.

If we were to do something like this, I would recommend a re-examination of our whole policy regarding patents with an eye to strengthening incentive to the fullest. Patents are private property, and they should be protected by law, if properly used, as is other private property. The courts should be encouraged to <u>uphold</u> proper patents, and should not apply the philosophy that patents are not in the public interest.

In summary, I believe that innovation <u>is</u> lagging in this country, and that this is not due to any lack of creativity among Americans; we have our share of original ideas. And, while I believe that we must continue research at a high level in order to keep our storehouse of fundamental knowledge well filled, I do not believe that research alone will solve our problems. It seems to me that, unless we find ways to make research productive in a practical way, those who control the purse strings may have second thoughts as to the effectiveness of research in providing solutions to the sociological problems that confront us as a nation.

It seems clear that our problem is one of breaking the innovation barrier. Part of what must be done is to change the philosophy concerning intellectual property, so that incentives for the entrepreneur once again outweigh the risks. But I also believe that this alone is not enough. We must modernize our laws so that they fit realistically the considerably changed conditions under which today's inventions are made.

I have cited the Xerox case frequently, for it is such a clear example of what I believe is a major problem today. I am convinced that there are many "Xeroxes" lying around that, unfortunantely, are not in the hands of the inventor, but are controlled by someone else who lacks either the vision or the appetite to innovate even if the necessary incentives were restored. In this connection it is interesting to note that, when Carleson did finally make a sale of his Xerox process, it was to a company that desperately needed a new product line to save it from bankruptcy. It wasn't one of the successful industrial giants.

And so I believe it is essential that we make provision for the alternative routes through which the inventions, so to speak, can meet up with the appropriate entrepreneur. If we do these things, there is every reason to believe that the new businesses and new jobs that we so sorely need will come.

I would like to close by paraphrasing my earlier quotation from Vice President Humphrey: It is truly the responsibility of the federal government to create the proper conditions in which technical entrepreneurship and innovation can flourish.

THE NATIONAL SCIENCE FOUNDATION: CURRENT TRENDS

Leland J. Haworth, Director, National Science Foundation

In discussing the National Science Foundation, I shall deal especially with the trends in its research support, and the impact of those trends. First I'd like to talk quantitatively about some aspects of these matters.

We might look for a moment at the recent budget history of the Foundation. It's striking that from 1957, just before Sputnik, to 1965—that is, eight steps in the budget—the budget increased by an order of magnitude, from $40 million in 1957 to $420 million in 1965. But there has not been a constant first derivative or even a constant logarithmic derivative; the growth has had its ups and downs. I might give some actual figures for the last few years.

In 1961, the budget was $176 million. The next year it went to $263 million, an increase of 50 per cent; in 1963, it went to $323 million, an increase of 23 per cent. In 1964 it went to $353 million, an increase of only 9 per cent, and in 1965 it went to $420 million, which is an increase of 16 per cent. So you see there has been a series of ups and downs in the relative rate of growth.

If one looks back a little farther, it's interesting to note that there has been about a four-year cycle. For two years the increases would be large, then for two years they would be small. I might illustrate this by saying that from 1957 to 1959, the increase was 235 per cent; from 1959 to 1961, it was only 31 per cent; from 1961 to 1963, it went back up to 85 per cent; and from 1963 to 1965, it went down to 30 per cent.

Thus I have optimistic feelings in terms of this cycle with respect to the next couple of years. The request before the Congress at the present time is for $530 million as compared to $420 million appropriated this year. This would be an increase of about 26 per cent.

Now where does the money go? Because I believe the questions before us are intimately connected with the distribution of funds. At the risk of boring you with more figures, I will speak of what we anticipate will be the distribution in the current year. Now, these numbers are geared to our budget make-up, or format; for historical reasons, it has some peculiarities, so it is a little hard to sort it out very clearly for your purpose, but I will do the best I can.

