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ABSTRACT

These hearings, which focused on the goals and objectives of national science policy, include discussions, questions and answers for the record, and, when applicable, prepared statements. Individuals appearing during the hearings include: (1) George C. Pimentel; (2) Alex Roland; (3) John S. Foster, Jr.; (4) James B. Wyngaarden; and (5) Lewis M. Branscomb. Included in an appendix is the report "Research in Prevention, Fiscal Years 1981-83 (1984 Estimated) Budget Information and Program Highlights," United States Department of Health and Human Services, Public Health Service, National Institutes of Health, June 1984. Among the areas and issues explored are: financial support for science; funding of research universities; support for arts and humanities compared to that for science; extent to which goals and objectives of U.S. science policy have changed since 1945; U.S. biomedical research programs; the demand for doctoral level engineers as opposed to the demand for bachelor's and master's degree level engineers; and industry funding of science oriented projects. (JN)

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ERIC

Science Policy Study—Hearings Volume 1
GOALS AND OBJECTIVES OF NATIONAL SCIENCE POLICY

HEARINGS

BEFORE THE

TASK FORCE ON SCIENCE POLICY

OF THE

COMMITTEE ON SCIENCE AND TECHNOLOGY HOUSE OF REPRESENTATIVES

NINETY-NINTH CONGRESS

FIRST SESSION

FEBRUARY 28; MARCH 7, 21, 28; APRIL 4, 1985

[No. 46]

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CONTENTS

WITNESSES

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GOALS AND OBJECTIVES OF NATIONAL SCIENCE POLICY
(With Dr. George C. Pimentel)

THURSDAY, FEBRUARY 28, 1985

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
TASK FORCE ON SCIENCE POLICY,
Washington, DC.

The task force met, pursuant to notice, at 8:40 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. FUQUA. The task force will be in order. Today we begin hearings with outside witnesses regarding their views and ideas on the subject of science policy before the special task force, and we are very pleased this morning, for several reasons, to have Dr. George Pimentel, who is professor of chemistry at the University of California at Berkeley. He is the former Assistant Director of the National Science Foundation. He is currently Chairman of the National Academy of Sciences' Committee to Survey Chemical Sciences, and yesterday he was honored by the President of the United States with a Medal of Science that was presented by the President.

We are very pleased to have you, George. You've been here many times, and you have certainly contributed a great deal to the deliberations of our committee on various subject matters. We are glad to have you today to be our leadoff witness in what we hope will be a constructive study of our science policy of the United States.

[A biographical sketch of Dr. Pimentel follows:]

DR. GEORGE C. PIMENTEL

Professor George Claude Pimentel assumed Directorship of the Laboratory of Chemical Biodynamics, a Division of Lawrence Berkeley Laboratory and an Organized Research Unit of the Chemistry Department, University of California, on July 1, 1980. He came to that post after having served as Deputy Director of the National Science Foundation (NSF) for three years, October 1977 to June 1980. Dr. Pimentel has been a member of the chemistry faculty at the University of California at Berkeley since 1949. He is widely known both for his scientific contributions and also for his excellence in teaching.

Dr. Pimentel's research has been in the fields of infrared spectroscopy, chemical lasers, molecular structure, free radicals, and hydrogen bonding. Dr. Pimentel's interests have centered on the application of spectroscopic methods to the study of unusual chemical bonding. A major contribution was the development and exploitation of the matrix isolation method for the spectroscopic detection of highly unstable molecules This involves stabilization of such molecules in a matrix of frozen inert gas, such as argon, at very low temperature to permit leisurely spectroscopic study.

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Application of this matrix isolation method led to the discovery of many unusual and highly reactive molecules that could not otherwise have been detected.

His pioneering development of rapid scan techniques for infrared spectroscopy extended to the gas phase these spectroscopic studies of normally transient species. This work led to the design of a unique infrared spectrometer for the 1969 Mariner interplanetary spacecraft to determine the composition of the atmosphere of Mars.

During studies of photochemical reactions, Dr. Pimentel and his students discovered the first chemically pumped laser. Flash photolysis methods on the microsecond time scale permitted the measurement, through the laser emissions, of nascent population inversions produced in the normal course of a chemical reaction. Quite a variety of chemically pumped vibrational and rotational lasers have been discovered in his laboratory, providing valuable state-to-state kinetic information.

An enthusiastic teacher, Dr. Pimentel currently lectures in freshman chemistry at Berkeley as he had done for six years before accepting a Presidential appointment as Deputy Director of the National Science Foundation. He is also chairman of the committee appointed by the National Research Council to identify prime areas for research in the chemical sciences. He is coauthor of seven books, four of which are textbooks, and three of which concern areas of his research. He has long been concerned with the quality of teaching in secondary schools and was editor of the CHEM Study project which was devoted to the development of a new high school chemistry textbook. The text, titled Chemistry—An Experimental Science, was published in 1963 and is now used in high schools in every state. More than a million copies have been sold, with all royalties going to the U.S. Treasury, and the text has been translated into 13 languages, including Russian. Dr. Pimentel also has collaborated in the production of several chemistry educational films, incluidng one which concerns the impact of science on the quality of life. In 1958, Dr. Pimentel received the Campus Teaching Award at the University of California on the basis of student nominations and evaluations. In 1971 he received the Manufacturing Chemists Association College Chemistry Teacher Award. His name is listed in Outstanding Educators of America.

Over the years, he has received many honors and awards for this scientific contributions. He was awarded a Guggenheim Fellowship in 1955 and then, in 1957, he received the American Chemistry Society California Section Award. He received the American Chemical Society Precision Scientific Award in 1959. He was elected a member of the National Academy of Sciences in 1966 and two years later he was elected a Fellow of the American Academy of Arts and Sciences. In 1972 he received the Dickerson College Priestly Memorial Award and in 1974 he won the Spectroscopy Society of Pittsburgh Award. Dr. Pimentel was selected to participate in the 1973-74 U.S.-Japan Eminent Scientist Exchange Program and was an Alexander von Humboldt Senior Scientist Awardee in 1974. In 1979 he was awarded the E.K. Plyler Prize in Molecular Spectroscopy and the UCLA Distinguished Alumus Award UCLA. He was selected as the 1980 recipient of the Ellis R. Lippincott Medal and also received the Distinguished Service Gold Medal from the National Science Foundation in 1980. He was selected to receive the 1982 Linus Pauling Medal from the ACS Puget Sound Section and the 1983 Peter Debye Award in Physical Chemistry from the ACS. He also received the 1983 Madison L. Marshall Award from the American Chemical Society's North Alabama Section. He received the Wolf Prize in Chemistry, 1982. In 1985 Dr. Pimentel received the Franklin Medal, the William Proctor Prize, and the National Medal of Science Award, considered the Nation's highest scientific honor.

Born May 2, 1922, in Rolinda, California, Dr. Pimentel received a Bachelor of Arts degree from the University of California at Los Angeles (UCLA) in 1943. After a year on the Manhattan Project at the Berkeley campus and more than two years in the Navy, he returned to Berkeley and completed his graduate work. After earning a Doctor of Philosophy degree in 1949, Dr. Pimentel was appointed a member of the faculty at Berkeley and 10 years later he had attained the rank of professor.

From 1966 to 1968 he served as Chairman of the Chemistry Department. He served on the University of California Select Committee on Education in 1965–66. He was a member of the Lunar and Planetary Missions Board, an advisory unit to the National Aeronatics and Space Administration (NASA), from 1967 to 1970 and a member of the National Academy's Committee on Science and Public Policy from 1975 to 1977. Dr. Pimentel was elected President of the American Chemical Society. He serves as President Elect during 1985, President in 1986 and Past-President in 1987.

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STATEMENT OF DR. GEORGE C. PIMENTEL, PROFESSOR OF CHEMISTRY, UNIVERSITY OF CALIFORNIA AT BERKELEY, BERKELEY, CA

Dr. PIMENTEL. Thank you very much. I certainly appreciate your letting me come today and speak on these very important issues. I think you know, sir, that I consider these issues of extremely great importance. I have spent much time thinking about them.

I would like to begin by commending, if you will let me, the committee for undertaking this systematic study. It is certainly beneficial to articulate clearly and periodically and remind ourselves of why the Federal Government should be investing not inconsiderable sums in the advancement of science.

I have, of course, a background which colors my views, as everyone has, and I will speak about that background so that you can see how my background and biases, if you like, have been developed.

In the first instance, let me speak about where we are now in science. In replying to my own question there, I should remark that I just returned from Britain 2 days ago, where I had the opportunity to speak with some of the science policy leaders of the United Kingdom. In December I had meetings with their counterparts, this time in Stockholm, Sweden, and last year, earlier, in Germany. And I can tell you what I think you already know, that we are, simply stated, the envy of the world in the strength of our science and the strength of our institutions to pursue science.

One of the aspects of our institutions is the pluralism of support, so that we have a variety of agencies here in Washington considering the areas of science most relevant to the particular societal needs that their mission defines. We also have the National Science Foundation with the more general mission of ensuring the health of science across the board.

None of these countries that I mentioned has quite that pluralism of support and, as I say, abroad it is considered to be one of the great advantages we have over them in pursuing science. So I return to my remark that we are the envy of the world in the strength of our science. How did we get here? Now, in this I would like to engage in a bit of reminiscence, if I might, which will explain, in part, the attitudes that I shall be presenting to you.

My experience with science policymaking began by accident just after the end of World War II when, while I was still in uniform in the illustrious command status of Ensign in the Navy, I was sent to Washington, DC, just at the end of the war to work in an office that was headed by Captain Conrad and a civilian physicist named Allan T. Waterman. This office was called the Office of Research and Inventions, and while I was there, its name was changed to the Office of Naval Research.

In a very real sense, I think that that was the beginning of the post-World War II concept of how the United States was going to pursue science. And my view of the philosophy that was put into being there was that we should fund our most creative people to

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pursue their most ambitious and their most adventurous ideas on how to advance human knowledge. Stated negatively, it was, "Fund creative people, but don't tell them what to do."

Another very fundamental concept that more or less had its origins there, I believe, was that we should engage the academic community fully in the research enterprise. And I think as is pointed out in the book, that again is a particular aspect of the support of U.S. science, much more so than others in the more advanced, scientifically advanced countries.

This, I believe, is again one of the aspects of the U.S. scene that gives us enormous strength. Engaging the academic community in the research enterprise links very tightly the advancement of the scientific frontiers to the training and education of the next generation of American scientists. I think this is one of the most impressive aspects of our science establishment and one that we should always value very highly.

Of course, Allan T. Waterman became the first Director of the National Science Foundation and proceeded to put into effect the policies that I have tried to describe. At least that was my view of the policies that were brought into being in the Office of Naval Research—put the money where the people are and fund the person, not the proposal—and that will make an optimum climate for eliciting creativity.

Now I would like to turn to the several questions that have been posed and rather briefly provide you with some views of my own about what might be the kinds of answers that you would be finding and, with your permission to restate some of the questions, perhaps, to perhaps point to answers and new directions.

The first question I find is: What are we as a nation aiming for in providing support for science? My answer to that, in what you refer to as the most general terms, is to assure societal access to the benefits that inevitably flow from a better understanding of ourselves and the world around us.

I think that we can regard that as a sufficient justification for pursuing science, and with particular emphasis on the fact that societal benefits flow, we can see the support of science as similar, but much more crucial perhaps, to the support of cultural activities and the arts. I certainly, as much as anyone, regard the advance of understanding of ourselves and the environment as one of the important parts of our cultural ethos, and science, I believe, deserves to be supported only with that as a justification.

However, I believe that the size of the investment that is made in this country's future through its support of science should be very much larger than might be justified on the cultural benefits alone, and that's because we have ample evidence at hand, and it's easy to project into the future, that societal needs are answered by drawing on the reservoir of knowledge that's been accumulated in fundamental research over the decades. So science and technology are the hallmarks of our time, and assuring that our society has full access to the benefits which will flow from such activities fully justifies, I believe, the support of science.

The second and the third questions refer to goals for science and how they relate to our other national needs. Are the goals for science internally consistent? I am not exactly sure what is meant by this reference to goals other than what I have indicated; that is to say, the expansion of human knowledge with particular emphasis on those areas of human knowledge that might lead to a response in the future to human and societal needs that could not be anticipated today.

The words "national goals," again expressed in number four, makes me just a little bit nervous. And I have underscored the language in this brochure the expression, "Goals should avoid a high level of generality, which is easier to develop, but less useful as a guide." I think I understand what's meant by that. It's one thing to say, "Well, we're just advancing frontiers," but it's another thing to understand what that means as one tries to make decisions about the level of support and distribution of support among activities.

Nevertheless, I would like to add at least to that statement that, "Goals should avoid a high level of generality," the converse: I think our goals should avoid a high degree of specificity that might constrain or limit creative advances and adventurous challenges to existing dogmas. I do believe that we're trying to advance frontiers, and, by definition, this means moving into areas where we're not exactly sure either what we will find or what will be the outcome, and what we want to be careful about is to avoid being so specific in defining our goals that we, in essence, restrain ourselves to stay within existing bounds.

And then the last two questions I find: "Have our goals changed," and, "To what extent must changes now be made," I do believe that changes continually ought to be brought into consideration.

I find myself asking the question, "Do we have big problems that are connected with changing goals and insufficient responsiveness to the needs of today, they being different from the needs of yesterday?" I would have to answer, "No," if the question is phrased the way I phrased it, "Are there big changes needed?" Because of the word "big," I don't believe that big changes are needed.

I would assert that we need to bring into the funding equation somewhat more explicitly than we might have in the past the question of the resources needed in a given area in contrast to the probable societal benefit. But as I say that, of course, I want to say again what I just said earlier: without being so specific that we constrain ourselves.

We need to pose perhaps more explicitly and clearly the question of resources needed in a given area to maintain its health and vitality versus the manpower needs that we find society is expressing. That is to say, one of the most important activities of a research activity, particularly of course in the universities, is to prepare the cadre of scientists, young scientists, who will advance our science and technology in the future. And, as we see them, research in universities as a contribution to our manpower pool, we should have in mind those areas of science where the manpower are going to be most needed.

But once again I would admonish us not to feel that we can predict the future so precisely that we would impose on ourselves bounds that later we might find inappropriate.

But summing all of it up, I am brought back to the beginning, where I would reiterate that we have a magnificent science and technology position in the world, and I guess I would sum up with the old expression that, "When it works, don't fix it."

Thank you very much, Mr. Chairman.

DISCUSSION

Mr. FUQUA. Thank you very much, George.

Without using too many generalities, but more specifically, you did touch on where goals have changed. How would you evaluate our ability to attain those goals, including, as you say, "If it ain't broke, don't fix it?" And of course, it's not our purpose to be critical of science policy. We're trying to see if we can make it work better.

Do you have any suggestions for us as to what you think would be beneficial in trying to achieve our goals, whatever they are—and maybe they should be somewhat better defined? But has policy changed over the years? Certainly, the world has changed rather dramatically in the last 40 years, or better than that. Do we have a mechanism that can cope with that as we look 40 years down the road rather than back 40 years?

Dr. PIMENTEL. Well, I think we do have the mechanisms, and I think it's primarily a matter of being sure that we put them to work. I mentioned already the benefits the U.S. society has from the plurality of support areas, and that means that we have a series of Federal agencies, each of which is encouraged and, I believe, obliged by law to advance science in areas that it perceives to be important to the accomplishment of its mission.

I think one of the ways in which to get the full benefit of this—and I think this is responsive to your question—is continually to remind mission leaders, mission agency leaders, and to ask them to restate the way they see their mission and how their existing programs are seen to be working toward that end.

I think that the more or less continuous review of how the leaders of each mission agency see their mission and how they can defend and argue that their program is moving toward that end is itself a significant answer that permits a wider access to the rationale for the program and opportunity for committees like this to carry out their responsibility of seeing to it that the interests of the United States are well pursued.

Now, it may be that—and this might seem repetitious and a little tiresome; I don't think it is—I think it is quite appropriate right now to have these various agencies continually reminding themselves and all of us how they're moving toward their mission.

Mr. FUQUA. You mentioned that you just returned from Great Britain, and that is a country that has spent for many, many years a considerable amount of sums for basic research. Yet we find their economy has not responded likewise.

Take a country like Japan, which has not funded a great deal of money in basic research. Yet their economy has been booming. Is there a correlation in that? I know you're a chemist and not an economist, but is there a correlation between that and what we envision the role is in this country? It appears to me that if we are strong in basic research and we provide the foundation for economic advancement, and continue to be competitive in the world marketplace, there we have two examples where just the opposite has occurred.

Dr. PIMENTEL. Yes. I know that you pose that question. I am aware of the social context of the development for the last couple of decades that are surely extremely influential. To be specific about that and not feeling that I am telling you anything at all that you are not already aware of, the economic context of the situation Britain finds itself in has to do with social developments that are, so to speak, coming home to roost and they're having great difficulties in determining how to restore their economic strength that the country used to have. So certainly there are societal developments there that transcend the institutional mechanisms by which they pursue science.

The Japanese situation, I think, is a little more interesting in a certain sense, in that we have seen Japanese science and technology over the last two decades emerge in quite a remarkable fashion.

The one factor I think somewhat disarms your remarks about the lesser investment that is being made there is the simple fact that the Japanese economy does not make heavy investments in defense programs—of course, that being something that we ourselves are responsible for. And this has very important implications, I think, for their ability to use much smaller sums on a smaller scale than either we or, for instance, the United Kingdom, and make very significant advances in their scientific posture.

I apologize as a chemist and a person who is not qualified in these areas to be answering your question with answers that lie in economics and social structure, but I must admit that that's where I feel the major explanation is to be found.

Mr. FUQUA. The Chairman of the National Academy of Space Science Board recently noted that there are no scientific criteria we can develop for science policy as a whole. I quote, he said, "We are experts in setting priorities within any one field of science. The astronomer, for example, finds it difficult to judge impartially the value of research in the life sciences. The ultimate judgment about priorities are made adequately by the present method of relying on a complex democratic process to make essentially political decisions." What is your opinion about scientists and politicians making decisions about scientific priorities?

Dr. PIMENTEL. In the first instance, my experience at the National Science Foundation—1977 to 1980 was the period I was there—is more or less consistent with the first statement that you read. It did seem that we had advisory committees within the various subdisciplines that were extremely effective in facing these very difficult issues of relative importance within that subdiscipline—very difficult decisions, but nevertheless a sufficient mutual understanding of relative importance, relative potentiality, to be able to fight out those hard decisions.

What I found most difficult, and perhaps most lacking, was any willingness, readiness, or ability to make that kind of contrast between different areas of science. I think it's very important, but one of the more difficult things we do.

I tried to look for indices that might be used to try to make such difficult decisions, and in fact they were more or less implicit in what I indicated were the rather modest needs for change in our techniques. They're not that earthshaking, but I think they're real, tangible, and useful. One of them is to look around us and see—we have the data at hand as to where scientists end up: in industry, in Federal Government, in State government, in education—how many people are needed to keep our industries healthy and going, of each of the various subdisciplines, and have that in mind as one of the criteria by which we make our policy decisions about the relative importance of funding one area compared to another.

And then the other part of the answer would be what I have said before, that asking each of the mission agency scientific policymakers to remind us of how the existing research program is seen to be directed toward the goals of that agency, I think again is a very effective way of making sure that the agencies' policy decisions are considering appropriately the long-range mission of the agency—again without being too constrictive, but making sure that that's foremost among the considerations.

I think those are useful ways to keep this question in the open and properly addressed.

Mr. FUQUA. Thank you.

Before I recognize Mr. Lujan, I notice we have in the audience Dr. Fred Seitz, who is former president of Rockefeller University, a member of the National Academy of Sciences, and a very distinguished scientist in his own right.

We are very pleased to have you here.

Mr. Lujan.

Mr. LUJAN. Thank you, Mr. Chairman.

Congratulations to you on your award.

I do have a few questions of Dr. Pimentel.

The subject you were discussing with the chairman bothers me somewhat, the statement of let the democratic process determine the science policy, and let the political process finally emerge. I don't feel comfortable with that. There are some areas, of course, that I have some knowledge about and have maybe some opportunity to help in forming policy, but in general, in talking about computers and medicine and those kinds of research programs—there I feel totally inadequate.

I tell you how I make my decisions. When somebody talks to me about it, explains the program, and if it happens to be a favorite of mine—frankly if it has some down-home advantage to it—those are the kinds of things that help me make a decision. And I am just not comfortable with letting the political process set the priorities.

With that, you say that we should not be too specific. I am wondering what you mean. Let me give you some examples. Do you think statements such as, "encourage the use of robotics"—I am just reading some—"man's presence in space; smaller fission and fusion machines"—which happens to be one of my soapbox themes—"attract international cooperation; consider the payoff"—are those too restrictive or too specific? Do you think those are too specific or wide enough to set the policy and to let people within the scientific community work on their own projects as long as they are pointed in those directions, those examples that I just gave you?

Dr. PIMENTEL. Well, let me say I didn't find any of the examples that you read excessively constrictive if interpreted with some flexibility. I guess I would want to distinguish at this point between the support of fundamental research, however one wishes to make the definition of fundamental research, and applied research, and make sure that in our zeal and understandable interest in promoting societal benefit, that we don't lose sight of the long-range aspects of research while we're trying to get short-range benefits.

To make this point—you mentioned robotics, and of course, one can approach the subject of robotics at a variety of levels—I think we can anticipate quite clearly that robotics has a very important role in our technological future, and regarding this as an area of appropriate significant investment seems to me quite wise, and then I would add, of course, viewed with a great deal of flexibility and breadth to make sure that we are not only thinking about the particular assembly line that might immediately benefit from bringing in robotics.

I can remember 5 years ago being confronted by Senator Proxmire on one of our grant proposals at the National Science Foundation that was built, or designed, I guess, to get a large object to walk. And Senator Proxmire wanted to know what this object might actually do. And the principal investigator, thinking hard to try to say something that could explain why he wanted a large object to be able to walk, indicated that it might be good to move across tundra.

Senator Proxmire indicated there's no tundra in Wisconsin and he knew of no need for this object, and consequently he found it not necessarily a worthy investment.

My feeling is—without arguing for this particular project—that he was using too narrow a definition of how this particular scientific exploration might lead to understandings that later would be beneficial. And so I guess again I will say your list, I found worthy, and with proper flexible interpretation, not one that I would consider too constrained.

Mr. LUJAN. I guess you know, on reflection, all of those five lead to payoffs somehow. Maybe that's my big interest in this whole thing. One of the statements you made, differentiation between the short-term and the long-term goals and assigning maybe heavier emphasis on the short-term gains rather than long-term gains, whichever you see as the most productive.

Dr. PIMENTEL. You see, I would argue that the different agencies would take a different, and each one an appropriate, let us say, balanced view between short-range opportunities and longer range opportunities. I see the National Science Foundation as the agency, the institution that we have set up to make sure that we have some people pursuing advanced knowledge more or less unfettered by the need to justify in practical terms the outcome, and in contrast to that, other agencies like the National Institutes of Health and Department of Energy and Department of Defense I think also should be supporting fundamental research but obviously they must as well engage in what I will call somewhat more applied research and actual development work in the accomplishment of their mission. So a single unilateral definition probably is not warranted, but each agency should justify its own program in this sense.

Mr. LUJAN. Thank you.

Thank you, Mr. Chairman.

Mr. FUQUA. Mr. Walker.

Mr. WALKER. Thank you, Mr. Chairman.

If you will forgive me for just a second, I am going to raise something that I think is a problem that's developing in science policy. And I need just a couple of seconds here to frame it. It goes back to the questions that the chairman asked you and, to some extent, what Mr. Lujan asked you. The more we get the Federal Government involved in the issue of funding science, the more you end up with political decisionmaking. Is there not a danger that that becomes then a real problem in science, because politicians are ultimately going to act like politicians, we ultimately are going to define things in terms of our constituency? We ultimately are going to define science in terms of things that we understand and the things that we don't understand we are ultimately going to dismiss as being maybe even silly, and that tends to add a bureaucratic element.

Even more disturbing is the fact that we tend to begin to try to get around scientific processes; for instance, like peer review. We're already beginning to see a trend develop in Congress where you sidestep the whole peer review process and fund projects that have a political appeal. Often, the political appeal is who has the power to get the money at any given time, and that causes a concern.

And then the Federal Government tends to set up things which become increasingly bureaucratized; for example, the national labs, which have done some fantastic work. But the fact is that once you bring somebody into the national lab system and he or she is there for maybe 10 or 12 years doing a project, they are then there long enough that they look at the 20-year point when they could retire from the Federal Government and so they stick around for maybe 8 more years when they're not really doing much more than defending that which they did early in their career. And so we lose a lot of the innovativeness as a result of that bureaucratic structure.

Now, you know, I see the Federal Government involved and that having some dangers of that type that really impact on science policy then in the future. And I would just appreciate your comments.

Dr. PIMENTEL. All right. I will begin by saying that very much the concern you have just expressed I would endorse and agree with. The business of sidestepping the peer review process and letting the political process, with the inevitable appeal to particular constituencies, invade our science policy decisions, I think is very dangerous and definitely to be avoided.

In a way, your comments, I hope, will be kept in mind throughout the discussions that you have about the questions that have been posed in this book. And in a certain sense I feel the kinds of remarks that you've just made are one of the justifications for the whole study, because I indicated at the beginning that you are investigating very important questions and it's healthy to consider and then articulate clearly not only why the Federal Government is engaged in the support of science but how it should be engaged in the support of science. And I think clear-cut statements about the possible pitfalls is just as important an element of the outcome of your deliberations as the implications of, say, the policies that should be modified somewhat.

So ultimately, I would say that the outcome of a study could have a very beneficial impact in avoiding the pitfalls that you're talking about, and that's where I believe and have confidence in the vision and wisdom of you people in coming up with a final articulation that will be a healthy one and that will benefit us throughout the next few decades.

Mr. WALKER. Thank you, Mr. Chairman.

Mr. FUQUA. Doctor, I might point out to the other members that we do have another hearing starting in here at 9:30.

Mr. PACKARD. Thank you, Mr. Chairman.

There are several questions that could be asked, Mr. Chairman, but I will only ask one.

In a very practical sense, Doctor, we're faced with budget constraints more and more at this time and we're being called upon to fund education at the entry level and make equal access to the opportunity for higher education through student loans and grants at the same time we're asked to fund scientific and technological research and studies.

With the constraints on the budget, where do you believe would be the better place for our tax dollars to be focused: at the entry level, or at the research level where we have already proven and educated people?

Dr. PIMENTEL. May I ask, do I understand the entry level to be, let us say, precollege?

Mr. PACKARD. No; I am talking about primarily opportunity for higher education after high school.

Dr. PIMENTEL. I see. Well, if I understand the distinctions that you made there, I think there is no doubt that one wants to focus the resources on the research level and that, of course, implies at the graduate educational level. But focus does not imply, of course, that there is no attention given to the other area. What one wants to do is make a wise decision or, in any event, to charge the appropriate agency heads to make their best decision about the appropriate level to fund the entry-level aspect of education.

The one aspect of this that must be kept in mind is that we won't have graduate students, we won't have graduate students either of the ability level or the state of preparation unless we have a healthy entry level educational system, so we cannot neglect it and we cannot put it aside. Certainly that is consistent with the earlier remark that you focused on the graduate level and research aspect.

Mr. FUQUA. Mr. Lewis.

Mr. LEWIS. I have no questions.

Mr. FUQUA. Mr. Boehlert.

Mr. BOEHLERT. No questions.

Mr. FUQUA. George, thank you very much for being here today. You have contributed a great deal not only because of your background as a distinguished scientist but also as an administrator of science programs in the National Science Foundation. You have been very, very helpful.

Thank you very much.

Dr. PIMENTEL. Thank you.

Mr. FUQUA. We will meet again next Thursday morning, same time, same place.

[Whereupon, at 9:20 a.m., the task force recessed, to reconvene the following Thursday, March 7, 1985, at 8:30 a.m.]

[Answers to questions asked of Dr. Pimentel follow:]

Replies to Questions for the Record
Professor George C. Pimentel

1. In your view, should one of the goals of government science policy be to achieve and maintain, as a matter of national prestige, U.S. leadership across the spectrum of science, or should we share or yield leadership in some areas of science to other countries?

(ans.) 1. National prestige should not be an overt goal of government science policy. Such a motivation could cause us to distribute our national research investment unwisely and inefficiently. Thus, national prestige might lead us to build a huge facility (e.g., a planetary space probe, a large accelerator, a large telescope) when international collaboration is more appropriate because the number of scientific questions likely to be addressed does not warrant costly redundancy.

To the contrary, international scientific cooperation in the most fundamental areas of science speeds the advancement of knowledge and makes it more efficient. Then, in the course of this cooperation, there is little room for doubt that the contributions of U.S. scientists will indeed sustain our national prestige.

Two caveats are appropriate. First, we should not contemplate deliberate yielding of leadership in any frontier area of science that is rich in promise for fundamental advances and potential for application to societal need. Second, we must recognize that the desire to maintain economic competitiveness does provide one basis for U.S. support of scientific activity where U.S. leadership is sought. It is entirely sensible that one factor in the decision process by which research resources are distributed should be the expectation that our economic competitiveness and societal well-being will be enhanced by increased knowledge in particular areas.

Industrial employment of scientists is another factor to be weighed as we try to link federal support of science to increased economic competitiveness. We must try to attract talented young people into those scientific fields needed by industry to furnish the scientific manpower with the requisite fundamental background and interests.

2. It is well recognized that the potential payoff in medicine or technology from an individual research project can not be predicted. However, we also know that broad fields, such as chemistry, yield significant practical benefits. To what extent can and should the expectations of such payoff be used to determine the levels of funding for science and for the individual disciplines?

(ans.) 2. It is surely not possible to predict that a particular scientific project will ultimately have a technological payoff. What can be predicted with confidence, however, is that advance of the frontiers in certain scientific fields will surely have this outcome because the field plainly relates to societal needs. Then it is only sensible to allocate enough resources and a high enough priority to such a field to be sure we capture the long-range benefits to be won.

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3. In discussions of the government science budget, much stress has been placed on providing new funds for new initiatives in emerging areas of scientific promise. Why should we not expect a comparable group of areas within each discipline which have "peaked" or been "mined out" and where consequently some funding decreases can be made?

(ans.) 3. I can speak particularly about my own field, chemistry. In this field, areas that have been "mined out" are rapidly put aside voluntarily by the active research community and forcefully reduced by the peer review process. As is characteristic of "small science" there is neither a large capital investment nor vested institutional interest in maintaining an activity as its productivity wanes. Instead, there is strong peer pressure urging movement into new and promising opportunity areas.

4. The current Administration has shifted the principal rationale for government funding of research. Instead of emphasizing the technological pay-off, the stress has been on the training of a new generation of scientists as the principal benefit yielded by research grants. In your view, how many scientists do we need in the coming decades and to what extent will the current levels of research funding meet that need?

(ans.) 4. A crucial goal of federal support for fundamental research in Universities should be the attraction of talented young people into those fields needed by U.S. industries to maintain our technological leadership in the world scene. Such young people can bring to our industries first-hand knowledge of the active research frontiers and first-hand experience with state-of-the-art techniques. Whatever the federal investment in fundamental research, its distribution among the disciplines should be consciously aware of the current industrial employment of scientists from those disciplines. Chemistry provides an example that shows that this is not the case at this time. U.S. business and industry employs more doctoral chemists than the sum of those employed with doctorates in the biological sciences, mathematics, physics and astronomy combined but federal research support for chemistry is only a small fraction of that for the other disciplines.

5. To what extent is government support of science comparable to government support of the arts and the humanities? Is there a "need" in our society for the kind of science that satisfies public cultural demand and can this serve to suggest the level of funding for science?

(ans.) 5. Our cultural ethos warrants federal support of research that carries deep philosophical significance and without immediate regard for likely practical outcome. In that sense, some government support of science should have the same cultural origin as government support of the arts and humanities. It would be unrealistic, however, to expect that the large sums presently directed toward scientific research could be sustained without reference to the fact that our standard of living and technological strength are derived from such activity. Our task is to assure that those sums are sensibly distributed among those disciplines that can influence our societal well-being and economic competitiveness.

6. Most studies of science and most agency budgets for science are future oriented. They speak of future opportunities, future projects, and future results. Retrospective discussions are limited to anecdotal cases of successes, while little has been done to look carefully at entire programs and the ratio of those which lead to technological successes and those which do not, however measured. Why should not more such comprehensive evaluations of past programs be done?

(ans.) 6. Research activities appropriate for federal support should have long-range significance and feature fundamental investigations that industry is not likely to undertake because the payoff horizon is too distant. Such activities are intrinsically high-risk and their practical importance is difficult to perceive quantitatively, and evaluation programs have difficulty finding real measures of success. Hence I am not optimistic that we would gain useful guidance from more evaluations relative to what common sense and objective judgment already provide.

7. As you look beyond the current studies and science budgets for the next few years, what changes or adjustments in our goals and objectives do you foresee for the decades after year 2000?

(ans.) 7. We should direct a larger fraction of the federal R and D investment into the R end of the spectrum, the most appropriate place for federal activity. We must find more reliable criteria for deciding upon the distribution of federal support among the disciplines. Without converting to emphasis on short range, sure-thing and "better mousetrap" projects, we should place more emphasis on areas that undergird our technological industries and that respond to society's needs.

8. In view of the many problems and difficulties which are facing the universities, how do you view the longer term future of the nation's research universities?

(ans.) 8. The U.S. dependence upon its Universities as prime sources of fundamental research is one of our major advantages over our competitors abroad because it couples the research function with preparation of the next generation of scientists. Both functions benefit enormously from the coupling. In the national interest, the country's research Universities should be kept active and healthy.

9. With the fluctuations in enrollment and the resulting limits on faculty hiring, should alternative institutional mechanisms for research be sought to supplement the universities as performers of research, or should the number of research universities be contracted or expanded?

(ans.) 9. Even the most richly supported research institutes abroad are constantly struggling to find ways to avoid stagnation and to maintain the vitality that is constantly injected into our University research laboratories by the presence of bright young graduate students and reinforced by the teach-

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ing function. As to limits on faculty hiring, we have almost passed through the worst of the age uniformity generated by the rapid University growth during the 1950s. Retirements are beginning and University hiring practices are now more attentive to age distribution. No major changes are needed with respect to the research institutions we now have except to ensure that their research capacity is being well utilized and adequately supported.

10. Overhead or indirect costs paid on research grants have generally been justified as needed to pay for the costs associated with the performance of research, but they have generally been limited to current operating costs. In your view, should indirect costs be broadened to recover, as well, the capital costs and other non-operating costs of the universities?

(ans.) 10. In the national interest, the federal government must be concerned with the health of the University research enterprise. Hence, it should seek an appropriate participatory role in the provision of the University's capital and building needs ("bricks and mortar"). An appropriate way to determine the distribution of such resources would be to tie its amount to the total federal research support competitively won at a given institution.

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GOALS AND OBJECTIVES OF NATIONAL SCIENCE POLICY

(With Dr. Alex Roland)

THURSDAY, MARCH 7, 1985

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
TASK FORCE ON SCIENCE POLICY,
Washington, DC.

The task force met, pursuant to notice, at 8:30 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. FUQUA. The task force will be in order.

This morning we continue the review of science policy. We are very pleased this morning to have Dr. Alex Roland, associate professor of history at Duke University, where he teaches military history and the history of technology. His research and teaching interests include the history of science and technology in the West, 20th-century technology and science policy in the United States, and the history of aeronautics and space flight. His most recent book is *Model Research: The National Advisory Committee for Aeronautics, 1915–1958,* written while he was an historian with NASA.

Dr. Roland, we are very pleased to have you with us today. You may proceed.

[A biographical sketch of Dr. Roland follows:]

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ALEX ROLAND

November 1984

PERSONAL

Born 7 April 1944; Providence, Rhode Island

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EDUCATION

PhD, Duke University, Military History, 1974
MA, American History, University of Hawaii, 1970
BS, U.S. Naval Academy, 1966

EMPLOYMENT

1981-present: Associate Professor of History, Duke University
1973-1981: Historian, National Aeronautics and Space Administration
1966-1970: U.S. Marine Corps; captain at time of leaving the service

GRANTS AND FELLOWSHIPS

Woodrow Wilson Dissertation Year Fellowship, 1972-197[illegible]
Duke University, Trinity College of Arts and Sciences, course development grant, 1983
Duke University Research Council Summer Fellowship, 1984

PROFESSIONAL SERVICE

Society for the History of Technology
    SECRETARY (1984-)
    Committee on Research (1979-1984)
        CHAIRMAN (1979-1984)
Aerospace Historian, Editorial Advisory Board (1981-)
Isis, Editorial Advisory Board (1982-)
NASA History Advisory Committee (1983-)
Naval Undersea Warfare Museum Foundation
    VICE PRESIDENT (1984-)

UNIVERSITY SERVICE

Program in Science, Technology, and Human Values
    Steering Committee (1981-)
    DIRECTOR (1984-)
Undergraduate Faculty Council of Arts and Sciences (1982-83, 1984-85)
    Curriculum Committee (1982-)
        CHAIRMAN (1982-1984)
    Executive Committee (1984-1985)
Academic Council (1984-85)
Triangle Universities Security Seminar (1981-)
    Steering Committee (1983-)
Technology and the Liberal Arts Program
    Steering Committee (1984-)
Officer Education Committee (1981-)
Student Symposium Committee, 1985
    Faculty co-advisor
Department of History, Honors Committee (1983-1984)
    CHAIRMAN (1983-1984)

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STATEMENT OF DR. ALEX ROLAND, ASSOCIATE PROFESSOR OF HISTORY, DUKE UNIVERSITY, DURHAM, NC

Dr. ROLAND. Thank you very much, Mr. Chairman.

I am pleased and honored to have this opportunity to share with you one historian's perspective on the timely and important task you have set for yourself. For reasons that I will discuss in my concluding remarks, I believe that this kind of activity is indispensable to a healthy relationship between science and the Federal Government.

My belief that I might have something to contribute to your deliberations is based on my research and teaching. My basic proposition is that there are at least three kinds of science that need to be considered and at least three different rationales for the Federal Government to support them.

I would like to begin by differentiating among those three kinds of science: physical sciences, life sciences, and social sciences. Their histories are quite different and they may well require different science policies.

The world is still in the throes of an enthusiasm for the physical sciences that dates to the Newtonian revolution. By reducing the movements of the planets and the apocryphal apple to a single, simple equation, Newton stimulated a faith in man's ability to understand nature, which was probably a good thing, and a belief in quantification, which has been a mixed blessing. The physical sciences have advanced most dramatically since Newton, in part because they are more readily quantifiable and in part because we have viewed the inorganic as more manipulable than the organic.

Outside of medicine, the life sciences have focused until recently on taxonomy, which increases understanding without necessarily making comparable increases in our ability to manipulate nature. Genetics now shows signs of effecting a revolution in the Newtonian sense, but it is too early to tell. The social sciences have trailed far behind, counting aggregate data and proving singularly unable to identify any laws of nature. I propose to address the physical sciences and life sciences during most of my remarks, turning to the social sciences only at the end.

I draw this distinction because it bears on the committee's decision to concentrate on science policy to the exclusion of technology policy. Most research in the physical sciences, and an increasing amount in the life sciences, is inextricably intertwined with technology. Either it has technological applications or its experimental pursuit requires technology. The physical sciences employ increasingly sophisticated technology, the development of which occupies a significiant proportion of our research and development; the life sciences are moving in the same direction. Particle accelerators and Viking spacecraft for the Mars landing are as complicated as the scientific research they support, making the separation of science and technology virtually impossible in the modern world.

It has become a commonplace in the last 100 years to view technology as applied science, by which we usually mean applied physical science. Throughout most of human history, however, technology has led science; that is, we have learned how to manipulate nature long before we understood why it operated the way it did.

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Man was making steel, for example, two millenniums before he understood its chemical and molecular composition. What is more, technology has very often stimulated scientific investigation, as Watt's steam engine prompted Sadi Carnot to develop thermodynamics. Even today, much of our technology precedes and stimulates scientific discovery, current research on memory storage and retrieval for computers outstrips our understanding of the molecular physics at work.

This is not to deny that, especially in the last 100 years, much of our technology has become science based. Increased understanding of the laws of nature has led to practical applications, a fact reflected in the large science component in modern engineering education.

Understanding why something works is indeed preferable to simply knowing that it does work, but understanding that something is true—for example, that hydrogen is theoretically the most efficient combustion fuel—may leave us a long way from a practical hydrogen engine. The most important distinction to be drawn is whether to consider science as an end unto itself, or to see it as a means to an end.

In the United States, the Government has historically seen science as a means to an end, and this has dictated the nature of Federal support. Science in the colonies and the early republic was largely a private enterprise, funded by amateurs of independent means like Benjamin Franklin or by universities and foundations. The Government provided support mostly for agriculture and military applications, clearly expecting some practical return on its investment, usually some technological application.

At the turn of the century, the Government began to increase its support of science, creating such agencies as the National Bureau of Standards and the National Advisory Committee for Aeronautics. But again, it was for practical applications, usually technology, not for basic science. Most research was done in-house, on the model of military arsenals and agricultural research stations.

Our modern view of the relation of government to science formed in World War II, to be refined in the succeeding years of cold war. Vannevar Bush and his colleagues in the wartime Office of Scientific Research and Development were chiefly responsible. For better or for worse, Bush's "Science—the Endless Frontier" has shaped our science policy for the last four decades. Several basic tenets of "Science—the Endless Frontier" warrant special attention. Most are explicit in the report; one is implicit, but nonetheless decisive.

First, Bush argued that what he called scientific research was indispensable to the military and economic security of the United States.

Second, the Government should fund this research on a continuing and substantial basis, in contrast with the irregular and inadequate patterns of the past.

Third, scientists should have control over how these funds were distributed, to ensure that the best science was supported as it had been by OSRD during the war.

Bush was not, however, asking for free access to the Treasury; funds expended in this way would represent only a small proportion of those spent on research and development through the mission agencies of the executive branch, as in fact OSRD had accounted for only a part of the military R&D in World War II.

Fourth, the scientists should remain in their home institutions, primarily universities, and not be drawn into Government laboratories, with their inevitable emphasis on applications and politics.

Fifth, scientists should participate with Government leaders and industrialists in shaping R&D policy.

The unspoken assumption behind these recommendations was that scientists understand nature's laws better than anyone else; they are in a better position to see the potential applications of their understanding. It behooved the Government, Bush believed, to support the expansion of that understanding, to create a reservoir of people capable of seeing how that understanding might be reduced to practical applications. Let the scientists themselves pursue their own agenda, identify and conduct the best science as an end in itself. In an emergency, the country could draw upon this reservoir of talent, even as it had gathered the country's nuclear physicists together in the Manhattan Project.

Thus, even Bush's formula, though it called for scientific autonomy, based its argument on the traditional American belief that pure research will lead in the end to practical applications, that science leads ultimately to technology. Of course, Bush was referring principally to the physical sciences; he made special provision for medicine, which has comparable applications, but otherwise slighted the remaining life sciences.

But if Bush's assertion were true, that basic research leads to technological applications, it would create a serious contradition. Scientists conducting truly basic research rely on publication. Their only reward system is recognition by their colleagues, and this recognition goes to first publication. Why should the Federal Government support basic research if it is going to give away the results, if it is not going to get a monopoly on the applications? Why pursue basic research at all if the results are free for the taking in the international marketplace of scientific literature? Why not support only applied research?

Bush would have argued that basic research creates that reservoir of people best able to apply their own results. But this only begs the question why other scientists, working on applications, cannot understand the theoretical literature.

This has not become a fundamental problem since the Bush formula was proposed because the Cold War has provided yet another premium for basic research, one with applications entirely different from those envisioned by Bush.