In the line item that we call basic-research project support, from which comes the great bulk of the support of individual university scientists, we have assigned this year about $122 million, which is about 28 per cent of the total budget. For the item called "National Programs," which include things like the concerted program in the Antarctic, Project Mohole, the Indian Ocean expedition, and various programs of that sort, we expect to spend $42 million, or 10 per cent. Much of this support is also, of course, in the form of grants similar to those in the first item. Incidentally, this year is the peak year in Project Mohole because we expect to make a commitment for the "floating platform."

Nearly $27 million, or 6.5 per cent, will go for the so-called special research facilities, which include many things, ranging from oceanographic research vessels and major equipment such as accelerators down to $20,000-$50,000 equipment items in some cases. About $28 million, or about 7 per cent, is committed to the so-called graduate facilities, which means laboratory buildings built at the universities. The so-called institutional-base grants, which consist of completely flexible money given to the colleges and universities on the basis of a formula applied to the support they get in the regular research grants, will account for about $11 million, or 2.6 per cent.

The national research centers—the Kitt Peak optical observatory; the Green Bank, West Virginia, radio-astronomy observatory, and the Atmospheric Sciences Laboratory at Boulder—add up to about $19 million, or 4.5 per cent.

This year there is a new program called "Science Development," which most of you have heard about and which I will come back to later; it represents an attempt to give to each of a relatively small number of institutions a sizable grant through which it can make a quantum jump, as it were, in some fairly broad aspect of its scientific programs. This program will account for $28 million, or 6.6 per cent. The remainder, including our out-and-out education programs, our science-information programs, our so-called planning function—which includes all the statistical work that is done on behalf of all the agencies and our own internal planning, and a certain amount of analysis that is carried out on behalf of the federal government as a whole—add up to about $135 million, or 32 per cent of the total. And finally, our own administrative costs are about $13.5 million, or about 3 per cent.

The situation has changed during the last several years. There was a time when a much larger proportion of the funds went into the so-called research project grants. But there have been new programs added, including the national programs that are groupings of the same sort of projects, and the support of the national centers, and there has been, perhaps, increased relative emphasis on the educational programs.

To give you an idea of distribution between the various scientific disciplines: In round numbers, in 1964, of the total basic research project grants, 55 per cent of the funds in the item called basic research projects went to mathematics, the physical sciences, and engineering; about 36 per cent went to biology and medicine; about 9 per cent to the social sciences.

Next I might discuss for a moment: How well are we doing in terms of supporting the requests that come in for research support? Here, just because the statistics happen to be put together that way, I will talk only about the basic-research project grants, the item that accounts for about 28 or 30 per cent of the total. I will not include the national centers or the national programs like the Antarctic Program.

In fiscal year 1964, we granted money to almost exactly 50 per cent of the original applicants. However, those people who were given grants received in total only about 44 per cent of the funds that they requested; moreover, the average grant duration was less than requested. The net result is that we are awarding about a quarter of the total dollars that have been requested of us.

However, included in these total statistics for the Foundation are a significant number of withdrawals. These come about for several reasons. One, and perhaps the largest, single reason is that an application was made to more than one agency and another agency gave an award, so the request to us was withdrawn. A second reason is that an applicant may decide after thought that he can make a better proposal; he withdraws the original one and re-submits, and unfortunately the case is counted twice in the statistics. I cannot sort out these effects on the basis of figures I could get in the last few days for the Foundation as a whole, but I can sort them out for the mathematical, physical, and engineering sciences for fiscal year 1964.

Here, about 44 per cent of the proposals resulted in grants, a little less than the average for the Foundation; about 36 per cent were rejected, and about 20 per cent were withdrawn. It would be more revealing if all our statistics eliminated the withdrawals, even though there are very few instances in which a proposal was withdrawn for fear of a denial.

So if I eliminate those that were withdrawn, about 55 per cent of the proposals resulted in grants and 45 per cent resulted in rejection. Turning to the amounts involved, in the same fields, we made grants totaling $64 million, or about 23.5 per cent of the funds requested in all proposals. Proposals totaling $86 million were rejected completely, which is about 32 per cent of the request; proposals totaling $21 million, or about 8 per cent, were withdrawn.

If we look at the acceptances in the same group, $167 million was reqeusted; $64 million was granted, or in other words, there was a reduction of $103 million, or about 62 per cent, of the amount requested even in the grants that were given. Of this reduction, about 40 per cent resulted from the fact that we did not give grants of as long duration as was requested, and about 60 per cent from reductions in the rate of support.

The figures are not quite so discouraging in the biological and social sciences, but they are bad enough.

Now, let us see what these figures mean—how significant they are in terms of our ability to support good proposals. Various division heads in the Foundation estimate that 75 to 90 per cent of the proposals are really good and should be supported, if we had adequate funds—and presumably about the same fraction applies in terms of dollars.

So you see that, whereas 75 to 90 per cent of the proposals are judged to merit support, only 50 per cent are actually being supported, and the funds given are only 25 per cent of those requested. So clearly we do not have adequate funds.

Switching to another point for a moment—there has been a trend downward in the duration of grants that have been given. A few years ago, the average was well above two years; now it is definitely under two years, and, for example, it is down to about 1.7 years in mathematics, the physical sciences, and engineering. It is still about two years in the biological sciences and a bit above two years in the social sciences.

Now this trend downward, of course, has reflected an attempt to answer the leveling off in the budget. Harvey Brooks probably mentioned yesterday the general belief that, in order to hold our own in the universities, we should have an annual increase in total federal support of about 15 per cent. In the last few years it has been definitely less than that—something like 10 per cent. Consequently, in trying to support as many people as possible without reducing the level of individual support too drastically, there has been a tendency for the program directors gradually to shorten the duration of grants in the hope that the shortening can be recouped in some later year when funds are more readily available.

Now, of course, one can do this only so long. As a matter of fact, the effect is worse than is immediately obvious because of what I call "the bunching disaster." Let me vastly oversimplify and exaggerate what I mean by this. If, in a given year, one makes only three-year grants, during the next year only two-year grants, and in a third year only one-year grants, they all come up for renewal in the fourth year and one is in serious trouble. In a far less drastic way, we have been in trouble this year because of this sort of piling up—in some programs more than in others. There is a large number of renewals that we would like to make this year, but we cannot make them all because we simply cannot afford to cut the average duration any farther.

We have given a great deal of thought to what we should do about this, and a great deal of thought has been given, as you know, by the Office of Science and Technology—by Donald Hornig particularly—and by the Bureau of the Budget, to this whole picture. I might talk a little bit about what the plans are, hopefully, for the coming year. I will confine my remarks to what I will call "academic science," that is, the support of science in the universities proper, not counting the contract laboratories outside of the regular academic structure.

As I mentioned a moment ago, it is generally believed that, just to hold our own, support of this sort should increase about 15 per cent per year. The reasoning goes something like this. The growth in student population over the next decade is predicted to average about 10 per cent per year. Now, if only the same proportion of students go on from their bachelor's degrees into graduate school, then, of course, we will soon have a graduate population increasing 10 per cent per year. This means that, unless we are going to have more students per faculty member, we should increase the faculties by 10 per cent per year. If they are to be active, vigorous people, they should do research, and so the research support should increase about 10 per cent per year for this reason alone—just to hold our own.

Furthermore, we all know, of course, that the cost of research per investigator increases each year, quite apart from general inflation. This condition arises from many causes—increased complexity of experiments, more complicated equipment, use of computers, and so forth. It also is related to increases in salaries; this last is not an insignificant factor, because, even though salary scales might stay the same, the scientific fraternity is by and large a young fraternity, and the average age is about at the point of maximum relative increase in salary.

The total effect on research costs is something on the order of 5 to 7 per cent per year. Thus, the 10 per cent increase in university population and the 5 to 7 per cent increase in relative cost of doing research combine to create a need for an annual increase of at least 15 per cent.