At least since Sputnik, we have come to believe that scientific achievement, independent of material application, contributes to national security. Scientific eminence lends prestige, and prestige weighs in the calculus of the social, ideological, and political competition between East and West. We go literally to the ends of the solar system and the bowels of the atom, not just in search of understanding, but in large measure to demonstrate our scientific virtuosity.

Each year we quickly convert the Nobel Prizes into a box score of which nation is doing best. And we realize practical returns on our life sciences that were impossible when the principal criterion was technological application. Basic research has become a tool not only for manipulating nature, but for influencing public opinion—yet another practical application in the American tradition.

Bush and the cold warriors who followed him have both then argued for Federal support of science, one on the basis of the traditional view of practical applications, the other in the belief that national prestige contributes to national security.

From an historian's perspective, I would argue yet a third, one that has been lurking beneath the surface throughout our national history but far too seldom advanced. We should patronize basic science on a substantial, nontrivial, and continuing level, not only in expectation of some measurable return on investment but simply because understanding is a fundamental human activity, the support of which becomes a great nation such as ours. Just as we support, however modestly, the humanities and the arts, so too in principle should we support the investigation of nature as an end in itself.

No one can foresee the practical advantage in knowing how the universe was formed or why the dinosaurs disappeared or why grass is green, but we will be a poorer society if we stop asking, if we continue to demand of our science only that it make us richer and safer. For this part of the scientific agenda, that is, for truly basic research, I agree with Bush that the scientists themselves are the best judges of how and where to dispense what the Government can afford.

All the rest of scientific activity is more or less directed or applied, that is, it is conducted on the assumption that increased understanding will eventually serve some direct, utilitarian end. Here I harbor some strong reservations about the Bush formula. First, except in that peculiar realm of truly pure research, I do not believe that scientists should have complete autonomy, any more than any other group in our society should set its own agenda for dispensing public funds. Given that all other science is applied or directed in some conscious way, the Government should retain a clear voice in the application and the direction.

Second, I do not share Bush's faith in institutional arrangements. Institutions come and go, and while they may help to shape policy and channel research funds, they cannot guarantee the isolation of process from policy. No agency is immune to politics; no institutional form remains static.

The creation of a separate civilian space agency, for example, did not ensure the separation of civilian space activities from military, nor has it prevented the militarization of space. Similarly, the shuffling of Federal energy agencies in the 1970's reflected policy more than it shaped it. The National Science Foundation is a far different instrument of the national will than Bush's proposed National Research Foundation, on which it was based. Only the continual refinement of policy, such as this committee is now embarking upon, can ensure that public funds serve the public good.

Finally, I would place the social sciences in the same category as pure science and recommend that the Government support them in the same way and for the same reasons. We should not expect the social sciences to produce practical applications like those derived from the physical sciences and now the life sciences, for nature is knowable in a way that societies are not. But we should nonetheless provide the social sciences continuing and nontrivial Government support as ends in themselves. Like pure science, the humanities, and the arts, they are hallmarks of a vital and curious society where understanding is its own reward.

In conclusion, then, I would argue that there are at least three good reasons for the Federal Government to support science, but that these reasons apply differently in the different scientific fields. The physical sciences continue to dominate the public imagination, in part because of the tremendous advances since Newton, climaxing perhaps in the atomic bomb. Practical, that is, technological, application remains the most telling argument for basic research in this field, with prestige and the abstract search for knowledge playing lesser roles.

Genetics and medicine have provided the most dramatic practical applications in the life sciences and the greatest international prestige as well; other life sciences deserve support almost entirely on the basis of their contribution to our understanding, though we have not begun to tap the potentials of this field. With few notable exceptions, like the Nobel Prize in economics, the social sciences continue to make their greatest contribution in our understanding of ourselves.

Perhaps in our increasingly technological world that will prove in the long run to be the greatest contribution of all. How to manage the clones and the neutron bombs may be the most important questions we face.

[The prepared statement of Dr. Roland follows:]

"Goals and Objectives of National Science Policy"

by

Dr. Alex Roland
Associate Professor of History
Duke University

TASK FORCE ON SCIENCE POLICY

Thursday, March 7, 1985

8:30 a.m., Rm. 2318

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"Goals and Objectives of National Science Policy," testimony by Alex Roland before the Task Force on Science Policy of the House Committee on Science and Technology, 7 March 1985.

I am pleased and honored to have this opportunity to share with you one historian's perspective on the timely and important task you have set for yourself. For reasons I will discuss in my concluding remarks, I believe that this kind of activity is indispensable to a healthy relationship between science and the federal government. My belief that I might have something to contribute to your deliberations is based on my research and teaching in the history of Western science and technology, with some emphasis on the history of twentieth century science policy and technology in the United States. My basic proposition is that there are at least three kinds of science that need to be considered and at least three different rationales for the federal government to support them.

I would like to begin by differentiating among the three kinds of science: physical sciences, life sciences, and social sciences. Their histories are quite different and they may well require different science policies. The world is still in the throes of an enthusiasm for the physical sciences that dates to the Newtonian revolution. By reducing the movements of the planets and the apocryphal apple to a single, simple equation, Newton stimulated a faith in man's ability to understand nature, which was probably a good thing, and a belief in quantification, which has been a mixed blessing. The physical sciences have advanced most dramatically since Newton, in part because they are more readily quantifiable and in part because

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In the United States, the government has historically seen science as a means to an end, and this has dictated the nature of federal support. Science in the colonies and the early republic was largely a private enterprise, funded by amateurs of independent means like Benjamin Franklin or by universities and foundations. The government provided support mostly for agriculture and military applications, clearly expecting some practical return on its investment, usually some technological application.

At the turn of the century, the government began to increase its support of science, creating such agencies as the National Bureau of Standards and the National Advisory Committee for Aeronautics. But again, it was for practical applications, usually technology, not for basic science. Most research was done in house, on the model of military arsenals and agricultural research stations.

Our modern view of the relation of government to science formed in World War II, to be refined in the succeeding years of cold war. Vannevar Bush and his colleagues in the wartime Office of Scientific Research and Development were chiefly responsible. For better or for worse, Bush's SCIENCE--THE ENDLESS FRONTIER has shaped our science policy for the last four decades. Several basic tenets of SCIENCE-THE ENDLESS FRONTIER warrant special attention. Most are explicit in the report; one is implicit, but nonetheless decisive. First, Bush argued that what he called scientific research was indispensable to the military and economic security of the United States. Second, the government should fund this research on a continuing and substantial basis, in contrast with the irregular and inadequate patterns of the past. Third, scientists should have control over how these funds were distributed, to ensure that the best science was supported as it had been by OSRD during the war. Bush was not, however, asking for free access to the Treasury; funds expended in this way would represent only a small proportion of those spent on research and development through the mission agencies of the Executive Branch, as in fact OSRD had accounted for only a part of the military R&D in World War II. Fourth, the scientists should remain in their home institutions, primarily universities, and not be drawn into government laboratories, with their inevitable emphasis on applications and politics. And fifth, scientists should participate with government leaders and industrialists in shaping R&D policy.

The unspoken assumption behind these recommendations was that scientists understand nature's laws better than anyone else; they are in a better position to see the potential applications of their understanding. It behooved the government, Bush believed, to support the expansion of that understanding, to create a reservoir of people capable of seeing how that understanding might be reduced to practical applications. Let the scientists themselves pursue their own agenda, identify and conduct the best science as an end in itself. In an emergency, the country could draw upon this reservoir of talent, even as it had gathered the country's nuclear physicists together in the Manhattan project. Thus, even Bush's formula, though it called for scientific autonomy, based its argument on the traditional American belief that pure research will lead in the end to practical applications, that science leads ultimately to technology. Of course, Bush was referring principally to the physical sciences; he made special provision for medicine, which has comparable applications, but otherwise slighted the remaining life sciences.

But if Bush's assertion were true, that basic research leads to technological applications, it would create a serious contradiction. Scientists conducting truly basic research rely on publication. Their only reward system is recognition by their colleagues, and this recognition goes to first publication. Why should the federal government support basic research if it is going to give away the results, if it is not going to get a monopoly on the applications? Why pursue

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basic research at all if the results are free for the taking in the international marketplace of scientific literature? Why not support only applied research? Bush would have argued that basic research creates that reservoir of people best able to apply their own results. But this only begs the question why other scientists, working on applications, cannot understand the theoretical literature.

This has not become a fundamental problem since the Bush formula was proposed because the cold war has provided yet another premium for basic research, one with applications entirely different from those envisioned by Bush. At least since Sputnik, we have come to believe that scientific achievement, independent of material application, contributes to national security. Scientific eminence lends prestige, and prestige weighs in the calculus of the social, ideological, and political competition between East and West. We go literally to the ends of the solar system and the bowels of the atom, not just in search of understanding, but in large measure to demonstrate our scientific virtuosity. Each year we quickly convert the Nobel prizes into a box score of which nation is doing best. And we realize practical returns on our life sciences that were impossible when the principal criterion was technological application. Basic research has become a tool not only for manipulating nature, but for influencing public opinion--yet another practical application in the American tradition.

Bush and the cold warriors who followed him have both then argued for federal support of science, one on the basis of the traditional view of practical applications, the other in the belief that national prestige contributes to national security. From a historian's perspective, I would argue yet a third, one that has been lurking beneath the surface throughout our national history but far too seldom advanced. We should patronize basic science on a substantial, nontrivial, and continuing level, not only in expectation of some measurable return on investment but simply because understanding is a fundamental human activity, the support of which becomes a great nation such as ours. Just as we support, however modestly, the humanities and the arts, so too in principle should we support the investigation of nature as an end in itself. No one can foresee the practical advantage in knowing how the universe was formed or why the dinosaurs disappeared or why grass is green, but we will be a poorer society if we stop asking, if we continue to demand of our science only that it make us richer and safer. For this part of the scientific agenda, i.e., for truly basic research, I agree with Bush that the scientists themselves are the best judges of how and where to dispense what the government can afford.

All the rest of scientific activity is more or less directed or applied, i.e., it is conducted on the assumption that increased understanding will eventually serve some direct, utilitarian end. Here I harbor some strong reservations about the Bush formula. First, except in that peculiar realm of truly pure research, I do not believe that scientists should have complete autonomy, any more than any other group in our society should set its own agenda for dispensing public funds. Given that all other science is applied or directed in some conscious way, the government should retain a clear voice in the application and the direction.

Second, I do not share Bush's faith in institutional arrangements.

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Institutions come and go, and while they may help to shape policy and channel research funds, they cannot guarantee the isolation of process from policy. No agency is immune to politics; no institutional form remains static. The creation of a separate civilian space agency, for example, did not ensure the separation of civilian space activities from military, nor has it prevented the militarization of space. Similarly, the shuffling of federal energy agencies in the 1970's reflected policy more than it shaped it. The National Science Foundation is a far different instrument of the national will than Bush's proposed National Research Foundation, on which it was based. Only the continual refinement of policy, such as this committee is now embarking upon, can ensure that public funds serve the public good.

Finally, I would place the social sciences in the same category as pure science and recommend that the government support them in the same way and for the same reasons. We should not expect the social sciences to produce practical applications like those derived from the physical sciences and now the life sciences, for nature is knowable in a way that societies are not. But we should nonetheless provide them continuing and non-trivial government support as ends in themselves. Like pure science, the humanities, and the arts, they are hallmarks of a vital and curious society where understanding is its own reward.

In conclusion, then, I would argue that there are at least three good reasons for the federal government to support science, but that these reasons apply differently in the different scientific fields. The physical sciences continue to dominate the public imagination, in part because of the tremendous advances since Newton, climaxing

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perhaps in the atomic bomb. Practical, i.e., technological, application remains the most telling argument for basic research in this field, with prestige and the abstract search for knowledge playing lesser roles. Genetics and medicine have provided the most dramatic practical applications in the life sciences and the greatest international prestige as well; other life sciences deserve support almost entirely on the basis of their contribution to our understanding, though we have not begun to tap the potentials of this field. With few notable exceptions, like the Nobel Prize in economics, the social sciences continue to make their greatest contribution in our understanding of ourselves. Perhaps in our increasingly technological world that will prove in the long run to be the greatest contribution of all. How to manage the clones and the neutron bombs may be the most important questions we face.

DISCUSSION

Mr. FUQUA. Thank you very much, Dr. Roland, for a very excellent paper.

Dr. ROLAND. Thank you.

Mr. FUQUA. We are very interested in this subject and I think it outlines very well some of the issues with which we are faced here.

One of the things in your conclusion, you talked about all of the three sciences you outlined. You mentioned particularly the physical sciences—probably people see more end results from the physical sciences perhaps than in the others. Yet we, the Government, are politicians, not scientists, trying to make public policy decisions affecting various things and also we must extract money from the taxpayers in order to finance these programs.

Dr. ROLAND. Yes.

Mr. FUQUA. When the public clamors for more support for basic sciences versus the social sciences, for example, how, then, do we factor into our policymaking decisions to offset the public clamor for one science over the other?

Dr. ROLAND. I guess what I am really trying to suggest is that it behooves all of us, your committee in particular, to try to be more clear in our public discussion of these issues; that is, if we understood there were different kinds of sciences that have different needs, then people might be more receptive to an argument on principle.

I firmly believe that it behooves the Government to support on a certain continuing level, no matter how modest it has to be, the basic sciences, and that we should make known to the American people the belief that this is in the long term best interest of the Government.

The others are much easier to defend to the public, and the more direct and immediate the application is, the easier it is to defend. However, I think the result of that is that we tend to support technology-related sciences, those which can show immediate tangible returns, and we ignore some of the others. I think that is to our peril. I think everyone is capable of understanding this if we conducted public debates in these terms instead of lumping all of science together, because I really believe that when most people hear science or applied science all they think of is physics, and the immediate technological returns we get on that kind of research.

Mr. FUQUA. Let me say that I agree with you. I was asking the question more as the devil's advocate. I think it is poor public policy to set science policy as a result of the weather vane. Many times we hope we have the foresight to evaluate conditions and needs of the country and make those hard decisions that we have to make rather than what appears to be perhaps the most politically or publicly popular programs. That is the difficult part of it.

On page 3 you mention that the computer is outstripping the understanding of molecular physics, and some of the problems we are facing today, for instance in hazardous waste. We did not know what to do back 30 or 40 years ago with regard to disposing some of these waste. That can continue to be a problem. There are things we are involved in today that we really don't understand.

What comes to mind is bioengineering. Is there something unforeseen? How does public policy try to take into consideration those measures?

I think there is a higher awareness factor today of these things than perhaps existed back 30 or 40 years ago. However, at that time I was not as involved in public policy as those of us who are sitting here today.

Is there a way in science policy that we can be sure we do not repeat mistakes of the past?

Dr. ROLAND. I do not think there is any guarantee. I have two answers to your question. One, I think this is where the social sciences can serve a role, not in predicting the future but in pointing out instances in the past where our public policy has not been adequate to anticipate problems in the future.

The specific one you raise about nuclear waste is particularly poignant and it is appropriate to what I am trying to suggest to the task force. A very distinguished nuclear scientist told me that the nuclear waste problem is a problem today because when they first faced it the scientific community believed it was readily solvable and they did not take the time to bring the issue out into the public debate. They simply advised policymakers that that was manageable. He says that they now regret having made that decision; that is, for better or worse, they could not predict the future fully either, but if they had conducted public debate rather than guaranteeing that the problem was solvable they would not look as fallible today as they do.

To my mind, that is another reason for not giving the scientists carte blanche; at least make them come out in public forum and explain what they anticipate or what they predict about the future so it becomes a public debate rather than a guarantee from an expert.

Mr. FUQUA. I will have to excuse myself, as I stated earlier. I thank you very much for being here. I would like to be here longer and hear the responses to the other questions. I will turn the meeting over to Mr. Brown. I am sure he and the other members will have some very interesting questions for you. Thank you very much.

Dr. ROLAND. Thank you.

Mr. BROWN [acting chairman]. Mr. Lujan.

Mr. LUJAN. Thank you, Mr. Chairman.

What you say about the nuclear waste issue is true. We were always told that the technology is there and it is a solvable problem. While it was almost true, it was not totally true.

I have been sitting here as you were going through your statement and trying to place myself in whatever little box you were talking about. I divided it up into two areas—the knowledge seekers and the applications proponents.

Dr. ROLAND. Yes.

Mr. LUJAN. I guess we do that even, as you characterize it, with our Nobel Prize box scores. It gives us world leadership. That is why every country in the world wants to get into the space business as a means of their own national self image.

I guess I fit into the applications proponents box from the descriptions you have given. I see nothing wrong with it.

Dr. ROLAND. Oh, no.

Mr. LUJAN. Science is a means to an end. I would like to discuss that a little bit. Why should it not be that all research, whether we put it into this pool for use later on, into the bank account, but all research should be directed at eventual commercialization or for defense, one or the other, which in itself is a means to an end?

Dr. ROLAND. I guess what I am trying to suggest is that that one seems a natural to me. I think, as you do, most people understand that; that is a practical reason for pursuing scientific research. I think there are at least two others.

One is this national prestige, and the space program is a good example. We have received lots of practical returns from that, but also some of the activities conducted by NASA are simply the pursuit of knowledge as ends in themselves, and that, too, should have some place, however modest, in our Federal budget. It behooves us as a nation to do that, in part because of the prestige that comes, for example, from the Viking exploration of Mars. We still don't have any tangibles and practical returns on that, but the increase of our understanding and the national prestige are both world-worthwhile activities that the Government should be supporting and pursuing.

The further you get from practical applications, especially for you to consider, the more difficult it becomes to convince the taxpayers that this is something that should come out of the Federal Treasury.

Mr. LUJAN. Unless you can give them a practical application for the future.

Dr. ROLAND. Yes; exactly.

Mr. LUJAN. Can you do that?

Dr. ROLAND. What I am saying is that the national prestige is a legitimate, practical application; that is, part of the contest between East and West now is conducted on terms of how did the Third World countries perceive the drift of events; which society with its form of government and organization is making the most progress? That is significant It does contribute significantly to our international stature.

The third one is the most difficult to define, that is, the pursuit of knowledge for its own sake is a legitimate enterprise that the Government should be funding. My only argument for that is that it behooves a society like ours to contribute some proportion of the Federal Treasury, however small it is, in principle to enhance understanding.

Mr. LUJAN. Give me your thoughts on what percentage. I look at funding the National Science Foundation, for example.

Dr. ROLAND. Yes.

Mr. LUJAN. Let's say the Department of Energy or NASA, funding those for what good they do for mankind.

Dr. ROLAND. Yes.

Mr. LUJAN. How should we weigh that? Can you give me a percentage?

Dr. ROLAND. I can't begin to give you numbers, but I think it is comparable, though I hate to draw this comparison because of how modestly we support the arts and humanities, but I put the pure sciences in understanding for its own sake in that same category. It is a hallmark of a society that is not so self-absorbed in wealth and security that it can think of nothing beyond immediate practical returns. That is, a curious and outward-looking society is a healthy society, and a society that can find no money to support arts and humanities and understanding for its own sake is an impoverished one, indeed. Exactly what the numbers are I would not begin to recommend to you.

However, I think that Congress should make the case to the people that this is just something we should do and identify it, that a certain portion of our Federal expenditure in science is not because we are trying to make ourselves richer or more secure but just because we are trying to make ourselves wiser.

Mr. LUJAN. Thank you, Mr. Chairman.

Mr. BROWN. Mr. Reid.

Mr. REID. Thank you, Mr. Chairman.

Looking at your biographical outline, I am envious. You have studied some things I would like to have studied. You have had a great education. I think we on this committee should hear more from people like you.

I was especially impressed with your direction which is basically that there is more to science than a man in orbit.

Dr. ROLAND. Yes.

Mr. REID. Can you give me your opinion as to how we are doing as a nation in the social sciences compared to the physical sciences? I understand your theory, but how are we doing?

Dr. ROLAND. Compared with other nations, I think we do quite well. Compared with the physical and life sciences, the social sciences have not—and it is my belief—cannot produce comparable results. The physical sciences in particular produce dramatic results because nature keeps behaving the same way every day, no matter what we do about it.

Mr. REID. Do you talk about that in your paper?

Dr. ROLAND. Yes; and society does not. Social sciences never will discover laws of human activity comparable to the laws of nature, the physical sciences and the life sciences. Therefore, in a certain sense, the name "social sciences" is a misnomer, but it is appropriate because what they are undertaking to do is to apply a variety, a form of scientific methods to the study of society. That is admirable but they are never going to reach conclusions that are comparable.

Mr. REID. If I may interrupt, I guess the direction of my question is this: Do you think we are doing enough in the social sciences?

Dr. ROLAND. No; social sciences need considerably more support. As I suggested in my conclusion, as we become an increasingly technical society, I think that places a higher and higher premium on the social sciences because the most important thing to understand now perhaps is how to handle this wealth of scientific and technological capability.

Mr. REID. Your last sentence was quite enlightening: "How to manage the clones and the neutron bombs may be the most important questions we face."

You know, all across the United States there is an effort, especially at the secondary school levels, to take out of the curriculum social science courses.

Dr. ROLAND. Yes; I know.

Mr. REID. In my State there is a big battle in the State legislature now as to what teachers should be able to teach. They say the only thing you should teach is reading, writing, and arithmetic.

Dr. ROLAND. That is right. I hate to say that it is finding its way into the colleges and universities as well; that is, a perception that a crucial ingredient of liberal education for the world that today's college graduates will face is more technical training. Technical literacy is the term used most often now.

I have no argument with that whatsoever. I think it is helpful for everyone to have some understanding of what science is, what technology is, and how they function. However, if this is done at the expense of the humanities and social sciences, we are likely to lose exactly those skills and understandings that will be necessary to control the science and technology. It becomes pervasive.

Mr. REID. You state again, in the last sentence, it is through courses like this, no matter how mundane they might seem, that we are able to attempt to manage clones and neutron bombs?

Dr. ROLAND. Exactly.

Mr. REID. Mr. Chairman, I appreciate very much the staff and the chairman who have produced this witness. We need more of this. We have to recognize there is more to life than trying to figure out a physics problem.

Thank you very much. I compliment you on your educational background.

I am sorry, Mr. Chairman, but I have a meeting I must attend.

Mr. BROWN. Mr. Volkmer.

Mr. VOLKMER. Thank you very much. I am sorry I was not here to hear your testimony. I have quickly reviewed it and find it most interesting and most thought-provoking.

One of the things that struck me immediately as I reviewed your testimony and heard some of your answers here is this: Back in the late seventies, when some of us were looking at an energy policy for this country, we thought we were doing some basic research on energy matters, geared to application, and now we have seen in the last 3 or 4 years those things have dropped off.

What is notable to me is what I read in here, even though we are now down to what we would call in the physical sciences more basic research, still that basic research is not truly basic research. It is still applied research in a sense.

Dr. ROLAND. That is right.

Mr. VOLKMER. In the sense that it is done with the idea it will have increased technological applications; is that correct?

Dr. ROLAND. Exactly.

Mr. VOLKMER. That is what you are saying?

Dr. ROLAND. Yes. As a matter of fact, an anecdote, I learned just recently after drafting this testimony—a colleague of mine at Duke, an engineer, read it and told me a story of an experience that he had at one of our national laboratories, which shall remain nameless. He said when reports were handed in there, scientific reports that he had conducted, the secretary would call him and ask him after the fact was this basic or applied research. He would pick one of the terms, and he said he had the feeling that if for the term he picked the money had run out it would just be called the other.

I think one of the problems we are facing—in fact, I suspect it is laced through my testimony, too—is that we use these words very loosely and we are not entirely clear on what they mean.

The distinction that I would draw is pure research, research that is for knowledge for its own sake. While there may be practical applications, the researcher does not have any in mind. He or she is just pursuing a scientific agenda.

All else, I believe, is directed or applied in some sense or another, that is, that even though the researcher is trying to increase his or her understanding of the phenomenon, they hope that in the end it will serve some purpose. That is a little bit different, I think, and a distinction worth drawing.

Mr. VOLKMER. In other words, what we are doing, in the field of aeronautics and in the field of space, or anything else, they are all geared to an application. Anything that is done in grants with regard to supercomputers, artificial intelligence, it is really not basic research

Dr. ROLAND. Not pure. "Basic" is a term that has slipped in and serves both purposes. Some people understand it as what I would call pure research; other people understand it as what I would call directed research.

NASA is one of the few mission agencies I know of which does conduct some pure research, as I understand it. That is exploration of the solar system, that aspect of the solar program, which is comparably small. To my mind, it is pure research. There is no foreseeable direct payoff. It is just understanding.

Mr. VOLKMER. As one who is not a researcher and does not have a background in this type of thing, NASA—engineering, physics, chemistry, or anything else—can you give us an example of what you would call a pure research in the physical sciences?

That is what bothers me—the limit of my ideas. You have a lot more knowledge.

Dr. ROLAND. I am going to cop out and choose one I am most familiar with and which comes quickly to mind. Again it is from the space agency. Astrophysics is fundamentally understanding how the universe was formed and how it functions. There may someday be some practical payoff in that but I don't think they are conducting it with that in mind. They are just trying to understand the nature of the universe.

Mr. VOLKMER. The reason I ask that question is that in reviewing this I begin to think and come up with this: Are we now so knowledgeable and have we done so much research that the areas of pure research are now limited?

Dr. ROLAND. No. I think it is going quite the other way. If you look, for example, at physics in the 20th century, it has gone through cycles. There have been periods where just this belief was at large, that we understood the atom so well now we had essentially solved the riddle, and then we moved a little bit further and found it was an entirely different structure than what we imagined. We do not know where the end of that is. I do not know any physical science, let alone the life sciences, where people operating, especially at the theoretical end of it, would say that they are anywhere near a complete understanding.

Mr. VOLKMER. As we look at the structure of what we have been working with in institutions, universities and laboratories, where would we find people who would be willing to do this pure research in the physical life sciences?

Dr. ROLAND. I think there are large numbers of them. This is just an impression now, but probably larger numbers than those who are actually working on pure research, because they cannot get funding for pure research. They do directed research because that is where the money is. Large numbers who don't do that kind of work now would be happy to do it if there were adequate funding for it.

Mr. VOLKMER. Thank you very much, Mr. Chairman.

I appreciate your testimony very much. Perhaps we can incorporate some of this into the policy report.

I also have to leave for another meeting.

Mr. LUNDINE. I have no questions, Mr. Chairman. I appreciate the historic review and I will pay close attention to this testimony because Dr. Roland comes from the finest university in America. [Laughter.]

Mr. BROWN. Dr. Roland, your contribution is extremely valuable, probably more than even you realize. It helps us get at the fundamentals of the policy issues involved in support of science and technology. Only when we get at the fundamentals will we be able to formulate secure policies which would best serve the country.

I specifically would like to commend your emphasis upon the contributions which applications and technology make to science, the conventional wisdom today being that the path is all the other way.

I think your discussion of basic, pure, and applied science contributes to an important debate, but it does not resolve it. In my experience over the past 20 years these distinctions always are in the eye of the beholder, not in the reality.

Dr. ROLAND. Yes.

Mr. BROWN. The contribution you make in focusing on that third reason for supporting basic science is that it is an indispensable ingredient in the human condition which deserves to be supported. I think that is the major contribution that you make, and it needs to be emphasized in many ways.

We have this tendency always to think in terms of cost-benefit. What this country needs today is a vision which will captivate them, not a cost-benefit analysis.

Dr. ROLAND. I quite agree.

Mr. BROWN. That may well be the most important political lesson we can learn.

Dr. ROLAND. Yes.

Mr. BROWN. The history of science would indicate that its very beginnings stem from that insatiable curiosity to know what constituted the universe. We hear from myth it may have been shepherds sitting around at night in fields looking up at the stars that created this problem. There is no question that was basic research because there was no practical application. It was also a good illustration of how all science begins, in a taxonomic way. They were there regularly, and so on.

I think you were presumptuous in some of the statements you made about the distinctions between physical and social sciences.

Dr. ROLAND. Yes.

Mr. BROWN. The beginnings of physical science were not based upon the knowledge of the regular world. They were based upon an effort to understand a very mystifying world. There was a lot of praying to the gods to help understand what made thunder and lightning but no idea that it would be really understood.

We need to understand and think about these things, and your contribution compels us to do that in a very important way.

Let me ask you this; the question of whether we should support the social sciences and even some more obscure fields is an important policy issue. Your contribution as an historian helps build a case for the support of the social sciences.

You also have had some experience with the program on Science, Technology and Human Values which is threatened with extinction. Rather than my making a strong statement on this subject, would you care, based on your own experience, to indicate what you consider to be the ability of such a program?

Dr. ROLAND. Yes. I believe there are several, but perhaps the most compelling is that as we come to live in an increasingly technological world, that is, in a world shaped increasingly by science and technology. This should not drive the humanities and social sciences into the background but in fact bring them more fully to the fore, because the emphasis on technical education will continue and we will be forced to function with the technology we operate with. That is no guarantee that we will understand the social implications. In the final analysis, the social implications are surely the most important criterion. What does it benefit us to control nature if we cannot control ourselves, if we cannot develop a society in which our scientific and technological advance creates more wealth and security for us?

As we become increasingly enamored of and controlled by our science and technology, we are going to need more, not less, skills in understanding how to manage them socially and make them redound to our real long-term benefit rather than immediate material benefit.

We conduct at Duke a program in science, technology, and human values which has this in fact as its major focus. It is originally directed toward students in the sciences and engineering, premed students, to give them an opportunity to sample courses, hear lectures, engage in discussions on the social implications, the careers that they envision for themselves. The student response to that, I understand, is significant enough to suggest that this is a real concern to thoughtful people going into these fields in the future. We need more, not less, of that in the future.

Mr. BROWN. Mr. Lundine, I have preempted this because I waited until last. If you would like to interrupt at any point, please do so.

Mr. LUNDINE. Thank you. I appreciate that.

Dr. ROLAND. I would like to add, if I might, that I read with great interest the agenda that your task force has prepared for itself. I am very, very impressed with that. It suggests a very informed committee and a very informed staff which understands all the ramifications of what science policy is.

One of the things I was trying to suggest in my presentation is that it behooves us to see all the complexities of this.

Mr. BROWN. At the risk of alienating my good friend, Dr. Holmfeld, who did most of the work on this, I would say it is far from perfect but it is a good start. [Laughter.]

It is based upon long appearance before this committee of these issues and questions in one form or another and a need to look at them systematically.

I want to go back and focus on this issue you raised so well, the importance of supporting science because of the need to encourage the exercise of unrestrained human curiosity, sort of science for itself.

Dr. ROLAND. Yes.

Mr. BROWN. I do that because obviously I agree with your point about the importance of it. I want to stress again, however, that there is no way I have ever found that you can separate this out. If you take the purest work of Einstein, the relativity theory, it took another generation before the development of the nuclear weapon.

Dr. ROLAND. That is quite right. The distinction I would draw is that was not Einstein's purpose. That is the distinction I would recommend in dividing these up for different funding purposes, different rationales. If that researcher is just pursuing understanding and has no long-term agenda for how that understanding might be applied, then that is pure or, if you prefer, basic research. However, if the research is being conducted in the belief that increased understanding of this field will lead to some practical applications, then that is directed. Both of them are worthwhile and both should be supported. However, I think we should understand the distinction between the two. They are very different rationales. The taxpayer can understand one, I think, much better than the other.

Mr. BROWN. The scientists coming before us like to have it both ways.

Dr. ROLAND. I can understand that.

Mr. BROWN. They always claim that there is an inevitable social benefit from the pure research. Then, on the other hand—they claim that pure research without any idea of benefit ought to be supported for itself.

Dr. ROLAND. I understand.

Mr. BROWN. They want to get all of the benefits and none of the problems. I say this about some very dear friends of mine who testify before this committee.

What they are missing is the vital importance of the point you are making, that no society can claim leadership which does not support it. The earliest support—Newton, for example—came because of Newton's prestige. This great scientific leader was important to the King or Queen of England; I am not sure who was in charge at that point. It was not done because Newton was going to contribute to development of something.

Dr. ROLAND. One of my colleagues suggested to me upon reading this testimony in draft that, as he recalled reading Bush, he always believed that Bush had one agenda to himself and made a different one because he thought that was what could sell. I think that is true. I think the scientific community is still functioning under the Bush agenda and that creates the inherent contradiction. They believe one thing, that pure science or basic science is worthwhile to fund as an end in itself but they are not sure that they can sell that argument, so they make an argument on practical applications. We would all be much better off if we just got out in the open and said that and tried to make a case for pure science.

Mr. BROWN. That is exactly why I am belaboring the point with you. I would like the record to fully reflect that we are missing a great opportunity to inspire the people of this country and to search for world leadership much more effectively when we fail to recognize the importance of supporting the sciences, probably the most important characteristic of the human condition.

Mr. LUJAN. Would the gentleman yield?

Mr. BROWN. Yes; I want you to help on this.

Mr. LUJAN. Why is it important you make a distinction, anyway? In other words, if your colleague's experience is typical and you can justify basic research as applied or vice versa, it would seem to me it would be just as easy to contaminate pure and directed. I am not sure why we need to make that distinction.

Dr. ROLAND. Because we are not having enough people do real research. Picture the scientist in the university who is looking for a research agenda. He might have a very theoretical, very abstract pure science question that he or she would like to pursue but they cannot get funding for that because they cannot argue any practical return, so what they do—again, the terminology is very confusing—very often they will call it basic research that has some foreseeable payoff. As soon as that intrudes upon the question, to my mind at least, that is not real pure science. That is a form of directed research, and we ought to have some funding for science that is simply for pure research and then let the scientists by their peer review system determine which proposal is most compelling for getting those funds.

Mr. LUJAN. In your view should we have some kind of a set-aside for pure research?

Dr. ROLAND. Yes. In many ways that is what Bush had in mind for what he called the National Research Foundation which became a considerably different thing as the National Science Foundation. The distinction he drew was layers of Government officials standing between the scientist and the Congress, and what he wanted in his National Research Foundation was an agency of scientists that went directly to the Congress and said: Here is our proposal for the research we think is most important to do. We cannot justify it all. We cannot tell you what the payoffs will be, but it is our best judgment that this is the best research to be doing. And then let the mission agencies of government within the executive branch pursue the directed research and conduct energy research.

No matter how basic or pure people within the Department of Energy would argue that research is, surely they have in mind there will be some long-term practical use.

Mr. LUJAN. You are not suggesting we return to that concept where all of the NSF funding be pure research and the other departments of Government would do all of the directed?

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Dr. ROLAND. Because it becomes different from what Bush envisions, perhaps not. I would be comfortable with what he originally suggested, that is, some agency or branch, some portion of the NSF budget which was argued simply on the basis of pursuit of understanding without any foreseeable or projected payoff.

Mr. LUJAN. Not that I cannot understand that, but my heart is in politics. I would much rather go across the street to that great deliberative body with a budget or proposal which had two-thirds applied and one-third basic or pure. Then I could talk about the former and say we have set aside so much for the latter. I would not go across there and justify any amount of money without being able to explain the application.

Dr. ROLAND. That is why we have the problem we have, because we have not educated the American people to understand these distinctions so we don't dare try to make an argument about pure research. We call it all directed in some way or another and predict a payoff from it which often does not come true.

Mr. BROWN. The point I appear to be making is one of unqualified support and endorsement of your theories, the significance of pure research. I think a more important point to be made is that we have a seamless web. The key ingredient is the human curiosity aspect. It is just as pure for that curiosity to be applied for a practical problem as it is an abstract problem.

Dr. ROLAND. Of course.

Mr. BROWN. I find myself fighting today probably even more than for the support of basic research, for appropriate applied research which contributes to the welfare of the country in a significant fashion.

Dr. ROLAND. Yes.

Mr. BROWN. I don't make a distinction in terms of its importance, prestige, or other things of that sort, nor do I distinguish, sometimes for political reasons, between the significance of physical or biological or social science research as long as they represent what needs to be supported to help human beings.

What I am seeking to do is to create sufficient, first, political and congressional understanding and then public understanding of that so that the entire basis rests on a firm foundation.

Dr. ROLAND. Yes.

Mr. BROWN. Let the record show you enthusiastically agreed with me.

Dr. ROLAND. I enthusiastically agree with everything you say.

Mr. BROWN. We have come to the end of time alloted for us. Another committee will usurp our place here. We will stand adjourned until the same time next Thursday when our witness will be Dr. Lew Branscomb, well known to most of us, and whose many roles in science are well known.

Thank you for your contribution.

Dr. ROLAND. Thank you.

[Whereupon, at 9:33 a.m., the task force recessed, to reconvene Thursday, March 14, 1985.]

[Answers to questions asked of Dr. Roland follow:]

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QUESTIONS AND ANSWERS FOR THE RECORD

Professor Alex Roland

1. To what extent have the goals and objectives of U.S. science policy changed since 1945? To what extent have these changes been a response to "crises", and to what extent have they been in response to broader changes in society's needs and wants? What elements have contributed or deterred from the flexibility and responsiveness of our science policy?

I don't believe the United States has ever had a science policy, not even since 1945. We have, however, expected science to serve several goals and objectives, and we have developed policies and institutions intended to reach these goals and serve these objectives. There are numerous instances of this; perhaps a few examples will suffice. The creation of the Atomic Energy Commission proceeded from the assumption that science had delivered to the United States a new technology fraught with promise and danger; only a special institutional arrangement would guarantee that the potentials of this new technology were controlled and exploited. The National Science Foundation was a compromise of Bush's plan for postwar mobilization of science. It excluded the military and medicine and it gave the scientific community less autonomy than it wanted but more than it had enjoyed before World War II. In response to the crisis of Sputnik, the United States created the President's Science Advisory Committee, the National Aeronautics and Space Administration, and the Advanced Research Projects Agency, and passed the Defense Reorganization Act of 1958 (creating a Director of Defense Research and Engineering) and the National Defense Education Act. As these actions, some in response to crises and some not, suggest, we do not have a policy; rather we respond ad hoc to problems as they arise. This has the advantage of great flexibility and responsiveness. It has the disadvantage of inconsistency.

2. Have Bush's rationales for the support of basic research proven valid over the past 40 years? For example, has the historical record, in your view, shown scientific research to be indispensable to the military and economic security of the U.S.? Based upon the history of the twentieth century technology, should we question the truism that science leads ultimately to technology?

It must be recalled that Bush's rationales for the support of basic research were just that--rationales. He believed that there were real reasons and good reasons for supporting basic research. The real reasons--knowledge as an end in itself and basic research as an undifferentiated pool from which to draw future technology--would not sell. So he invented good reasons: economic and military security. This case he could rest on the record of his wartime Office of Scientific Research and Development. His scientists had been demonstrably more productive following their own noses than the rest of the scientific and technical community who had been dancing to the tune piped by the military. And their work paid off on the battlefield. By simple extension it followed that if a comparable group of scientists could work under similar conditions in the post-war world they would produce comparable wonders of practical application for the military and economic well-being of the country. Security and prosperity, after all, sell in Washington.

3. You mentioned the Bush Report of 1945. As a historian, can you shed any light on the Steelman Report which was done by a staff member in President Truman's White House and which was published in 5 volumes in 1947, two years after the Bush Report? Why was that report commissioned and why do we today hear so much less about it?

I have heard of the Steelman Report only briefly and in passing. Short of engaging in original research (which I would be willing to do if you are really interested), I would suggest that your question helps to explain the obscurity of the report. Bush knew that five-volume reports never get read. He limited that basic text of Science--The Endless Frontier to 34 pages.

4. Some, including some historians and social scientists, have suggested that the relationship between science and the Federal Government is in the nature of a social contract: The government provides certain resources for scientists to expend in return for which they provide society with certain benefits. How do you view this analysis, and has it changed over the years?

It is accurate enough to view the relationship between science and the federal government as a social contract, but not, I think, very helpful. The real issue is always what the government gets--or expects--in return for its support. Primarily it has wanted throughout our history practical returns on its investment. It is just as much of a social contract, however, if the government gets only advancement of knowledge that pure research brings.

5. You argue that the U.S. Government has historically viewed science as a means to an end rather than as an end unto itself, and that government support of science has reflected that view. Yet you advocate the support of basic research - especially in the life and social sciences - for their own sake. What historical precedents exist for pursuing such a policy? What are the historical limits likely to be imposed on such a policy by the American political system?

The precedent for government support of basic research is in the arts and the humanities. The government supports those activities (however modestly) on the faith that art and culture are hallmarks of a healthy society. We do not ask what a poem or a painting do in return for government support; the return is intangible and assumed.

The problem, of course, is that the government has historically provided little more than token support for the arts and humanities. Some believe that such activities are best supported by private funding. Others find distasteful particular art or humanistic projects the government funds. Still others, immersed in the deep current of anti-intellectualism that flows beneath the surface of American culture, consider all such enterprises to be a waste of time and money. Basic research is vulnerable to all the same objections. Additionally, poets and painters are at least harmless; scientists might blow up the world.

6. You note that throughout human history technology has led the way for science rather than the other way around, and that only in the last 100 years has it become commonplace to view technology as applied science. Where do we stand today? Is it possible to distinguish some areas of technology as clearly science based and other areas of technology as clearly not. Do you see that as a steady-state situation or as evolving, and what are the policy implications?

Indeed some technologies are clearly science-based. Equally clearly some sciences are technology based; i.e., we are able to do things before we understand why they work the way they do. Aerodynamics is a classic example of a modern science whose theoretical base has long trailed empirical capabilities. Many other sciences, from solid state physics to molecular chemistry, share the same characteristic. If there is a trend at work here it is probably that science, especially the physical sciences, are becoming increasingly empirical while technology is becoming increasingly reliant on scientific knowledge or at least scientific method. It is more and more difficult to distinguish between empirical science and technology, except that their goals are different.

7. You suggest that the idea that scientific research will lead to practical applications is the "traditional American belief". In your view, how far back in our history does that belief go, and do you see it as uniquely American?

Most colonial science in America was natural science, collecting flora and fauna to add to the store of European knowledge. By the time of the Revolution, however, our premier scientists, men like Benjamin Franklin, Thomas Jefferson, and Benjamin Rush, were turning their attention to physical sciences with practical applications. Ever since the Revolution, Congress has been more willing to fund scientific activity with practical application than science for its own sake. This is not peculiarly American, but it does suit our national penchant for pragmatism and practicality.

8. There has been much emphasis on the need to maintain U.S. leadership in science across a broad front in order to allow the U.S. to remain strong in technology and international trade. Yet some countries with a strong science base, such as England, have fallen behind economically, and some countries with a weak science base, such as Japan, have surged ahead economically. What is the relationship between national and international strength in science and economic strength?

Nathan Rosenberg has addressed the relationship of science and technical progress in his recent book INSIDE THE BLACK BOX: TECHNOLOGY AND ECONOMICS (Cambridge: Cambridge University Press, 1982). In surveying the literature on the subject he concludes:

> "What is clear and is borne out by the histories of England, France, the United States, Japan, and Russia over the past two and a half centuries or so is that a top-quality scientific establishment and a high degree of scientific originality have been neither a necessary nor a sufficient condition for technological dynamism" (pp. 13-14)

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I agree wholeheartedly and I recommend to you Professor Rosenberg's entire book, which speaks to many of the questions raised in your letter. The chapter "How Exogenous Is Science" is the best rebuttal I know of the conventional wisdom on technology as applied science.

9. It is well recognized that the potential payoff in medicine or technology from an individual research project can not be predicted. However, we also know that broad fields, such as, for example, chemistry, yield significant practical benefits. To what extent can and should the expectations of such payoff be used to determine the levels of funding for science and for the individual disciplines?

As I suggested in my original testimony, I believe that the disparity in predicted payoff of different scientific enterprises requires that science be funded under two rubrics. Directed or applied science should be funded at a level commensurate with the expected payoff and the perceived public need. Pure or basic research should be funded independent of any expectation of direct return on investment. This level of funding would of course be far more arbitrary, but then so too is current funding for the arts and humanities.

10. To your knowledge, have there been any retrospective analyses made to systematically evaluate the nation's science programs in order to determine the ratio of projects which led to technological payoffs and those which did not? What are the inherent pros and cons of such studies? How might the discipline of history be put to use to assist in the formation of a rational, comprehensive science policy?