In the budget analysis last fall and early winter, this was given a great deal of thought. To oversimplify again, the National Science Foundation budget was held for consideration until the last. A look was then taken at the provisions made in the fiscal year 1966 budgets of the

Department of Defense, the National Institutes of Health, the National Aeronautics and Space Administration, the Atomic Energy Commission, and other agencies for academic research. But their missions are specific and—although they are all very conscientious about their responsibilities to the universities and their own future needs for scientists and engineers—their charters do not, in general, include support of the universities per se. Hence, as their total budgets get tighter there has been a tendency for their support of basic research, and consequently of academic research, to level off even more than ours has.

When the situation was analyzed it was found, again, that the growth in support that could be expected from the totality of all those agencies, save the National Science Foundation, would not amount to the 15 per cent, but to something more like 10 per cent.

By agreement between Dr. Hornig and the Budget Director and myself, it was proposed that the National Science Foundation budget be increased radically more in this category than it had been in recent years, and the President agreed. In the President's budget, the Foundation item covering most of our support of academic research is about 60 per cent higher than that for this year.

This, it seems to me, has two very significant policy implications, as far as the Executive Branch is concerned. The first is of general interest, namely, that there is a policy decision to attempt to increase the support of academic research in the ways that I have described. The second is of great importance to the Foundation and indirectly in many ways to everybody, namely, that the Foundation is now actually to be, in this sense, the balance wheel that it has always been intended to be, but hasn't had enough moment of inertia to really be.

Nobody knows, of course, what will happen in the Congress, but we hope very much that this increase will survive.

Internally in the Foundation, we've been giving a great deal of thought to what we can do about the question of grant duration. I will make two or three points that may or may not have occurred to you.

In the first place, to try suddenly to increase the duration of the grants in some one year would require a substantial increment in obligational authority. It would not cost the federal treasury any additional funds in that year, because when the money is actually spent is what counts from that standpoint, but it would swell the administrative budget that the President places before the Congress.

Another point is that grants of long duration impose a time-lag between any increase in appropriation and the resulting increase in level of effort.

To use a simple case, if there is a constant increment in the budget each year, then, in equilibrium, the level of effort will lag by half a year under a system of two-year grants, by one year for three-year grants, and by a year and a half for four-year grants—or, speaking generally, by $(T-1)/2$ where T is the grant duration in years.

Hence, although we would like to give longer-term assurance in many cases, committing our appropriated funds on longer-term grants would result in a lowering of the level of effort so long as the budget is rising. But we can give legal commitments only to the extent of the actual appropriations. And so, after a great deal of thought, we decided that we will adopt a policy something like the one that the National Institutes of Health has followed, namely, of funding grants for an initial period and making a statement of intent for an additional period.

For example, suppose that on substantive grounds we believe it appropriate to support a given project for five years. Initially we would commit funds, in the full legal sense, at full level for two years, or at full level for the first year, at three-quarters level for the second year, and at half level for the third year, or something of that sort. In addition, we would give our own assurance, which has no legal standing, of course, but constitutes a moral commitment, that if funds were made available we would continue to support the project up to the full five years.

This would mean, of course, that the last three years would have triple-A priority when those years came around. We would also undertake to do the refunding well in advance—say, six months or more. In other words, we are trying hard to give more assurance of continuity to the people working in the universities, without tying up too much obligational authority at any one time, and without requiring, as we say, a quantum jump in an administrative budget.

We believe this will work. It certainly has worked for the National Institutes of Health, which funds only a year at a time, and, as I say, we would undertake to fund initially for a couple of years and then always refund six months or more in advance. The latter, incidently, is better than we are doing now because we don't usually manage to renew grants that easily.

This also would have the advantage that we would not need to go through a full-scale reassessment as often with such longer-range projects as we now do. This would lessen the burden on the scientists who serve on our panels and also cut down on the administrative costs within the Foundation.

Now I don't suppose that initially more than, say, half of our funds would be used in this way, but there are many cases in which it is perfectly obvious that we're going to continue to fund a given man indefinitely, and we ought to find some way to do it rationally.