Numerous studies have been undertaken in the last two decades evaluating more or less directly the contribution of scientific research to technical advance. The DoD HINDSIGHT study of 1969 and NSF's TRACES study of 1968 are the best known, but there are several others equally important. HINDSIGHT and TRACES have both been criticized for methodology and for reaching foregone conclusions. More importantly, perhaps, they and most of the other studies reveal the inadequacy of the social sciences to answer the basic question of how scientific and technical advance take place. The processes are simply too varied and complex and our methodology and data are too incomplete. What are needed are more dispassionate, scholarly, comprehensive case studies, such as, for example, Edward Constant's THE ORIGINS OF THE TURBOJET REVOLUTION. If we had enough of these they might teach us only that there is no simple pattern to scientific and technical advance, but that would be valuable knowledge in itself.

11. You note that the physical sciences are "more readily quantifiable" than the life sciences and the social sciences. Is this, in your view, a condition that will continue indefinitely, or should we expect the life and social sciences to reach the same level of quantification and exactness as the physical sciences?

All oxygen atoms have eight protons and all combine with two atoms of hydrogen to form water. It is possible that one day we will know as much about cells and how they function, but that time is not yet. We are still classifying and gathering data. Our ability to predict is primitive compared to the physical sciences. In the social sciences, we are still further away from real quantification. We do not even know what justice is, let alone how to count it. What we do count in the social sciences is behavior--how people vote, what they buy, how they respond to questionnaires. Until we learn why people behave the way they do, until we produce some Newton of the social sciences, we will still function like the ancients, gazing at the stars and concocting erroneous models to describe what we cannot explain. I do not expect that Newton of the social sciences will ever appear; if he or she does, free will and life as we know it will change irrevocably.

GOALS AND OBJECTIVES OF NATIONAL SCIENCE POLICY
(With Dr. John S. Foster, Jr.)

THURSDAY, MARCH 21, 1985

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
TASK FORCE ON SCIENCE POLICY
Washington, DC.

The task force met, pursuant to notice, at 8:40 a.m., in room 2325, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. FUQUA. Our task force will be in order.

We are very pleased to have a gentleman I have known for a long time. He has had a distinguished career as the director of one of the major national laboratories, Livermore; served as Director of Defense Research and Engineering for the Department of Defense; has served as vice president of TRW; and has received many distinguished awards for his service and as a member of the National Academy of Engineering; Dr. John S. Foster, Jr.

We are happy to have you here and we will be pleased to hear from you.

[A biographical sketch of Dr. Foster follows:]

Dr. John S. Foster, Jr. was appointed to his present position in June, 1979, and is responsible for providing leadership to the company's engineering, manufacturing, and research and development activities.

He joined TRW as vice president, energy research and development, in 1973 and was named vice president and general manager, TRW Energy Systems Group, in 1976.

Dr. Foster was born September 18, 1922 in New Haven, Connecticut. He received his B.S. from McGill University, Montreal, in 1948. He received his Ph.D. in Physics from the University of California, Berkeley, in 1952 while serving as a staff member at the University's Lawrence Livermore National Laboratory in California. In 1979 he received an honorary Doctor of Science from the University of Missouri.

In 1952 Dr. Foster became a division leader in experimental physics at the Lawrence Livermore National Laboratory. He was promoted to associate director in 1958, and director of the Livermore Laboratory and associate director of the Lawrence Berkeley National Laboratory in 1961.

In 1965 Dr. Foster was named Director of Defense Research and Engineering for DoD, leading the Department through one of its most critical periods of technological development.

Between 1942 and 1945 Dr. Foster worked in the Radio Research Laboratory at Harvard University, spending 1943-1944 as an advisor to the 15th Air Force in the Mediterranean Theater of Operations. He spent the summers of 1946 and 1947 with the National Research Council at Chalk River, Ontario.

Dr. Foster served on the Air Force Scientific Advisory Board until 1956. Then he served on the Army Scientific Advisory Panel until 1958 and was a member of the Ballistic Missile Defense Advisory Committee, Advanced Research Projects Agency, in 1965. He also served, until 1965, as a panel consultant to the President's Science

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Advisory Committee. Dr. Foster is an ex officio of the Defense Science Board, presently serving as Senior Consultant.

Among the awards he has received are the Ernest Orlando Lawrence Memorial Award of the Atomic Energy Commisison in 1960; the 1971 H H. Arnold Trophy; the Defense Department's Distinguished Public Service Medal in 1969; election to the National Academy of Engineering in 1969; the James Forrestal Memorial Award in 1969; the Crowell Medal in 1972; the WEMA award in 1973; and in 1974 he received the Knight Commander's Cross (Badge and Star) of the Order of Merit of the Federal Republic of Germany. Dr. Foster is a commander, Legion of Honor, Republic of France.

He is a member of the National Conference on the Advancement of Research, American Defense Preparedness Association, Los Angeles World Affairs Council, the National Petroleum Council, Stanford Research Council, Caltech Energy Advisory Board, National Advisory Board of the American Security Council, National Security Industrial Association, American Institute of Aeronautics & Astronautics, and the committee on The Present Danger, Department of Energy, Energy Research and Development Advisory Board. He is a member of the President's Foreign Intelligence Advisory Board.

STATEMENT OF DR. JOHN S. FOSTER, JR., VICE PRESIDENT, SCIENCE AND TECHNOLOGY, TRW INC., CLEVELAND OH

Dr. FOSTER. Thank you, Mr. Chairman. I welcome the opportunity to spend a little time with you here this morning. At the outset let me just say that I had the privilege of reading your report. I must say you have taken on a very challenging task. You have raised dozens of issues. There is no way I can possibly address any of them in a substantive way this morning. However, I want to thank you for the opportunity to think about it because to me it was quite an educational exercise to read your report and then try to think about what one might do with regard to each of those issues.

Now let me come to my visit with you this morning. I have four or five points which I think might be worthwhile commenting on at the outset. The first point is that it seems to me that U.S. science and its capabilities can be steered by the Government. It can be because it has been. The example one might give depends very much on one's vantage point in looking at the last 40 years. Let me just suggest one from my vantage point an example.

First a word or two about this science and its capability This is a fantastic national asset. The United States is preeminent in science. We have some of the best people in the world, some of the best facilities. We achieved those in part because of people who came to the United States, just as many of our forefathers did. They came because of the people who were already here, because of the way we run our science program, because of the facilities we have, and because of the country we have. You probably cannot measure the contribution of each of those. Anyway, that is what makes this one of the greatest assets of our society. And that science that we have is linked in an intimate way to the technological base we have.

There is a particular characteristic of this science capability we have that I want to address when it comes to the matter of whether or not you can steer it, and how.

The essence of science is that everything can be questioned, including the method. As a consequence, we found in this country that our science community is largely self policing, and that fact has important implications about the kinds of things one might want to do when it comes to steering the scientific community.

The example to which I would like to turn has to do with the period of the sixties and seventies, the period when the United States called on the scientific community to perform the feats that we see pictured on the walls of the room here. When President Kennedy called on the scientific community to perform that task, our universities and a large fraction of our industries turned their attention from a joint effort—namely, the universities feeding their major customer, industry, with the best people and best practices for manufacturing, maintaining our industrial competitive base—turned those people to this massive challenge, and we succeeded.

In the late seventies and early eighties, the Government turned down funds to universities, and as a consequence, at least from my perspective, the universities then sought other customers, particularly industry, so in recent years we have seen a growing number of university-industrial arrangements growing up. It is coming at an important time for industry because we have, in the last decade, realized that industrial competition is now a global matter with global markets, and we find in a number of areas we have to pull up our socks if we are to be competitive in the next decade or two. Therefore, here is a priceless asset, our scientific and technological community, which was steered by the Government, and has had major consequences both to the prestige of the United States in world opinion—we won the Science Olympics—but in the process we made a major change in the orientation of the universities from industry toward Government as a customer and now more back toward industry.

My second point, it seems to me that it is important to make it easier for the Government to attract good scientists into the executive and legislative branches. Putting it the other way around, I think we have made it too hard. I remember when I first went into Government. That was the year it was decided that relocation expenses would not be borne by the Government, and the University of California paid my way.

More recently the conflict-of-interest concerns have made it more difficult to attract people to Government who have any kind of holdings in industrial concerns which might represent a conflict. And, even more recently, there are restrictions on employment one can seek after leaving the Government.

I know this is a very complicated thing but it really seems to me that the national loss from having people who are short of the very best in the country is far greater than what we gain by putting everyone through a screen to be sure there is no way he can possibly have a conflict of interest.

I think somehow we ought to trust those individuals who choose to serve to really do the best they can, and, I suppose, run the risk that occasionally some of them will do something, inadvertently perhaps, that looks as though it was in his personal interest. Therefore, I think better people will lead to wiser Government practices when it comes to steering this critical science capability.

My third point, it seems to me that there is a bridge out between Government R&D and industrial R&D, private industry R&D. Not a bridge everywhere but here and there, there is a bridge out. Let me see whether I can explain that.

In recent years it seems to me there is a Government emphasis on taking on the long-range or high-risk, high-payoff efforts, whereas industry, probably not in its best interest, has looked more at short-range and low-risk efforts. This naturally leads to a considerable gap between the kinds of efforts pursued by those in response to Government effort and those in response to private industry effort, and it raises the question as to whether industry could ever leap that gap, because if the Government research and development products are to provide a payoff they then have to be used by industry.

In my experience, in looking particularly at the energy area, it seems to me that a number of the Government efforts were too much of a stretch for industry to reach for, so the Government effort, when it was completed, lay fallow, at least from the point of view of U.S. industry. In a number of instances foreign industry picked up that lead and used it. It is not really their fault. It is half ours. We should have found a way for Government and industry to bridge that gap.

My fourth point, and this is an old one for you gentlemen, but I would agree we need to modernize our university facilities. Thirty years of the project grant system, which has a lot of advantages, seems to me to have put an awful lot of focus on the one investigator-one project approach, and that focus has turned the support away from the funding of instruments and facilities. Now, then, we now have the feeling in the community, it seems to me, that our universities lack modern facilities.

An example that came to mind recently was a visit from Professor Lange from the University of Stuttgart when he came to the company and gave a number of lectures to our people. The thing that stuck with me was recognition that in his institute they have a number of machines, a number of facilities, that are quite similar to those used in their industrial firms, whereas in our universities we seem in recent years to be able to afford only models of those industrial machines.

A fifth point, Government laboratories. It seems to me that improving the trend we have seen in the last few decades in our Government laboratories requires a joint executive-legislative branch effort. I do not see that you can improve that trend by the executive branch alone or, in fact, the legislative branch alone. Government is a customer for those laboratories. The value of the laboratory over the years depends probably more on the importance of the mission than anything else. If the mission is not very important, so probably is the laboratory.

It seems to me unrealistic to expect that the laboratory will be very effective in serving either industry or universities because its prime customer is the Government.

Therefore, if the mission is no longer important or if it no longer exists, what we might think about is building down, to take a phrase from the military side of things. If somebody wants another laboratory in some region of the country, maybe we ought to see if we can close two and build one, but we ought to face up to it.

I believe accountability is a necessary function of the Government. Perhaps it is useful to think about this accountability at perhaps three different levels. There is the individual investigator. He will always argue that it will take years, even decades, to see the results of his efforts, but it does seem to me reasonable to give him a year or two or five, but not to ask him every year how it is going.

At the program level it seems to me quite reasonable for an agency to make a review each year by a panel of experts in that area, at the field of science level. I would say every 5 years it is quite reasonable to ask the NRC, as it is now doing, to undertake a review of some major scientific field such as fusion physics, high-energy physics, chemistry, or materials.

I would also ask and suggest that the Congress and the executive branch ask the directors of the national laboratories each year what the laboratory has contributed for the last 10 years. Admittedly there are big contributions and then nothing might happen for a year or two, and then another contribution. However, over the years, if one funds a laboratory at, say, $200 million a year, you know after 10 years it is reasonable to ask, "What did we get for the $2 billion?" It is important that those people know that that question is there and that they are going to have to address it.

Finally, let me just make a general observation. Recently President Reagan called on the scientific community to look at this question of whether or not one could provide for a strategic defense. That brought to mind the thought that it may be quite reasonable for the customer, that is the people of the United States and perhaps mankind, to call on the scientific community from time to time, the international scientific community, and ask them to address some of the major problems. We have some important international problems, the weather. It is important that we understand what makes the world's weather. If there is a CO_2 question, it is important that we understand what causes the various levels of CO_2 in the atmosphere, what would be the effect as the temperature goes up at the poles, reducing the density, changing the position of the winds, and, therefore, the world's weather. These are important to this country, important to all industrial and underdeveloped countries.

Currently there is great concern about the progress of the arid lands in Africa. That is an important question not just to the people in Africa but to the United States and around the world. Have we really asked the scientific community to help us understand what causes that and how we can come to grips with it?

I am just suggesting that as long as we are funding science as a customer, it is important that we provide a little market pull on that capability, so it is not just enough to fund the technological push. One needs to provide a little market pull, so if one can do it at the international level it could be a very important contribution to international cooperation.

That is all I have, Mr. Chairman.

[The prepared statement of Dr. Foster follows:]

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Statement of

Dr. John S. Foster, Jr.
Vice President, Science & Technology
TRW Inc.

before the

Task Force on Science Policy
Committee on Science and Technology
U.S. House of Representatives
March 21, 1985

Thank you, Mr. Chairman. I welcome the opportunity to spend a little time with you with you here this morning. At the outset let me just say that I had the privilege of reading your report. I must say you have taken on a very challenging task. There is no way I can possibly address any of them in substantive way this morning. However, I want to thank you for the opportunity to think about what one might do with regard to each of these issues. I have selected six points which I think might be worthwhile commenting on.

First, however, I would like to say a word or two about our capability in sciences. This capability represents a fantastic national asset. The U.S. is preeminent in science. We have some of the best scientists and facilities in the world. We achieved these in part because of the people who were already here, because of the way we run our science program, because of the facilities we have, and because of the country we have. You probably cannot measure the contribution of each of them. Nevertheless, our ability to attract them is one of the greatest successes of our society. And it is our strong capability in science is linked in an intimate way to the technological base we have.

The essence of science is that everything can be questioned, including the method. As a consequence, we find in this country that our science community is largely self-policing, and that fact has important implications about the kinds of things one might want to do when it comes to steering the scientific community.

This brings me to my first point, which is that U.S. science and its capabilities can be steered by the Government. It can be because it has been.

The example one might give depends very much on one's vantage point in looking at the last 40 years. Allow me to suggest one from my vantage point. It has to do with the period of the sixties and seventies, a period when the President called on the scientific community to perform the feats in space that we see pictured on the walls of this room. When President Kennedy called on the scientific community to perform that task, our universities and a large fraction of our industries turned their attention away from their joint efforts of maintaining our industrial competitive base. Many of the best people and best practices that were being provided by universities to industry were directed to this massive challenge, and we succeeded.

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Then in the late seventies and early eighties, the Government began limiting funding to the universities. As a consequence, at least from my perspective, the universities sought other customers, particularly industry. Consequently, in recent years we have seen a growing number of university-industrial arrangements being established. These are coming at an important time for industry because in the last decade we have realized that industrial competition is now a global matter involving global markets. We find that in a number of areas we have to pull up our socks if we are to be competitive in the next decade or two.

In this example, we find a priceless asset, our scientific and technological community, being steered by the Government with major consequences to the prestige of the United States in world opinion. We won the Space Olympics, but in the process made a major change in the orientation of universities from industry toward Government as a customer and now are steering it back again more toward industry.

My second point has to do with the importance of Government making it easier to attract good scientists into the Executive and Legislative branches. Putting it the another way around, I think we have made it too hard. I remember when I first went into Government. That was the year when it was decided that relocation expenses would not be borne by the Government, and the University of California paid my way.

More recently, conflict-of-interest concerns have made it more difficult to attract top people to Government who have any kind of holdings in industrial concerns which might represent a conflict. And, even more recently, restrictions on employment that one can seek after leaving Government have been tightened considerably.

I know this is a very complicated issue, but I believe that what the nation loses from having people who are short of the very best in the country is far greater than what the nation gains by putting everyone through a screen to be sure there is no way he or she can possibly have a conflict of interest.

I believe we ought to trust those individuals who choose to serve to really do the best they can. I suppose this will run the risk that occasionally some of them will do something, inadvertently perhaps, that looks as though it was in his or her personal interest. Nevertheless, I think better people will lead to wiser Government practices when it comes to steering our nation's critical science capability.

My third point is that here and there a bridge is out between Government R&D and private industry. Let me see if I can explain that.

In recent years, Government emphasis has been directed toward taking on the long-range, high-risk, high-payoff efforts, whereas industry, probably not in its best interest, has focused more on short-range, low-risk efforts. This naturally leads to a considerable gap between the kinds of efforts pursued by those responding to Government efforts and those responding to private industry efforts. It raises the question whether industry can ever leap that gap. If Government research and development products are to provide a payoff they have to be used by industry.

In my experience, looking particularly at the energy area, a number of the Government efforts were too much of a stretch for industry to reach for. Therefore, when these efforts were completed, the result lay fallow, at least from the point of view of U.S. industry. In a number of instances foreign industry picked up the lead and used it. It is not really their fault. It is half ours. We should have found a way for Government and industry to bridge that gap.

My fourth point, which is an old one for you gentlemen, has to do with modernizing our university facilities. Thirty years of the project grant system, which has a lot of advantages, has put a lot of the focus on research support to the one investigator - one project approach, and that focus has turned support away from the funding of instruments and facilities. We now have a feeling in the community that our universities lack modern facilities.

An experience which comes to mind was a recent visit from Professor Kurt Lange from the University of Stuttgart who came to our company to give a number of lectures to our people. What stuck with me was the recognition that his institute has a number of machines and facilities that are quite similar to those used in West German industrial firms, whereas in our universities in recent years we seem to be able to afford only models of those industrial machines.

My fifth point refers to Government laboratories. It seems to me that improving the erosion trend in our Government laboratories we have seen in the last few decades requires a joint Executive-Legislative branch effort. I don't believe that you can improve that trend by the Executive branch alone, or, in fact, the Legislative branch alone. Government is a customer for those laboratories. The value of these laboratories over the years probably depends more on the importance of their mission than anything else. If the mission is not very important, so probably is the laboratory.

It seems unrealistic to expect a Government laboratory to be very effective in serving either industry or universities when its prime customer is the Government. Therefore, if the mission is no longer important or if it no longer exists, then we might think about building down, to take a phrase from the military. If somebody wants another laboratory in some region of the country, maybe we ought to see if we can close two and build one, but we ought to face up to it.

My sixth point is that I believe accountability of investments in science is a necessary function of the Government. Perhaps it is useful to think about accountability at three different levels. The first level involves the individual investigator. He will always argue that it will take years, even decades, to see the results of his efforts. It seems reasonable to give him a year or two or five, but not to ask him every year how it is going.

At the program level, however, it would be quite reasonable for an agency to make a review of that program each year by a panel of experts. Finally, at the field of science level it is quite reasonable every five years or so to ask the NRC, as it is now doing, to undertake a review of such major scientific fields as fusion physics, high energy physics, chemistry, materials, etc.

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I would also like to suggest that the Congress and the Executive branch ask the directors of national laboratories each year what the laboratory has contributed over the past 10 years. Admittedly, major contributions may come about only after intervening periods of two or three years. However, over ten years, if one funds a laboratory at $200 million a year, it is reasonable to ask, "What did we get for the $2 billion?" It is important that directors of laboratories know that that question is going to be asked and that they are going to have to address it.

Finally, let me make a general observation. Recently, President Reagan called on the scientific community to look at the question of whether or not one could provide for a strategic defense. That brought the thought to my mind that it may be quite reasonable for the customer, that is the people of the United States and perhaps mankind at large, to call on the international scientific community from time to time to address some of the major problems facing the world. We have some important international problems facing the world. It is important that we understand what makes the world's weather. What would be the effects if the temperature goes up at the poles of reducing atmospheric density, changing the position of the winds, and therefore, the world's weather? If there is a question about CO_2, it is important that we understand what contributes to the various levels of CO_2 in the atmosphere. Currently, there is also great concern about the progress of arid lands in Africa. Such problems are important not only to the United States but also to all the other nations of the world. Have we really asked the scientific community to help us understand these problems and suggest how we can come to grips with them?

I am suggesting that as long as the Government is funding science as a customer, it is not enough to fund just the technological push. One needs also to provide a little market pull, if one can do it at the international level it could result in very important contributions to international cooperation.

That is all I have, Mr. Chairman.

DISCUSSION

Mr. FUQUA. Thank you, John, for a very thoughtful statement.

As to the last part of your statement, I recall a specific example a couple years ago of an incident at Boulder. They just concluded a series of studies on wind shear which I thought was very important. I asked them whether they had shared that information. Nobody had asked for it. I wanted to give that study particularly to the Department of Defense because I thought it was important for flying and teaching. And the response of the Department of Defense was, "Don't call us, we'll call you." Here is something that affects lives, and yet if we add up for a year the number of flight hours the Department of Defense has—not only that but mercial and general aviation communities—you wonder how you lead the horse to water and make him take sustenance once he gets there. That is one of the problems.

You mentioned Africa. We have a lot of work going on in meteorology in basic research throughout our colleges and universities. Maybe we should direct our efforts more toward problems you identified as well as in the African continent. Do you have an answer to that? Do you have a thought about it, perhaps?

Dr. FOSTER. Mr. Chairman, not much more than to suggest that there is a tendency to be preoccupied with whether or not to fund this, that and the other request at the expense of some others inasmuch as there is only a limited number of dollars available. I am suggesting there is another side of the coin, and that is what do we want this capability to do for us? Perhaps we don't think enough about whether or not a capability can help people who are paying the bill, and in return perhaps we can provide some money to those who would like to examine this or that area of science.

Mr. FUQUA. You mentioned the national labs. You have a vantage point, having been the director of one, and working perhaps as a user in the Department of Defense and also with industry. One of the concerns we have had is that we recognize we have a great asset in our national labs, a great resource. I am convinced we are not making adequate and full use of that for industry as well as academia. It is like pulling teeth to try to get the labs opened up so the graduate students can perform work there which industry can utilize. It has been a very difficult thing.

I know David Packard headed up a committee for the President. I met with Mr. Packard several times. I think he made some very good suggestions about that.

Dr. FOSTER. Yes, sir.

Mr. FUQUA. Do you have any suggestions about that? How do we make them function more fully for the support of society's needs as a whole?

Dr. FOSTER. I have perhaps given up a little on this. My view is that the first and most important aspect of this is to make sure that the laboratory has an important mission. If it has a very important mission, then I would argue the next thing to be sure it does not stray off into half a dozen other fields. However, if it does not have a mission, then we had better find one or close it.

In the process of looking over that range of things, I realize that a number of thoughtful people have suggested that there is a great facility and a good opportunity for graduate students and a great opportunity for the universities to become involved and for industry to become involved. I am trying to suggest that from my vantage point that is a kind of 10 percent thing. These laboratories are very, very expensive. They are great national assets, and as the customer, the agency and Government share the responsibility to be sure that the purpose of the laboratory is really there and is important. If it is not, they share the responsibility to see what to do about it.

Mr. FUQUA. When Vannevar Bush studied the conclusions made 40 years ago, he stated that it was important to have Government involvement in basic and applied research. However, if Government became too dominant in the field, then you might impede the interest of private industry also to fund that type of project. Do you see that imbalance today? How do you perceive that as functioning today?

Dr. FOSTER. First, Mr. Chairman, I agree that one needs to have a balance in both Government and industrial involvement in that research effort, that national research effort. I believe it was imbalanced in the late sixties and early seventies, but it is hard to make a decision as to whether that was right or wrong. We were responding to a call, a national call.

I believe it is now coming back into balance, and it seems to me that the Government, in reducing the funds to the universities and industry picking up joint arrangements with the universities, is moving in the right direction. At the moment it is probably industry which is lagging a bit. It needs to be galvanized a little more in its own interest in hooking up with the universities.

Mr. FUQUA. You think the R&D tax credits have attracted more industry funds into that effort?

Dr. FOSTER. It certainly has helped some, sir. I don't know how much is due to the R&D tax credit but it has helped.

Mr. FUQUA. Thank you.

Mr. Lujan.

Mr. LUJAN. Thank you, Mr. Chairman.

John, you make a very interesting point combined with some others we have heard. Let me tie them together as far as policy is concerned and what we are to do about it. You talk about Government steering the whole course of science. That is true because we fund universities who do a particular kind of research, the laboratories with specific missions and in contracting with the various companies to get done what we want done, so we do steer.

Dr. Pimintel, our first witness, said we are the envy of the world in our scientific endeavors, and if it ain't broke, don't fix it. Leave it alone; it is functioning.

If you could sort of summarize this whole thing as to whether we need to change our science policy or merely define it a little clearer so that we understand what we are doing, looking at it from the standpoint of whether we should be more interested in end use or knowledge and just research, a Nobel box score and that sort of thing, and the long term versus the short term, how are we better off? Is it by changing it or simply redefining the definition or steering us into a new course?

Dr. FOSTER. Mr. Lujan, the question at first blush seems to me to be a little too tough.

Mr. LUJAN. That is what we are trying to do with this Science Policy Task Force. What do we do? I am asking you the same question.

Dr. FOSTER. I understand, but you will have 2 years and I have 2 minutes. I understand Dr. Pimentel's thought about if it ain't broke, don't fix it. There is a problem, however. I don't believe it is busted, but I am not convinced it is not bent. It might be bent a little here and there. Because it is such an important asset, because it can be so valuable to our future, it seems to me we ought to see that if it is bent in the wrong direction we ought to do something about bending it in the right direction. I believe that is important.

An important thing, perhaps, is to recognize that our scientific capability is so pervasive in hundreds of large universities, hundreds of large industries, that it cannot move left or right on an instant's notice. It in fact takes years, perhaps even a decade, to make a rather major change. If that is the wrong direction, it then has to take decades to come back. The cycle inside a university is like a decade, so it is very important to do in fact what the description of your purpose says; namely, take a very careful study before one chooses to make even a slight change in course or a slight change in practice.

I have suggested that it is perfectly reasonable for the Government to expect an accounting for the expenditures. And I have suggested it is important for the Government to think about what it would like out of those expenditures as well as to review what the scientific community offers and wishes to do. The combination of those two things perhaps represents our best interest. I have suggested there is an important international aspect here. We have magnificent tools to look at a number of big problems and we have a lot of brains. But there is a real opportunity here in these international problems to use not only our tools but others, and not only our brains but others, and together the challenge between these nations is a very healthy challenge. We will get more for our investment.

Mr. LUJAN. You discussed the laboratories with the Chairman. There is one area where we have complete control over what we will pursue by the method of funding. Those discretionary funds that the directors have, are they used primarily for specific short-term projects or are most of them used for pure science? What use is made of those discretionary funds generally in your own laboratory and in the others?

Dr. FOSTER. It has been 1965 since I was director of the laboratories so I am not sure to what degree my experience at that time relates to what is going on now. Let me make two or three points about the matter of discretionary funds for the director.

First of all, I think they are important. We ought to have them. How much in terms of percentages is not a good guide. It is more in the nature of what is going on in the laboratories. In my experience the funds were extraordinarily useful to take care of exciting opportunities, immediate problems, where you knew that this was something that a laboratory really ought to do with a small amount of effort, a really important thing to examine, or this is a problem which has to be taken care of right now, the thing to do was to put the people on it, and at the same time go and explain to those to whom you report, if it looked like it was going to be a rather substantial amount of money, to gain their understanding of how you are managing the program and their understanding of what was going on. Without that, I think you begin to deny the management of the laboratory's sense of responsibility to do the right thing.

Mr. LUJAN. Not define it to the point of where we want to shape what they are going to do? That is what you are saying?

Dr. FOSTER. Yes.

Mr. LUJAN. I agree. I was curious as to where most of it goes, whether to put out the fire or really to do some substantial building.

Dr. FOSTER. My experience was with relatively short-term money because next year, if it turned into a relatively large opportunity, there would be a description of the program or if a problem remained there would be a description of the problem, so it is a sort of 1-year thing.

I do feel strongly not only in having that flexibility but also in holding the manager accountable.

Mr. LUJAN. Thank you.

Mr. FUQUA. Mr. Brown.

Mr. BROWN. Dr. Foster, you raised a number of interesting policy areas that we still do not seem to be able to get a handle on. In the third point you made in your list you talked about the bridges between Government and industry in terms of the scientific research programs not being adequate, and you suggested that in part it was because of the Government's focus on the long range, more fundamental research, and industry on the opposite end of the spectrum and a gap in between.

We observed that over a number of years but we have not been able to come up with a solution to that problem which is adequate except in a few cases. We seem to have done a little better job of solving that in the agricultural field where from the beginning we created the whole spectrum.

Dr. FOSTER. Right

Mr. BROWN. And we did not seem to have a fear that Government was going into the agricultural business in competition with the farmers, but we do have that in industry to a considerable extent. There is almost a paranoia that if the Government gets involved in that place in the middle of the spectrum where there is a potentially large commercial payoff that that is inappropriate. We are grappling with that right now in the space program. We want to divest the Government from the Land Satellite Program, but we are not doing a good job of it.

The question is, How do we provide the long-range research, transition it to the stage where it really does have commercial application, and take care of it in adequate fashion so it becomes part of our commercial-industrial base?

We even made mistakes in terms of the communications satellite. We spun it off at Comsat. We got out of the basic research when we should not have, and then we got back into it again.

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How do we cover that policy area in an adequate fashion, minimizing the distress it seems to have caused, and recognize that our ultimate goal—and I know of no disagreement on it—is a healthy, full-fledged, private enterprise operation of those programs which are important, but with an adequate transition to that situation where it is necessary?

Dr. FOSTER. Mr. Brown, I think you understand the problem better than I do. To me what you said would seem to be rather key, this business of a continuum that we have in agriculture, an ongoing continuum. Some of those programs you are dealing with, which seem to be problems, are not ongoing, however. They are a project, admittedly very expe ive project.

One thought that might b seful to think about is the example we have in the Defense Advance Research Projects Agency created right after Sputnik to guard against future Sputniks. I recall setting up, as a matter of policy, a requirement that when DARPA wanted to get into something they had to have a plan at the outset of how they were going to get out of this. There were two ways out of this, because the program was only a few years long. Either it was to stop or it would be picked up and continued by an operating service. Therefore, from the outset, the service was brought in, like the Air Force or the Army or Navy, they were brought in in the planning stage so it was aware of what it was DARPA had in mind—the big far-reaching, exciting high-risk, high-payoff project—and realized that the service had to decide whether to pick it up and continue its funding or express no interest, in which case the project would die. It seemed to me that recognition from the outset, that termination date was there and those alternatives were clear, helped to make the program more effective and to bring about the transition in the most sensible way.

Perhaps we have not found the best way to involve industry at the outset. Maybe we have not made it a requirement for industry to be involved in a serious way at the outset.

Mr. BROWN. Obviously part of the problem has been that situation we describe as the adversarial relationship.

Dr. FOSTER. Right.

Mr. BROWN. Which has been fairly common between industry and Government.

Dr. FOSTER. Right.

Mr. BROWN. And I think we see signs it has been somewhat ameliorated.

Dr. FOSTER. That is right.

Mr. BROWN. But we have not yet gotten over the hurdle. DARPA has a major situation in the computer communications field where they are funding a long-range supercomputer program, and they will have to get out of it at some point.

Dr. FOSTER. That is right.

Mr. BROWN. After they have met the needs of their own customers, when they have been convinced private industry will pick it up and meet the needs of customers in that way.

Dr. FOSTER. Yes.

Mr. BROWN. What I am talking about here is some focus of decisionmaking which will decide—has DARPA done enough in this area? Is it being transitioned adequately? I am not confident we are making an adequate judgment in that area. Maybe DARPA is doing better than average, I don't know. However, there are other areas where I know we are not doing as well as we should be doing. I guess you don't need to answer that question.

Let me take up one additional point. You sort of offered up a challenge that we ought to do something to stimulate the worldwide scientific community to look at the big problems; you mentioned the weather, and so forth. We tried to do that. It has been 15 years ago that the scientific community said we ought to take a big look at global weather. We ought to have an overall program. We enacted the legislation to do that. It has been a flop, frankly. It has not been a complete failure, but certainly it was not as adequate as it should have been.

The reason for that is that I think there are ebbs and flows in the appreciation of what is a big global problem. What is attractive? CO_2 was in all the headlines for a long time. I have not seen a word about it recently. You can name other large scientific areas in the same category.

We tried to institutionalize the process of keeping these in the forefront. We asked, when we set up the Science and Technology Policy Act, that there be a mechanism where we take a 5-year outlook, and we do this every year, to keep elevating the high priority problems. Yet that seems to have faded in terms of the effectiveness. I think the reason that has faded is because we did not conceive of the entire process. To be effective, that needs to permeate not only the scientific community but the public because the public makes the final decisions as to what is important enough to budget another $100 million, for example. I do not think we have done that. Can we do a better job in that area?

Dr. FOSTER. Obviously we can. Obviously we have to. I agree with you that it is a matter of doing a more complete job rather than just asking questions once a year, laying the plan of expectation, and that the complete list and consideration of each of the elements in that list will be examined in some detail. The expectation of that and funding for efforts depend on satisfaction in analyzing and reviewing each of those elements. Then it seems to me we will do better.

Mr. BROWN. I hope so.

Mr. FUQUA. Mr. Lewis.

Mr. LEWIS. Dr. Foster, let me apologize for missing your early presentation. I am interested in your opinion, and you may have covered this, whether we are observing a degradation of our scientific faculty and community, and what should the role of Government be in relation to the private sector to try to upgrade our scientific community if we are, in fact, falling behind?

Dr. FOSTER. Mr. Lewis, I don't know really whether our scientific community is improving in quality or degrading, really. One thing is probably true, and that is that our role, compared to that of other nations around the world, probably of necessity will decrease as other nations make investments in science and technology, nations that have not done it in the past, as they go from near zero to some very small amount by our scale. We no longer have the dominant and commanding position we once had. That isn't bad. In fact, that is probably good.

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I am aware that there are a number of people who are concerned because on the faculties of U.S. universities and in the graduate student program there are more and more people from other nations, and this seems to be of concern. It is not clear to me that that is bad. You know, the fact is that this is a nation of immigrants, and it is what made us great. The people who come to this country are generally the very best, and we have done well because of that in the past. Therefore, I am not so sure that we should take action to prevent those who come to us from other countries from getting a first-class education.

We should also make sure, of course, that those who would like to get a first-class education in this country can do so.

Mr. LEWIS. One final question on this. Do you feel we should be looking at upgrading our disciplines? Are we in need of more Ph.D.'s and more masters in engineering and science than we are bachelors?

Dr. FOSTER. I don't believe I am the best witness on that question, Mr. Lewis. I think obviously it depends very much on which field you are talking about.

Mr. FUQUA. How about coming from industry? Does your company have enough engineers?

Dr. FOSTER. It is my job in TRW to ask those kinds of questions, and having been at one time a scientist, and so on, I asked that question. I have some expectations. You see, I feel we can always be better off if we had more Ph.D.'s, and so on.

I find that in some areas the answer is yes, we would like to have more Ph.D.'s, and in other areas the answer is no, we don't think we would be better off with more Ph.D.'s. We need more people, for instance, who have a B.S. or an M.S. in mechanical engineering.

More recently, what we have been trying to do is to encourage the universities to turn out students who are trained in manufacturing. There was a tendency during the sixties for the universities to turn from what you would call engineering practices to engineering science. When they made that turn, then the product from the university going into industry was much more interested in the scientific aspects of industry than they were in, say, the manufacturing aspects of industry. That hurt us. Now, then, since we are in this global challenge, and competitiveness is the name of the game, we are looking for people who really understand manufacturing. Yes, then, there is a shift there now, and a number of universities are beginning to go back and examine what it takes to provide excellence in manufacturing.

Mr. LEWIS. Thank you.

Mr. BROWN. If I may follow up on that.

This condition of getting adequate supplies of trained manpower seems to follow curves.

Dr. FOSTER. That is right.

Mr. BROWN. There is a shortage of good nuclear engineering right now.

Dr. FOSTER. Yes.

Mr. BROWN. It used to be an exciting field and many people wanted to get into it. The same thing influenced electric utilities and water utilities. Yet today these are becoming high national pri-

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orities, to get people into these fields who understand the problems, which are changing very rapidly after having been static for so many years. Of course, we assume that the normal forces of supply and demand will meet this, but it does mean a 10-year lag or something.

Dr. FOSTER. Exactly.

Mr. BROWN. We need more foresight to reduce the lag and not substituting some fiat but substituting a little smarter strategic planning or something of that sort.

Dr. FOSTER. Yes.

Mr. BROWN. I wondered about whether we should address that problem in some fashion as part of our science policy. I have not seen a good handle on it. I bring it up because perhaps you can think of a better way we can reduce that lag as problems become urgent and yet we do not have the trained manpower and do not develop it for a number of years, although ultimately we do—and then we get a surplus for a while.

Dr. FOSTER. I agree with everything you say. It is just a fact. Despite the excellence in our educational process and understanding of the supply and demand law, we manage to go through the most violent cycles decade after decade—dire predictions of shortages only to be faced with surpluses within a matter of 10 years. This is such an expensive thing in terms of the lives of individuals that it probably deserves a little more attention than we have been giving it. I was delighted to see, in reading your study report, that you plan to examine what has happened here historically and try to get a handle on the mechanisms which drive this.

Obviously there are very different constants involved. When industry finds that it would be useful or necessary to go into a certain field, it then imposes a very high demand. It can do that in a matter of months. Yet it can take the universities 4 to 8 years to respond, so you have a mismatch in the time constants.

There is another alternative to this and that is to retrain, to transfer over from one particular adjacent field into the one that is in need. Perhaps when we see these crises, we do not first turn to that alternative and arrange training procedures to permit a more rapid response.

Mr. BROWN. A closer coupling between the universities and industry will help.

Dr. FOSTER. Yes.

Mr. BROWN. You commented we are moving toward that.

Dr. FOSTER. Yes.

Mr. BROWN. We are looking at that in this study, the longer-range demographic trends which bear on this. It is a separate curve, but it relates to the changing needs of industry in a very important way sometimes.

Thank you.

Mr. FUQUA. Dr. Foster, thank you very much. We appreciate your sharing your time with us. Your contribution has been very valuable. You have given us the benefit of your thoughts which are important to us.

Dr. FOSTER. Thank you, Mr. Chairman. I enjoyed being here. I learned a lot.

[Whereupon, at 9:40 a.m., the task force recessed, to reconvene Thursday, March 28, 1985.]

[Answers to questions asked of Dr. Foster follow:]

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QUESTIONS AND ANSWERS FOR THE RECORD

Dr. John S. Foster, Jr.

QUESTION #1

In your view, should one of the goals of government science policy be to achieve and maintain, as a matter of national prestige, U.S. leadership across the spectrum of science, or should we share or yield leadership in some areas of science to other countries?

ANSWER #1

I believe it would be inappropriate for the U.S. to seek predominance across the entire spectrum of science solely for the purpose of national prestige. However, I believe we should pursue science to serve human and national needs, and to do so we should seek and maintain the capability of making pathbreaking discoveries across the entire spectrum. In some areas we should maintain scientific leadership as a matter of national security and welfare; such as national defense, energy, food production, health and medicine, natural resource development, weather prediction and control, etc.

QUESTION #2

There has been much emphasis on the need to maintain U.S. leadership in science across a broad front in order to allow the U.S. to remain strong in technology and international trade. Yet some countries with strong science, such as England, have fallen behind economically, and some countries with a weak science base, sucn as Japan, have surged ahead economically. What is the relationship between national and international strength in science and economic strength?

ANSWER #2

National strength in science needs to be balanced with strengths along the entire technological chain if scientific discoveries are to be translated into useful products and services. With today's speed of communications, scientific discoveries and refinements are transmitted around the world almost instantaneously. The nation best positioned to capitalize on scientific potentials for their "downstream" technological and marketing developments will reap the benefits in terms of economic development and international trade. We need to constantly examine if any bridges are out along our own scientific-technological chain in order to be first to benefit from our strong scientific base.

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QUESTION #6

The current Administration has shifted the principal rationale for government funding of research. Instead of emphasizing the technological pay-off, the stress has been on the training of a new generation of scientists as the principal benefit yielded by research grants. In your view, how many scientists do we need in the coming decades and to what extent will the current levels of research funding meet that need?

ANSWER #6

I have no hard data upon which to respond to this question. However, I am of the opinion, based on current recruiting trends, that the numbers of engineers and scientists being matriculated in the fields of electronics, computer sciences, and communications are running far short of demand. Furthermore, the disparity between supply and demand will likely increase in coming decades. The choke point is generally attributed to an insufficient supply of professors for educating the needed talent. I believe that if increased research support could have the direct effect of increasing the number of available faculty in these fields some of the expected shortfalls in talent could be corrected.

QUESTION #7

In your experience, is the problem of foreign-national scientists and engineers working in industry on government contracts a serious one today?

ANSWER #7

We find it very difficult to employ foreign-national scientists and engineers in our government contract research because of the necessary restrictions of physical access and requirements for securing clearances. I assume it is not a serious problem for industry at large engaged in non-classified government work.

QUESTION #8

Industry has always provided modest amounts of funds for specific research projects by university professors. Recently, this has received increased attention and some growth of funding. Under what circumstances does industry elect to provide such support? Should government policies and incentives be changed to influence the types and levels of such funding?

ANSWER #8

I believe that much of the recent growth in government-university cooperative support of university research has been tied by industry to the recruiting of top talent in selected areas and good will. Therefore, the degree to which this type of support can be further leveraged by government policies and incentives is probably limited. Tax incentives which will further increase industry investments in that university research focused on industry's needs could result in some additional funding growth.

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QUESTION #3

In the last few years the Defense Department has resumed a stronger role in funding university research. This has met with support from those arguing that support from all possible sources, including all the mission agencies, must grow, and with concern from those arguing that a growing military presence on campus is undesirable. What is your view of DoD support of university research?

ANSWER #3

The DoD is dependent on the best scientific minds in the country to assist in developing those defense technologies essential to our national security. In our society most of these great scientific minds reside at universities and colleges. Generally, research sponsored by the DoD at these universities and colleges is basic or applied research suitable for graduate research dissertations and publications. I believe this research is appropriate and necessary. Most concerns in recent years have arisen over research that involves publication restrictions in the interest of classification or technology transfer controls. Generally, research of this nature is more appropriately conducted at off-campus research institutions, industry, and DoD laboratories.

QUESTIONS #4

It is well recognized that the potential payoff in medicine or technology from an individual research project can not be predicted. However, we also know that broad fields, such as chemistry, yield significant practical benefits. To what extent can and should the expectations of such payoff be used to determine the levels of funding for science and for the individual disciplines?

ANSWERS #4

I believe that the quality of research, ability to perform, and scientific integrity are the principal criteria to apply to the sponsorship of university research. However, "expectation of utility" would be an additionally useful determinate in setting levels of funding for the engineering science disciplines.

QUESTION #5

In discussions of the government science budget, much stress has been placed on providing new funds for new initiatives in emerging areas of scientific promise. Why should we not expect a comparable group of areas within each discipline which have "peaked" or been "mined out" and where consequently some funding decreases can be made?

ANSWER #5

I believe that in a budget-constrained environment, peer and internal reviews practiced not only by the NSF but also by other government research granting agencies will cull out many of the research initiatives which have "peaked" or are "mined out". Periodic program reviews by external expert reviewers are also helpful in closing down programs that are no longer productive in order to make room for new initiatives.

GOALS AND OBJECTIVES OF NATIONAL SCIENCE POLICY
(With Dr. James B. Wyngaarden)

THURSDAY, MARCH 28, 1985

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
TASK FORCE ON SCIENCE POLICY,
Washington, DC.

The task force met, pursuant to notice, at 8:33 a.m., in room 2318, Rayburn House Office Building, Hon. George E. Brown, Jr., presiding.

Mr. BROWN [acting chairman]. The task force will come to order.

We are very pleased to have with us this morning to discuss the important subject of science policy, Dr. James B. Wyngaarden, who is the Director of the National Institutes of Health. He has had a distinguished career in the health sciences and in policy issues with regard to science in general.

We are delighted that you could be with us to participate in this exercise, Dr. Wyngaarden, which I am sure you understand is a rather lengthy effort to review where we stand and where we are going in the general area of science policy and see whether we can sort of reevaluate the status of science today and perhaps 25 years after to see whether there is some course and direction we might follow.

This will not be formal. I would like to have you take as much time as you would like to present your own ideas. Then we will have a little discussion with you on these matters. Other members will wander in as you proceed.

Welcome to our meeting this morning.

Our ranking Republican member, Mr. Lujan, is here.

Mr. LUJAN. I have nothing to say at this point except I am glad to see Dr. Wyngaarden.

[A biographical sketch of Dr. Wyngaarden follows:]

Wyngaarden, Dr. James B., Director, National Institutes of Health,

Born.—October 19, 1924. East Grand Rapids, Michigan.

Education.—Calvin College, 1942-43; Western Michigan University, 1943-44. M.D., University of Michigan Medical School, 1948.

Professional History.—1948-52, Intern and Resident, Massachusetts General Hospital, Boston 1952-53, Visiting Investigator, Public Health Research Institute of the City of New York, New York. 1953-54, Investigator, National Heart Institute, NIH, 1954-56, National Institute of Arthritis and Metabolic Diseases, NIH. 1954-56, Clinical Instructor in Medicine, George Washington University, Washington, D.C. 1956-59, Associate Professor of Medicine, Duke University Medical Center, Durham, North Carolina. 1959-61, Associate Professor of Medicine and Biochemistry, Duke University. 1961-65, Professor of Medicine and Associate Professor of Biochemistry,

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Duke University. 1963-64, Visiting Scientist, Institut de Biologie-Physiochemique, Paris. 1965-67, Frank Wistar Thomas Professor and Chairman, Department of Medicine, and Professor of Biochemistry, University of Pennsylvania School of Medicine, Philadelphia. 1965-67, Physician-in-Chief, Medical Service Hospital of the University of Pennsylvania. 1967-82, Frederic M. Hanes Professor and Chairman, Department of Medicine, Duke University. 1967-82, Physician-in-Chief, Medical Service, Duke University Hospital. 1981-82, Chief of Staff, Duke University Hospital.

Professional Organizations.—American Academy of Arts and Sciences, American Association for the Advancement of Science, American Board of Internal Medicine, American Clincal and Climatological Association, American College of Physicians, American Federation for Clinical Research, American Rheumatism Association, American Society for Clinical Investigation, American Society of Biological Chemists, Association of American Physicians, Endocrine Society, Institute of Medicine, Interurban Club, National Academy of Sciences, Southern Society for Clinical Investigation, Sigma Xi, Council of the Government-University-Industry Research Roundtable.

Honors, Awards.—University Scholar in Professional Schools (Medical), University of Michigan, 1946. Alpha Omega Alpha (University of Michigan), 1947. Cum laude with First Honors, University of Michigan, 1948. Dalton Scholar in Medicine, Massachusetts General Hospital, 1948. Consultant to the Durham Veterans Administration Hospital, 1956-65 and 1967-82. Honorary Membership in the Italian Society of Rheumatology, 1961. Consultant to the Philadephia Veterans Administration Hospital, 1965-67. Consultant to the Office of Science and Technology, Executive Office of the President, 1966-72. Sesquicentennial Award, University of Michigan, 1967. Appointed to the President's Science Advisory Committee, 1972. Consultant to the Food and Drug Administration, 1972-73. Modern Medicine Award for Distinguished Achievement, 1974. Election to the National Academy of Sciences, 1974. North Carolina Governor's Award in Science, 1974. Appointed to the President's Committee for the National Medal of Science, 1977-80. Founder's Medal Southern Society for Clinical Investigation, 1978. The John Phillips Memorial Award American College of Physicians, 1980. Honorary Membership in the Socied d Medical Santiago de Chile, 1981. Fellow of the Royal College of Physicians of London, 1984. Distinguished Alumuns Award, Western Michigan University, 1984.

STATEMENT OF DR. JAMES B. WYNGAARDEN, DIRECTOR, NATIONAL INSTITUTES OF HEALTH, BETHESDA, MD

Dr. WYNGAARDEN. Thank you.

I appreciate this opportunity very much and commend the committee for this undertaking of a very important subject.

I thought I might begin with a number of historical references, perhaps well known to everyone, but nevertheless I cannot resist pointing out we are approaching the 100th anniversary of the National Institutes developed in 1887. We trace our origins to a one-room laboratory on Staten Island set up in 1887, in what was then the Marine Hospital Service, designed primarily to address problems of infectious disease of immigrants and merchant seamen. The problems that dominated the scene then were typhoid and cholera. Four years later that laboratory was moved to Washington and had several locations in the District before eventually being moved out to Bethesda in the thirties.

During the Second World War, as you know, many parts of the Nation were mobilized for the war effort, including many university scientists who participated in contract research of value to the military. After the war those contracts were moved to the NIH to be administered.

In 1944, as a consequence largely of the Bush Report, the Public Health Service Act created the National Institutes of Health and combined two laboratories previously really unrelated. One was the National Cancer Institute that had been started in 1937, and the other was this descendant of the Staten Island Laboratory, which at that time was called the National Institute of Health and was largely concerned with infectious disease problems. The National Institutes of Health was born then. In the subsequent several years additional institutes were added.

Essentially, when Dr. Shannon became Director in 1955, the total budget for the NIH was in the neighborhood of $98 million. When he retired 13 years later, it had passed $1 billion. During that 13-year stretch the average rate of increase in purchasing power was 24 percent per year. About 1965 or 1968 that leveled off, and when the budget passed $1 billion, there was a fairly extensive congressional review of the NIH activities, headed by Congressman Fountain from North Carolina, and the budget since that time has grown at a much slower rate. In fact, the overall rate of growth from 1968 through 1984 was 2 percent per year in purchasing power, so you can see the NIH has had definite phases to its growth—very slow growth. It was still a very small Institute in the 1940's, and then it had a remarkable period of growth and much more of a steady status in the Institutes for the past 15 years.

During those days of expansion from, say 1950, a number of important principles were established. One of these is the peer review system, which developed in two phases. One was the initial review by a disciplinary study section for committees that would evaluate grants for technical merit—scientific merit, and feasibility.

Then a second level of review was made by the councils of the individual funding Institutes, which looked once again at the decisions of the study section but considered other issues as well—policy issues, program relevance of the proposed research, geographical distribution, and other matters of that sort. That two-tiered peer review system has stood the test of time very well. It has been emulated by many other groups around the world.

The primary mission of the National Institutes of Health as defined in the 1944 legislation is to conduct research of potential benefit to the health of the American people, and that has been our overriding sense of mission ever since. It has some corollary features, one of which is to supply training for the scientists who conduct this research. Since a pattern developed that 80 percent of this research is done through grants and contracts to university scientists, the work has been predominantly conducted not in national laboratories but in academic settings. Corollary features are those which concern the infrastructure; that is, the adequacy of facilities, including the equipment used in the laboratories. Those four factors have been major features; that is, the support of the research project itself, support of training, support of the equipment, and support of facilities.

We use other mechanisms to accomplish our work, but the bulk of the work is still done through the project grant mechanism, but we employ contractors from time to time. Those are really questions of whether a proposal may involve a product to be acquired or work to be conducted to produce a specified result, in which case a contract may be useful. We use contracts for clinical trials where we control the multiinstitutional activity.

However, the bulk of our support is in the research project grant which is viewed not as a contract but as a grant in aid to enable the scientist or a group of scientists to pursue the ideas which they

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have. They are clearly structured along a defined and predicted line, but there is a great deal of flexibility built into that, because in the case of the biological sciences more than in other sciences, we are much more dependent upon the unexpected discovery than we are on the completion of a tightly designed project.

I have the sense that physicists can gather around a table and, based on existing data, predict the existence of some particle that has not been discovered and then set out precisely to discover that particle. Biological science rarely works that way. It is very much more dependent on a scientist doing work and discovering something unexpected and then finding that it is a clue to a potential discovery that has not really been anticipated, and then moving in that direction following up these very exciting new leads.

We feel that our mission involves a balanced investment in the pursuit of new knowledge and in the application of that knowledge to better define predictable outcomes. We have protected the NIH budget for the aspect of discovery. At present something more than 60 percent of our budget is classified in this standard system as being in support of basic work; that is, pursuit of basic knowledge which at the time it is conducted does not have a precise application in mind. It is simply an investment in new knowledge in biological science.

We have, of course, an aspect of accountability in this. Most of our awards are made for 3 years. My own view is that that is a little short in many cases. We are addressing that question, as to whether we should move back toward longer awards, which was the case a decade or two ago. At any rate, at some point—3, 4, or 5 years—the scientist reapplies, and we have a chance to review the progress and decide whether the high promise has been fulfilled and whether it is merited to continue the award for another defined period of time. There is an aspect of accountability built into this, but it is not an annual complete review, though we do have annual progress reports which are studied. We want to make sure that work is going forward as proposed and consistent with the original application.

In the field of biomedical science we are in a stage of halfway technologies in many areas. Lewis Thomas has a classic example of this, which is not a new one but it still applies. That is the iron lung stage of polio treatment, which represents a complex stage of incomplete understanding and a very expensive one. That, of course, is replaced when it is possible to prevent the condition, in this case by the vaccine.

We have many examples of that. I think we are at the halfway technology stage in heart disease with bypass surgery, and in kidney disease with dialysis and transplantation. While we are doing what needs to be done to handle the care of patients with the most modern scientific and technological approaches possible, we are also investing in further understanding of basic phenomena in the hope that we can prevent more and more of these conditions. In fact, prevention strategy is one of the very prominent themes of biological research. Using rather standard definitions of public health schools and text, we classify about 25 or 28 percent of all the research that we do as in the prevention category. This includes, just as one example, our accelerated vaccine development program. We have identified 10 or 12 conditions of very high priority for development of new vaccines.

There is great emphasis currently on the field of technology transfer. There was a time when NIH felt that its mission was adequately addressed simply by promoting scientific research in a variety of fields and publishing that research. Half a dozen years ago or more, the emphasis became somewhat broader than that. We consciously developed mechanisms for accelerating the application of that knowledge in the practice of medicine. We set up a new office called the Office of Medical Applications of Research in the Office of the Director. This Office does a number of things.

One of its important activities is the organization of consensus development conferences. These cover maybe 8, 10, or 12 topics a year, bringing in experts with a variety of points of view about that development to discuss the state of that field and issue a statement which is not an NIH statement. It is done by contract. This is a public statement. That statement advises the medical profession on the application of research developments in that particular area.

We have had a very recent conference on obesity, which has received some publicity. We have had one on control of serum cholesterol values, diet, that sort of thing. We are quite conscious of now supporting everything from the very basic exploration of new ideas to the application of those ideas in clinical applied work, to the evaluation of those new developments in terms of their optimum application in the practice of medicine.

I liked the statement that Jay Keyworth published some time ago which summarizes, I think, the general attitude that we share about basic research: that is that it is something that can only be done on a scale that is currently practiced with Federal support. Basic research warrants Government support because it is an investment in the future and in a better quality of life, better security, better economy, and simply a better understanding.

We have for 40 years taken as our mission, as I indicated earlier, the conduct of research of potential benefit to the health of the American people. We are currently examining a somewhat broadened sense of that mission. This is, again, stimulated in large part by some of the comments that come out of OSTP having to do with the responsibilities of the agencies such as NIH toward maintaining industrial competitiveness and technological leadership.

We have scheduled a meeting of the Director's Advisory Committee in June to examine that issue. The whole field of biotechnology has grown up in large part because of NIH support. In the fifties and early sixties we had a very large investment in bacterial physiology, bacterial genetics, simply because we thought that it was worthwhile to develop a better understanding of cellular machinery. There was no suggestion or dream at that time that it would spawn an entire new industry, but it has.

As a consequence, we are now at a stage of enormous contributions to health based on the use of bacteria as factories for producing new proteins and new agents of various kinds, which is extending far beyond biomedical science into agriculture, chemicals, and so on. That grew out of work 90 percent of which was NIH supported over the past 20 or 30 years. There is a question whether we are doing all we should be doing in terms of ensuring the health of the biotechnology industry and our national leadership in this area. We are going to be examining that issue more carefully.

In my view, it has a parallel in the story I just told about the shift in our sense of responsibility for the use of knowledge developed in the biomedical field. We have moved past the point of feeling that our responsibility has been met simply when the work is published. We have, also, a role to play in ensuring the application, the appropriate application, and periodic evaluation of the use of that knowledge. We are going to be exploring this with a number of outside consultants and the NIH Director's Advisory Committee in June.

I mentioned a few minutes ago that 80 to 81 percent of the NIH budget is spent in other institutions. Actually 12 or 13 percent of the budget is spent intramurally on research conducted at the National Institutes of Health. About 81 percent is spent in grants and contracts to some 1,250 institutions throughout the United States. We support somewhere in the neighborhood of 50,000 or 55,000 individual scientists to some extent in their work through a total array of 22,000 or 23,000 different grants and contracts.

In addition, we have a small amount of our budget spent in international work. That figure has been fairly stable at about 1.5 percent of our budget for the past 10 or 15 years. It consists of half of that amount in projects conducted overseas or in other countries. A great deal of this is in Canada. About half of that is in support of scientists working in this country or in international conferences, that is, without any line budget for that. That is just the way it turned out. It has been fairly stable.

With respect to the four-fifths of the NIH budget expended in grants and contracts to other institutions in this country, additional statistics may be of interest that indicate the scope of the collaboration in health research between the Government, academia, and increasingly also industry; 60 percent of all research funded by the NIH is performed by universities.

Mr. REID. What was that?

Dr. WYNGAARDEN. Sixty percent. This difference between the 60 and the 81 percent consists of research conducted in perhaps free-standing hospitals or institutes or industry; 60 percent is in the universities.

We estimate that, of the health R&D funds used by universities, 77 percent comes from the Federal Government, chiefly from NIH, so the extent of interdependence there is quite clear.

In 1983 the total national support for health R&D was about $10.4 billion. Of that, 37 percent was supplied by the NIH, 38 percent by industry, and 25 percent by other Federal, State, and local governments, and private nonprofit organizations.

Of the amount supported by the NIH, we classified about 61 percent as basic work, somewhere around 31 percent as applied, and a small amount, 8 percent, in what we call development work. Even there that is not quite the same way industry would use the term "development." For example, we use "development" for the late stages of vaccine programs when they are at the stage of clinical testing.

From the standpoint of industry, on the other hand, about 10 percent is basic, and the rest is applied developmental work. There

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is a continuum there with a little overlap, but most of the basic research done in industry is still fairly well product directed for that industry's interest, whereas ours can support good ideas wherever they may potentially lead. In our view this represents a very nice balance and excellent collaborative venture between the public and private sectors.

I might say a word about the patent side of work supported by NIH. In many areas of Government, patents are obtained, but they are on inventions that are not marketed. I understand in Defense there may be a procurement issue there that has a different goal from ours.

We have sought since 1968 to capitalize on any kind of discovery made with support of NIH funds. Since then, we have negotiated institutional patent agreements with 80 universities through which they can retain ownership of grant-generated inventions.

However, since the patent and trademark amendments of 1980, that concept has been applied to all Federal agencies. Our interest is not a financial one; it is one to make sure that any discovery of potential benefit to the American people is explcited. We have march-in rights if there should be some failure to do that. We have never had to use them.

Of the 1,226 NIH-sponsored patents issued since 1961, both extramural and intramural, 452 have been licensed. That represents a 37 percent licensure rate, which is a substantial rate of commercialization when compared to the Government-wide average of less than 2 percent.

We are now in the third year of the small business set-aside program which has increasingly brought small businesses into the arena of NIH-supported work. In the first year of that we awarded $6.5 million, actually exceeding by $500,000 our quota for that year. The following year we made 201 phase I awards and 46 phase II awards, amounting to about $21 million, again exceeding our set-aside requirements by, in this case, $275,000.

That has brought a new category of institutions into the portfolio of NIH-supported work. There are still a few rough spots in most relationships, but I think it is going very well. This year we are increasing by law the amount that is in that program to 1 percent of our R&D budget.

We have a lot of collaboration with industry that stretches back over the years. Pharmaceutical companies, for example, have frequently donated drugs for use in clinical trials. In fact, we have a few examples in which the actual expense of the clinical trial has been shared between NIH and industry. We have had jointly sponsored conferences in many areas.

We are also developing some new programs of interaction. I have already mentioned the Director's advisory committee meeting on biotechnology to be held in June. In all likelihood, this will open up some new opportunities for collaboration with industry.

We have also met recently on two occasions with some of the officers of the Industrial Research Institute—their Federal Science and Technology Committee—to explore ways in which we could interact even more effectively. I plan to address their major fall meeting to discuss some of the opportunities and policies at NIH.

We will schedule perhaps two major scientific conferences at the NIH to which we will invite industrial leaders.

We are exploring ways in which more members of industry's scientific staffs might spend short periods of time, perhaps even a whole year, in our intramural program as part of the company's sabbatical system. We have such people now. We have a few from American industry. We have to have more from foreign industries. We may have been overlooking an opportunity that should be developed domestically. We plan to do that.

I might say a word about return on investment, because there have been substantial sums spent in biomedical research. There was a book published in 1979 by Selma Mushkin entitled, *Biomedical Research: Costs and Benefits*, in which she and her coauthors address the degree to which biomedical research has accounted for trends in reduced costs in illness, as measured by reduction in premature death and loss of work time.

She concludes that 30 to 40 percent of the reduction in mortality rate and 39 percent in the reduction of objective sickness rate can be attributed to biomedical research. In fact, in terms of econometric models, she states that, overall, the return on the investment in biomedical research from 1900 to 1975 averaged 13 to 1. For every dollar invested, there has been a thirteenfold return to the general economy. That rate was higher in the early part of the century, when the infectious disease rates were coming down and the expenditures were small, but for the last 30 years of that, from 1945 through 1975, the rate was still 6 to 1.

In addition, our Office of Medical Applications of Research studied some time ago some developments that grew out of support of our medical science that have entered the general economy outside the health care sector. There are some very interesting examples of that. For example, the freeze-drying technique was originally developed as a method of preserving proteins against deterioration. Now, of course, it is the basis of—I am not sure about this coffee, but at least a lot of instant coffee and other foods. It is a very important component of the industry. There are other examples: flexible endoscopes, enzymes used in the stabilization of beer, and so on.

They took 10 such discoveries and they found these returned $37 billion to the general economy. That happened to be a year when the NIH budget was $3.7 billion, a very convenient 10-to-1 ratio.

I might just cite a couple more recent developments that I think are very exciting. One is from cardiology and one is from neurology.

Prior to birth, there is a slot between the left and right side of the heart that enables blood to circulate in the fetus without having to go through the lungs completely. That little artery normally closes off at the time of birth or shortly thereafter. However, in some children it does not close and requires a surgical procedure. That has been done for many years.

Recently, it has been discovered that a drug, indomethacine, will promote closure of that slot, thereby obviating the need for surgical procedures. That drug was developed in large part with NIH support. We estimate we put perhaps $5 million over many years into the development of that drug and its scientific basis. We also recently conducted a multicenter clinical trial that cost another $5 million. So we invested about $10 million in that drug and procedure.

The former cost of care for about 15,000 to 20,000 infants per year we estimate as being close to $200 million. That is the cost of surgery plus a week in the intensive care unit. The total cost of treatment with this new drug for the entire country is $800,000. That is a development that has reduced the cost for the individual patient from $9,000 to $40.

We have another development in the field of plasmapheresis for Guillain-Barre syndrome, a neurological complication of viral infections. It is a paralytic state that requires extensive time in intensive care units. The use of the plasmapheresis procedure has greatly shortened the length of time that such patients require in the respirator by 11 days and has reduced the time that is required to recover the ability to walk by somewhere between 30 and 90 days. We estimate that that procedure, the trial of which cost about $900,000, has saved $35 million a year in costs for these patients at the hospitals in the general economy.

We could cite others. Those are two recent ones, but I think it does illustrate that the investment is paying off handsomely.

The new vaccine for hepatitis B, if fully used, has a potential of saving $4.3 million per week in hospitalization costs for that condition.

I mentioned earlier that the budget of the NIH has been more or less stable now for 15 years with an overall growth rate of about 2 percent per year in purchasing power. During that period of time, we have shifted resources into the project grant category because of the large number of excellent projects which have been proposed and because of our declining ability to fund as much of the work as we might have otherwise funded.

This shift into research grant categories obviously was at the expense of certain other mechanisms, including contracts, clinical trials, and training. So about 5 years ago, as a consequence of the study on research goals that was conducted when Mr. Califano was the Secretary, a policy of stabilization was proposed. The full expression of that policy called for protection of all of these categories to maintain a certain balance, but also suggested that a minimum of 5,000 new and competing awards be made each year as a device for smoothing out the enormous fluctuations that had occurred over the previous several years which had resulted in figures as low as 2,900 and as high as about 6,500 such grants being funded. It was felt there was considerable merit in developing a policy of a predictable number of such awards each year. Otherwise, the new people coming along really faced a very uncertain future.

That has been done, and we have since that time been able to maintain the 5,000 number as essentially a target figure. It was conceived initially as a floor. It has since become both a floor and a ceiling, but, at any rate, it has provided a substantial measure of stability.

We have during this period of time, in the early seventies until now, been able essentially to double the number of such awards made per year in this investigator-initiated project grant category from something around 9,000 or 9,500 in 1970 to about 18,000 overall last year. At the same time the applicant pool has tripled. While we have doubled the number of awards, the applicant pool has tripled. I think that is an expression of the number of talented people who have found biological science an exciting career to enter. At present, we are funding an average of one-third of the projects that are approved by study sections, whereas roughly one-half was the figure a decade ago. It does mean we have to be very selective in assigning priorities to what we perceive to be the most highly promising work. That is where the peer review system that I mentioned earlier is so indispensable.

We have a number of topics on our agenda looking at the extramural awards system. My sense is that the degree of competition that has grown up in the past decade has had some effects on the system that were not entirely anticipated. It has been a subtle shift, it seems to me, from the investment philosophy in science and scientists to one of more of a procurement mentality with a great deal more careful and, in some cases, overly picayune review of grants, looking for minor flaws that would justify a lower priority because the competition is so intense.

We have some [illegible]tions as to whether this may not have had a subtle negative eff[illegible] on the creativity of the scientists. It might be hard to document, but there is a tendency to be very cautious, to propose only things that are reasonably sure of execution. Many of our advisers have commented in the same manner to us, that they think this perhaps has not been the healthiest development. We are looking at ways in which we can simplify the system, perhaps move it back more toward the investment mentality, perhaps stabilize investigators somewhat more by providing longer awards where they really are warranted.

We have addressed that question during the last meeting of the director's advisory committee and other outside consultants and are in the process of drawing up some implementation plans right now.

DISCUSSION

Mr. BROWN. Dr. Wyngaarden, we would like to save a few minutes for questions.

Dr. WYNGAARDEN. This is the end of my prepared remarks, so it is a good time.

Mr. BROWN. You anticipated us.

Your remarks have been extremely valuable in illuminating this whole area. We very much appreciate it.

I am going to recognize Mr. Lujan first for questions.

Mr. LUJAN. I have just a couple of quick ones.

Is there anything that inhibits your running of NIH as you would in the absence of those obstacles? Are there some things that you would like to proceed with?

Dr. WYNGAARDEN. No; I don't see any major constraints of that sort. I think in common with every other agency and every other institution in the world we have to live within a budget. There are some things that we cannot do for lack of funds, but actually the budget has permitted a stable program now for a decade [illegible]r more. We are able to support the very best science.

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Mr. LUJAN. In the area of medicine, that costs big bucks, particularly today—take heart transplants, for example, and that kind of market—as a very basic question, can we depend on those big dollar incentives to really drive research?

Dr. WYNGAARDEN. There has been a substantial reduction in the incidence of heart disease. The death rate from coronary disease has declined by about 25 percent in the past decade; by stroke, even more than that. That is in large part attributable to improved health care, some of it directly related to NIH-sponsored research, such as better control of hypertension, which is maybe the single most important factor.

I think there is a greater appreciation for preventive measures for atherosclerosis, which is control of the fat content of the diets, that grows out of NIH research.

We do not presume to take credit for all of that because some of it is due to lifestyle changes and other factors that are certainly not directly NIH-related. That is bringing down the costs.

The other side of that, of course, is that procedures that are used as part of the halfway technology stage I mentioned earlier are very expensive. We do continue to evaluate those. A large study funded by the Heart Institute helped, I think, to define the criteria for cardiovascular surgery, for bypass surgery, and recommended that there was a fairly substantial group of patients who had minor symptoms which could be controlled medically that did not need surgery. We do address those issues, although our charge is not primarily the cost of the health care system. Obviously, we impact on it.

Mr. FUQUA. Dr. Wyngaarden, you indicated that about 60 percent of your budget went into basic research.

Dr. WYNGAARDEN. Yes.

Mr. FUQUA. It was generally on a 3-year cycle, most of the grants were.

Dr. WYNGAARDEN. Yes; the average was 3.

Mr. FUQUA. Last week we had Dr. John Foster here. I don't know if you know Dr. Foster or not. He has been the Director of the Livermore Lab and has been involved more in physics than in any type of research that NIH would be connected with.

He indicated from his perspective that when they set up DARPA, I think it was, they warned him that when you started into basic research, they wanted you to have a plan for when you got out; when would it be complete or when would it be accomplished? Do you work that way, does NIH, or do you see that as an impediment to further research?

Dr. WYNGAARDEN. I think there is a difference between research in physical science and biological science. Some of our most basic research would be very difficult to analyze in that manner at the time it is done. I think the example I gave was bacterial genetics, which led to DNA discoveries. This new industry could never have been predicted in that manner in the fifties and sixties when it was started.

It was started then with the feeling that, if we knew more about genetics and more about cellular development and control, it would open up new avenues, new insights for understanding disease, which indeed it has done. However, the industrial outcome of that I think in a way came as a surprise to most people.

I think we feel in a general sense that new knowledge is going to be useful in the understanding of disease and in the development of therapeutic and prevention strategies, but it is very hard to predict just where a given piece of new information or a given theme of research will find its application.

In fact, it even illustrates, I think, the declining rationale for some of the NIH organizations that we have, in that two of the most important discoveries in diabetes in the past year were not even made in our Diabetes Program. One came out of the Dental Institute and one came out of the Eye Institute.

Mr. FUQUA. That is very interesting.

Dr. WYNGAARDEN. There is an enormous sense of confluence of science, and the common language is science. Whether it is labeled immunology or bacteriology or physiology, nevertheless, it is the mechanism for coming together. The tools are so powerful, the DNA techniques and antibody techniques are becoming standard tools in all branches of biological science. Therefore, it is very hard to know where a discovery will have its ultimate application.

Mr. FUQUA. On a broader philosophical vein, the last major report on science policy was the Bush report about 40 years ago. Do you see that the importance of science and Government's responsibility in science has changed very much in 40 years? What do you see for the next 40 years?

Dr. WYNGAARDEN. I think the insights of that report have been amply validated. I think the next 40 years will continue to require a major Federal investment in the support of basic science. The institutions that have evolved, and collaborative relationships between Federal support of academic institutions, and the linkage of academic institutions and industrial components for their capitalization or for commercial applications, represent a very healthy system. I cannot really see any substitute for the investment of large amounts of Federal money in basic science. Even the entry of more private sector money into this field—it may double or triple—is nothing on the scale of the Federal support. I cannot imagine any major shift in that distribution of responsibility.

Mr. FUQUA. Thank you. Mr. Packard.

Mr. PACKARD. Doctor, higher and higher health costs have become a great concern to the American people.

Dr. WYNGAARDEN. Yes.

Mr. PACKARD. Research and development have produced technology and equipment that has generally increased the cost and certainly increased the quality of service to the people. The costs certainly have reflected that increased technology. You have given two rather graphic examples of how technology can reduce the cost of health care. Is it possible to develop a strategy and a policy that would lead to a greater emphasis on seeking cost-saving research and equipment in contrast to the rapid increase of health care costs as a result of the high technology we have developed in the health care field?

Dr. WYNGAARDEN. I think the answer is yes. I am not sure on what sort of microscale, but certainly as an overall philosophy and policy statement I agree with that. It is reflected, for example, in our emphasis on prevention because the ultimate saving is in prevention.

In those areas where there is a possibility of moving faster toward prevention strategies, we are attempting to do so. One of those I mentioned is the accelerated vaccine development program. We have about 10 candidate vaccines that we have given very high priority to in the sense that, if we put a larger resource, a larger effort there, we may make progress more rapidly. I think that is important.

In terms of the large degenerative diseases or ones which cost so much in terms of health care, cardiovascular problems, for example, we need to make sure that we do not overlook any opportunities in developing further understanding of the basic path of physiological change. Arterosclerosis, for example, is behind a great many of these. Immunological responses in kidney disease are ones that we need to gain insights into.

We have to pursue those with a steady vigor to turn out the kind of discovery that will turn these fields around. We are making progress, but still care is going to be expensive.

Mr. PACKARD. My interest in this direction would even be more acute among the elderly. If my figures are correct, 80 percent or more of the entire lifetime cost of health care falls within the last year of a person's life, and therefore that is where the greatest burden of cost falls. It would be of interest to me if we could develop our policies to encourage research and development in the area of cost-saving mechanisms and approach to medical care particularly among the elderly.

Dr. WYNGAARDEN. The arterosclerosis example, of course, is one, but a better one for your purpose right now might be Alzheimer's disease. That is a good example because our concept of Alzheimer's disease has changed remarkably in the past decade. We have a major emphasis on that field.

The burden of illness and health care costs certainly is one of the factors we take into consideration in setting priorities.

Mr. PACKARD. I have other questions, but I will put those into the record.

Mr. BROWN. I would like to interject one comment before I recognize another member.

Mr. Packard's question shows we are all faced with this problem of cost, not just in health but in other fields. I will cite the example of agriculture where a very productive and beneficial research system has resulted in huge extra costs to the taxpayers as a result of subsidy payments.

Dr. WYNGAARDEN. Yes.

Mr. BROWN. We need to ask the same question there. The problems that cause these costs are institutional problems such as third-party payment for health care or the very success of the research which is the extension of life. In the case of agriculture, it is the overproduction resulting from science coupled with an institutional system which pays for the overproduction. What we really need to look at is the institutional system, but we do not have any research in that area. We neglect that as an area where we can fruitfully devote funds for development of better policies.

I don't want to blame that on NIH. That has to be blamed on Congress. It leads to the question of whether or not we could not more fruitfully develop some areas of policy research.

Mr. PACKARD. If the chairman would yield on that point, that brings it down to the bottom line. The function of this particular committee is to establish a long-term national policy which will include a health policy or a biomedical research policy. I believe in establishing such a policy we ought not to overlook these thrusts we can incorporate into cost-reducing processes.

Mr. WALGREN. You mentioned the emphasis on prevention. I wonder if it might be proper to ask for some submission in greater detail of your NIH emphasis on that and how it occurred.

The question I want to raise for discussion would be the strength of our ability to focus and emphasize areas of research or approaches such as cost reduction mentioned by Mr. Packard. You said in the sixties we put emphasis on disease reduction, but other spinoffs occurred. Certainly the fact there are spinoffs is not a reason not to have a very focused direction.

Could you describe or comment on the strength of the directing power over and above the peer review system? To a certain extent, the peer review system is down there pulling these resources in very specific directions without regard to any overall policy thrust.

How strong is our ability to develop direction and emphasis on an overall level as opposed simply to putting the money in and seeing where it goes?

Dr. WYNGAARDEN. We have a variety of mechanisms which impact on priority setting. I think in terms of great discoveries it is hard to order those. Those come out of the work of scientists at the bench and their insights, and frequently through unexpected developments. Beyond that, we have obviously a variety of priorities which are set. They are in a way also representative of the names of the institutes as they were established along the way. There is the Heart Institute, the Cancer Institute, that represent public policy statements.

In addition to the thrust of research that results from the receipt of applications generated by the scientific community, we give some guidance to the scientific community frequently reflecting Congressional directives, reflecting administration priorities, reflecting issues brought to our attention by voluntary health agencies, and efforts of our own advisory councils as they look over our entire portfolio to identify areas which we need to stimulate more work which may result in requests for applications, requests for proposals.

Mr. WALGREN. What I am asking is whether or not you can detail the structure of that level of decisionmaking, perhaps not here but perhaps in a submission, because somehow or other it seems to me that our question would be whether that level of influence is strong enough. Is it being exercised directly enough? I don't know the answer to that. I am sure there are some yeses and noes.

Dr. WYNGAARDEN. My general answer would be yes, but there may be specific examples.

Mr. WALGREN. If you could give us some kind of submission describing the process you use to bring that element to bear.

Dr. WYNGAARDEN. Yes.

Mr. WALGREN. And what levels in your organization are involved in that and when they see the flow of the money and get their opportunity to direct it. That would be helpful.

Dr. WYNGAARDEN. I will be happy to supply that information. [See appendix.]

Mr. BROWN. Mr. Lewis.

Mr. LEWIS. Doctor, you mentioned 60 percent of your budget is for research. How do you make a determination of what percentage of your budget goes for research, advanced research, research for AIDS, respiratory diseases, and other areas? How do you determine how to allocate those funds?

Dr. WYNGAARDEN. We have 16 separate budgets at NIH. There is no such thing as an NIH budget. There is a budget for the National Cancer Institute and for each of the others. The overall allocation of funds in a given field is really set by the Congress.

Within that, the managers of the National Cancer Institute, for example both in terms of the budget they prepare and defend, may ask for funds for specific components, but in the end they have a great deal of flexibility in pursuing what they judge to be the greatest areas of scientific promise.

You asked about AIDS. AIDS represents not only a national tragedy, but a scientific opportunity of enormously intriguing potential to the scientists who work in the retrovirus field, for example. This disease is one in which a selective cell, the T cell, was destroyed by, in all probability, a virus. People who are working in T cell biology and retrovirus work moved into this field because it was so exciting.

Then we developed, on the basis of their estimates, requests for funding that would permit the best work to go forward. It is a combination there of, let's say, Administration policy, extramural demand from the affected segments of society, and scientific opportunity that come to bear to define the budget.

The budget for AIDS work in the NIH this year is around $60 or $61 million. In the Department as a whole it is about $95 million. That has come from virtually nothing 5 years ago. We have responded to these varieties of influences to fund what we think is an appropriate level of activity.

Mr. LEWIS. Do you think funding is the greatest priority you may have in the life sciences and biomedical medicine for advancement of human welfare?

Dr. WYNGAARDEN. I think funding is at a good level. We were talking before this hearing began about the effect on the cancer field of the enormous influx of money in the early 1970's, in response to a public demand for more work in cancer and the National Cancer Act. The budget for the Cancer Institute was essentially doubled. To some extent, that came at the expense of other institutes which lost ground for a few years, but the result of that was an enormous stimulation of work in the cancer field.

It came at a time when research developments had moved to the point where that investment was appropriate. Ten years earlier I am not sure it would have been. By the early seventies, there was a large amount of available information and the influx of new money did several things. One is that it attracted scientists whose work was perhaps relevant to cancer, but it could have gone in other directions than into the cancer field. It also sent a very powerful signal to the young people entering biological science, who then chose the cancer field.

Whereas the cancer field had, on the average, not maybe the quality of scientists in the fifties that it does now, it now has spectacular people in it. There was a lot accomplished by the funding of that field at a time when the opportunities were there to make use of the funds.

Mr. LEWIS. Thank you, Mr. Chairman.

Mr. BROWN. Dr. Wyngaarden, your presentation and discussion raise many, many questions which we will not have an opportunity to explore because we will have to leave shortly.

Let me bring up a couple items which seem to be particularly relevant. In this discussion of funding of basic biological research in the various Institutes, it raises some questions about the normal definition of basic research. It is not aimed at a specific goal or target but the exploration of human knowledge. As far as I can tell, you can fund good basic research on a particular problem in almost any of the Institutes, it seems to me.

Dr. WYNGAARDEN. That is true.

Mr. BROWN. How do you go through the decisionmaking process which determines which Institute will fund good basic research?

Dr. WYNGAARDEN. As the applications arise, if they have some kind of linkage with the particular Institute's program, they are likely to be assigned to that Institute.

Mr. BROWN. By "might," you mean in the field of biological science it may have an important component in that particular Institute?

Dr. WYNGAARDEN. Yes. For example, right now retrovirus work is supported by the Cancer Institute. That reflected a decision of some years ago, when it was not thought that retroviruses had much relevance to human disease unless it was to cancer, that they would be pursued by the Cancer Institute. Now we know better, but that is where that work is supported.

If work is coming along that we cannot peg, we put it into an Institute set up precisely for that function, the General Medical Sciences Institute. It supports basic research that may be fundamental to two or more programs or without specific foreseen application. Most of the work in the bacterial genetics and viral genetics that eventually grew into the AIDS Program was supported by that Institute.

Mr. BROWN. It occurs it is not that important as long as there is a rational process involved.

Dr. WYNGAARDEN. Yes.

Mr. BROWN. Members of Congress tend to look at this from the standpoint of what is the rational process. Sometimes they are not too rational themselves in doing that.

The other kind of question I have is again sort of an allocation kind of question, but at a different level. It turns out, of course, that biotechnologies, which I agree with you stem from the support we gave to basic cellular research for many years, have applications which go beyond human health. At least I do not think improving the manufacturing of beer is tied to human health, and of course in agriculture.

The question I have stems from what is happening in agriculture. We saw this week in the *Post* an article describing the revolution that will take place in agriculture.

Dr. WYNGAARDEN. Yes.

Mr. BROWN. It is obvious that for many years the Department of Agriculture did not adequately fund plant biotechnology, and even perhaps some aspects of animal biotechnology, particularly relevant to agriculture, although animals benefit from human biotechnology certainly.

How do we bring about at a Government-wide level an appreciation of a proper distribution of the funding of research in important areas so that we do not miss any major paths?

For example, you mentioned some aspects of the work you have done which have important industrial and agricultural applications.

Dr. WYNGAARDEN. Yes.

Mr. BROWN. Important applications in other fields of science.

Dr. WYNGAARDEN. Yes.

Mr. BROWN. How do we get that kind of proper focus on a broader level than just the Institutes of Health?

Dr. WYNGAARDEN. That is a difficult question. My quick answer to that would be that a system has evolved where we have a large investment in basic and differentiated work but we also have ways of feeding and channeling that work beyond where it was intended. The work in AIDS is a good example of that. There do not seem to be many barriers to free exchange of information and utilization of those discoveries in our particular capitalistic system of industrial development. If there is any merit in commercialization of these ideas, they will be commercialized.

We are getting at this problem of closer linkage with industry, I think, and we hear from some of the industries that they are not as well informed regarding what we do as they would like to be. We are addressing that. That is important.

Mr. BROWN. I will not belabor it.

Mrs. Schneider.

Mrs. SCHNEIDER. Could you elaborate a little bit regarding which of those 16 different budgets you have at NIH focus on preventive medicine and looking at human health in a more comprehensive way by including physical, mental, and emotional aspects of various diseases?

Dr. WYNGAARDEN. The 16 budgets include those of 11 categorical institutes, several divisions, and one of them is essentially for buildings and facilities. Excluding the latter, I would say virtually all of those have a prevention component.

For example, the Child Health and Human Development Institute has a large component which would be considered prevention. So would the Allergy and Infectious Diseases Institute which deals with viral and bacterial vaccines.

We can get you precise figures on that. The figure I gave of 28 years, NIH's overall figure, it is higher in some institutes and lower in others. It is a question of the kind of work they do. The more basic the work, the less you use.

With respect to the behavioral and social science research, about $60 million is in two Institutes, $40 million in Child Health and

Human Development, and $20 million in Aging. Those would be the largest Institutes invc .ved in behavioral research.

In all of those, the earlier question was when this behavioral prevention emphasis developed. I think it has been there all along. We didn't discuss it in those terms 20 years ago but back in the early fifties there was a lot of work done in understanding the biochemistry of fatty substances, for example. Now we classify that as related to atherosclerosis.

Mrs. SCHNEIDER. It concerns me that as we listen to testimony in various hearings we have, there is such an emphasis on what appears to be the high technology curative approach as opposed to really any kind of analysis as to some of these origins or causes which might be more low tech types of things.

For example, in the area of cancer prevention, I think there is a great deal of evidence emerging—I believe it has been around for a while—but now it is emerging in popular magazines and other more widely read journals, that stress is an important element of cancer. I think we have known all along there is a chemical reaction that takes place when one experiences certain emotional changes, whether it be fear, anger, stress, or whatever.

It concerns me that all too often our budgets are focused on hardware and new diseases as opposed to looking at what would be more obvious.

One other example I would like to share with you and ask you about is the area of air pollution. It is indicated that indoor air pollution is responsible for many different illnesses we experience, either temporary or long term, particularly in the area of lung cancer. I believe 50,000 of the lung cancers which occur each year have a connection with radon which is trapped within heavily insulated homes or workplaces.

Dr. WYNGAARDEN. Yes.

Mrs. SCHNEIDER. It seems to me that the amounts of dollars that the consumer is paying through their health bills and through their taxes, which ultimately goes into research for the cure of lung cancer, this would be an area where NIH would be anxious to conduct research on the impact of various chemicals that react on people, radon and others, on the human body. Is there work going on there?

Dr. WYNGAARDEN. Those are excellent points. I will say something about these.

On the aspect of stress in general, I think one other thing is that there has been a general suspicion that stress plays a large role in many illnesses, perhaps including cancer. What is needed is a way of reducing that general question of [illegible]iosity to some mechanism that one can study and measure. That is where basic science development defined in the past few decades 30 or 40 new chemical messengers in the brain. We used to think there were 4 or 5; we find another 50 or 60. There may be many more.

We can now begin to approach in a more qualitative and scientific manner the explanations of the question you raise: Does stress play a role? If so, how? We are making good progress in that general field.

As to pollution, we have the National Institute of Environmental Health Sciences, which deals with the question you raised. We are

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not charged with the responsibility of regulating the workplace or exposure levels, but of defining the scientific rationale for perhaps regulatory decisions or changes in health practices and industrial practices. We do that vigorously.

The question you raise of radon exposure will be pursued.

Mrs. SCHNEIDER. The administration is eliminating the research into indoor air pollution. Is that a wise idea?

Dr. WYNGAARDEN. I am not aware of that. It has not come down to me.

Mrs. SCHNEIDER. I just told you. I would be curious about your opinion.

Dr. WYNGAARDEN. We are continuing research in the areas I indicated, which would include any biological factors which depend on cancer and other health problems.

Mrs. SCHNEIDER. Sorry to put you on the spot.

Mr. BROWN. Dr. Wyngaarden, we are grateful to you for your appearance this morning. It has been very stimulating and helpful to our pursuit of questions in these areas. We hope we can get you back again.

Dr. WYNGAARDEN. Thank you very much.

Mr. BROWN. The task force will be adjourned until next Thursday at 8:30. The postponed appearance of Dr. Lewis Branscomb from IBM will occur at that time.

[Whereupon, at 9:50 a.m., the task force recessed, to reconvene the following Thursday, April 4, 1985.]

[Answers to questions asked of Dr. Wyngaarden follow:]

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Question 1

Q: Some, including some historians and social scientists, have suggested that the relationship between science and the Federal Government is in the nature of a social contract: The Government provides certain resources for scientists to expend in return for which they provide society with certain benefits. How do you view this analysis, and to what extent does it apply, in your view, to the field of biomedical research?