So much for this type of problem. Let me just mention, briefly, a few of the other problems that we are concerned with, all of which relate to support.

One of the questions that I will mention, but not discuss, is the perennial question of "big" versus "little" science. The Foundation has moved in the direction of supporting more "big" science in the last few years—the three National Centers, Project Mohole, and so forth. We are not in the gigantic accelerator game, as the Atomic Energy Commission is, nor do we have as large-scale projects as does the National Aeronautics and Space Administration, but nevertheless, we do have this problem.

A second problem is the question of flexible money. The research-project grant scheme, of course, has unquestionably accomplished a great deal. It was a great invention. It has given life to American science that probably could not have been given in any other way because, with funds available to support only a fraction of the amount needed, this was unquestionably the best way to be sure that the best scientists and the best ideas were supported.

But it does give rise to some problems that have been discussed many times. One of them is that the institutions are sometimes hard put to provide many things, such as machine shops and computers—in other words, the types of facilities and related activities required to support many pieces of research. In the research project grant mechanism there is no provision for supporting this sort of thing, except through concerted action by many principal investigators, who aren't always completely generous about it, I am told.

So we've been looking for ways to give some flexibility at, say, the department level to take care of this kind of need, by one means or another, including provision of some flexible funds. Whether this will turn out to be separate grants, or freedom to use a small fraction of the regular grant money for other purposes than the direct research, I don't yet know, but certainly we will come to something of that sort.

I won't get into the institutional questions that revolve around the inflexibilities forced on institutions by virtue of this support. I mean the inability to make normal administrative decisions about who works in the summertime and who doesn't, and so on. There are other problems—the overhead, the faculty salary questions, and so forth—but I would like to turn to another one, and that is a question with which we are definitely faced—one that is going to be more and more important. That is—and I say it in quotes, because I don't like the term—"geographical distribution."

I firmly believe that we must increase the number of first-class educational institutions in the country, and we should try to upgrade all such institutions. Of course, the Science Foundation's obligation here is limited to the fields of science. The Science Development Program that I mentioned is one method of trying to help do this. Here the intent is to assist a number of institutions to take some major step or steps forward in a department or a group of related departments.

Institutions making proposals are required to develop plans for improvement. These plans may include new faculty, equipment, facilities, or what have you. Grants will be made to give substantial help in pursuing the plans over the first three years, with the possibility of renewal for two more years, after which the institution is supposed to carry on from normal sources of support. We will give grants of the order of up to five or so million dollars to a selected few of the proposers. This year we expect to give seven to ten such grants from the $28 million available. We have requested $40 million for additional grants next year.

Now to get back to "geographic distribution." It seems reasonable to me to give thought, in making Science Development grants, to the needs of certain regions of the country for better institutions. It also seems reasonable that we try to find mechanisms akin to the research-initiation grants that our own Engineering Division has used, to help people get started on research in places where, for one reason or another, they have not had a chance to make a reputation. In other words, I think we should find ways to upgrade more institutions, and that in doing this we should take account of geography. I don't consider geography the sole, or even the primary, reason, but I believe that, since we do need more good institutions, we should try to distribute assistance in ways that will help all regions of the country develop strong educational institutions.

I'm happy to say that, among the great majority of the members of Congress, it is not a simple pork-barrel instinct that leads them to ask that there be funds for research and development and education in their districts or their states. Rather it is because they recognize the need for strong educational institutions in their regions. And they tend to think regionally rather than by district, recognizing that strong educational institutions in their regions are not only desirable but a necessity. So that both the instinct and the objective are right.

These are some of the problems we face. There has been a trend toward more and more institutional support as compared to project-grants support. As I said, there has been a trend toward support of a bit more big science, although we still don't have very much, but we hope to keep on supporting the individuals and individual research projects. We certainly do not intend in any way to let the good scientists or the leading institutions suffer from our attempts to upgrade others in the various ways I have described.