A: The reciprocal obligations between biomedical scientists and the society that provides their support can indeed be viewed as a social contract. Biomedical research derives the vast majority of its financial support from Federal funds. Clearly, this support is predicated on the public's belief and trust that from the results of such research will ultimately be derived the tools for better diagnosis, treatment, and prevention of disease and the reduction of premature death and disability. And, in fact, substantial benefits have already been derived from research in the form of new drugs and other treatment modalities, new vaccines and other means of disease prevention, and new and improved screening and diagnostic tests and procedures. Recent progress and advances in research now offer even greater promise for future improvements in health care.

The NIH honors the implicit terms and conditions of this contract through the process by which research priorities are established and funding decisions are made in the broad allocation of research resources. In setting research priorities, the NIH gives consideration to the concerns and wishes of the public, expressed directly and through congressional and Executive Branch actions. Authorizing legislation and appropriations influence our research planning and the conduct of our programs. The views of professional societies, voluntary health organizations, and the general public are sought also through a variety of means ranging from structured activities such as national advisory councils, task forces, and

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commissions, to unstructured individual interaction with representatives of such groups.

However, the research ideas contained in unsolicited grant applications provide a major influence on priority setting and the NIH must meld scientific considerations with broader policy considerations. To accomplish this each Institute weighs (a) the state of knowledge in areas of science underlying the various diseases; (b) the public health importance of a disease; (c) the availability of trained manpower, facilities, and equipment to mount major initiatives; (d) the views of various constituency groups; and (e) the thrust of congressional mandates and directives.

The public, in turn, must understand that the nature of the research process dictates to a large degree the manner in which health problems can be addressed. Biomedical research is an investment in the future which involves a continuing search for knowledge. Basic biomedical research is, by its nature, an unpredictable undertaking; there is no way of forecasting which problems will yield easily and quickly, nor when solutions will be found. This has always been characteristic of the course of basic biomedical research and will continue to be. Nevertheless, we are coming closer than ever before to understanding the mechanisms of the living processes in cells and tissues, and there is a high degree of confidence that the underlying mechanisms of disease are becoming approachable because of these insights. This view is rapidly replacing the view held

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by many that the study of disease is quite a separate endeavor from basic research. The evidence is also growing that human diseases are not the completely separate and apparently unrelated entities that they were once believed to be. And as we continue to identify and sort out the participating factors in the causation of disease, the knowledge gained will advance our understanding on multiple fronts.

The social contract has also heightened concern for the rapid utilization of the results of biomedical research. To the generic mission of basic research has been added the responsibility to assure that the knowledge gained in research settings is: (1) assessed for its potential clinical usefulness and applied as soon as possible to medical practice; (2) applied widely in disease prevention; (3) provided to agencies responsible for the regulation of health procedures; (4) transferred to industry for application in health, agriculture, and environmental protection; (5) provided to organizations responsible for health care financing; and (6) translated into information appropriate for professional and public education. In response to these continuing mandates the NIH has developed a variety of mechanisms to facilitate the transfer of new technologies to improve the quality of health care in the Nation.

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Question 2

Q: To your knowledge, have there been any retrospective analysis made to systematically evaluate the nation's biomedical research programs in order to determine the ratio of projects which led to technological payoffs and those which did not? What are the inherent pros and cons of such studies? In general terms, what have been the results of the evaluation studies which the NIH has been mandated by the Congress to spend a small percentage of its funds on?

A: During the past decade many retrospective analyses of NIH programs have been conducted. Individual Institutes that have conducted such analyses usually select an identifiable program or program segment and combine peer group perceptions of the state-of-the-science with review of program structures, etc. Examples of this type of evaluation study that have been particularly effective are studies of the National Institute of Dental Research (NIDR) programs in Periodontal Disease, Caries and Craniofacial Anomalies, and a National Institute of Arthritis, Diabetes, and Digestive and Kidney Diseases (NIADDK) study of the Muskuloskeletal Diseases program. The National Institute of Child Health and Human Development, the National Eye Institute, and NIDR have incorporated state-of-the-science analyses into comprehensive plans for institute programs. The National Heart, Lung, and Blood Institute and the National Cancer Institute have emphasized studies of the effectiveness of education and technology transfer programs, and of such broad data collection activities as the SEER program. Comprehensive lists and examples of reports of institute evaluation studies are available in the Science Policy Research Division of the Library of Congress.

Comprehensive retrospective analyses are conducted centrally at NIH. These programs of study use objective methods to address manpower issues

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and to assess research program performance. Manpower studies examine the subsequent career development and research productivity of individuals who have benefitted from various types of training support. A recently completed example is the report "Career Achievements of NIH Trainees and Fellows," an analysis of outcomes of predoctoral support. An analogous study of postdoctoral training support is underway. NIH-wide research program performance is assessed by examining the quality and quantity of research journal publications resulting from NIH grant and contract support. Comprehensive analyses of publications resulting from NIH extramural and intramural research support programs from 1970 to 1983 are in preparation The Science Policy Research Division of the Library of Congress has copies of several reports of studies of this type.

Retrospective analyses of the types described above do not provide information about "technological payoff". While it is possible to identify a technological advance and to trace its development back through the scientific journal literature to the fundamental discoveries that were its necessary precursors (several such studies have been and are being done), the opposite, "forward" tracing of the journal literature is not technically feasible. Retrospective tracing is accomplished by searching the references given by the key authors at each stage of an advancement. Forward tracing requires that one attempt to track through successive generations of papers that cite a target paper. Any paper that receives even an average number of citations will, in only a few "generations" (of tracking the papers that cite the paper that cited the target) result in hundreds or even thousands of possible research directions in a wide variety of disciplines. Without clues to guide the forward search, it is hopeless to attempt to determine which direction may eventually lead to a technologic advance. Furthermore, the traces methodology has demonstrated that the trails from basic research to technology may extend into decades.

Clearly, retrospective traces reveal only the most significant contributors to a particular technologic development. For each such contributor there may be dozens who serve primarily to confirm or to refine the breakthrough discoveries. While such refinements may actually make possible the next level of discovery by revealing a new direction or application, the traces methodology would be unlikely to accord them recognition.

To conduct traces studies of all technological advances that occurred during even a short period of time in an attempt at comprehensiveness would be prohibitively costly; n.b., a current traces study of a mere dozen technologic advances in cancer research will cost over a half million dollars.

The logical alternative to traces studies for broad scale program analysis are publications analysis and peer judgements of the status of the science. The latter type of analysis provides substantive information but is subject to suspicion of bias. Publications analysis, or "bibliometrics", provides no substantive information beyond literature

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titles and abstracts, but allows for objective analysis of the relative utility or merit of large aggregates of the published results of projects. On the individual project level there is little doubt that the judgement of a group of peers is most likely to lead to a fair assessment of the nature of the contribution of an individual grant. On the other hand, when the issue is the overall performance of the often very large number of investigators whose research support constitutes a "program", bibliometric analysis will provide the most comprehensive and objective assessment of overall performance permitted by present day technology.

Evaluations mandated by the Congress are usually conducted by the National Academy of Sciences. In general, these efforts result in scholarly reports that contribute much to the consideration of policy issues and alternatives, but are, nevertheless, of limited value and applicability to NIH program policy development. Their limitations reflect the absence of intimate knowledge and/or understanding of the full complex of factors and forces the effects of which must be integrated in arriving at policy decisions.

Question 3

Q: The Chairman of the National Academy's Space Science Board recently noted that there are no scientific criteria that can be developed for science as a whole. He said that "we are experts at setting priorities within any one field of science. The astronomer, for example, finds it difficult to judge impartially the value of research in the life sciences. The ultimate judgment about priorities are made adequately by the present method of relying on a complex democratic process to make essentially political decisions." What is your view of the roles of scientists and politicians in making decisions about scientific priorities?

A: How much the Federal Government allocates to competing areas of science and by what criteria these decisions can be made have always been important considerations but they assume an even greater urgency as a result of the current climate of fiscal austerity coupled with the ever-rising costs of performing new, sophisticated research. Such allocations will probably become an increasing necessity since what society is willing and able to spend on all of science will undoubtedly never be enough to satisfy all worthy claims on the available funds.

Obviously, the issue of establishing definitive priorities among diverse fields of science is fraught with conceptual and technical difficulties and has long eluded any satisfactory resolution. At the very highest levels of aggregation it appears that the various broad branches of science are, indeed, incommensurable and cannot be measured by any universal or uniform standards. At least, I am not aware of any adequate internal criteria that can be extended and applied to compare the relative "worthiness" of these far-ranging basic fields of science. Consequently, decisions concerning the broad allocation of funds to such disparate fields as radio astronomy and molecular biology involve criteria external to science <u>per se</u> and require the exercise of value judgments which are different in kind from the scientific judgments that are made in

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considering choices within a discipline or research area. Such decisions may be informed by considerations of the state of the art within a particular field and the potential for substantial research progress but these decisions ultimately require the type of adjudication of conflicting claims for public monies that, in my opinion, can best be achieved in a political context. In this sense, I agree with the views expressed by the chairman of the National Academy's Space Science Board.

Within such fields of science as biomedical research I believe that internal criteria and scientific judgments are of paramount importance in determining the allocation of resources. Obviously, they cannot serve as the only criteria and the melding of scientific considerations with concerns for the relevance of research to pressing health needs will, in my view, always be a hallmark of the U.S. system of research support.

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Question 4

Q: In discussions of the government science budget, much stress has been placed on providing new funds for new initiatives in emerging areas of scientific promise. Why should we not expect a comparable group of areas within each discipline which have "peaked" or been "mined out" and where consequently some funding decreases can be made?

A: It is true that there are both areas of expanding opportunities in science and areas that have yielded their greatest contributions. However, scientific opportunities are currently expanding at a prodigious rate. This is due to our existing body of accumulated knowledge which has opened entirely new areas of research. These novel areas of research add to the collection of scientific knowledge and, in turn, expand the number of promising research leads to be pursued. A major challenge facing NIH today is to maintain the national research capability in a time of limited resources so that the exceptional opportunities afforded by the current biological revolution can be exploited.

As particular areas of science become less productive sources of new and useful knowledge, funds are diverted from those areas and into more fruitful ones. This is an ongoing process which is an intrinsic aspect of the scientific enterprise. There is, however, little publicity given when an area of research is constricted, hence, there is almost no awareness publically about these funding decreases which occur continually.

For instance, the NIH peer review system places a great deal of emphasis not only on the quality of a proposal but on the significance of the research and its relevance to institute goals and the overall mission of NIH. The competitive nature of research support, particularly in biomedicine, rarely allows research to continue in areas acknowledged to be devoid of significance. In FY 1985, for example, it is estimated that

NIH's competing research project grant applications will have an award rate of approximately 30 percent. Given such conditions, only exemplary research proposals stand a chance of securing competitive funding.

Generally, research prooosals are also reviewed at their parent institutions for quality, merit and importance of the intended research. Investigators are also aware of the fact that their research results will eventually be scrutinized by the editorial review boards of scientific journals. These boards judge submissions in terms of importance and originality. Finally, the selection of a research problem is guided by the individual investigator's desire to gain recognition and stature in the scientific community through the significance and creativity of his or her efforts.

Categorical decisions to decrease funding in large areas of scientific investigation are usually obviated by these highly refined informal and formal processes which ensure that research investments are focused on areas of scientific promise.

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Question 5

Q: In a recent article Dr. David Hamburger emphasized the need to place stronger emphasis on "the entire gamut of factors affecting health, from basic research to health care delivery," a process he described as placing equal emphasis on all links in the chain. NIH has recently been asked to do this, in particular in the area of biotechnology. To what extent should Federal agencies supporting scientific research play an active role in seeing that research results are translated into practical application?

A: In its early years, NIH's primary concern was to develop a strong science base which would underpin efforts to attack specific health problems. However, as the state of the art progressed in many scientific disciplines and opportunities for the development of useful medical interventions began to emerge, the NIH actively sought ways to increase the transfer of this information to the health care system and to promote the commercial application of relevant technologies. We believe firmly that the quality of medical care is dependent upon the timely and appropriate transfer of medical technologies from research settings into medical practice and that--as a health agency--the NIH has a major responsibility to facilitate that transfer process.

The degree to which the various technology assessment and transfer mechanisms are utilized by NIH varies according to the needs of each BID's constituencies. Nonetheless, several major types of activities are common to many of the BIDs. These include support of clinical trials, specialized centers and clearinghouses; development and dissemination of scientific publications; conduct of state-of-the-art workshops and conferences; and evaluation of biomedical interventions and monitoring of patent and licensing activities.

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The primary means for NIH's transferring new treatment methods is dissemination of information about them through a number of conduits including: scientific publications, brochures, and pamphlets; staff attendance at professional meetings; and BID public information offices.

In recognition of the need to strengthen this transfer function, the Director, NIH, established in 1977 the Office of Medical Applications of Research (OMAR) to develop procedures for transferring knowledge to promote its effective application in community settings. The functions of this office are to:

- Coordinate, review, and facilitate the systematic identification and evaluation of clinically relevant NIH program information;

- Promote the effective transfer of such information to the health care community and to other agencies requiring such information;

- Provide a link between technology assessment activities for the BIDs and the Office of Health Technology Assessment, National Center for Health Services Research; and

- Monitor the effectiveness and progress of NIH assessment and transfer activities.

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In general, OMAR's mission is twofold: to conduct technology assessment and transfer programs such as the NIH Consensus Development Program and technology assessment conferences, the NIH/DHHS Patent Program, and review and analysis of issues relating to Health Care Financing Administration's policies on Medicare coverage of medical technology; and to conduct research and evaluation of technology assessment and transfer methods. These activities are coordinated by OMAR's full-time professional and support staff working together with numerous BID staff members and receiving assistance from the NIH Coordinating Committee on Assessment and Transfer of Technology.

The Coordinating Committee on Assessment and Transfer of Technology (CCATT) was established by the Director, NIH, to provide a mechanism for the coordination of NIH policy and activities related to health technology assessment and transfer and to share information on these activities with other Federal agencies.

These activities represent a strong commitment to the transfer of new knowledge from the basic laboratory to the health system, thus enabling NIH to effectively carry out its mission to improve the health of the American people.

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Question 6

Q: NIH is unique among the Federal science agencies in that it ties its budget request to a certain number of grant awards each year. Without getting into the current controversy about that number, could you discuss in more general terms how that approach originated at NIH, how it has worked, and whether, in your opinion, it may be applicable to other Federal science agencies?

A: The NIH is commitied to maintaining a strong science base as the means for improving the health of the American people. Several mechanisms are employed to ensure the continued vigor of the biomedical research enterprise including: grant-supported research projects, grant-supported research centers and resources, research contract projects and intramural research. Investigator-initiated research project grants form the vanguard of our research effort, paving the way in the search for new knowledge. Therefore, the highest priority has been placed on the support of this type of award during periods of overall budgetary constraint.

The NIH experienced a period of rapid expansion between 1955 and the late 1960's. However, as the growth curve began to level off NIH found that it could support the increasing pool of excellent regular research project grant proposals only by shifting funds from other program mechanisms. In 1979, the NIH led a Department effort to convene over 100 representatives of research and health organizations to address the increasingly critical need for a comprehensive plan for health research. The resulting report identified "stabilization of the science base" as the most important research planning need, with investigator-initiated research project grants (R01s) receiving top priority.

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I should note that the NIH portfolio of R01s is composed of two groups: (1) new and competing grants requested to initiate or renew a particular research activity; and, (2) noncompeting grants that had received approval earlier through the peer review process. NIH typically approves grants for a three year period but funds are awarded one year at a time as long as progress has been satisfactory. Continuation of funding is deemed a moral commitment that affords a high degree of confidence that there will be no disruption of support during the approved project period. Because this expenditure is fixed, the number of new and competing grants that NIH can afford to fund is subject to all the vagaries of the annual budgetary process.

The concept of stabilization was advanced as a solution to the wide fluctuations in support for new and competing grants which was fostering considerable uncertainty and anxiety in the research community. The logic behind stabilization was adopted both by Congress and the Administration as a means of maintaining a predictable level of support in an era of fiscal constraint. There was general agreement that the chances for an applicant's success should depend solely on the relative merit of the proposal and not on the fortuitous fiscal circumstances of the year in which it is submitted.

The goal of a minimum of 5,000 new and competing awards to be funded each year represented a compromise between what was considered to be desirable and what was considered realistic. The result was that the oscillations that occurred before the inititation of this policy were in fact dampened and the number of new and competing grants has remained relatively constant since 1980.

The desired result of creating a climate that encourages the entry of new young scientists into the system has been achieved but not without cost. One of the assumptions underlying the concept of stabilization was that funds would be available to support about 16,000 research project grants at a level sufficient to outstrip the rate of inflation and to maintain other program activities at their existing levels of effort. This has not been the case. Inflation has taken its toll and the costs of conducting research have outpaced the funds made available for research support. Since 1979, the proportion of the extramural budget devoted to research project grants has risen from 44 percent to 54 percent creating serious imbalances among other program mechanisms. Thus, the commitment to fund 5,000 new and competing grants has been honored largely at the expense of other support mechanisms.

The stabilization concept served NIH well in maintaining the vitality and momentum of the research effort during troubled times. Today, however, several concerns indicate the need to reassess the value of adhering to a policy based upon an arbitrary figure. Prominent among these is a recognized need to introduce sufficient flexibility to permit the exercise of professional judgment in adjusting the allocation of resources to meet competing and changing program demands.

Various routes to "stretch" the research dollar have been examined and found deficient in many respects. For example, payments have been negotiated downward in an effort to fund the greatest number of proposals, but excessive pruning risks damaging the project and losing the original investment. Therefore, NIH intends to fund all research project grants at essentially the full amounts recommended by peer review groups in order to assure the most effective conduct of biomedical research.

- The NIH experience with stabilization indicates that maintaining a steady level of support for a constant number of investigator-initiated research project grants does indeed encourage research advances as fresh new minds enter the field of biomedical science. This approach holds value for other agencies whose missions involve research support. However, the advantages of enjoying widespread support for a particular number of awards must now be weighed in the context of new pressures arising from ever-more intense competition for the research dollar.

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Question 7

Q: Most studies of science and most agency budgets for science are future oriented. They speak of future opportunities, future projects, and future results. Retrospective discussions are limited to anecdotal cases of successes, while little has been done to look carefully at entire programs and the ratio of those which lead to clinical successes and those which do not, how ever measured. Why should not more such comprehensive evaluations of past program be done?

A: The future orientation of science studies and of agency budgets for science is related to the purposes of the documents. Future oriented science studies are usually analyses of the state of the science and are intended to serve as guides to the research community concerning observed areas of research need and opportunity. Budgetary documents outline anticipated uses of budgeted funds.

The tendency to confine retrospective discussions in such documents to anecdotal evidence is unfortunate, though it may not be due to the absence of more comprehensive information. Often such documents require a brevity that does not allow for the explanation of complex evaluative material. Unfortunately also, success or failure in research suoport cannot be described in terms of a simple ratio such as the proportion of projects that lead to clinical successes. Few research support programs are, in fact, aimed directly at producing specific clinical advances, and even those that are must develop balanced programs. One or more aspects of research in a program area may be ready for studies involving clinical application while a dozen other areas require that many questions of a very basic scientific or technical nature be answered before applications questions can even be formulated. A balanced program must attempt to encourage progress in many directions, both basic and clinical. Focus on a criterion such as a ratio of projects leading to clinical successes could lead to a harmful effort to capitalize on only obvious superficial applications possibilities.

Advances in the basic sciences rarely lead directly to application. Often an advance may have significance for an entirely different application area than was intended, and usually, it is only the confluence of many different basic science advances that result in a readiness to attack a clinical problem. By definition, the time it will take to find a solution to a basic scientific unknown cannot be predicted. The critical fundamental discovery that ultimately makes possible a clinical advance may precede that advance by decades, and its relevance to the clinical question may not even be recognized until many years after the event.

All of this is not to say that more comprehensive evaluations of past programs should not be done. The question is what kinds of studies are most useful and effective. The principle of requiring evidence in the support of claims and proposals for change is a sound one, though the urgency for action may militate against delay. NIH has embarked on the development of several databases and analytic methods that are aimed at increasing the capacity to present sound, objective, and timely retrospective evidence of performance. These capabilities have now been developed and refined to where comprehensive analyses of programs can be performed unobtrusively and within a period of time that can effectively serve the needs of program and policy development. In another year the timeliness of analytic capability will be still further advanced as

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bibliometric data will become available in less than a year after publication. It can therefore be said for NIH that more comprehensive evaluations of past programs can and should be done.

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Question 8

Q: Some have observed that in the area of health in the United States communicable infectious diseases play a less significant role while chronic diseases are more prevalent. If this is the case, how should our thrust in biomedical research be changed to reflect that shift?

A: Over the years, as effective therapies and preventive measures evolved for some of the major communicable infectious diseases, biomedical researchers have in fact turned their attention increasingly to the more intractable problems of chronic diseases. This process began decades ago and continues to this day. One indication of this trend, for instance, is seen in the fact that the budgets for the Cancer and Heart Institutes alone constitute approximately 40 percent of the entire NIH budget.

Although a relative shift in emphasis has occurred toward more research directed to the chronic and debilitating diseases which effect increasing numbers of Americans, we have not lost sight of the fact that communicable infectious diseases still profoundly effect the health of our citizens. Infectious diseases result in approximately 27 million patient days of acute hospital care each year. For instance, genital herpes, AIDs, and hepatitis are a few infectious diseases of tremendous national concern which require a commensurate investment of research resources.

In addition, many chronic diseases may have an infectious component. For instance, recent findings have established the viral etiologies of several chronic diseases such as subacute sclerosing panencephalitis, progressive multifocal leukoencephalopathy, kuru, and Creutzfeldt-Jakob disease. Also, human T-cell leukemia/lymphoma virus is now considered the direct causative agent for some human cancers. The fact that such diseases are caused by persistent viral infections suggests that other chronic diseases

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of unknown etiology in man and animals may be caused by persistent infection with known or as yet unrecognized viruses. It is recognized that the full scope of persistent viral infections of medical and economic importance to man is not known today.

When one examines the emphasis that is currently placed on chronic diseases it is clear that an enormous portion of our resources are appropriately devoted to this area, and I can see no compelling reasons calling for a major realignment of current resources.

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Question 9

Q: The Task Force has had some anecdotal evidence suggesting that senior scientists are growing reluctant to serve as peer reviewers for grant proposals because of the workload involved, or because of the detailed personal disclosure requirements, or because of the shortage of funds to support a reasonable fraction of the available proposals. Do you see the emergence of such a reluctance to serve on study panels, and if so, what is the longer term solution?

A: The NIH grants peer review system is oriented toward obtaining the consensus judgment of knowledgeable advisors about the quality of each proposed research activity for which support is being sought. The system depends upon a national pool of scientists for assistance and advice in the selection of meritorious research with the highest scientific promise and technical quality. NIH draws heavily upon the nation's nonfederal scientific community for the expertise needed in making these critical judgments.

NIH has no higher priority than keeping the NIH peer review system strong and highly regarded by the community it serves. The process is frequently studied for imperfections so that improvements can be made. NIH officials are always eager to examine valid concerns. To this end, NIH devoted the November 1984 meeting of the Director's Advisory Committee to an examination of questions that have been raised regarding the growing complexity of the grants award system. Candid exchanges at that meeting did include discussion relative to reviewers: What was their publication record? Were some institutions over-represented on review panels? Were reviewers true "peers," able to deal effectively with grant applications in rapidly changing and newly developing areas of science?

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The task of finding qualified investigators who will serve on review panels is not a trivial one. Much attention is given to the selection process. Only investigators currently productive in research and recognized for their achievements in a particular area of scientific inquiry are invited by the NIH Director to serve. Care is taken to achieve a balance in the scientific disciplines represented on a review panel. In addition, geographical balance is sought, and there is a commitment to appoint qualified women, minorities, and young investigators.

Responding to concerns raised about the availability and quality of reviewers, NIH recently examined in detail the characteristics of study section members over a ten year period. It would appear that by most objective criteria, the scientific competence and professional stature of current and recent members of NIH peer review panels have not declined and that we are, in fact, still able to recruit the services of the most able scientific talent available. This, however, does not assure that the beginnings of problems are not evident. Thus NIH will continue to monitor the process and to guard against potential threats to the quality, efficiency, and effectiveness of the peer review system.

Question 10

Q: Overhead or indirect costs paid on research grants have generally been justified as needed to pay for the costs associated with the performance of research, but they have generally been limited to current operating costs. In your view, should indirect costs be broadened to recover, as well, the capital costs and other non-operating costs of the medical schools and universities?

A: Although both the direct and indirect costs of research are considered legitimate expenses incurred in the conduct of research, they are calculated and managed separately. Direct costs are those which can easily be assigned to an individual project and are subject to peer review and evaluation for relevance to that research effort. These include personnel, equipment, supplies, etc., necessary to accomplish the activity being funded. The indirect costs of research are those expenses that cannot readily be traced to specific projects. Usually included under this classification are expenditures for such items as utilities, depreciation, maintenance, departmental, general and research administration, and libraries. Consequently, some capital costs are in fact allowable under the definition of indirect costs.

Indirect costs are fully reimbursed in accordance with a negotiated rate based upon the allowability and research relevance of particular expenditures. Because research costs are often difficult to distinguish from other functions of a university, e.g., teaching, the terms under which expenses are allocated are prescribed in OMB Circular A-21. Circular A-21 has been revised as the result of several years of negotiation between OMB and the academic community.

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Under the terms described in OMB A-21, capital costs for acquisition, operation, and maintenance of research facilities and equipment may be assigned to the indirect cost category. However, one should not assume that this allowance is sufficient to offset the effects of the demise in 1968 of the Health Research Facilities Act which was the major source of support for the research infrastructure. As existing equipment and facilities deteriorate and become increasingly obsolete, this approach may place additional burdens on research dollars.

The issue of indirect costs continues to be a major cause for concern among the research community and funding agencies. The problem is not that the definition of allowable expenses is too narrow, rather, that indirect costs consume an increasing proportion of the research dollar. In 1966, 15 percent of the total costs of research grants were devoted to indirect costs. By 1985, this figure had risen to 31.7 percent with over 32 percent projected in 1986.

This problem is not unique to NIH but is common to all Federal agencies. In recognition of the cross-cutting nature of the issue, the Office of Science and Technology Policy (OSTP) has undertaken a study of options to contain the growth of indirect costs associated with research awards. Staff of the Department of Health and Human Services are providing data and technical assistance for the OSTP project. This study is part of a wide-ranging inquiry into the financial health of research-intensive

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universities, including the nature and extent of funding needed to help these institutitons remain at the forefront of scientific disciplines relevant to the national security, economic competitiveness, human health and other indicators of the well-being of society.

Question 11

Q: The current Administration has shifted the principal rationale for government funding of research. Instead of emphasizing the clinical and technological payoff, the stress has been in the training of a new generation of scientists as the principal benefit yielded by research grants. In your view, how many scientists do we need in the coming decades, and to what extent will the current levels of research funding meet that need?

A: The principal rationale for NIH support of basic and clinical biomedical research continues to be the development of new knowledge leading to improved diagnosis, treatment, and prevention of disease. The current Administration has not shifted the principal rationale for government funding of biomedical research. It is true, however, that a significant amount of training does occur under research grants since the serving of an "apprenticeship" is a valuable part of the process by which research skills are learned. This training, although extremely important, is a secondary feature of the research grant and by no means constitutes the primary rationale for the support of research project grants.

The NIH has developed other, more direct mechanisms to provide a comprehensive program of research training. Some of these mechanisims incluoe:

Individual Fellowships for postdoctoral research training in which recipients, selected through national competition, are granted a stipend based on their years of experience. An allowance is also provided to the institution to offset training-related expenses. These grants may not exceed three years without a waiver.

Institutional Research Training Grants which may be awarded to a domestic public, nonprofit private, or Federal institution to support a training program in a specific area of research.

The Medical Scientist Training Program which provides support for a six-year program of study leading to the simultaneous award of the M.D. and Ph.D. degrees.

Short-Term Training to exposè students in health professional schools early in their professional studies to the opportunities inherent in research careers. These experiences are usually conducted during off-quarter or vacations periods.

Post Sophomore Fellowships to provide support for selected highly qualified students in health professional schools who are willing to interrupt their professional education for a year of professional training.

Minority Access to Research Careers Program which through institutional fellowships and traineeships strengthen the faculty at minority universities and colleges.

The National Research Service Award Act of 1974 (P.L. 93-348) (NRSA) recognized that there is a close and reciprocal relationship between the continued productivity of research and the availability and replenishment of the supply of well-trained investigators. Their availability wholly

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determines the ability to conduct research. This Act mandated that a continuous strong supply of well-trained scientists be available to carry out the research necessary to meet national health goals.

Section 472 of the PHS Act requires that the Nation's personnel needs for biomedical and behavioral research scientists be met through Federal Government financial support of trainees. The level of such support is based on recommendations by the National Academy of Sciences (NAS) in their continuing study of future needs. This study takes into account training activities that occur under research grants.

Every year each institute at NIH reviews the composition of its research training activity by program, the number of individuals receiving research training in that program, and the level at which training is being received, i.e., predoctoral, postdoctoral physician and postdoctoral Ph.D. Emphasis is then placed on preparing investigators in those areas in which it appears that future research advances will require trained investigators.

Institute plans are reviewed by the NIH Coordinating Committee on Manpower (CCM) and the Director, NIH. The CCM reviews plans of the various institutes paying particular attention to the balance maintained between support for predoctoral students, postdoctoral students and minority training programs. In doing so, the CCM takes into consideration the recommendations made by the National Academy of Sciences.

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Since 1980 the overall level of full time training positions has been approximately 10,000. The resources devoted to NIH extramural manpower and training activities each year represent roughly 5 percent of the total NIH budget.

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Question 12

Q: As you look beyond the current studies and science budgets for the next few years, what changes or adjustments in our goals, objectives, policies and practices do you think are needed in the decades ahead?

A: In seeking to develop a science policy which will serve as a strong framework for the future development of biomedical research, I believe it is essential to achieve explicit and wide-spread agreement on the need to assure a steady and predictable amount of support for basic research which also provides for some established incremental level of growth. It is essential to signal clearly our intention to make vigorous Federal support of basic biomedical research an indispensable and continuing foundation of our national science policy if we are to continue to attract the Nation's brightest minds into careers in biomedical research. Such a policy is also necessary if we are to avoid the type of wasteful disruption of productive research programs which results when large fluctuations are permitted in the amounts of funds available from year to year for research grant awards.

Additional means of increasing the stability, efficiency, and effectiveness of the research system are being sought through efforts to address possible shortcomings that are perceived in the current NIH extramural awards system. It has been suggested, for instance, that one of the factors that may be contributing to the workload of both the grant applicant and the NIH peer review system is the excessive complexity and sheer bulk of the research grant application and that a greatly simplified application form would, of itself, help to reduce the workload involved in preparing and reviewing research grant applications.

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It is also felt that the current average award period of 3 years places first-time recipients at a distinct disadvantage in competing for continued research grant support since the investigator has only about 18 montns in which to "start up" the project and accumulate research findings before starting to write a new application. Consequently, the NIH plans to extend the length of award for first-time recipients to 5 years.

The means by which research support is provided to established investigators is also under examination. These researchers have demonstrated their expertise through outstanding research accomplishments and are widely recognized as leaders in their fields. Yet, they must continue to compete in the standard manner, at frequent intervals, in order to receive continued research support. This practice is viewed by many observers as a wasteful diversion of creative talents. Accordingly, we plan to lengthen the awards for many such individuals to as long as 7 years and will place greater emphasis on the "track records" of these investigators.

In addition to adopting a policy of providing adequate levels of predictable funding for basic research, I feel we must also assure a greater degree of flexibility for health agencies in the allocation of these funds among the many competing research areas. It is somewhat ironic that during a time when science is becoming increasingly unified--as we approach studies at the cellular and molecular level--external forces are creating pressures which tend to compartmentalize the allocation of research funds. While it is understandable that the recent fiscal climate has intensified the efforts of special interest groups within the health field to place their concerns before the Congress, I believe these special pleadings must be placed in greater perspective if we are to avoid serious distortions in research priorities.

I would like to note, that as we seek changes to strengthen and improve the research system, we should also, perhaps, reaffirm those principles and policies that continue to form the bedrock of progress in biomedical research. Prominent among these is the reliance placed on the support of the investigator-initiated research project grant. I believe this will continue to be the major vehicle for promoting and maintaining a vigorous base of free-ranging scientific inquiry which has proved to be so effective in generating new knowledge. However, as recent experience has taught us, we must constantly guard against the possibility of creating program imbalances through preoccupation with selected program components. To avoid these past mistakes we must, for instance, (a) provide funds necessary to support a relatively constant number of trainees to assure a cadre of new scientists to meet our national research needs; (b) maintain support for research centers which combine basic research with clinical application; (c) continue clinical trials to provide evidence of the safety and efficacy of new medical interventions; (d) maintain the NIH intramural research program which performs laboratory and clinical research across the full spectrum of disease areas; and

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(e) strengthen biomedical communications involving the acquisition, storage, and dissemination of information needed in biomedical research, health professional education, and the delivery of health care.

Finally, I should add that no assessment of future goals for science policy would be complete without addressing the growing need to find appropriate ways to strengthen and upgrade the research infrastructure. Over the past decade, increasing concern over the deterioration of the research environment has been widely expressed. The most prominent concerns are for the growing shortages and obsolescence of research instrumentation and the physical deterioration of laboratories, animal buildings and other research facilities.

The full magnitude of the problem is not known, and the NIH has two studies in progress to provide data to better assess both the nature and the severity of the problem. When the results of these studies are available, we should be in a much better position to begin to seek broader solutions to this potentially severe impediment to future research progress.

GOALS AND OBJECTIVES OF NATIONAL SCIENCE POLICY

(With Dr. Lewis M. Branscomb)

THURSDAY, APRIL 4, 1985

HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE AND TECHNOLOGY,
TASK FORCE ON SCIENCE POLICY,
Washington, DC.

The task force met, pursuant to notice, at 8:35 a.m., in room 2318, Rayburn House Office Building, Hon. Don Fuqua (chairman of the task force) presiding.

Mr. FUQUA. The task force will be in order.

This morning, in continuing our hearings on the science policy review, we are very pleased to have a very distinguished scientist with us, Dr. Lewis Branscomb. He is former head of the National Bureau of Standards, senior vice president of IBM, and most recently was Chairman of the National Science Board of the National Science Foundation. He has many other honors, well deserved, to his credit.

Lew, I am very pleased to have you here with the very wide background you have in science and science policy. We are very glad to have you here.

[A biographical sketch of Dr. Branscomb follows:]

Dr. Lewis M. Branscomb, Vice President and Chief Scientist of International Business Machines Corporation, and a member of the Corporate Management Board, is responsible for guiding the corporation's scientific and technical programs to ensure that they meet long-term needs. He joined IMB as chief scientist in May 1972 and was then elected an IBM vice president. In March 1983 he was named a member of the Corporation Management Board.

A research physicist, Dr. Branscomb was appointed director of the National Bureau of Standards by the President in 1969. He joined the Bureau in 1951, served as chief of the NBS Atomic Physics Division, and was chairman of the Joint Institute for Laboratory Astrophysics at the University of Colorado before his appointment as director of NBS.

In 1979 Dr. Branscomb was appointed by President Carter to the National Science Board, and in 1980, he was elected chairman. He is also a member of the President's National Productivity Advisory Committee and chairs its Subcommittee on Research, Development and Technological Innovation.

Dr. Branscomb was graduated from Duke University summa cum laude in 1945. He was awarded M.S. and Ph.D. degrees in physics by Havard University in 1947 and 1949. During his career, he has taught at University College, London, the University of Maryland, the University of Colorado, and Harvard where he was a member of the Society of Fellows.

Dr. Branscomb has received the Rockefeller Public Service Award, the Samuel Wesley Stratton Award, the Gold Medal for Exceptional Service from the United States Department of Commerce, the Procter Prize from the Scientific Research Society of America, and the National Civil Service League Award. He holds honorary

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doctor of science degrees from Duke, Western Michigan, and Rochester Universities, the Universities of Colorado and Alabama, Polytechnic Institute of New York, Clarkson College of Technology, and Lycoming College, and an honorary doctorate in humane letters from Pace University.

Among his affiliations, Dr. Branscomb has been a member of the President's Commission for the Medal of Science and the President's Science Advisory Committee. A member of the National Academy of Engineering, the Institute of Medicine, the National Academy of Sciences, American Philosophical Society, Royal Society of the Arts and past president of the American Physical Society. He has served on the U.S. Department of State's Advisory Committee on Science and Foreign affairs, and is a former member of the board of directors of the American Association for the Advancement of Science.

Dr. Branscomb is a director of General Foods Corporation and Mobil Corporation, and a trustee of the Carnegie Institute of Washington and Vanderbilt University.

Dr. and Mrs. Branscomb have two children.

Mrs. Branscomb is an attorney.

STATEMENT OF DR. LEWIS M. BRANSCOMB, VICE PRESIDENT AND CHIEF SCIENTIST, IBM CORP., ARMONK, NY

Dr. BRANSCOMB. Thank you, Mr. Chairman.

I would like to compliment you and the committee on quite an extraordinary piece of work you have undertaken to review the Nation's science policy. It has been 40 years since a 9-year debate began, almost a decade's debate, in the Congress on what our science policy should be. Among the protagonists was Senator Killgore of Tennessee, who took a very pragmatic view of the utility of science, and a very distinguished scientist, Vannevar Bush, who emphasized the importance of pure science. President Truman resolved those conflicts in the establishment of the Science Foundation.

The policy that we have been taking wonderful advantage of for the past 32 years is perhaps best described in Vannevar Bush's book, *Science—The Endless Frontier.*

I think this committee has recognized that it is now 1985. The cornerstone of U.S. science policy must still be a national commitment to excellence in science and engineering, but it takes more than explorers and homesteaders and trappers and prospectors to build a nation. We need farmers, roadbuilders, school teachers—the infrastructure of a modern nation.

While "The Endless Frontier" is as vital and important as ever, there is a lot to do this side of the frontier to insure that the benefits of science are properly made available to our people.

For scientific and technical achievement, like entrepreneurial skill and athletic prowess, are elements of our culture, and we measure the vitality of our society by our attainments in those areas, just as we measure the quality of our society by the prevalence of justice, equality, and caring. But scientific and technical achievement is more than culture; it is a means to a broad variety of ends.

A science policy must focus on more than just strengthening science, but on the processes through which a strong science benefits current and future generations.

I believe that is why developing a consensus around a national science policy is so difficult—for we can all agree on the importance of leadership in science. It is harder to agree on policies to improve the effectiveness of the mechanisms that harvest the fruits of science.

I think the difficulty in part stems from economic realities that so much influence that process. We are all aware that the economy historically has been very effective at converting scientific discovery into innovation, and thus new jobs and higher living standards. There are other countries, like the United Kingdom, that have superb science that have not been as successful. We tend to take that linkage of science to jobs for granted, but in fact we cannot build a healthy economy just on science and patents and the sale of technology. We have to manufacture here in this country. Americans have to do the work that converts those fruits of science.

Today the overpriced dollar is driving an enormous negative trade balance which itself reflects a loss of benefits to U.S. technical leadership to people of other countries, to which manufacturing is rapidly moving. So closing the budget deficit is a key element in national science policy.

Fluctuations in the macroeconomic environment will always dominate apparent technological performance of our industry. People don't always realize that. When the economy gets very healthy in international trade terms, suddenly our technology looks like it is more vital. To some degree, that is in fact simply a reflection of economic realities.

But those economic fluctuations also mask the issue of the systematic basic strengths and weaknesses in the economy and in the technology specifically. So my point is that, while a national science and technology policy has to be grounded on economic policy that provides the climate in which science can serve the public effectively, one cannot dismiss the flight to offshore production of high tech products as solely due to an overpriced dollar.

Rapidly industrializing countries like Korea and Taiwan and Singapore are showing impressive capability at managing, absorbing, and producing economic benefits from the fruits of our science.

I think the economic environment for science policy is changing in two other very important ways. One is that these increasingly knowledge-intensive activities do not represent an economic sector in itself that will bring the benefits of science to the public. Knowledge-intensive activities are not an alternative to manufacturing and services and agriculture. They are the means whereby all kinds of work become more productive.

So if the knowledge base or information sector does not contribute to that productivity, and hence competitiveness, we won't be able to afford the investment to keep that knowledge sector moving.

Yet, in much of our science policy discussion—and I must here exempt Mr. Brown, who has led the emphasis on this whole area of information technology and policy—there is still a tendency not to deal with the software side of the technology as though they were an important mainstream of modern science and technology. One reason for that is that they are not easily defined and encompassed like physics or electrical engineering. Once you get into the software side of technology, you begin having to deal with managerial and cultural and even aesthetic values that are inseparable from the computer communications and programming skills that are more technical.

In this sense, we need to look hard at the social sciences, in particular the more quantitative side in social sciences, to identify those that really are in a position to make a contribution to this new kind of technology and science that we have to deal with. And to continue the old line of thought that says the social sciences are simply an academic reflection of what goes on in society viewed from afar is, I believe, an unfair characterization of what the social sciences need to be doing as a part of the overall technical effort.

The other point I want to make is that the fact that knowledge-intensive work is an increasingly important part of technology has implications for the global character of the competitive arena. Our very strength in science and engineering, and information science in particular, positions us well for competition globally, but our public attitudes toward technology transfer and the role of science in international affairs is not always in accord either with the economic reality of the global marketplace or the government's current focus on free trade.

Free trade in ideas and information must follow free trade in goods if we are to capitalize on our natural advantages in the information-rich, high-tech economy, because those things are inseparable.

Now I would like to focus for just a moment on the changing nature of science, for it is changing not only in encompassing areas of intellectual work that we might have thought of as social science in the past, but the practice of science and engineering is profoundly changing, and this committee in fact has been central in that recognition—that the distinction between science and engineering is beginning to blur and they are becoming increasingly interdependent.

Indeed, science is becoming more dependent on technology, just as technology is becoming more dependent on science. But more important, the combination of science and technology is becoming more complex and more capital-intensive. This is putting a lot of pressure on our scientific institutions.

Leadership increasingly depends on system and software science, on disciplinary approaches, on sophisticated intelligent instrumentation. At the same time individual creativity remains the keystone to excellence. New ways of helping our universities learn to manage in that environment are going to be needed.

The scientific basis for technology has been understood to be important for a long time. We have to invest in the technology for doing and using science. I think the new policy on engineering worked out by the National Science Board during the last 4 years, and the recognition of its importance by this committee, is of great potential importance. The first grants, in fact, under that engineering program were announced in the newspaper this morning.

I think, however, we have a long way to go. I would call attention to the fact that the universities are ready to respond with modernization of their engineering capabilities. One evidence of that is that NSF received something like 2 billion dollars' worth of proposals for those new engineering research centers.

Another piece of evidence is that, when IBM offered a competitive grant program to the universities in manufacturing systems engineering technology, we thought we would be lucky to get 8 or

10 good proposals; 172 universities responded. Many of them have moved ahead in this area without, indeed, having received any support from us.

Support for the private sector's technological competitiveness has now emerged as a primary requirement for Federal investments in the research base. That is new since Vannevar Bush's day.

Yet, in spite of a remarkable increase of university-industry cooperation, the agencies that support the great majority of university research, and indeed the universities themselves, have little capability to respond quickly and effectively to new areas of research promise that arise from that cooperation.

The role of the national laboratories, perhaps with the exception of NBS, is still undefined in relation to their economic value to our society. Indeed, the Bureau is shrinking and changing at a time when I believe greater reliance should be placed on it.

On the other hand, I want to be clear that I oppose the direct Federal support of private sector commercial innovation which has been advocated by some people under the general label "industrial policy." But the investment that the Government makes in university and national laboratory research should be guided, to an appropriate extent, by the potential for dramatic advances in technology as well as by intrinsic scientific interest—technologies that can serve our economic roles.

I believe the best way to achieve that is to encourage, through tax policy and other means, the voluntary collaboration of private industry with universities and national laboratories, with the Government agencies adjusting their program priorities for science support to respond appropriately to opportunities that are identified by the academic scientists after they have had their relationship with their industrial peers.

I would like to make a few remarks about information policy for science and science information policy, two important but different ideas.

We clearly need a more sophisticated view of the Nation's intellectual assets that provide for nurture, protection, and sharing in appropriate balance—avoiding the extreme of self-defeating protectionism but recognizing that technical leadership is the primary value added for our economy and for our defense.

At this time the most visible issue in that general area is the Government's attempt to find policies and regulations that will slow the diffusion of important knowledge in science and engineering to the Communist bloc countries while basing our own defense strategy on high-tech science leadership that can only be maintained by the most extensive and open scientific communications within the United States and with our NATO allies.

Knowledge is not free. It must be husbanded, but the husbandry may be a thoughtful policy of encouraged diffusion, reserving protection to a carefully chosen limited set of critical assets. The policies to guide that selection process in the total national interest are making good progress, but they are not yet established.

The most critical problem is not East-West information exchange, but the health of scientific cooperation and competition with the Western industrial democracies.

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The debate over export policies on technical information can breed distrust in the alliance, as Europeans may suspect that U.S. policy is aimed as much at retaining U.S. commercial superiority as it is at preventing potential enemies from turning our own technology to their military advantage.

Of course, Americans may, with some justification, suspect that policies of other countries in tariffs, industrial standards, and regulatory administration are themselves tainted with protectionist motives.

This is a complicated world we now live in, and just as many companies have to learn to manage relationships with other companies simultaneously as customer, competitor, and supplier, Americans will have to learn how to share our science, compete in high-technology commerce, and share our defenses with allied nations whose governments invest directly in their national enterprises and will necessarily have mixed motives on matters of science information and technology transfer policy.

Scientific and technical information are increasingly critical to both public and private decisions, especially decisions on the uses of technology. This trend is part and parcel of an increasingly information-intensive economy and was given great emphasis by the OECD over a decade ago.

Yet, we read only last week that OMB is planning to cut even further back statistical data collections of the Federal agencies. Many of these systematic data collections must be considered as part of the technical infrastructure that underpins our future. Science for policy is as important as policy for science.

Public concern for quality of opportunity in the economy of the future, not only in international competition but in States and communities, will accelerate as political initiative for economic promotion continues to shift to the States. National science policy has meant Federal policy for four decades. It must now shift and focus on State and private sector policy as well.

At State level the linkage of educational quality, scientific research, and the growth of high-tech employment is an article of faith now. At the Federal level responsibility for supporting excellence in fundamental science, primarily through university research, has been a cornerstone of U.S. science policy for decades and is well accepted. These two strategies—State and Federal—are not conflicted but they are not coordinated. As a result, confusion reigns over the matter of responsibility for institutional infrastructure for science and the need to coordinate investment strategies.

This is a much more substantial and complex issue than arguments about geographical distribution of research grants. The coupling of Federal and State interests in scientific development takes place primarily through the Congress, for the tenuous relationships between Federal science agencies and the State houses have been almost completely dismantled.

Some universities have been criticized for lobbying their friends in Congress for appropriations for research facilities, bypassing peer review. I agree that the trend is reason for concern, primarily because it may threaten benefits in scientific excellence that result from vigorous but fair competition within the academic community.

However, it is unrealistic to believe that science and engineering capability in local communities can be seen as the route to jobs and a better life without engaging the political process in the development of that capability. Thus, the decentralization from Federal to State level of initiative for high-tech economic development makes sense, but closer coordination of Federal investments in scientific infrastructure with State strategies for economic development must be sought. In fact, the linkages, as I said before, in intergovernmental coordination have become somewhat weakened in recent years, although the Governors' conference I think has shown great leadership and brought the Federal community into its work.

I have been speaking about equal opportunity for communities and States. Equal opportunity for careers in science and engineering has always been an important element of social equity. In a knowledge-intensive economy more than the elimination of prejudice is required. The quality of public instruction will increasingly determine the meaning of equal opportunity for all citizens.

This educational dimension of science policy has evolved from the need to train future science specialists to the opportunity for all our young people to prepare for the careers of the future.

I think I would like just to mention two other things and then allow the committee to direct its discussion however it would like.

First, I would like to go back to my comment about interdisciplinary opportunities. Last year when this committee entertained and heard from our newest Nobel laureates, your hearing was followed by a seminar at the Academy in which four of our former laureates addressed the issue: What is happening in their areas of science? These four distinguished laureates were in the fields of organization theory, economics, biology, and physics. They spanned the disciplines pretty well.

They told me they had not compared notes in advance on what they would say, and yet each of the four said exactly the same thing. Each one said the most important ideas and exciting opportunities in my field are now being seized by people who are able to reach into many disciplines, all the way from mathematics to more applied areas, and combine the results to fruitful purpose.

That led this small audience to a free discussion of why was it so difficult to do interdisciplinary science when the leaders of science all recognize that that is where the action is, where the progress is being made. The observation emerged in that discussion that the disciplines are terribly important because they are the gatekeepers of quality standards in our science without which science would be of no value and make no progress.

So we have a balancing act to do between maintaining the standards which are done by the disciplines and yet somehow responding with speed and with concentration of flexible resources for these much more cross-discipline opportunities.

I think that is one of the greatest challenges facing our universities. I have the feeling that our university funding agencies, while they understand that dynamic intellectually, find it difficult to operate their grant programs in such a fashion that they truly respond to those kinds of opportunities. Indeed, I suspect that peer review is harder to do in those kinds of environments than it is within the narrow disciplines. I regard that as a challenge which we will have to learn how to master without giving up the virtues of peer review and the disciplines for maintaining quality standards.

Finally, I would like to come back to my brief comment about national laboratories. They represent a very substantial part of our research investment, Federal research investment, and they have an enormous capability. If you look at the scientists and engineers who work there and the facilities they have, and, indeed, they, like corporate research laboratories, do not suffer from this difficulty with respect to interdisciplinary work to the same degree as do most universities.

Yet, there has been an endless number of studies, I remember, on how to make better use of this great capability.

I remember that in 1968, when I was on the President's Science Advisory Committee, Professor Hill of MIT was asked to do one more study about the national laboratories. Don Hornig, the science adviser, thought this would be a very time-consuming task. Hill, in fact, came back in 2 weeks with his work, which was represented on a single page. He made a chart, rows and columns, in which in the first column he listed 14 previous Federal studies on how to make better use of our Federal laboratories, and across the top row he listed all possible recommendations that you could make. Then he put a checkmark in this plot everywhere one of those studies had made one of those recommendations. The chart was a forest of checkmarks. He said, "Here is my study. I suggest you get on with these recommendations." [Laughter.]

I would suggest to you that what that proved was that it is a much tougher problem than those 14 studies appreciated.

My own belief is that the national laboratories could serve the country much better if they were in a position to be more flexibly managed, and that rather than trying to invent new missions for them in these kinds of studies, we might do better to try to invent a new organizational structure within the Government that would permit their redeployment, or at least their partial redeployment, in a swifter way when problems come along that need urgent attention and that appropriate for them to work on.

I remit that these ideas are not totally new, because they have been suggested in hearings in this committee before, and they are very difficult to accomplish. But, of all of the notions that have been put forward from time to time under the general heading of department of science or science reorganization, one that has always appealed to me was something that would give a top science executive in the Federal executive branch, whether the science adviser or a minister of science or perhaps even the director of a major agency, the freedom to deploy some fixed percentage of certain selected national laboratories, preselected national laboratories, to redeploy those on urgent new tasks without having to come first through the budget bureau and the Congress for authorization, in order to be able to put 50 people or 100 people or 500 people on such a problem with the stricture that that program could not continue for more than 2 years or 3 years without coming back to the Congress, the agency that is responsible for that laboratory, for confirmation that this was appropriate work and for its proper budgeting and review.

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Maybe that idea is not terribly practical, but somehow it seems strange in the world in which we know it is not appropriate to try to redeploy our university resources to meet urgent near-term needs. We all know the experience with the RANN Program with NSF, which was not very successful. That is not the right role for the universities, but we do need the ability to deploy interdisciplinary, broad-ranging, advanced scientific capability against urgent environmental needs or other questions that emerge in a more flexible manner than we have been able to do in the past.

That idea has been inspired perhaps by the fact that it was successfully done, at least for a period of time, in the French Government when Pierre Aigrain was Minister of Science—sorry, when he was head of DGRST. He had resources that he could deploy of that character within the CNRS. I thought it an interesting idea.

Mr. Chairman, I think I have imposed on your patience long enough.

[The prepared statement of Dr. Branscomb follows:]

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FUQUA HEARING OUTLINE HEARING MARCH 14, 1985

Goals and Objectives of National Science Policy

Introduction:

The cornerstone of U.S. science policy must be a national commitment to excellence in science and engineering. Scientific and technological achievement, like entrepreneurial skill and athletic prowess, are important elements of our American culture. We measure the vitality of our society by our attainments in these areas, just as we measure the quality of our society by the prevalence of justice, equality and caring. But scientific and technological achievement is a means to a broad variety of ends.

A national policy for science must focus on more than strengthening science, but on the processes through which a strong science will benefit current and future generations of Americans. This is why generating a consensus behind a national science policy is so difficult. We can all agree on the importance of world leadership in basic science; it is harder to agree on national policies to improve the effectiveness of the mechanisms that harvest the fruits of science.

This difficulty stems from the economic realities that so strongly influence the value of scientific leadership to the society. We are all aware that the US economy has, historically, been very effective at converting scientific discovery into innovations that when commercialized create new jobs and higher living standards. Other countries, like the U.K., have failed to benefit to the same degree despite the impressive performance of their scientists.

Economic Environment for Science Policy

We Americans must not take this linkage of science to jobs for granted. We cannot build a healthy economy relying only on science, patents and sales of technology. Americans must be able to manufacture, sell and service the resulting products. For if all the high tech production goes off-shore, the experience others gain with the technology through manufacturing will soon erode our technical lead. The revenue stream from production is required to finance the technology needed for competitiveness.

Today the overpriced dollar is driving an enormous, negative trade balance, which itself reflects a loss of the benefits of U.S. technical leadership to the people of other countries where manufacturing is rapidly moving. Thus closing the budget deficit is a key element in the national policy for science and technology.

Fluctuations in the macroeconomic environment will dominate the apparent technological performance of U.S. industry. But, they will also mask underlying systematic strengths and weaknesses. My point is

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that a national science and technology policy must be grounded on economic policy that provides the climate within which science can serve the public effectively. One cannot simply dismiss the flight to off-shore production of high tech components as solely due to an overpriced dollar. Rapidly industrializing nations like Korea, Taiwan and Singapore are demonstrating impressive capability at absorbing managing and producing the fruits of American scientific and advanced development talent.

The economic environment for science policy is changing in two other very important ways. First, we all know the statistics that show how our modern economy is redeveloping its workflow from agriculture and manufacturing into activities sometimes called services, and especially into information-related activities.

A Knowledge-Based Economy

What is not well understood is that these knowledge-intensive activities are not an alternative to manufacturing, services and agriculture; they are the means whereby all kinds of work become more productive. Indeed, if the knowledge-based or information sector does not contribute to productivity, and hence competitiveness in manufacturing services and agriculture, we will not be able to afford to continue to invest.

Yet, our science and technology policy discussion - indeed our educational institutions - have not come to grips with these "software" technologies, in part because they do not stand alone as a special skill--like physics or electrical engineering. Managerial, cultural and aesthetic values are inseparable from computer, communications and software skills. Public services, equality of opportunity and other issues call for tradeoffs between market forces and the public good as a guide to future development.

A Global Competitive Arena

Flowing directly from the importance of knowledge-intensive work is the global character of the competitive arena. The very strengths of the American society - and especially of our science and engineering - positions us well for competition in a global marketplace. However, our public attitudes toward technology transfer and the role of science in international affairs is not always in accord with either the economic reality of a global marketplace or the governments current focus on free trade. Free trade in ideas and information must follow free trade in goods if we are to capitalize on our natural advantages in the information-rich, "high tech" economy -- they are inseparable.

This committee's review comes at a watershed time in the relationship of American science and engineering to the nation's future, for many circumstances have changed since basic science policy was set in the 1950's. Let me summarize some of the key changes, and the policy issues they raise.

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a) THE CHANGING NATURE OF SCIENCE

The practice of science and engineering is profoundly changing, blurring the distinction between them and enormously increasing their power for progress and for application. But this comes at the cost of increasing complexity and capital intensity, putting great pressure on scientific institutions. Leadership increasingly depends on systems and software science, on pan-disciplinary approaches and on sophisticated, intelligent instrumentation. At the same time individual creativity remains the keystone to excellence. New ways of funding equipment and creating new research activities across traditional disciplinary boundaries are needed.

The scientific basis for technology has been understood to be important for many years; now we must also invest in the technology for doing and using science. The new policy on engineering worked out by the National Science Board during the last four years, and the recognition of its importance by this committee are of great potential importance. The nation's universities are eager to respond, as indicated by the 172 universities that responded to IBM's manufacturing systems grant competition and the $2 billion in proposals for the NSF Engineering Research Centers. But the administration and congress have not yet faced up to the implications of supporting the kind of technology base our universities should be providing.

b) TECHNOLOGICAL COMPETITIVENESS

Support for the private sector's long term technological competitiveness emerges as the priority requirement for federal investments in the nation's research base. On this point everyone in the debate on industrial policy agrees. Yet, in spite of a most remarkable increase of university-industry cooperation in many fields, the agencies supporting the great majority of university research have little capability to respond quickly and effectively to new areas of research promise that arise from that cooperation. The future role of the national laboratories, except for NBS, is still undefined, and the Bureau is shrinking at a time when greater reliance should be placed upon it.

I oppose, as impractical, the direct federal support of private sector commercial innovation advocated by some under the label "industrial policy". But the investment government should and will make in university and national laboratory research should be guided by the potential for dramatic advances in technology, as well as by intrinsic scientific interest. The best way to achieve this is to encourage, through tax policy and other means, the voluntary collaboration of private industry with universities and national laboratories, with government agencies adjusting their program priorities for science support to respond to the opportunities identified by academic scientists and their industrial peers.

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c) INFORMATION POLICY FOR SCIENCE

We must have a more sophisticated view of the Nation's intellectual assets that provides for nurture, protection and sharing in appropriate balance, avoiding the extreme of self-defeating protectionism but recognizing that technical leadership is the primary value-added for our economy and our defense. At this time the most visible issue is the government's attempt to find policies and regulations that slow the diffusion of important knowledge in science and engineering to the communist bloc countries, while basing our own defense strategy on high-tech science leadership that can only be maintained by the most extensive and open scientific communications within the U.S. and with our NATO allies.

Knowledge is not free; it must be husbanded. But the best husbandry may be a thoughtful policy of encouraged diffusion, reserving protection to a carefully chosen limited set of critical assets. The policies to guide that selection process in the total national interest are not yet established.

The most critical problem is not East-West information exchange, but scientific cooperation and competition within the Western industrial democracies. The debate over export policies on technical information can breed distrust in the alliance, as Europeans suspect that U.S. policy is aimed as much at retaining U.S. commercial superiority as it is at preventing potential enemies from turning our own technology to their military advantage.

Of course Americans may also suspect that other policies - in tariffs, industrial standards and regulatory administration by our Allies are themselves tainted with protectionist motives.

Just as many companies have learned to manage relationships with other companies as customer, competitor and supplier simultaneously, Americans will have to learn how to share our science, compete in commerce, and share our defenses with allied nations whose governments invest directly in their national enterprises and will necessarily have mixed motives on matters of science information and technology transfer policy.

d) SCIENCE INFORMATION FOR POLICY

Scientific and technical information are increasingly critical to both public and private decisions, especially decisions on the uses of technology. This trend is part and parcel of an increasingly information-intensive economy, and was given great emphasis by the OECD over a decade ago. Yet, we read only last week that the OMB is planning to cut even further back the statistical data collections of the federal agencies. Many of these systematic data collections must be considered as part of the technical infrastructure that underpins our future. Science for policy is as important as policy for science.

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e) COMPETITION FOR OPPORTUNITY: STATES AND COMMUNITIES

Public concern for equality of opportunity in the economy of the future, not only in international competition but in states and communities, will accelerate as political initiative for economic promotion continues to shift to the states. National science policy has meant federal policy for four decades; it must now focus on state and private sector policy as well.

At state level the linkage of educational quality, scientific research and growth of "high tech" employment is an article of faith. At the federal level, responsibility for supporting excellence in fundamental science, primarily through university research has been a cornerstone of US science policy for decades and is well accepted. These two strategies - state and federal - are not conflicted, but they are also not coordinated. As a result confusion reigns over the matter of responsibility for institutional infrastructure for science, and the need to coordinate investment strategies.

This is a much more substantial and complex issue than arguments about geographical distribution of research grants. The coupling of federal and state interests in scientific development takes place primarily through the Congress, for the tenuous relationships between federal science agencies at the statehouses has been almost completely dismantled. Some universities have been criticized for lobbying their friends in Congress for appropriations for research facilities, bypassing peer review. I agree that the trend is reason for concern, primarily because it threatens the benefits in scientific excellence that result from vigorous but fair competition within the academic community. However, it is unrealistic to believe that science and engineering capability in local communities can be seen as the route to jobs and a better life without engaging the political process in the development of that capability. Thus the decentralization from federal to state level of initiative for "high tech" economic development makes sense. But closer coordination of federal investments in scientific infrastructure with state strategies for economic development must be sought. In fact, the linkages for intergovernmental coordination have become weakened in recent years.

f) COMPETITION FOR OPPORTUNITY: EDUCATION AND THE DISADVANTAGED

Equal opportunity for careers in science and engineering has always been an important element of social equity. In a knowledge-intensive economy, more than the elimination of prejudice is required. The quality of public education will increasingly determine the meaning of equal opportunity for all citizens. The educational dimension of science policy has evolved from the need to train future science specialists to the opportunity for all young people to prepare for the careers of the future.

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PROPOSITIONS FOR A NEW SCIENCE POLICY

1] A national policy is not just federal policy; the states and private sector now have major responsibilities.

2] Excellence in science is not enough. Science policy must encompass the processes for public benefit from science.

3] Economic environment dominates the short-term benefits from science and technology and must be managed to permit the private sector to sustain technological competitiveness.

4] Federal role in commercial technological competitiveness should focus on support for the research and educational base, primarily through the colleges and universities.

5] Industry-university cooperation should be encouraged to maximize economic return on the federal research investment.

6] The new software sciences underlying a knowledge-based economy must be encompassed in science policy. They include cognitive, behavioral and aesthetic dimensions, less easily separated from their social/economic context than traditional "hardware" sciences.

7] Knowledge-intensive technologies are essential for competitiveness in all sectors: manufacturing and agriculture as well as the "high-tech" sector. The entire technology base of the economy must be strong, not just one favored sector.

8] America must compete in a global economy, and must have access to world markets. Free trade in goods requires free trade in services, patents and information. Information policy must be a part of science policy, and must strike the balance between asset protection and asset exploitation.

9] Research to support public and private decisions on technologies will be increasingly important, as will the maintenance of statistical databases on which policy-relevant research must rest.

10] Science policy must include means to increase R&D productivity. Science information services are an important element of that strategy.

11] State and local governments use research and education investments to compete for economic opportunity. Federal policy must recognize, indeed encourage this initiative, and insure that federal research investment strategy is compatible with state goals. States must focus their higher education strategies to match the realistically available research resources.

12] Scientific progress increasingly requires integration of ideas from many disciplines, while the disciplines serve to maintain the quality standards of science. Our universities must be helped to capture these interdisciplinary opportunities without sacrifice of their stewardship of quality standards.

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13] Engineering is the vital link between science and its economic benefits. Further, science increasingly depends on the fruits of technology as research becomes increasingly dependant on instrumentation and information system support. Federal support for universities must encompass a more effective balance between manpower and facilities investments.

14] Engineering education and research must strike a better balance between research and development and design and production if the U.S. is to be economically competitive.

15] The quality of public education has always been important in preparation of scientific careers, but now becomes the most important element of equal opportunity for every one who will work in a knowledge-intensive economy. Thus science policy must embrace effective education for all, not just the future technical professionals.

16] The national laboratories are a great and underutilized asset. The best way to update their missions is to structure a government organization capable of redeploying them to priority federal R&D needs whenever that is required.

17] Science is an increasingly important tool of foreign policy, but is increasingly difficult to manage effectively as foreign governments get more sophisticated about their interests in technology, and the U.S. has goals requiring access and negotiation in foreign countries. U.S. policy formation processes are inadequate for the future.

DISCUSSION

Mr. FUQUA. Thank you very much, Lew.

You touched on the interdisciplinary and the fact that some disciplines have difficulty understanding others. The Chairman of the National Academy of Space Science Board said recently that "we are experts at setting priorities in any one field of science. The astronomer, for example, finds it difficult to judge impartially the value of research in life sciences. The ultimate judgment of our priorities is made adequately by the present method of relying on the complex democratic process to make essentially political decisions."

I guess my question is this: How do you resolve that issue of science and the political issue? Many times we have in this committee taken initiatives that were not forthcoming, say, by the Science Foundation or by other groups that we felt was in the national need. One was in the area of science and engineers in education. We felt it was very important. Another was an initiative in supercomputers. Those are just two recent ones that come to mind.

Is that the way it should work or should we wait for the scientific community to come forward in understanding that, particularly in the Federal Government process they have to go through OMB and there are certain restrictions, and free thought sometimes ceases once they make a decision? That may be based on budget decisions, not on a policy decision. How do we do that? Are we an impediment? Can we foster good science?

Dr. BRANSCOMB. You certainly are not an impediment, for the Congress, in my view, is and has been the most effective steward of our scientific capabilities in the past 30 years that we have in this country.

Let me try to answer your question by an analogy with a question that was once asked of me and my company shortly after I went there. I was asked, "Could you please tell the management how we ought to decide how much money to spend on R&D, and we are interested in how much money we should spend on R as well as the separate question of how much we spend on D."

My answer was that, first of all, the decision on how much money you should spend on development is not a global decision that you make at all, for development serves a very clear purpose. It is to achieve a certain business opportunity. So that decision should be made by examining all these opportunities for their value—in this case, to the company by analogy to the country—and whatever of those opportunities seem worth pursuing, then you must do the appropriate science and engineering work to achieve that end result.

My first answer to your question is, in all the areas where we see science or research or engineering as useful tools to achieve a goal, then whatever element in our society has the opportunity to get on with achieving that goal should proceed to do so.

The scientific community should be called upon to help out. If it means to follow, because this involves something innovative, fine, then let the need pull the science.

But I also said that, if we left it solely—if we could leave it solely to the business process in my company or the political process in the country, to determine all of that science that is needed, then we wouldn't have to do anything else. It would pull so much far-reaching thinking and so much good educational investment that we would achieve our objectives in a very demand-pull-oriented environment.

But we, in fact, all know that won't work. In a business where the business elements are pressing very hard to be competitive and cut costs and shorten the development cycle, they simply cannot take the time to invest in those long-term issues and, in any case, they are not really equipped to make those judgments or to nurture the kind of people who do forefront work in science.

Therefore, you have to take a piece of the investment and split it off and protect it. In my company that is like 10 percent of the total. In that piece, now you have to leave it to the judgment of the scientists who manage it to decide what the internal distribution of investment should be for those opportunities that are science driven, driven by perception of scientific opportunity. Those things need to be decided by scientists.

The analogy in the country is that we do need agencies like NSF, like NIH, where their job is to have the scientists pursue the intellectual opportunities that over the long term will give our people the best benefit from that investment. As Bob Wilson once said in testimony for Fermilab, make the country worth defending even if the investment doesn't help defend the country.

Therefore, the answer has to be both. I would dearly wish that in the case of NSF the Science Board truly did make the final decision on the allocation of investments across disciplines and that that was fully delegated by OMB. I respect, however, OMB's right and certainly their authority to express more of their opinions and to have that balance also reflect the President's judgments about what is important in the large, and of course those judgments need to be respected and in fact reflected in the statute as appropriate for NSF.

But nothing, I believe, should in any way deter either committees of Congress or committees of citizens to perceive a role that science can play usefully in our society and get on with trying to produce the necessary results. That, to me, is what has made our country great, and it is what makes the science budget not a fixed pie problem, even though it is often perceived that way by the universities. It isn't a fixed pie at all; we use a fraction of the ideas and knowledge and imagination that is available in this country to solve its problems. We don't need to throw money at that community, but we certainly shouldn't refrain from using brains wherever they can be deployed.

Mr. FUQUA. One of the conclusions of Vannevar Bush's study was that there was a proper role for the Federal Government in basic research, but he cautioned that we not do it—that there not be so much involvement of Government that it stymies industry from their appropriate role in that.

How do you see that working today? Is industry picking it up with tax credits, R&D tax credits? Is that a stimulus? It has not been in effect very long. I am not sure we have a good handle as to how effective it is.

Are we putting too much Federal funds into that to the detriment of industry picking up an appropriate amount?

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Dr. BRANSCOMB. You have asked two questions. Let me try to address both of them.

First of all, I believe that on the issue of the Federal strategy for investing in the fundamental science as a means to economic stimulation, we really have a pretty broad consensus in the country now that the best way to do that—there may be exceptions, but the best way to do that—is to invest in our university research sector, and that has two enormous strategies, which is the one that has been an important one in the last 30 years. It has two very important benefits.

One is that that probably is the best way to get postgraduate science education accomplished—by doing the science in the universities rather than in independent institutes, as would be typical in the Soviet Union or Australia or other places.

Second, so long as there is a healthy collaboration between industry and the universities, then the Government's participation with the universities permits the universities to be a healthy partner with industry. In my opinion, that is going exceptionally well today compared, say, to 10 years ago when there were great barriers between universities and industry.

In fact, it is quite extraordinary. My own company, at last count, had undertaken 1,200 independent projects with 130-something universities in this country since 1982. I put that only as evidence that the universities are receptive to this kind of collaboration.

My comments earlier were aimed at expressing a concern that when there is an area of great interest to our economy and to industry, that the universities also seem to be very interested in, such as computer-aided design and manufacturing and new manufacturing processes and ways of manufacturing, ways of managing production, the Government finds it slow and tortuous to find ways to deploy resources to help the universities in that collaboration. I would like to see that be a little more swift. I think the NSF program that they have now embarked upon is moving in that direction. So I am optimistic about the future.

The piece of that industry-university collaboration that is not yet functioning properly, which I hope the engineering investments will really make a difference in, is the middle to smaller-sized company. Industry-university collaboration works very well with companies that have corporate research and, therefore, have researchers who are very much like the university peers. They can talk to each other very easily.

It is harder for small companies that do only design and production engineering and do very little scientific research, but they need the intellectual help just as the larger companies do.

On the specific issue of tax credits, I believe that indeed that has had a positive effect. There is, in fact, a debate on the record on that subject between myself and Professor Mansfield at the Harvard Business School last year, and I know that manuscript is available to you.

My own belief is that there may be ways to improve that program. It certainly requires measurement and further study because it is relatively new. But in my own industry, the first year that that program was really fully available to use, there was a remarkable investment in advanced research and development in the computer industry, even by companies that were having tough times on the revenue and profit side. I believe the testimony of the individuals in those companies that it did, indeed, have a stimulating effect.

Mr. FUQUA. If it is not proprietary information, what is the R&D budget of your company? Last I heard it was two or three times that of the Science Foundation.

Dr. BRANSCOMB. I do not have the exact numbers. In 1984 it was somewhere between $3.5 and $4 billion.

Mr. FUQUA. Mr. Brown.

Mr. BROWN. Dr. Branscomb, following up on this last line of thinking about the role of the universities and the fostering of basic research as the model, as you know, there are other models. One which has proven to be somewhat successful is the institute model as represented by the Max Planck Institutes in Germany. It seems to me that when we think merely in terms of institutions we do not get at the root of the problem, which is the fostering and encouraging of creative minds.

The problem we have in tying ourselves strictly to a university, regardless of how good it is, is that it is an institution which has a life and death. It goes through demographic changes. It gets to the point where the faculty perhaps, because of one reason or another, becomes old and static and is not fostering creativity to the extent it should.

There are, for example, more brilliant young researchers available than there are faculty positions to use them, maybe even industry positions to use them. How do we gear up to handle that?

I think we are moving in that direction with this emphasis on university-industry cooperation. In many cases this leads to the establishment of jointly controlled research institutes allied with both single companies or groups of companies in an industry or even across industry lines in some cases.

It seems to me that we need to explore those problems keeping in mind our goals of providing opportunity and incentive to foster the creative mind.

Would you comment on that line of thinking?

Dr. BRANSCOMB. Indeed. The Max Planck Institutes are a model of highly creative science institutions that do have the virtue that they find it relatively easy to undertake interdisciplinary work within their field of science. In Germany there are Fraunhofer Institutes, perhaps not quite so successful, but they attempt to do the same thing with industry. Those are worthwhile models to examine.

My belief is that we have many examples of the interdisciplinary research institute that is either an intimate part of the university involving the participation of multiple departments or is attached to the university and with varying degrees of intimacy and linkage.

The virtue of having such institutes connected closely with the university is that they will help to prevent the very kind of situation you describe that occasionally happens at a university from having a devastating effect on the quality of education in those institutions.

For I believe the most remarkable achievement of American science policy in the last 30 years is that we accomplished an enormously important educational objective without having to go through the political process of deciding we wanted a ministry of education that would undertake to finance all the postgraduate education, which would never have been accepted by the American people.

We have met the output measure of that intellectual activity; namely, the science, drive science education at the postgraduate level. I think it has been very successful.

What I would observe is that that institute pattern is one which has not been established in some sense as one of the three arms of policy, the third arm being the national laboratories. In this country the equivalent of the Max Plank Institutes in Germany is much more nearly our national laboratories than it is the interdisciplinary institute on the campus.

In that, I would include among national laboratories the NSF-funded facilities like NCAR, and the like. Those are very important in achieving the objective you describe.

Mr. BROWN. You stress the objective yourself, that is, the stimulating creativity and so forth. You indicated there should be more leeway in the laboratories to pursue the nonorthodox, new idea, that there should be that opportunity included in the funding arrangement in the budget, something I assume comparable to the independent research allocation allowed defense contractors. What are they called?

Dr. BRANSCOMB. Yes; IR&D.

Mr. BROWN. Which is used by defense contractors for what little research they do. The labs could benefit from that. But the principle here is that we provide opportunity, that we stimulate people to follow the brave, new ideas. I want to continually hold up ways to do that as the objective rather than protecting some particular institutional arrangements that we have at the present time.

Dr. BRANSCOMB. Yes. I do believe that there must be ways that can be found in the mechanisms for funding science at the universities that give the universities positive incentives, not just the removal of obstacles, toward their own ideas about rearranging their research activities in order to seize new opportunities.

I think that is a difficult problem in the acquisition of expensive equipment that might be shared by investigators supported by many different resources of support, and I do believe that more flexibility available to the initiative of the institutions, both national labs and universities, would be very helpful in unleashing a fair amount of local imagination in achieving what you describe.

These kinds of research institutes do get created by NSF and other institutions, but they involve a great deal of discussion and struggle before decisions are made to do them, so it is very much a top-down kind of decision process today.

Mr. BROWN. Thank you. Mr. Lewis.

Mr. LEWIS. Doctor, what in your view is the relationship between national and international strength in science and economics? Should the United States take a broad lead in all fields of science such as countries like England which has had a strong science base while behind economically, Japan with a weak science base while ahead economically? What is our role in this area?

Dr. BRANSCOMB. I think, first of all, scientific excellence is a sine qua non for long-term competitiveness, and that is as true for Japan as it is for us. They are discovering that. As they succeed in their catch-up objectives, much of their economic strategy in the high-technology area has been aimed at acquiring a substantial market share of a market that already has been brought into existence by others. As they achieve that objective and try to get out ahead, they will not be able to do that without their own indigenous science and innovative capability.

But the primary answer to your question is that, especially for the United States, but for any industrialized democracy, a high level of achievement in both science and in engineering and the disciplines that are involved in the translation of science to benefit is the sine qua non for competition in the world today. It is certainly not wage rates.

The Japanese contribution to that recognition is that they have done an extraordinary job in production engineering. Their manufacturing engineers do things that in America are done by development engineers. As a result, they do them quicker.

They are just very focused at the whole notion of production as a very sophisticated technical challenge. In our country, for too many years in too many sectors of industry, and in too many engineering schools, the notion of engineering design and production as a challenging intellectual area for research and teaching has just not gained acceptance. As a matter of fact, the teaching of design disappeared in school after school for years. Now that recognition has been very substantially reversed, and the response to that IBM manufacturing systems engineering program I mentioned earlier is evidence of it. Indeed, we do now teach imaginative design. It can be taught, and it is taught at MIT and Berkeley and elsewhere.

In fact, on television last night I saw the results of a competition. The finalists came down to Berkeley and MIT. The competition for the students was to figure out why the perpetual motion machine worked. It was, of course, a fake perpetual motion machine. But the kinds of skills that those students brought to bear in that competition are the kinds of skills that are needed in design and not just scientific skills.

If we have a balanced investment in our national science and engineering capability, then I think we will have the tools, given a sound economic environment—and we absolutely have to have that—those two things will make us or allow us to remain the No. 1 economic performer in the world.

Mr. LEWIS. You mentioned in your presentation about universities needing research assistance by the Government. The Department of Defense has taken a greater role in university research. Is there any problem with the growing military presence on campuses at this time?

Dr. BRANSCOMB. I won't try to comment on any sociological consequence. I am not too concerned about that.

Mr. LEWIS. That is a concern, but what is your view of the DOD supporting universities?

Dr. BRANSCOMB. My view is that the DOD has not only an obligation to refurbish, to reinvest in the sources of new notions that they are depending upon as they exploit them, but that, indeed in

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their own self-interest, given the nature of our military strategy today, which is very much a high-tech strategy, it is vitally important that our military community have the broadest base of ideas and capabilities and skills to draw upon.

Indeed, my concern is not that there is too much Defense Department investment in universities, but that if you look carefully at what the Defense Department in fact is funding under the 6.1 budget category, which has always in the past meant fundamental research, I think you will find a great deal of activity there that is not the area in which the universities can best contribute. Therefore, the Defense Department is not playing the level of role in building our university capability that, given the priority this area has in the country today, they should be. I believe the universities in fact are prepared to do their appropriate role more aggressively for our defense purposes.

Mr. LEWIS. Thank you, Dr. Branscomb.

Mr. FUQUA. Thank you.

Mr. Packard.

Mr. PACKARD. Dr. Branscomb, you emphasized in your statement that it is not only a Federal policy that we should be developing, but we should be looking at State and even local involvement in that policy, particularly for the economic benefit of the State and local jurisdictions. How do you perceive that best to be accomplished?

Our goals at the Federal level may be motivated entirely by a different motivation than what they are defined at the State and further down to the local levels. How can that best be done? Is it not true that there is a tendency of each wanting to go their own separate ways in developing their own local policies or individual policies based upon different motivations?

Dr. BRANSCOMB. That tendency for the States and communities to go in different directions is, in my opinion, a very healthy evidence of an innovative, competitive spirit. I am a supporter of the notion that the States should take a leading role in the exploitation of intellectual investments for economic benefit, because I think it is appropriate for the States to compete with each other in this respect. Industry and science can vote with its feet on how it responds. That is the American way.

That diversity will, of course, leave much of the activity exclusively to the local arena, and there is nothing wrong with that. My concern is based on the fact that we have a national—let me take an example, which is science education in the public schools, or let's say education in general in the public schools.

The Federal Government in its leadership has clearly indicated that that is a matter of concern to the Nation. There has been a lot of analysis of the problem and exhortations for progress at the Federal level. The States have, many of them, undertaken quite imaginative activities.

It seems to me that the motivations are all correct, but the strategy for solving the problem is not yet really joined between State level and Federal level, or for that matter, I don't believe there is as broad an awareness in the private sector of the way in which the private sector can help with education as perhaps there should be.

I am not sure that we should leave it exclusively to the Governors' Conference and to other non-Federal bodies to take the initiative in that respect. I think we need a genuine partnership, so that the deployment of resources at the Federal level, whatever they may be in whatever is the appropriate Federal role, is matched as best it can be to the central strategy of the State and local communities, recognizing that that won't be the strategy for all.

Mr. PACKARD. I suppose that the task of this committee, then, in setting up a national policy incorporating the local and the State policy as well would be as a correlating or a coordinating task in that respect at least. Do you have any suggestions on how that could best be done?

Dr. BRANSCOMB. I think you are right about the role. It is an interesting idea. The National Conference of Governors has worked this problem pretty hard through a variety of mechanisms, and they continue to focus on it.

I would think it interesting perhaps for this committee to try to find some way of undertaking a collaborative activity with that organization, at least to be sure that you understand their views of the Federal efforts in this area over which you have stewardship.

Mr. PACKARD. Thank you, Mr. Chairman.

Mr. FUQUA. Mr. Wirth.

Mr. WIRTH. Thank you, Mr. Chairman.

Thank you very much for that great tour de force through various issues that we have to look at.

Let me ask you a bit of a different question. You talked this morning about international information, intergovernmental relationships, interdisciplinary studies, laboratory cooperation, really the process of science policy which is part of what we have to do.

It seems to me the other part of what we have to do is the level of Federal investment in this area. Historically, as I understand it, we have tended to say if we invested about 3 percent of our gross national product in research and development, that was about the rule of thumb, and that was invested about half by the public sector and about half by the private sector. That is generally what we have done in the past 25 years.

With the changing nature of our economy, with a defense establishment that has a higher technological interest, with an increasingly international structure in the economy, is that 3 percent still an adequate rule of thumb? Should we, as the Federal Government, be spending more in the area of not only science and technology, but the education of young engineers and young computer scientists and social scientists, and so on?

Dr. BRANSCOMB. I think we should be spending more, but I don't believe we can arrive at that conclusion by examining the 3-percent number. In fact, I don't think the 3-percent number came about that way.

My real belief is that we should spend more only when and where we have figured out how to do it well. That statement is a much more important stricture when we talk about fundamental science than it is when we talk about, let's say, engineering education.

In fundamental science you are really wasting the money unless you invest it in somebody who can make a significant incremental contribution to the body of world knowledge that exists. There, if we have the good fortune to have bright people with first-rate ideas who can really move the boundaries of knowledge ahead, then, as Mr. Brown suggested, we need to be able to create the opportunity for those people to make that contribution.

I think today there is no question but what we have that opportunity in many disciplines to the point where we really are wasting precious intellectual assets, and that increased investments are dictated.

But I believe there is another area in which it is quite clear we have to develop a companion piece to the character of the investment we have made in the past. In the past that investment has been primarily in forefront research in the various areas of science. We have done that in an educational context to a substantial degree to get the educational benefits.

We have to remain the leader in that area. That means increased investment. But we also have to address two other issues. One is the whole area of the technological base for the economy. Our economy has fundamentally changed in the past 40 years, and the whole area of the vitality of the engineering community is, in my opinion, of a piece with the vitality of the science community. We have to develop those capabilities at the same time we keep the science ahead, not at the expense of science.

As Mr. Bloch said the other day, I don't believe it is necessary to invest as much in engineering research in the universities as we do in science because there are other sources of engineering support, both of a more mission-oriented character in the Federal Government and from private industry, which will preferentially support engineering over science, probably.

Nevertheless, that is a big area and one which I think we can sit down and describe quite accurately what the shortcomings in our economy are with respect to engineering skills and knowledge, and the organization of the science information base for engineering use.

The other area where we need an add-on increment which would change that 3 percent is in the area of public education. Now I have to say here, having been a part of that problem when I was with the Science Board, that the biggest problem there is to understand what kinds of investments really help and really make a difference. That is a tough problem, and it is going to take time.

But I do believe that, once the NSF and the States and local communities get into a groove swing on that issue, that the investment required to make those educational reforms will be quite substantial, and that that has to be an incremental investment as well.

But fundamentally the best way to examine the problem, in my view, is to subtract out the military R&D investment, to look at the residual U.S. total R&D investment as a percentage of GNP, to compare with the principal competing nations like Japan which don't invest that much in R&D to support the military, and then to examine what our infrastructure needs are and theirs. I think you will see they need to make an expanded science investment; we need to make a very substantial investment in the base technologies that help us use that science.

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Mr. WIRTH. Having done that calculation you referred to, where you subtract out what you are doing for defense and look at where we are, it seems to me you can make the issue enormously complicated or relatively simple, as we talk about science policy. We could get ourselves deeply embroiled in a whole set of issues over which we may or may not have any influence or we can say, "Let's make it simple and look at the basics that make this whole system work," which I think you are referring to.

Dr. BRANSCOMB. Yes.

Mr. WIRTH. At least I would prejudge it in this way—that we ought to be investing more in the young scientists and the capability to train Ph.D.'s and keep young faculty at universities to develop the kind of institutes that I think George is talking about, that provide the ability to do interdisciplinary study, that does university instrumentation, that does university laboratory facilities, and it then goes to the question of science literacy. We know a lot about how to teach kids. We know a lot about that sort of thing, but we are not doing it very well.

You can go right back to basics and say, OK, why don't we go back to the numbers and go back to those basic investments, and everything else gets driven by those. If we are not willing to make those investments, why go through an enormous exercise to say, well, maybe we can do better in terms of interrelationships here or information dissemination over there? If we are not going to make basic investments, it doesn't make any difference to have another study on the wall.

Dr. BRANSCOMB. I think we should make the basic investments. As I said, if you compared our nondefense investments with those of our principal competitors, I think you get a guide as to at least a ball park figure we ought to be shooting toward.

I don't believe, however, that it is wise or, for that matter, very popular with the American people to try to drive the process by starting from a gross budget number and then figuring out how to allocate that. It leads to a lot of conflict in the allocation process.

But, most importantly, we clearly need to do that increased investment in a way that internalizes the discrimination between worthwhile investments and those that maybe are not really ready to be managed well.

If there is a way to make the investment either in a sharing way or a competitive way or a matching way or a collaborative way with other sectors that also are making investments, mainly the private sector and the States, then perhaps we have a device for insuring that that expanded investment is really well made.

In that sense, I think the States' competitiveness with each other is a useful tool. I could well imagine an expanded program of Federal investment in that basic resource driven by what the States also are going to do.

Mr. WIRTH. I am not suggesting that you start from a 3-percent figure and go from there. That is not the point. We tend to have about that level now. You say is that or is that not adequate, and you come back on top of that and look at all of the basic investments that ought to be made. Are those met by what we are doing now or not? If the conclusion is, no, they are not being met by what we are doing now, we ought to be doing more in terms of these other fundamentals. If we do those other fundamentals, those will in turn drive answers to the other questions that are being raised and are going to be addressed by this brilliant group of hearings that the chairman is setting up.

Money drives a lot of us.

Dr. BRANSCOMB. Indeed.

Mr. WIRTH. We cannot assume that that problem will go away even with the budget constraints of 1985.

Dr. BRANSCOMB. Absolutely. That is why I was as careful as I knew how to be to say the single most important change in science policy in this 30-year period is that the American people now put the economic health first, and that is what they want to see their science investment accomplish, as well as the national security application, and the fact that I believe we know what the investments are, the kinds of investments that are needed in order to have that economic benefit. Given a closure of that budget gap which is driving the dollar up to the point where we cannot keep the fruits of our science at home, and that closure process is going to make you folks struggle very, very hard with where you are going to get the money to make these investments, I just don't think—I want to make it clear that I think we in the community, the scientific community, that believe we can deliver the benefit to the country that more than pay for itself in economic terms, we have to show it. I don't think we can expect to get support without program by program understanding of those mechanisms and properly describing them and defending them.

But I believe that defense can be made, and it will drive an expanding investment even in this budget situation.

Mr. FUQUA. Mr. Volkmer.

Mr. VOLKMER. I am sorry I was not here to hear your full presentation. As I listened to your answers at the end of your presentation, I would like to ask you, Doctor, perhaps I have a wrong impression, but is not the industry investment in research and to a major extent the defense investment in research goal oriented?

Dr. BRANSCOMB. Indeed.

Mr. VOLKMER. Being such, and as you talked to the gentleman from Colorado, if we are driven in this research today toward moving our economy ahead and developing technology in order to do that, is not that goal-oriented research?

Dr. BRANSCOMB. Indeed, it is. That is the same goal the companies have.

Mr. VOLKMER. What concerns me about that is, if we put our funds into that in the Government area as well as industry doing it, and most of DOD doing it, what happens to our basic fundamental research?

Dr. BRANSCOMB. First of all, the tone of your question suggests to me that you believe that I was advocating a set of investments which were essentially defensive in character for American industry.

Mr. VOLKMER. Yes.

Dr. BRANSCOMB. That is not my image. My image is that the United States enjoys the leadership position today and it has every opportunity to sustain that leadership position, economically and scientifically. The cornerstone of doing that is to maintain the lead in science. Indeed, even though it is true that corporate investments are goal oriented, one of our goals is to have people in the company at the absolute forefront of basic science. That is an important tool in economic competitiveness. It is only one tool, however, and I am just trying to emphasize the need for a program that reflects what science is in 1985 and what science is going to be like in 1995 and the year 2000.

Science is already increasingly complex, supported with sophisticated instrumentation involving the collaboration of many different people with different backgrounds and skills working in teams. That is not to say that the individual creative genius isn't as important as ever; it is to say people have had to invent ways of mobilizing that kind of talent in this kind of environment.

If you want to take a pure science example of that, look at what it takes to make a great discovery in high energy physics. Those people are as brilliant and creative as you will ever find, but, my goodness, look at the technology they deploy in the process.

I simply observe there is a joining happening between what it takes to be a leader in science and the science it takes to be a leader in engineering. The Federal investment ought to recognize that balanced situation.

It is reflected in the increased amount of collaboration between scientists in industry and scientists in universities as well.

Mr. VOLKMER. In the allocation, then, of dollars, it is not necessary to look at the immediate payoffs of any individual scientific research project then as a criterion.

Dr. BRANSCOMB. That is absolutely correct, just as we do not either in our corporate research in industry.

When I suggested that Federal agencies that support science in universities should take into account the technological implications of the science as well as its intrinsic intellectual interest—that is the National Science Foundation's grant policy. It is not a new idea.

When they do that, I do not believe they should sit there and invest the money where a parade of industry representatives comes into Washington and tells them to invest it. I believe they should invest it where their university clients say they want to invest it, but I would like those university clients to have the opportunity, increasing opportunity, to collaborate with people in industry so they are aware of what the exciting opportunities are for their students in industry and, therefore, can make an informed choice on where the excitement lies. I think they will make that choice correctly, but in the past those university people have had—on the one hand, they have had an opportunity for imaginative agencies like ONR and NSF and NIH to follow the intellectual lead, but they have also had on their plate all the time—most of their money, in fact, came from the mission-oriented agencies who had quite clear goals to solve, goals in the military, goals in space, goals in energy.

The universities, to the extent that they let applications influence their judgment on the selection of fields for research, it was the Government's missions that influenced that judgment.

I say in the world today, where the Government's mission depends upon the health of the economy, it is important that the universities have equal opportunity to be influenced by those economic implications of science as well as by the military and space and energy implications.

Mr. VOLKMER. To get back to one other area, so we try to use funds as efficiently as we can in Government and so we look at allocation of funds with private industry and hopefully more from the States to the universities to research, how do we make a sound judgment and how do we allocate those funds so they are not really duplicative of what is being done elsewhere?

Dr. BRANSCOMB. I wouldn't——

Mr. VOLKMER. I see that as some of the problem.

Dr. BRANSCOMB. I think so long as the agencies that are spending the money that you authorize are held to a high standard on operating their grant mechanisms in a fair and competitive way, using the correct criteria for choosing who gets the grants, that is the best defense against the problem of wasteful duplication, because if three teams of scientists around the country are all rushing to attack the same challenging goal, to find out if the photon has a rest mass, for example, then there is absolutely nothing wrong with having three. As a matter of fact, you will get there more than three times as fast with three than you would with one. You will never get there with one. Competition works in intellectual life just like it does in business.

On the other hand, if you have a set of people who are proposing to do something that, in effect, really has already been done, then God save the taxpayers—let's don't spend that money.

The peer reviewers who look at that proposal who are familiar with the field will hopefully know that this is not truly original work and, therefore, it is inappropriate to invest in it, even though there might have been some educational side benefits from doing it.

Mr. VOLKMER. Thank you, Mr. Chairman.

Mr. FUQUA. Thank you, Mr. Volkmer.

Dr. Branscomb, we want to thank you very much for sharing your time with us this morning. It has been very enlightening. We appreciate your thoughts on this from the vantage point of your experience. It has been very beneficial to us. Thank you very much.

Dr. BRANSCOMB. Thank you. Good luck on this very important project.

Mr. FUQUA. Thank you.

[Whereupon, at 9:50 a.m., the task force recessed, to reconvene subject to the call of the Chair.]

[Answers to questions asked of Dr. Branscomb follow:]

QUESTIONS AND ANSWERS FOR THE RECORD

Dr. Lewis M. Branscomb

1. In your view, should one of the goals of government science policy be to achieve and maintain, as a matter of national prestige, U.S. leadership across the spectrum of science, or should we share or yield leadership in some areas of science to other countries?

Yes. As I said in my address to the National Science Board in May, 1984, our national goal should be to insure that American scientists have the opportunity to achieve world leadership in every important area of science. This statement does allow certain fields to be e[illegible]ted as insufficiently interesting or valid to justify federal investment. Importantly, it speaks to opportunity to compete intellectually; it does not guarantee success.

Since there are many very bright scientists in almost every country, we cannot buy scientific leadership in any case. So as an empirical fact, we will find ourselves "yielding" leadership to other countries from time to time. But if our educational system is strong the next generation of students will win back the lead. The implication of this policy goal is:

(a) Our best scientists must have access to the facilities and equipment without which they cannot compete for the lead;

(b) our universities must have first rate graduate research and education programs that cover all the important areas of science;

(c) our scientists must be encouraged to travel, to communicate and to collaborate with their foreign peers, and must be able to welcome them into our laboratories here, in order to learn from the best minds abroad, to ask them to help in our educational programs, and indeed to welcome the best into our own scientific community.

In my view this is a smart strategy, and not an expensive one.

2. One of the largest science facilities ever proposed, the SSC, or Superconducting Super Collider, is now under serious consideration. Its proponents argue that it will enable scientists to penetrate further into the ultimate structure of matter; its opponents argue that the cost of the SSC is too high for the benefits expected. In your view, is the SSC a device which is needed not only for research in physics but also to signal our continued commitment to U.S. leadership in science; or is it, like the U.S. SST - the Supersonic Transport - 20 years ago, a device which is technically feasible but so expensive that those resources could better be used elsewhere?

The comparison with the SST is unfortunate; that was to be a government-subsidized commercial development, using what was already known (and demonstrated in military aircraft) to meet a perceived commercial need. Its fault was that it was uneconomic. (It had only a 7% payload for passengers, by weight, for example.)

The SSC, on the other hand, would be a first-of-a-kind technical achievement, quite apart from the information it might give about the nature of matter and the origins of the universe. I believe the right way to decide about the SSC is as follows:

(a) First, get the scientists qualified to evaluate all the alternative scientific approaches to this class of questions about major facilities. Since I believe the high energy physicists are prepared to see their facilities opportunities for 10 years pooled into a single SSC project (once everyone is happy about how the project is managed), there remains the need to get the cosmologists and astronomers on board too.

(b) If the unanimity so achieved permits construction of the SSC within costs reasonably close to the current DOE high energy physics budget, by concentrating funds on this one project and spreading the expenditure over the appropriate time frame, I would decide to proceed.

(c) Once the decision to proceed is made, other nations should be invited to participate as full partners, with financial investments proportionate to their level of participation. This could significantly reduce the total cost to the USA, and increase the intellectual rewards to humanity.

(Note, that with satellite broad-band data links, participating countries will be able to operate the accelerator directly from their home laboratories. This will have been demonstrated by then both for Fermilab and the NSF "supercomputer" centers.)

In summary, the World should build and can afford the SSC; the US should lead the way, but has no need to bear the whole cost.

3. In discussions of the government science budget, much stress has been placed on providing new funds for new initiatives in emerging areas of scientific promise. Why should we not expect a comparable group of areas within each discipline which have "peaked" or been "mined out" and where consequently some funding decreases can be made?

You should indeed expect investments in old areas to decline. They do peak out. I believe that so long as the science support process is actively competitive, this squeezing out is going on at a great rate. The problem is that it is not very visible to the Congress and other observers from the outside.

The reason is that old areas do not get abandoned as a whole; they change as the old questions get answered, the old tools lose their usefulness, and they evolve into new combinations of ideas under new names. Most important, the process is evolutionary, the product of many small decisions by individual scientists, peer review groups, program officers. There should be no areas of knowledge declared to be no longer interesting, but the standard for what constitutes a useful research investment should constantly rise with time.

Thus, the best safeguard for the public purse is a vigorous, objective peer review process. Probably the most critical requirement is that "peers" not be narrowly defined by discipline, but should be persons of broad and deep knowledge. Otherwise, there is danger of islands of isolated scientists, pursuing ever finer points of diminishing importance, insulating one another from external scientific criticism by mutually supportive peer review.

4. In the last few years we have seen the merits of a number of science facility projects advocated on the floor of the Congress, and amendments for such projects have occasionally succeeded. As a result there has been a vigorous debate about the respective roles of political and scientific judgment and expertise in making decisions about when and where to construct such science facilities. Have you formed an opinion about how this matter should be dealt with?

I testified on this specific point in the informal hearing last March.

Briefly, I believe the scientific community is justified in their concern, wherever a public competition for facilities resources was in place, and Congressional action by-passed (some would say subverted) the fair competition process. But some have overreacted. The Congress must have a major role in balancing local, state and federal interests in scientific and engineering capability development.

In a democracy, politics is not the problem, it is the solution. Scientists and universities must participate in that process, which means they must take the interests of the citizenry at large into account, when new programs of facilities development are being sought. But once there is consensus in Congress on the need for a facilities program open to competition, that competition must be unsullied by manipulation or preemptive strikes.

I will confess to being perturbed by the new phenomenon of use of professional lobbyists by scientific institutions, even where political activity is not inappropriate. It somehow seems to introduce a new element which threatens to build barriers rather than bridges between the academic and political communities.

5. Overhead or indirect costs paid on research grants have generally been justified as needed to pay for the costs associated with the performance of research, but they have generally been limited to current operating costs. In your view, should indirect costs be broadened to recover, as well, the capital costs and other non-operating costs of the universities?

The government should be prepared to pay the actual costs of the research it supports, but not a penny more. Although I am no expert on this, I believe that current overhead accounting does allow reasonable rents on space, and to this extent compensates the university for use of capital assets.

But I am strongly in favor of allowing universities to capitalize scientific equipment purchases and facilities modernization investments, and charge reasonable depreciation rates back to all the research projects that directly benefit. This would encourage timely equipment investments, encourage equipment sharing across fields, and increase injection of private capital into the solution of university facilities needs.

6. Some, including some historians and social scientists, have suggested that the relationship between science and the Federal Government is in the nature of a social contract: The government provides certain resources for scientists to expend in return for which they provide society with certain benefits. How do you view this analysis, and has it changed over the years?

I like this model, for it conveys dimensions to the relationship between science and government that transcend the notion of procurement of services or an entitlement on behalf of universities. Only if both parties understand that the benefits to our society depend on an act of faith on both sides, accompanied by accountability, will those benefits be realized. One only has to look at the Soviet Union to see the failure of the materialistic, authoritarian approach.

7. To what extent is government support of science comparable to government support of the arts and the humanities? Is there a "need" in our society for the kind of science that satisfies public cultural demand and can this serve to suggest the level of funding for science?

Of course, science is part of our culture, perhaps a too dominant part. There is a proper role of government to express the collective desire of the people that our culture should be preserved, developed and appreciated.

But there is no way we can justify 50 billions in federal R&D as an investment in culture, or even 6 billion for academic research. The overwhelming majority of the federal investment in science is justified by expected returns to public benefits of a quite specific form: jobs, military security, better health, etc. Thus, the scientific community cannot expect to be supported at levels two orders of magnitude above the support level for humanities without facing up to the pragmatics of those public expectations. That is an important reason why academic engineering needs to be given serious attention in the federal research strategy.

8. Most studies of science and most agency budgets for science are future oriented. They speak of future opportunities, future projects, and future results. Retrospective discussions are limited to anecdotal cases of successes, while little has been done to look carefully at entire programs and the ratio of those which lead to technological successes and those which do not, however measured. Why should not more such comprehensive evaluations of past programs be done?

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I am all for it. Objective studies of past experience can only help decision about the future. But the work needs to be carefully done and subject to critical evaluation. Are there adequate funds for this kind of science policy research? I doubt it. Dorothy Nelkin, of Cornell University, has published a number of excellent critical evaluations of past government programs. Her view of this issue would be very constructive.

9. It is well recognized that the potential payoff in technology or medicine from an individual research project can not be predicted. However, we also know that broad fields, such as chemistry, yield significant practical benefits. To what extent can and should the expectations of such payoff be used to determine the levels of funding for science and for the individual disciplines?

Economic tests should be applied to development, test and production engineering activities. They should not be applied to fundamental research. We do not attempt this in industry, where our incentive to measure financial payback is even greater than in government.

However it is perfectly appropriate to ask what useful results can be expected from a line of research. Indeed, I cannot imagine anyone engaged in research who does not have a goal clearly in mind.

Benefits of science investments can best be judged by technical people whose careers are devoted to applying research for practical purposes.

Priorities for government research investments should be set by a balanced combination of people of this background and those more interested in the intrinsic mysteries of science.

10. The current Administration has shifted the principal rationale for government funding of research. Instead of emphasizing the technological payoff, the stress has been on the training of a new generation of scientists as the principal benefit yielded by research grants. In your view, how many scientists do we need in the coming decades and to what extent will the current levels of research funding meet that need?

I endorse this emphasis the question attributes to Administration policy. But the issue is not how many scientists we need, but what skills, imagination and values should they have? Quality is more important than quantity. However, if our strategy is successful, and the economy grows on strong technical people, we will find a need for growing numbers of technical people, even at a time when demographically the work force is shrinking. This says we must do a better job of insuring opportunity for all our young people to participate, especially young women and minorities. It puts great pressure on the quality of the pre-college educational system.

Will the current levels of research meet that need? I don't know, but as a guide one should compare total U.S. non-military R&D investments with those of Japan and West Germany. The ratio of those

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investments to GNP already exceed the U.S. ratio. This suggests that we do not yet know how to use the talent we are capable of producing.

11. Industry has always provided modest amounts of funds for specific research projects by university professors. Recently, this has received increased attention and some growth of funding has taken place. Under what circumstances does industry elect to provide such support? Should government policies and incentives be changed to influence the types and levels of such funding?

The question understates what industry is doing. IBM alone is funding over $65 million a year in donations to U.S. universities. In addition, we have initiated over 1,000 separate projects with 208 U.S. universities in recent years, with a total cost of over $130 million. Much of our donations program has been devoted to structural reform of science, engineering and business education at the post graduate level.

I can't speak for all industry, but from IBM's point of view, we regard the strength and modernity of the U.S. university system as critical to our long term international competitiveness. We are prepared to invest considerable sums to help universities develop and innovate.

A very important part of our help to university research is the donation of computers and other equipment. We and other companies have substantially expanded this form of philanthropic assistance to universities since the passage of the tax incentives for such donations. Given the $2B shortfall in equipment in our universities, it is important that these incentives remain in the tax law, as the President has proposed in his tax reform package.

12. In the field of engineering there has recently been a growing emphasis on the training of Ph.D.'s, and on government funding of research by engineering faculty. From the point of view of industry, is there an unmet demand for Ph.D.-level engineers as opposed to the demand for bachelor degree and master's degree-level engineers?

Yes, there is an unmet need for Ph.D. engineers, as opposed to B.S. and M.S. graduates, but except for certain shortage categories (computer and materials engineering, for example), the main reason is that new categories of skill need to be developed which will create their own demand. For example, the magnetic recording industry is a multi-billion dollar "high tech" source of growing employment, increasingly challenged by Japanese companies bringing to bear increasingly sophisticated technology. Until a number of U.S. companies recently funded expanded post graduate work in magnetic data recording technology (at CMU and UCSD), there was no Ph.D. level curriculum in U.S. engineering schools on this topic. As these two programs, and perhaps others, become productive demand for these doctoral engineers will appear.

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However, the bulk of engineering demand will continue to be for M.S. level. Nevertheless, Ph.D. programs with associated research are necessary for the proper training of these M.S. candidates as well as the development of new areas in engineering.

13. **With the fluctuations in enrollment and the resulting limits on faculty hiring, should alternative institutional mechanisms for research be sought to supplement the universities as performers of research, or should the number of research universities be contracted or expanded?**

There already are alternative institutional mechanisms for research; new ones do not require invention. First of all the tradition of the specialized research institute, located on campus and staffed by a combination of faculty and full-time research staff, is a well founded tradition. Second, the national laboratories (both federal and FFRC) already represent a very major employer, supplementing - and sometimes competing with - universities. Finally, there are specialized institutions operated by consortia of universities. In some cases national laboratories operate very succesful joint ventures with universities on their campuses (for example: Joint Institute for Laboratory Astrophysics [NBS and Univ. Colo.] and Smithsonian Astrophysical Observatory [at Harvard]).

Quality and coverage of important areas of research should guide the size of the federally funded research population. Institutional choice should be biased toward educational environments, except where scale of activity, applied objectives or special facility needs dictate otherwise.

On the number of universities engaged in research, I believe - on the basis of the quality of the research opportunities - we have enough, perhaps too many. But given the reality of faculty cut-backs because of declining student demographics, I believe that quality is today support, not talent limited.

I believe we need another "Centers of Excellence" program to help a geographically well distributed group of second tier institutions reach first tier quality in their selected areas of special strength.

But the primary criterion for investment should continue to be excellence and need for the work. I am convinced there are enough seriously underfunded areas of work which could make a direct and significant contribution to U.S. technical competitiveness, that remedying these shortfalls would already expand the current research effort level enough to compensate for the negative effects of demography.

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14. In view of the many problems and difficulties which are facing the universities, how do you view the longer term future of the nation's research universities?

Their future is very bright, for they represent such a record of astonishing success in research, and such a vital asset to the nation that I cannot imagine the American people allowing those very real problems to degrade their basic capabilities.

The heart of the matter is the concept of financing much of higher education in the sciences and engineering by funding long range research in academic rather than non-academic environments. In many cases, but not all, the university is a better bargain for federal fundamental research investment than the national laboratory, even without the derivative benefit of graduate education.

Second, indusry is learning not only to value the university, but to use it correctly and well. This will help motivate the leaders of industry to be even more vocal in their defense of American educational excellence than they already are.

I am seriously concerned about the health of the private research universities because of the rising costs to parents and students, and the curtailment of federal student aid and other sources of external assistance. But they are well led, resilient institutions and will continue to offer the government an invaluable resource for contributing to the public welfare.

15. As you look beyond the current studies and science budgets for the next few years, what changes or adjustments in our goals and objectives do you foresee for the decades after year 2000?

(a) Science is changing in many ways; federal policy must change, too. First, science is getting more technological. Second, there is a grand disciplinary re-unification of science taking place. Both these trends will affect institutional arrangements, project selection, and international cooperation (which must be substantially increased).

(b) Engineering and science will not be regarded as alternatives or competitors for federal policy, but as two aspects of a national effort to sustain our economy and the quality of life. Since Americans will be increasingly challenged by other nations that also put their faith in technology and knowledge, the consensus for making these investments will grow stronger.

(c) The states will play an increasingly important role in research and educational strategies and investments. The Federal Government must become a much more interested, willing and responsive partner with the states, for the present relationship is little more than benign neglect. It will not be easy, for the states compete with each other for economic opportunity, seeking advantage in "high technology". But the overall effect of this competition is very good for the country as a whole.

(d) Education will indeed be recognized as a lifelong necessity, and access to on-the-job training will become a sought after benefit. Already, the private sector spends over $800 millions annually, almost as much as the entire budget for our K-12 schools. Much of this is technical training. An integrated national strategy for technical education will be a necessity for the 21st Century.

APPENDIX

RESEARCH IN PREVENTION

FISCAL YEARS 1981 – 1983
(1984 Estimated)

Budget
Information
and
Program
Highlights

U.S. DEPARTMENT OF HEALTH
AND HUMAN SERVICES
Public Health Service
National Institutes of Health

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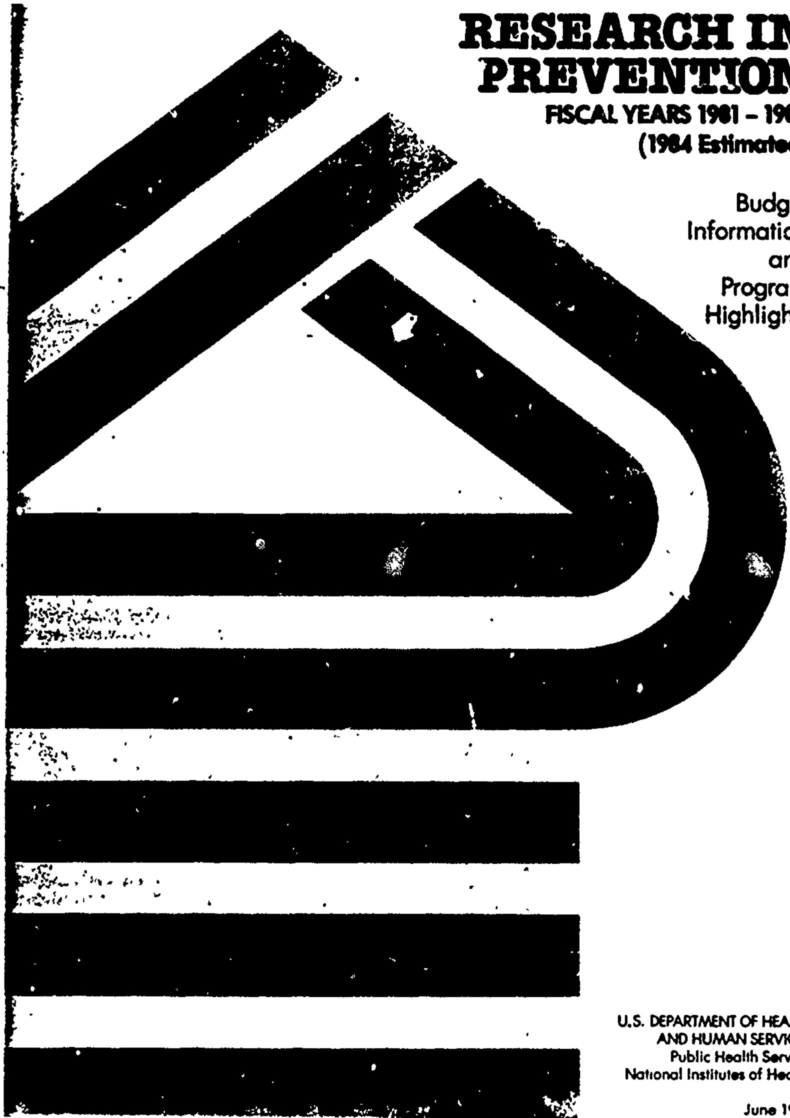

RESEARCH IN PREVENTION

FISCAL YEARS 1981 – 1983 (1984 Estimated)

Budget Information and Program Highlights

U.S. DEPARTMENT OF HEALTH AND HUMAN SERVICES
Public Health Service
National Institutes of Health

June 1984

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Key to Abbreviations

DHHS = (U.S.) Department of Health and Human Services

PHS = (U.S.) Public Health Service

NIH = National Institutes of Health

BIDs = Bureaus, Institutes, and Divisions (of NIH)

NCI = National Cancer Institute

NEI = National Eye Institute

NHLBI = National Heart, Lung, and Blood Institute

NIA = National Institute on Aging

NIAID = National Institute of Allergy and Infectious Diseases

NIADDK = National Institute of Arthritis, Diabetes, and Digestive and Kidney Diseases

NICHD = National Institute of Child Health and Human Development

NIDR = National Institute of Dental Research

NIEHS = National Institute of Environmental Health Sciences

NIGMS = National Institute of General Medical Sciences

NINCDS = National Institute of Neurological and Communicative Disorders and Stroke

DRR = Division of Research Resources

FIC = Fogarty International Center

NCC = Nutrition Coordinating Committee

NLM = National Library of Medicine

Notes on Charts and Tables

1. All figures refer to NIH obligations for prevention-related research (as defined), expressed in thousands of dollars.

2. In a few cases, totals may not add up precisely due to rounding.

3. In a few cases, individual prevention-related projects (as opposed to programs) could not be isolated from the BID budget.

4. In almost all cases, prevention-related figures are provided separately for grant, contract, and intramural programs of the BIDs.

FOREWORD

"Since it is infinitely easier to prevent physical ills than to remedy them once they have arrived, preventive medicine is the most important part of medicine, although it has been neglected for a longer time than any other part. . ."[1]

In the time since these words were penned by Johan Peter Frank two centuries ago, preventive medicine has scored some remarkable triumphs, most notably the sanitary reforms of the last half of the 19th century and the development of vaccines in this century As a result, the infectious diseases that were yesterday's scourges, such as smallpox, diphtheria, and polio, are no longer a threat to the public health Today, however, preventive medicine faces a different and more difficult challenge to eliminate those diseases that are chronic in nature, such as cardiovascular disease, cancer, and diabetes—diseases that swell the list of major causes of death in the United States and for which there appear to be no simple causes or solutions

Accordingly, prevention research has become both more comprehensive and complex—with the promise of unprecedented achievement Within the Department of Health and Human Services (HHS), disease prevention and health promotion have continued as major initiatives since 1979 Underscoring the importance of this "wellness" approach to biomedical research, health education, and health care services, HHS Secretary Heckler stated that prevention can "affect the health and vitality of the American people more than all of 20th century medicine"—and help stem the country's "soaring health and medical bills "[2]

Definition of Prevention

The FY 1983 and estimated FY 1984 figures and project descriptions presented throughout this booklet apply to research projects and programs that the institutes have classified as either "prevention research" or "prevention-relevant research," based on the following PHS working definition

- **Prevention Research:** Narrowly defined, prevention research includes only that research designed to yield results directly applicable to identification of risk, and to interventions to prevent disease or the progression of detectable but asymptomatic disease

  Pre-Intervention

  — Identification of risk factors for disease and risk assessment,

  — Development of methods for identification of disease controllable in the asymptomatic stage;

  — Refinement of methodological and statistical procedures for assessing risk and measuring the effects of preventive interventions

  Intervention

  — Development of biologic interventions to prevent disease occurrence or progression of asymptomatic disease;

  — Development of environmental interventions to prevent disease occurrence or progression of asymptomatic disease;

  — Development of behavioral interventions to prevent disease occurrence or progression of asymptomatic disease;

  — Conduct of field trials and demonstrations to assess interventions and to encourage their adoption

  Some interventions may be applicable to primary prevention as well as to disease treatment (e g., diet and exercise as components of rehabilitation for coronary heart disease). Research into such interventions is considered prevention research.

- **Prevention-Relevant Research:** More broadly defined, prevention research also includes research with a high probability of yielding results that will likely be applicable to disease prevention or health promotion Included are studies aimed at elucidating the chain of causation—the etiology and mechanisms—of acute and chronic diseases Such basic research efforts generate the fundamental knowledge that contributes to the development of future preventive interventions.

The National Institutes of Health (NIH) recently adopted these definitions to account more adequately for the scope of its total prevention program. Prior to FY 1983, NIH prevention data were reported according to a more restricted definition of "primary prevention"—intervention before the biologic onset of the disease in question Research related to "secondary prevention' —intervention when a disease can be detected but a step before it is symptomatic—was included only when it related to methods instrumental in preventing degeneration into more severe disease states following the onset of symptoms.

The increased dollars and numbers of projects cannot be accounted for simply in terms of "real growth," due to the fact that a new and more encompassing definition of prevention is being applied to the NIH research portfolio Therefore, the 1983-84 figures present a discontinuity when compared with the figures for 1981-82, which were compiled when a more restrictive definition was applied. Now that a definition has been agreed to by all the NIH institutes and other PHS research agencies as well as by the Assistant Secretary for Health, we can plan to use the FY 1983-84 figures as a base for measuring prevention research trends during the coming years

Recognizing that prevention clearly is the most useful and cost-effective extension of knowledge in the field of health, the NIH has long been involved in prevention-related research, even though these activities might not always have been termed "prevention." From basic laboratory investigations to community demonstration programs, NIH-sponsored research scientists throughout the country are searching for effective preventive measures to reduce the death, suffering, di· ·bility, and financial loss associated with diseas. and accidents. Indeed, the NIH mission in disease prevention and health promotion perhaps can best be framed by the words of Dr. Lewis Thomas:

> "When medicine has really succeeded brilliantly in technology the cost is likely to be very low indeed It is when our technologies have to be applied halfway along against the progress of disease, or must be brought in after the fact to shore up the loss of destroyed tissue, that health care becomes enormously expensive. The deeper our understanding of a disease mechanism, the greater are our chances of devising direct and decisive measures to prevent disease, or to turn it around before it is too late."[3]

Summary of Findings

A caveat to any summary of results in this survey must be that there is now greater agreement than ever before as to what constitutes prevention research; the operative definitions have changed considerably over the period in which the reported data were compiled Nevertheless, certain findings are both striking and indicative of the growing NIH commitment to research aimed at disease prevention. Chief among these indicators is the fact that the initial effort to make an accurate accounting of NIH prevention research funding, undertaken in 1979 by this office, revealed that $352 million was spent in FY 1978 on primary prevention research alone This third followup study, combining fiscal years 1981, 1982, and 1983, now indicates that the figure has grown to $957 million—an increase of 172 percent over the FY 1978 total and 3½ times the rate of growth of total NIH research funding (49%) during the same period.

Other significant findings of the FY 1981-1983 survey are that:

- Prevention research—through grant, contract, and intramural investigations—constituted 26.8 percent of the total FY 1983 budget for all NIH bureaus, institutes, and divisions.
- Twelve of the 13 BIDs (all but the overwhelmingly basic research-oriented NIGMS) reported that prevention research made up more than 10 percent of their total budgets for FY 1983; 10 BIDs did so in FY 1981 and 1982.
- Four institutes—NCI, NHLBI, NICHD, and NIEHS—accounted for nearly 72 percent of total NIH prevention research funding in both FY 1981 and 1982: in FY 1983, these institutes plus NIADDK each spent more than $100 million for prevention research, accounting for 77 percent of total NIH prevention research funding. For comparison, these four institutes accounted for an average of 52 percent of total NIH research funding in all areas during the period.
- About 31 percent of the $957 million allocated for NIH prevention research in FY 1983 was for funding of projects directly related to four DHHS prevention priority areas (in order): t· ic agent and radiation control, improved nutrition, pregnancy and infant care, and family planning.
- About 60 percent of total NIH prevention obligations in FY 1981 and 1982 were made through the research grant mechanism; this proportion increased to 65 percent in FY 1983.

These and other findings related to NIH prevention research are summarized in this document through tables graphs, and figures representing FY 1981, 1982, and 1983 actual and, where noted, FY 1984 estimated budgetary obligations. The data presented herein were compiled by the NIH Coordinator for Disease Prevention and Health Promotion, based on input from the prevention coordinators and planning and budget officers of the BIDs. The names and titles of the NIH prevention coordinators are listed at the end of this section.

In the compilation process, each BID was requested to select those projects which in its view involve prevention as denoted by the NIH working definition; the BIDs also categorized individual projects and funding levels by grants, contracts, and intramural projects Because the intent of this accounting effort was to identify prevention research activities, funding figures for management operations, and construction were excluded, although in some cases these activities do, in fact, relate to prevention

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Format of Report

Section I of this document presents brief narrative highlights of prevention research activities under way at NIH and NIH-supported institutions Though far from exhaustive, this sampling from all the BIDs illustrates the far-reaching progress being made by scientists in numerous and diverse fields

Section II contains tables and charts showing FY 1981, 1982, and 1983 actual budgetary information (and, where noted, FY 1984 estimates) for overall NIH prevention research efforts For ease of reference, each summary graphic is presented with the numerical table on which it is based

Finally, Section III presents NIH prevention research activities by individual BID, in both narrative and numerical form Tables showing FY 1981, 1982, and 1983 actual budgetary data (and FY 1984 estimated totals) for each BID are categorized by organizational structure or program area, these are preceded by brief descriptions of major areas of activity in prevention research for that BID

In reviewing these data, the reader is reminded that merely comparing prevention funding levels among BIDs can be misleading. Some BIDs, such as NIEHS and NICHD, have mandates that are inherently prevention oriented, thus, a large percentage of their funds are allocated to prevention research Others, such as NIGMS (which almost exclusively supports investigator-initiated basic research), do not have responsibility for specific disease areas, thus, their activities are by intention less directed and more general in nature This overwhelmingly basic research, while vital to our understanding of the underlying factors in disease causation and hence to the development of prevention measures, is not included in the prevention research data summarized in this report

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[1] Frank, J P A System of Complete Medical Police Baltimore Johns Hopkins University Press, 1976

[2] Heckler, M. M Remarks at the Public Health Service Awards Ceremony Washington, D C , May 26, 1983

[3] Thomas, L The Medusa and the Snail New York The Viking Press, 1979, p 173

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I. RESEARCH HIGHLIGHTS:

Progress for Disease Prevention at the National Institutes of Health

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One of the primary goals of the National Institutes of Health is to support research that could ultimately lead to the prevention of disease. In working toward this long-range objective, each bureau, institute, and division has actively pursued bo basic and applied research that shows promise of leading to methods of preventing or ameliorating disease. In certain instances these goals include finding methods to detect disease before it becomes manifest; in other instances the aim is to halt or reverse further development of existing disease. New and exciting prevention research findings are forthcoming from such diverse fields as biochemical epidemiology, nutrition, social and behavioral science, enzymology, developmental embryology, biochemistry, physiology, immunology, pharmacology, and many other areas. Progress in basic science has reached the point where we now have the knowledge to apply these findings at both the pre-intervention and intervention levels, so that health practitioners and the public can take measures to reduce risk and morbidity for acute and chronic diseases as never before. The following highlights typify the breadth of basic and applied research conducted and supported by NIH as it works toward its goal of preventing disease.

Biochemical Epidemiology

This exciting and important new area of NCI research combines epidemiological and chemical analytical approaches used in the past in other research areas that are now being applied to investigate the causes of cancer. Laboratory techniques have been developed that use biochemical measures to better characterize exposure to carcinogens, to serve as indicators during the course of malignancy, to identify interventions that halt or reverse this process, and to investigate mechanisms of human carcinogenesis Examples of studies in this area include

- Efforts to evaluate the body burden of chemical carcinogens in studies of occupational and general environmental cancer risk factors,
- Sophisticated analyses of air, water, and biologic specimens for carcinogenic and mutagenic substances, in conjunction with specific analytical studies,
- Search for evidence of viral infection including viral segments or oncogenes in the DNA of individuals at high risk of cancer that may be associated with infectious agents or heritable states,
- Evaluation of disturbances in immune function as they may relate to malignancies, particularly those of the hematopoietic system,
- Investigation of the relationship between micronutrients and a variety of epithelial cancers, and
- Determination of the relationship of macronutrients, including dietary fat, and hormonal changes to subsequent risk of breast, endometrial, and colon cancers

The potential of biochemical epidemiology to predict cancer risk on individuals, instead of at the population level, and before the onset of clinically evident cancer provides an exciting new opportunity in cancer research and prevention.

Oncogenes

Recent advances in molecular biology, including the development of recombinant DNA and nucleotide sequencing techniques, have made it possible to isolate and amplify oncogenes and to dissect their fine structure Oncogenes are dominant genetic elements whose expression within a normal cell leads to malignant transformation. Although the first oncogenes were demonstrated in DNA and RNA tumor viruses, oncogene sequences have now been found to be a part of the genetic complement of normal vertebrates, including humans.

Scientists supported by NCI have cloned viral oncogenes as well as their normal cellular homologs and are currently attempting to characterize their enzymatic functions and the targets of their transforming gene products Efforts to determine precisely how oncogenes are involved in human cancer are also under way. An important recent development in this area has been the detection and direct isolation of dominant transforming genes from human tumors. Oncogenes have been cloned from two human bladder carcinoma cell lines, and researchers have shown that a single codon change in the normal human allele results in its conversion to a gene with transforming properties. The cell contains tens of thousands of cellular genes, and in theory each could be the target of any number of genetic or environmental insults whose disruption could lead to cancer or to other diseases

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Heart and Lung Disease

In prevention research related to hypertension, new knowledge is being gained about the function of the nervous system in the regulation of normal blood pressure and in the pathogenesis and control of essential hypertension; the influence of local modulators of vessel wall resistance on blood pressure, genetic mechanisms, including those related to salt sensitivity and/or salt resistance, and sociological and psychological factors related to or involved in hypertension

In an important area of lung research, progress continues in identifying the mechanisms of lung damage that cause emphysema, a disease characterized by the destruction of a major structural protein of the lung, elastin Essentially, an imbalance of two groups of substances—the proteases, which break down elastin, and the antiproteases, which inhibit this breakdown—is responsible for this often fatal disease. Recent research by NHLBI indicates that inhalation of tobacco smoke elevates protease; smoking cessation, therefore, remains the most potent preventive measure against emphysema The Institute is continuing to support investigations attempting to increase lung protection against these proteases, an approach that has allowed the maintenance of "protective" levels of antiprotease activity in individuals with a genetic deficiency of alpha-1-antitrypsin An area of special promise is the intravenous administration of concentrated amounts of this natural antiprotease obtained from normal plasma

Periodontal Diseases

There is growing evidence that different forms of periodontal disease may be caused and aggravated by specific bacteria. Investigations to date by NIDR-supported scientists suggest that *Actinobacillus actinomycetemcomitans* and *Bacteroides gingivalis* are the etiologic agents in localized juvenile periodontitis, respectively However, no single species has unequivocally been shown to cause any of the periodontal diseases The current surge in research activities in this area should soon lead to the identification and characterization of pathogenic bacteria, allowing for more effective preventive and treatment measures

Diabetes

Although treatment with special diet, exercise, insulin, and other medications has extended and improved the lives of people with diabetes, such treatment has not prevented the development of the tissue-damaging aspects of the disease: heart attacks, strokes, kidney failure, gangrene, blindness, and damage to the nervous system Most of the morbidity mortality, and economic cost associated with diabetes is due to these degenerative tissue changes, but there is unresolved controversy as to whether they can be prevented by strict and precise control of blood glucose levels New technologies have now been developed that permit, for the first time, a study to assess whether strict metabolic control will prevent the serious clinical complications of insulin-dependent diabetes—an hypothesis supported by a growing body of preliminary evidence NIADDK, in addition to supporting a broad range of such explorations, is now initiating the Diabetes Control and Complications Trial to explore this potential method for preventing the devastating effects of insulin-dependent diabetes.

Vaccine Development

Since the mid-1960's, NIAID has led the NIH effort to develop vaccines for the control of infectious diseases. Presently there are more than 50 different antigens (proteins or other components of an organism that stimulate the immune response) being investigated in NIAID's Vaccine Development Program These antigens represent potential vaccines for a variety of diseases such as gonorrhea, herpes, malaria, parainfluenza, influenza, hepatitis, and meningitis Other institutes and Federal agencies are working on still other vaccines.

Recent events have led NIAID to implement a program to accelerate the development of new vaccines. The emergence of new knowledge—recombinant DNA and hybridoma technologies, new findings of how the immune system works, and biosynthetic technologies—permits new approaches and opportunities for vaccine development Use of these technologies promises to allow the development of vaccines several years earlier than otherwise would have been possible.

Mental Retardation and Developmental Disabilities

NICHD is supporting biomedical and behavioral studies to enhance knowledge of the basic causes of mental retardation, research that eventually may lead to the prevention or amelioration of this disability One such study deals with the initiation during the newborn period in children with phenylketonuria (PKU) of a diet that contains a limited amount of phenylalanine Measures of intellectual development, height, weight, and head circumference have shown that treated children with PKU, an inborn error of metabolism with an incidence of 1 in 14,000 births, achieve scores comparable to those of normal children.

Birth Defects

An estimated 250,000 babies are born each year in the United States with mental or physical defects

Accordingly, the long-term objective of the NICHD research program on birth defects is prevention Prevention of birth defects is achieved in part by genetic counseling and through prenatal diagnosis A more direct approach, however, is to eliminate the etiological factors that induce the defect Such an approach is most effective if the initiating agents, as well as the mechanisms through which they act, are known—a prerequisite not yet met for 65 to 70 percent of congenital defects. Therefore, NICHD focuses much of its research attention on the genetic and environmental control of developmental processes, an area of increasing progress and promise

Eye Disorders and Diseases

Basic and applied research by NEI has yielded considerable progress in the prevention of vision-related problems. For example:

- It has been demonstrated recently that appropriately timed laser treatment is very effective in preventing blindness from one form of aging-related maculopathy.
- Scientists are investigating the factors that promote recurrence of ocular herpes simplex infection in the hope of preventing or reducing the incidence of this disease.
- The efficacy of aldose reductase inhibitors in preventing the formation of diabetic cataracts in animals has been demonstrated, and current research is aimed at evaluating the role of such agents in preventing other diabetic complications
- Improved means for the early detection of individuals with elevated intraocular pressure is being sought to reduce the damage done in glaucoma
- When visual input is impeded early in life, there can be a profound and permanent impact on the development and function of the visual centers in the brain Studies are under way to prevent this early visual loss and to investigate the possibility of using drugs to reverse the effects of visual deprivation

National Toxicology Program

Administered by NIEHS, the National Toxicology Program coordinates research and testing activities and provides information about potentially toxic chemicals Use of this information by regulatory and research agencies and others may help prevent chemically induced diseases such as some forms of cancer and genetic damage. The National Cancer Institute's carcinogenesis testing program, which was transferred from NCI to direct NIEHS management in 1981, is an integral part of the NTP effort

A number of new assay methods being developed and validated by NTP hold promise for providing improved information for risk estimation. These include methodologies for measuring mutagenic activity of chemicals in human urine and blood; chromosomal analyses to measure genetic damage in humans; rodent liver tumor models for assessing initiation and promotion mechanisms of chemically induced immunotoxicity; automated procedures for measuring neurobehavioral toxicity in animals; methods for functional analysis and early diagnosis of kidney injury, and rodent embryo and cell culture systems as indicators of teratogenic potential

Nutrition

NIA's Epidemiology, Demography, and Biometry Program leads NIH's participation in the National Health and Nutrition Examination Survey (NHANES-I) Epidemiologic Followup Study of 14,400 people carefully examined for medical and nutritional status from 1971 to 1974 This is the first time a cohort of the National Health Survey has been traced and reinterviewed to study outcomes and identify risk factors As the largest followup study of nutritional outcomes conducted in the United States, NHANES-I will provide specific information on characteristics and conditions related to smoking, the use of alcohol, exercise, and changes in these behavioral characteristics over a 10-year period Blood pressure and weight will also be measured and related to previous measurements and outcomes.

International Issues

FIC's Advanced Studies Program recently conducted a series of prevention-related studies In 1980, an initial task force studying the approach taken to eradicate smallpox determined the applicability of that model to other infectious diseases Separate, in-depth analyses of three additional diseases (measles, poliomyelitis, and yaws) were recommended. Responding to the task force efforts, FIC sponsored an International Symposium on Measles Immunization in FY 1982 to discuss how available vaccines can be exploited to overcome the worldwide economic, logistical, and attitudinal barriers to immunization. As various countries achieve measles control within their own boundaries, the problem of importation of disease from countries where programs are less successful, or nonexistent, will become more apparent and increasingly troublesome. Thus, it is important for developed countries to provide assistance and knowledge to developing countries and to devise unique strategies to overcome disease transmission.

The elimination of measles as a universal cause of childhood misery and long-term disability will result in significant economic gains worldwide as health care costs for measles immunization and long-term care are eliminated. For example, in the United States alone, it has been estimated that in the 18-year period from 1963 to 1981 there were 4,840 lives saved and 16,100 cases of mental retardation averted When additional years of normal productive life are considered, as well as school days saved and physician or hospital costs saved, it is estimated the net benefits of measles control amount to more than $4 billion

Hansen's Disease

Hansen's disease (lepromatous leprosy) remains a major human health problem, affecting approximately 15 million people Scientists have been unable to develop a vaccine against the disease largely because they have not had a suitable animal model Following the detection of a spontaneous case of Hansen's disease in a monkey (sooty mangabey, or *Cercocebus atys*) and the subsequent successful induction of leprosy in additional mangabeys, the DRR-supported Delta and Yerkes Regional Primate Research Centers, in collaboration with the Armed Forces Institute of Pathology (AFIP), are now performing studies with this primate model A consortium grant was recently awarded by NIAID to support this work

Because the sooty mangabey is the first nonhuman primate model for leprosy studies, the existence of a breeding colony of this rare primate species at the Yerkes Primate Center, coupled with the laboratory resources and professional expertise of the Delta and Yerkes Centers and the AFIP, will greatly enhance progress toward a leprosy vaccine.

Lipid Storage Diseases

In several dozen inherited disorders, excessive amounts of natural metabolites (such as mucopolysaccharides and lipids) are stored, causing cell damage and, when the brain is involved, severe mental retardation. People with such disorders have been found to be missing certain enzymes necessary for the normal disposal of the accumulating material In disorders such as Tay Sachs disease and Gaucher's disease, the lack of specific enzymes leads to the accumulation of damaging fatty substances Using this information, NIGMS-supported scientists have helped develop amniocentesis procedures and carrier detection tests that have led to effective genetic counseling measures. Enzyme replacement measures are now under experimental testing. Advanced techniques to target appropriate receptors in the body are expected eventually to enhance, through genetic engineering, the development of enzyme production methods to prevent nerve cell damage and mental retardation and prolong life in these disorders

Neurotoxicity

Epidemiological studies on neurotoxic agents (lead cadmium, manganese, ferrous metal alloys, chemical synthetics, and heavy metals in coal, oil, and gasoline) conducted by NINCDS-supported scientists have revealed specific at-risk populations. Clinical studies to delineate early indicators of subtle nervous system injury, as well as environmental studies to define more accurately levels of exposure, have enhanced efforts to reduce environmental exposure to these agents and to develop adequate screening programs for people at risk. In addition, this area of prevention research has led to the development of technologies to produce protective devices for individuals employed in high-risk industries

II. SUMMARY DATA:

Funding for Research in Prevention at the National Institutes of Health

Fiscal Years 1981 – 1983 (1984 Estimated)

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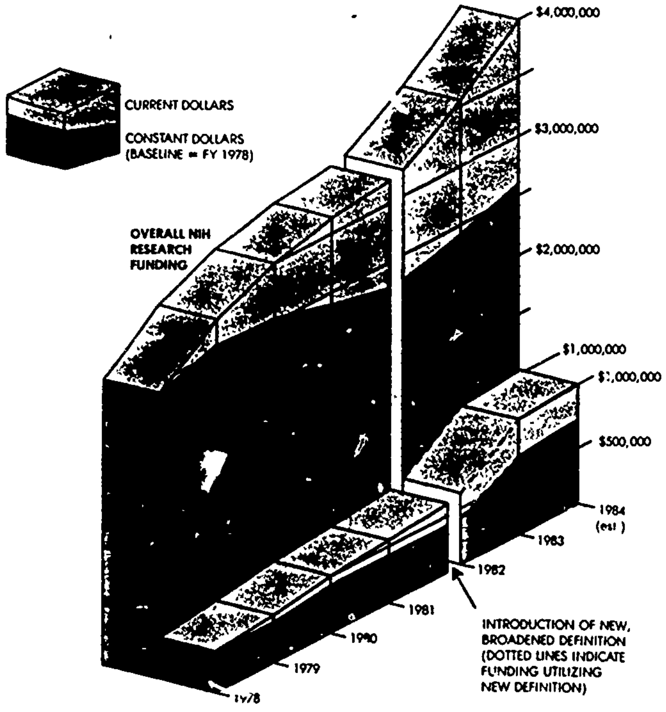
Trends in Overall NIH Research Funding Versus Prevention Research Funding



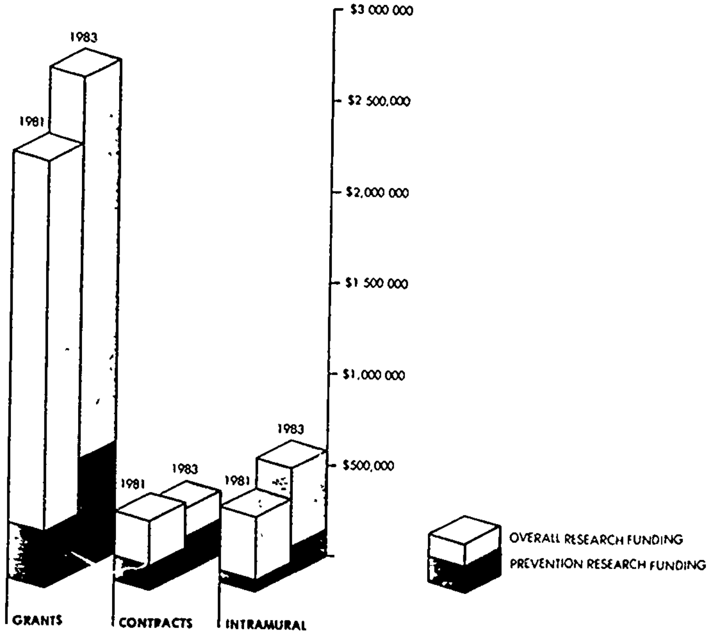
	Overall NIH Research Funding**		Prevention Research Funding		Prevention Funding As a Percentage of Overall NIH Funding	R & D Biomedical Deflator†
	Current Dollars	Constant Dollars	Current Dollars	Constant Dollars		
FY 1978	$2,367,554	$2,387,554	$ 352,121	$352,121	14 7%	100 00%
FY 1979	2,781,906	2,570,127	419,476	387,542	15 1	108 24
FY 1980	2,991,684	2,533,393	506,459	428,875	16 9	118 09
FY 1981	3,135,479	2,409,312	542,416	416,794	17.3	130 14
FY 1982	3,222,165	2,302,040	602,549	430,484	18 7	139 97
FY 1983	3,563,899	2,435,522	957,152	654,105	26 8	146 3
FY 1984 (est)*	3,995,742	2,608,016	1,070,072	698,435	26 8	153 21

* FY 1984 overall NIH obligations are estimates based on the 1984 column of President Reagan's 1984 budget FY 1984 prevention research obligations are estimates.

** NIH obligations include funds for research grants, R&D contracts, disease control, intramural research, and FIC direct operations (conferences fellowships, etc) excluding salaries and expenses

† Provided by the Division of Program Analysis, Office of Program Planning and Evaluation, NIH

Overall NIH Research Funding* and Prevention Research Funding by Mechanism of Support



		Overall NIH Research Funding		Prevention Research Funding	
		Dollars	Percentage	Dollars	Percentage
Grants	FY 1981	$2,343,849	75%	$ 318,141	59%
	FY 1982	2,420,088	75	364,772	60
	FY 1983	2,710,019	76	621,174	65
	FY 1984 (est)	3,090,758	77	710,433	66
Contracts —	FY 1981	375,871	12	168,227	31
	FY 1982	347,632	11	148,637	25
	FY 1983	345,493	10	190,064	20
	FY 1984 (est)	355,850	9	198,469	19
Intramural —	FY 1981	415,759	13	56,047	10
	FY 1982	454,445	14	89,139	15
	FY 1983	508 387	14	145,914	15
	FY 1984 (est)	549,134	14	161,170	15
Total —	FY 1981	$3,135,479	100%	$ 542,415	100%
	FY 1982	$3,222,165	100%	$ 602,549	100%
	FY 1983	$3,563,899	100%	$ 957,152	100%
	FY 1984 (est)	$3,995,742	100%	$1,070,072	100%

* Total NIH obligations excluding research in training construction and program management

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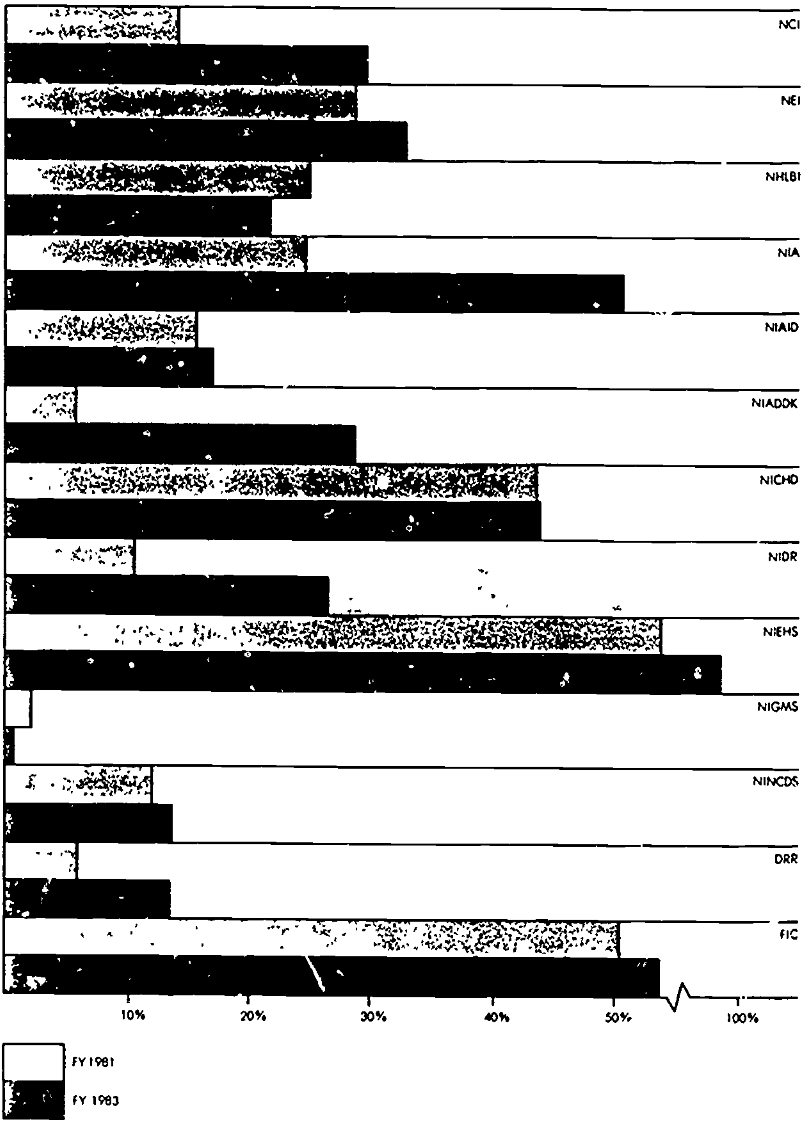
Institute (BID) Prevention Research Funding As a Percentage of Total Research Funding



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	Total Funds*					
	FY 1981 Obligations			FY 1982 Obligations		
	Total	Prevention	Percentage	Total	Prevention	Percentage
NCI	$ 910,449	$127,454	14 0%	$ 908,456	$132,869	14.6%
NEI	109,462	31,263	28.6	117,069	36,537	31 2
NHLBI	485,927	120,706	24 8	503,151	121,702	24 2
NIA	66,519	16,340	24 6	73,183	17,650	24 1
NIAID	211,198	32,948	15 6	215,880	30,345	14 1
NIADDK	336,425	19,438	5 8	335,906	28 474	8 5
NICHD	199,661	86,710	43 4	204,547	9[illegible] [illegible]58	46 4
NIDR	58,943	6,267	10 6	60,468	6,878	11.4
NIEHS	81,355	54,390	66 8	94,310	82,582	87 6
NIGMS	270,620	5,705	2 1	283,345	2,728	1 [illegible]
NINCDS	228,651	27,664	12 1	242,311	29,907	12 3
DRR	167,458	10,086	6 0	177,353	14,670	8 3
FIC	6,811	3,444	50 6	6,186	3,249	52 5
Total NIH	$3,135,479	$542,415	17.3%	$3,222,165	$60[illegible],549	18.7%

	Total Funds*					
	FY 1983 Obligations			FY 1984 (est.) Obligations		
	Total	Prevention	Percentage	Total	Prevention	Percentage
NCI	$ 906,777	$266,528	29 4%	$ 994,481	$307,894	31.0%
NEI	132,054	43,240	32 7	144,661	47,500	32 8
NHLBI	563,149	121,519	21 6	637,681	128,000	20 1
NIA	84,441	43,110	51 1	104,17[illegible]	54,803	52 6
NIAID	256,574	43,472	16 9	289,22[illegible]	47,652	16 5
NIADDK	378,720	109,148	28 8	426,560	121,825	28 6
NICHD	2[illegible]52	100,467	43 6	250,353	109,200	43 6
NIDR	67,381	17,752	26 3	77,732	18,995	24 4
NIEHS	151,478	138,797	91 6	166,331	151,764	91 2
NIGMS	309,566	2,462	0 8	353,958	2,960	0 8
NINCDS	269,765	37,183	13 8	307,786	42,417	13 8
DRR	206,249	28,529	13 8	234,697	31,572	13 4
FIC	7,392	4,945	66 9	8,103	5,490	67.8
Total NIH	$3,563,899	$957,[illegible]52	26 8%	$3,995,742	$1,070,072	26 8%

* Includes grants, contracts and intramural

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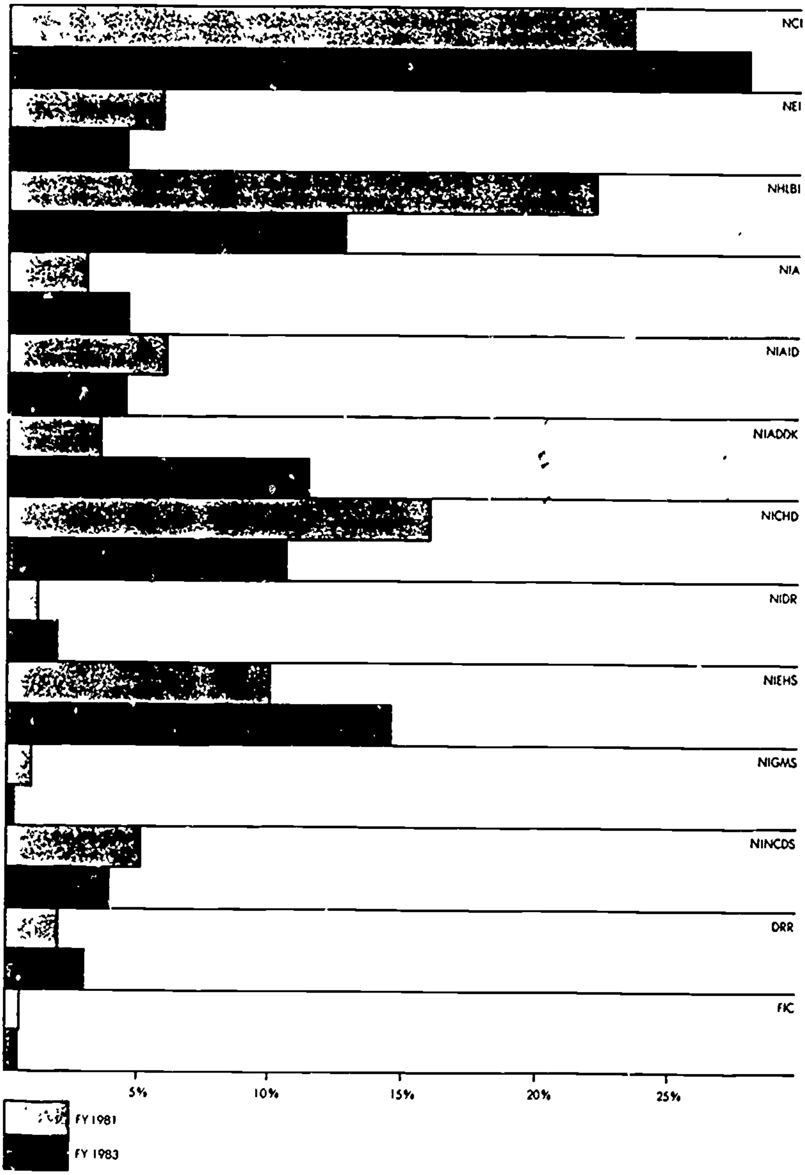
Distribution of Total NIH Prevention Funding by Institute (BID)



	FY 1981		FY 1982	
	Total Prevention Funding	Percentage of Total NIH Prevention Funding	Total Prevention Funding	Percentage of Total NIH Prevention Funding
NCI	$127,454	23 5%	$132,869	22.0%
NEI	31,263	5 8	36,537	6 1
NHLBI	120,706	22.2	121,702	20 2
NIA	16,340	3 0	17,650	2 9
NIAID	32,948	6 1	30,345	5 0
NIADDK	19,438	3 6	28,474	4 7
NICHD	86,710	16 0	94,958	15 8
NIDR	6,267	1 2	6,878	1.1
NIEHS	54,390	10 0	82,582	13 7
NIGMS	5,705	1.0	2,728	0 4
NINCDS	27,664	5 1	29,907	5 0
DRR	10,086	1 9	14,670	2.4
FIC	3,444	0 6	3,249	0 5
Total NIH	$542,415	100 0%	$602,549	100 0%

	FY 1983		FY 1984 (est.)	
	Total Prevention Funding	Percentage of Total NIH Prevention Funding	Total Prevention Funding	Percentage of Total NIH Prevention Funding
NCI	$266,528	27 8%	$ 307,894	28 8%
NEI	43,240	4 5	47,500	4 4
NHLBI	121,519	12 7	128,000	12 0
NIA	43,110	4 5	54,803	5 1
NIAID	43,472	4.5	47,652	4 4
NIADDK	109,148	11 4	121,825	11 4
NICHD	100,467	10 5	109,200	10 2
NIDR	17,752	1 8	18,995	1 8
NIEHS	138,797	14 5	151,764	14.2
NIGMS	2,462	0 3	2,960	0 3
NINCDS	37,183	3 9	42,417	4 0
DRR	28,529	3 0	31,572	3 0
FIC	4,945	0 5	5,490	0 5
Total NIH	$957,152	100 0%	$1,070,072	100 0%

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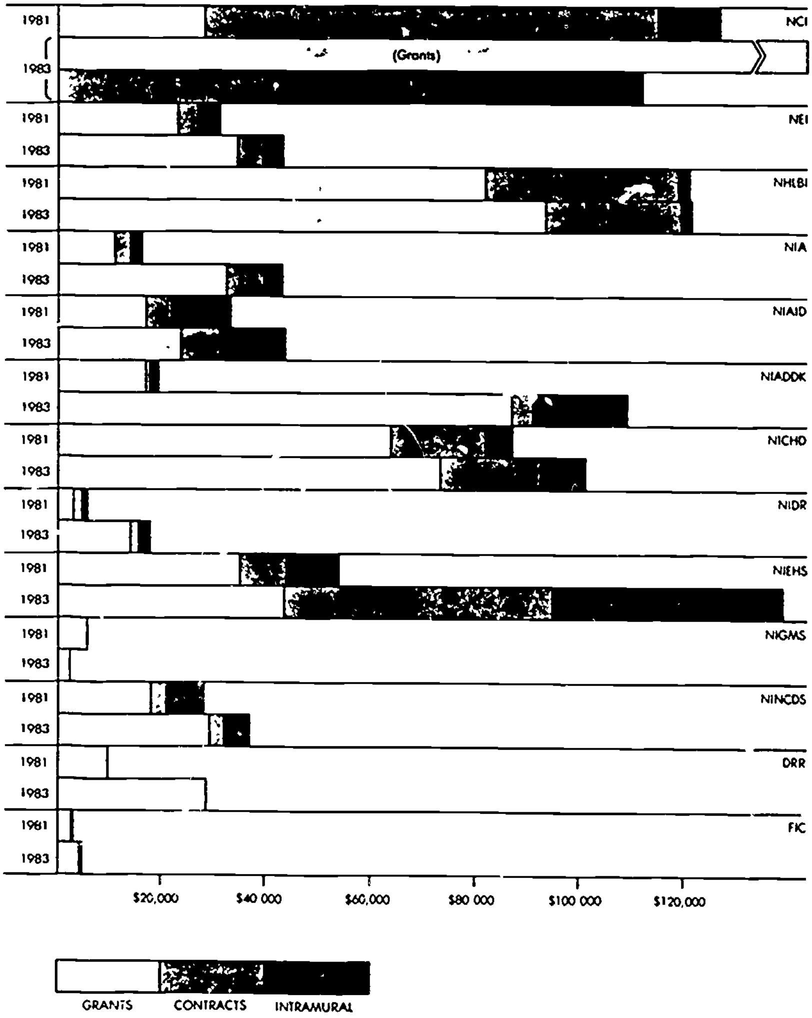
NIH Prevention Research by Mechanism of Support



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	FY 1981				FY 1982			
	Grants	Contracts	Intramural	Total	Grants	Contracts	Intramural	Total
NCI	$ 28,457	$ 87,206	$11,791	$127,454	$ 31,724	$ 84,265	$16,880	$132,869
NEI	23,227	3,977	4,059	31,263	28,376	3,238	4,923	36,537
NHLBI	81,776	36,888	2,042	120,706	100,186	19,626	1,890	1[illegible]1,702
NIA	10,700	3,340	2,300	16,340	12,655	3,267	1,728	17,650
NIAID	16,652	5,357	10,939	32,948	16,669	4,628	9,048	30,345
NIADDK	17,237	648	1,553	19,438	24,108	1,193	3,173	28,474
NICHD	64,126	17,841	4,743	86,710	69,979	16,758	8,221	94,958
NIDR	3,770	1,527	970	6,267	4,046	791	2,040	6,878
NIEHS	35,420	8,624	10,346	54,3[illegible]0	37,443	12,238	32,901	82,582
NIGMS	5,705	—	—	5,705	2,728	—	—	2,728
NINCDS	17,711	2,819	7,134	27,664	19,082	2,633	8,192	29,907
DRR	10,086	—	—	10,086	14,670	—	—	14,670
FIC	3,274	—	170	3,444	3,106	—	143	3,249
Total	$318,141	$168,227	$56,047	$542,415	$364,772	$148,637	$89,13[illegible]	$602,549

	FY 1983				FY 1984 (est.)			
	Grants	Contracts	Intramural	Total	Grants	Contracts	Intramural	Total
NCI	$154,374	$ 66,811	$ 45,343	$266,528	190,329	$ 66,959	$ 50,606	$ 307,894
NEI	34,388	4,729	4,173	43,240	37,510	5,440	4,550	47,500
NHLBI	93,771	25,764	1,984	121,519	98,9[illegible]	27,000	2,100	128,000
NIA	32,454	7,163	3,493	43,1[illegible]0	41,[illegible]99	8,469	4,835	54,803
NIAID	23,668	7,168	12,636	43,472	26,547	7,802	13,303	47,652
NIADDK	86,859	4,057	18,232	109,148	97,716	4,564	19,545	121,825
NICHD	73,305	18,756	8,406	100,467	79,965	20,000	9,235	109,200
NIDR	13,825	1,547	2,380	17,752	14,793	1,655	2,547	18,995
NIEHS	43,684	51,122	43,991	138,797	49,867	53,387	48,510	151,764
NIGMS	2,462	—	—	2,462	2,960	—	—	2,960
NINCDS	29,262	2,947	4,974	37,183	33,871	3,193	5,353	42,417
DRR	28,529	—	—	28,529	31,572	—	—	31,572
FIC	4,643	—	302	4,945	4,904	—	586	5,490
Total	$621,174	$190,064	$145,914	$957,152	$710,433	$198,469	$161,170	$1,070,072

NIH Funding for DHHS Prevention Priority Areas*

DHHS Priority Area	Supporting Institutes (BIDs)	Dollars in Millions FY 1981	FY 1982	FY 1983
Toxic Agent and Radiation Control	NCI	$ 61 2	$ 15 4	$ 20 0
	NHLBI	3 0	—	—
	NICHD	2 0	3 8	3 8
	NIEHS	51 0	79 7	138 8
	DRR	0 8	0 4	1.0
	FIC	0 2	—	—
	Total	$118	$ 99	$154
Improved Nutrition	NCI	$ 5 4	$ 11 4	$ 10 2
	NEI	8 2	9.0	11 1
	NHLBI	5 0	5 2	5 2
	NIA	1 6	1.3	2 1
	NIADDK	5 9	1 8	6 1
	NICHD	10 0	9 8	10 4
	NIDR	—	0 9	0 9
	NIEHS	2 1	1 6	—
	NINCDS	0 6	0 6	0 6
	DRR	2 9	6 1	13 9
	FIC	0 2	0 2	0 2
	Total	$ 42	$ 48	$ 61
Pregnancy and Infant Care	NCI	$ 0 5	$ 1 2	$ 1 6
	NEI	8 6	9 0	9 6
	NICHD	52 7	2[illegible] 6	23 7
	DRR	1 8	2 2	3 0
	FIC	0 2	0 2	0 2
	Total	$ 64	$ 37	$ 38
Family Planning	NICHD	$ 21 0	$ 33 7	$ 33 0
	DRR	0 4	0 1	0 5
	Total	$ 21	$ 34	$ 34
Surveillance and Control of Infectious Diseases	NCI	$ 1 3	$ 1 1	$ 5 5
	NEI	6 1	7 0	7.3
	NIA	—	0 1	0 4
	NIAID	6 8	7.0	10 1
	DRR	0 5	0 6	1 0
	FIC	1 8	1 8	2.7
	Total	$ 16	$ 18	$ 27
Immunizations	NCI	$ 0 6	$ 0 3	$ —
	NIA	—	—	0 2
	NIAID	20 7	14 8	21 3
	NIDR	—	0 9	—
	DRR	0 3	0 9	0 8
	FIC	0 3	0 3	0 5
	Total	$ 22	$ 17	$ 23
Fluoridation and Dental Health	NIDR	$ 5 0	$ 5 0	$ 15 0
	DRR	0 03	0 04	0 06
	Total	$ 5	$ 5	$ 15
High Blood Pressure Control	NHLBI	$ 9 0	$ 10 6	$ 10 9
	NIA	—	0 2	0 4
	DRR	0 3	1.3	1 2
	FIC	0 2	0 2	0 2
	Total	$ 10	$ 12	$ 13

* Taken from Prevention 80 DHHS (PHS) Publication No 81 50157

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DHHS Priority Area	Supporting Institutes (BIDs)	Dollars in Millions FY 1981	FY 1982	FY 1983
Occupational Safety and Health	NCI	$ 9.2	$ 8.7	$ 8 0
	NEI	1.0	1.0	—
	NHLBI	3 0	2.2	2 3
	DRR	0 2	0.1	0 1
	Total	$ 13	$ 12	$ 10
Smoking and Health	NCI	$ 4 1	$ 1.2	$ 4 8
	NHLBI	3 0	2.0	2 2
	NICHD	1.0	1.7	1 9
	NIEHS	1.3	1.3	—
	DRR	0 1	0.1	0.2
	FIC	0 2	—	—
	Total	$ 10	$ 6	$ 9
Sexually Transmitted Diseases	NCI	$ 0 01	$ 0 03	$ - -
	NEI	0 7	1.0	0 7
	NIAID	2.5	3 6	4 5
	NICHD	—	1.8	2.6
	DRR	0.3	—	—
	Total	$ 3	$ 6	$ 8
Control of Stress and Violent Behavior	NHLBI	$ 1 0	$ 1.8	$ 1.9
	NIA	4 4	0 6	1 0
	DRR	0 2	0 3	1 [illegible]
	Total	$ 6	$ 3	$ 4
Physical Fitness—Exercise	NHLBI	$ 1 0	$ 0 9	$ 1 0
	NIA	4 2	0 6	1.1
	DRR	0 2	0 3	0 7
	FIC	0 2	—	—
	Total	$ 6	$ 2	$ 3
Accident Prevention and Injury Control	NEI	$ 0 6	$ 1.0	$ 1 7
	NHLBI	1 0	—	—
	NIA	—	0 5	1 3
	DRR	0.01	0 01	0 03
	Total	$ 2	$ 2	$ 3
Misuse of Alcohol and Drugs	NCI	$ —	$ 0 02	$ 0 06
	NIA	1.1	0 5	0 4
	NICHD	—	0 8	1.5
	DRR	0 2	0 6	0 7
	FIC	0 2	—	—
	Total	$ 2	$ 2	$ 3
SUBTOTAL FOR 15 PRIORITY AREAS		$340	$300	$413
Cross-Cutting and Other	NCI	$ 45 0	$ 93 3	$216 4
	NEI	6 1	8.0	12 8
	NHLBI	93 0	98 7	97.9
	NIA	5 0	13 8	36 4
	NIAID	2 9	5.0	7.5
	NIADDK	13 5	26 7	103 0
	NICHD	—	18 8	23 6
	NIDR	1 3	1 0	1.9
	NIGMS	5 7	2 7	2.5
	NINCDS	27 1	29.3	36 6
	DRR	2.4	1.6	4 2
	FIC	0.2	0 6	1 0
	Total	$202	$304	$544
TOTAL		$542	$603	$957

III. Prevention Activities and Funding by Program Area

Fiscal Years 1981 – 1983 (1984 Estimated)

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National Cancer Institute (NCI)

- Determination of the molecular mechanism by which oncogenes act to transform cells
- Studies on the role of human T-cell leukemia virus in leukemia and other cancers and possible development of a vaccine for use in endemic areas
- Etiologic studies on acquired immunodeficiency syndrome (AIDS) and Kaposi's sarcoma with a search for a transmissible agent
- Evaluation of the efficacy in high-risk groups of hepatitis B virus vaccination in preventing hepatitis virus infection and development of hepatocellular carcinoma
- Development of improved methods for predicting carcinogenicity, mutagenicity, and teratogenicity
- Development of improved animal to-man extrapolation techniques
- Development of procedures for the qualitative and quantitative analysis of body fluids and tissues for the presence of chemical carcinogens, their metabolites, and their adducts with DNA
- Development of organ and cell culture systems, biological models, and bioassay systems for use in carcinogenesis studies
- Development of biochemical epidemiology as a multidisciplinary investigation into cancer etiology combining epidemiological and laboratory approaches for predicting cancer risk for individuals
- Studies to identify occupational causes of cancer and educational programs to reduce exposure to occupational hazards
- Studies to clarify the role of general environmental pollutants, medications, infectious agents, and genetic susceptibility as risk factors for cancer
- Smoking cessation research, education, and information programs, identification of high-risk groups especially vulnerable to hazards of tobacco
- Environmental carcinogenesis research, information, and education programs
- Experimental, epidemiological, and multidisciplinary research to elucidate the role of nutrients and other dietary components in causing and inhibiting cancer
- Studies of vitamins, vitamin analogs, and other dietary or nondietary substances that have potential as preventive agents
- Clinical trials of promising chemopreventive agents
- Studies on the role of natural inhibitors in cancer prevention
- Support of cancer control research units and cancer control science projects for defined population studies
- Collecting and disseminating technical information related to prevention
- Epidemiologic studies to delineate high-risk groups and individuals and to identify etiologic factors for cancer
- Studies of cancer risk from low-level exposure to ionizing or nonionizing radiation
- Studies to assess the efficacy of screening and to evaluate detection technologies and perform research to determine the methods of applying these technologies to defined populations
- Evaluate and analyze existing cancer data bases to obtain optimal utility and information pertaining to prevention

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Research Area	FY 1981 Grants No.	FY 1981 Grants Dollars	FY 1981 Contracts No.	FY 1981 Contracts Dollars	FY 1981 Total No.	FY 1981 Total Dollars
Division of Cancer Cause and Prevention						
Chemical and Physical Carcinogenesis	25	$ 2,400	37	$11,928	62	$ 14,328
Smoking and Health	—	—	3	675	3	675
Diet and Nutrition	28	2,723	1	200	29	2,923
Viral Oncology	46	5,000	7	1,192	53	6,192
Epidemiology	31	5,355	64	16,917	95	22,272
Total	130	$15,478	112	$30,912	242	$46,390
Division of Cancer Biology and Diagnosis						
Breast Cancer Task Force	4	$ 838	6	$ 211	10	$ 1,049
Division of Cancer Treatment						
Clinical Oncology	2	162	3	387	5	54[illegible]
Developmental Therapeutics	—	—	—	—	—	—
Radiation	1	146	—	—	1	146
Total	3	308	3	387	6	695
Division of Resources, Centers, and Community Activities						
Behavioral Medicine	7	617	1	7	8	624
Smoking and Health	7	761	4	588	11	1,349
Preventive Medicine	10	1,510	41	5,212	51	6,722
Education	20	1,870	29	1,850	49	3,720
Occupational Medicine	5	1,104	3	4,963	8	6,067
National Organ Site Program	47	3,487	—	—	47	3,487
Cancer Centers, including Centers Outreach	56	1,470	—	—	56	1,[illegible]70
Diet and Nutrition	—	—	2	641	2	641
Chemoprevention	1	337	—	—	1	337
Total	153	11,156	80	13,261	233	24,417
National Toxicology Program						
Carcinogenesis Testing	2	677	86	42,435	88	43,112
Total Grants & Contracts	292	$28,457	287	$87,206	579	$115,663

NCI Intramural Research

Research Area	Dollars FY 1981	FY 1982	FY 1983	FY 1984 (est.)
Nutrition	$ —	$ —	$ 250	$ 264
Chemical and Physical Carcinogenesis	7,432	10,317	14,832	16,590
Biological Carcinogenesis	778	630	19,602	21,276
Epidemiology	2,396	4,599	7,466	8,753
International Cancer Research Data Bank	602	702	—	—
Cancer Communications	542	632	—	—
Total Intramural	$11,791	$16,380	$42,150	$46,883

NCI (continued)

Research Area	FY 1982 Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Total No.	Total Dollars
Division of Cancer Cause and Prevention						
Chemical and Physical Carcinogenesis	30	$ 3,000	17	$ 6,271	47	$ 9,271
Smoking and Health	—	—	3	684	3	684
Diet and Nutrition	30	2,960	1	200	31	3,160
Viral Oncology	46	5,103	4	607	50	5,710
Epidemiology	31	5,671	46	13,379	77	19,050
Total	137	16,734	71	21,141	208	37,875
Division of Cancer Biology and Diagnosis						
Breast Cancer Task Force	—	—	—	—	—	—
Division of Cancer Treatment						
Clinical Oncology	3	269	5	454	8	723
Developmental Therapeutics	—	—	1	125	1	125
Radiation	—	—	—	—	—	—
Total	3	269	6	579	9	848
Division of Resources, Centers, and Community Activities						
Behavioral Medicine	18	2,630	3	205	21	2,835
Smoking and Health	4	450	2	609	[illegible]	1,059
Preventive Medicine	12	2,273	27	4,120	39	6,393
Education	18	1,700	39	4,515	57	6,215
Occupational Medicine	5	935	1	4,000	6	4,935
National Organ Site Program	33	3,290	—	—	33	3,290
Cancer Centers, including Centers Outreach	51	1,443	14	528	65	1,971
Diet and Nutrition	—	—	2	580	2	580
Chemoprevention	6	2,000	—	—	6	2,000
Total	147	14,721	88	14,557	235	29,278
National Toxicology Program						
Carcinogenesis Testing	—	—	1	47,988	1	47,988
Total Grants & Contracts	287	$ 31,724	166	$84,265	453	$115,989

NCI Prevention Obligations by Mechanism

Mechanism	FY 1981 No.	FY 1981 Dollars	FY 1982 No.	FY 1982 Dollars	FY 1983 No.	FY 1983 Dollars	FY 1984 (est.) No.	FY 1984 (est.) Dollars
Grants	292	$ 28,457	287	$ 31,724	1,057	$143,481	1,112	$175,906
Contracts	287	87,206	166	84,265	218	62,115	216	61,842
Intramural	*	11,791	*	16,880	*	42,150	*	46,883
Management and Support Costs**		—		—		18,782		23,263
Total	579	$127,454	453	$132,869	1,275	$266,528	1,328	$307,894

* Individual projects cannot be broken out by numbers

** Became a reportable prevention obligation in FY 1983

Research Area	FY 1983 Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Total No.	Total Dollars
Division of Cancer Etiology*						
Chemical and Physical Carcinogenesis	354	$ 38,720	53	$12,174	407	$ 50,894
Smoking and Health	8	1,440	4	827	12	2,267
Diet and Nutrition	37	6,895	0	0	37	6,895
Viral Oncology	308	50,364	16	3,208	324	53,572
Epidemiology	92	16,788	47	14,046	139	30,834
Total	799	114,207	120	30,255	919	144,462
Division of Cancer Biology and Diagnosis						
Tumor Biology	(8)	2,101	—	—	(8)	2,101
Total	(8)	2,101	—	—	(8)	2,101
Division of Cancer Treatment						
Radiology	49	5,919	9	2,721	58	8,640
Total	49	5,919	9	2,721	58	8,640
Division of Cancer Prevention and Control**						
Behavioral Medicine	(29)	3,721	(2)	103	(31)	3,824
Smoking and Health	(3)	660	(2)	286	(5)	946
Preventive Medicine	(6)	1,025	(39)	8,251	(45)	9,276
Education	(26)	2,174	(20)	5,514	(46)	7,688
Occupational Medicine	(7)	1,754	(1)	145	(8)	1,899
National Organ Site Program	(40)	4,106	(2)	150	(42)	4,256
Cancer Centers, including Centers Outreach	(65)	1,576	(16)	155	(81)	1,731
Diet and Nutrition	—	—	(1)	307	(1)	307
Chemoprevention	(21)	2,985	(4)	906	(25)	3,891
Cancer Control Science Program	(3)	2,400	—	—	(3)	2,400
Cancer Control Research Units	(1)	853	—	—	(1)	853
Total	(201)	21,254	(87)	15,817	(288)	37,071
Frederick Cancer Research Facility						
Biological Carcinogenesis	—	—	(1)	6,437	(1)	6,437
Chemical and Physical Carcinogenesis	—	—	(1)	6,885	(1)	6,885
Total	—	—	(2)	13,322	(2)	13,322
Total Grants & Contracts	(1,057)	$143,481	(218)	$62,115	(1,275)	$205,596
[FY 1984 total Grants & Contracts (estimated)]	[1,112]	[$175,906]	[216]	[$61,842]	[1,328]	[$237,748]

* Formerly the Division of Cancer Cause and Prevention

** Formerly the Division of Resources, Centers and Community Activities

National Eye Institute (NEI)

- Prevention of hereditary and developmental degenerations of the retina
- Prevention of proliferative diabetic retinopathy
- Prevention of retrolental fibroplasia and other proliferative retinopathies
- Prevention of blindness from branch vein occlusion
- Prevention of uveitis and other ocular inflammations through research on immune mechanisms
- Prevention of the toxic effects of drugs on the eye
- Prevention of recurrent corneal infection from herpes simplex virus
- Prevention of trachoma
- Prevention of human senile cataract
- Prevention of diabetic cataract
- Identification of risk factors related to the development of glaucoma
- Development of new drugs and treatments related to the prevention of glaucoma
- Research on the effects of visual deprivation related to the prevention of amblyopia and strabismus
- Prevention and/or control of eye diseases related to nutritional deficiencies
- Prevention of macular diseases and their consequences
- Prevention of visual impairment from corneal burns and ulcers
- Prevention of nearsightedness and other refractive errors

FY 1981

Research Area	Grants		Contracts		Intramural		Total	
	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Retinal and Choroidal Diseases	84	$ 8,108	24	$3,497	17	$2,464	125	$14,069
Corneal Diseases	60	5,754	—	—	2	290	62	6,044
Cataract	36	3,185	—	—	3	435	39	3,620
Glaucoma	25	2,048	—	—	6	870	31	2,918
Strabismus, Amblyopia, and Visual Processing	49	4,132	—	—	—	—	49	4,132
Other (Visual Acuity Impairment Study)	—	—	1	480	—	—	1	480
Total	254	$23,227	25	$3,977	28	$4,059	307	$31,263

FY 1982

Research Area	Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Intramural No.	Intramural Dollars	Total No.	Total Dollars
Retinal and Choroidal Diseases	111	$10,758	24	$3,139	15	$3,077	150	$16,974
Corneal Diseases	67	6,875	—	—	—	—	67	6,875
Cataract	46	3,933	—	—	3	615	49	4,548
Glaucoma	24	2,242	—	—	6	1,231	30	3,473
Strabismus, Amblyopia, and Visual Processing	56	4,569	—	—	—	—	56	4,569
Other (Visual Acuity Impairment Study)	—	—	3	100	—	—	3	100
Total	304	$28,376	27	$3,238	24	$4,923	355	$36,537

FY 1983

Research Area	Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Intramural No.	Intramural Dollars	Total No.	Total Dollars
Retinal and Choroidal Diseases	122	$12,988	28	$4,729	21	$3,043	171	$20,760
Corneal Diseases	71	7,577	—	—	—	—	71	7,577
Cataract	58	5,479	—	—	3	703	61	6,182
Glaucoma	37	3,633	—	—	5	427	42	4,060
Strabismus, Amblyopia, and Visual Processing	52	4,661	—	—	—	—	52	4,661
Total	340	$34,338	28	$4,729	29	$4,173	397	$43,240
[FY 1984 Total (estimated)]		[$37,510]		[$5,440]		[$4,550]		[$47,500]

National Heart, Lung, and Blood Institute (NHLBI)

- Smoking cessation research, education, and demonstration programs, identification of high-risk individuals
- Epidemiologic studies on asthma in families
- Evaluation of antenatal steroid therapy in neonatal respiratory distress syndrome
- National High Blood Pressure Education Program
- Research, information, and demonstration programs aimed at control of hypertension
- Prevention of deep-vein thrombosis
- Education and counseling programs for sickle cell disease
- Nutrition research, education, and demonstration programs aimed at lowering cholesterol, blood lipid, and weight levels
- Research into application of behavioral science to control of heart, lung, and blood disorders
- Demonstration of effectiveness of disease prevention/health promotion activities in certain settings such as the worksite
- Forging of collaborative education and control activities with the private sector, particularly industry and voluntary organizations
- Experimental activities featuring nontraditional use of media channels to inform and educate target audiences

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Research Area	FY 1981					
	Grants		Contracts		Total	
	No.	Dollars	No.	Dollars	No.	Dollars
Heart Diseases						
Arteriosclerosis	28	$ 5,579	21	$16,828	49	$ 22,407
Hypertension	95	10,222	14	8,133	109	18,355
Cerebrovascular Disease	1	57	—	—	1	57
Coronary Heart Disease	42	19,012	23	10,989	65	30,001
Peripheral Vascular Disease	1	138	—	—	1	138
Arrhythmias	10	715	—	—	10	715
Heart Failure and Shock	4	257	—	—	4	257
Congenital and Rheumatic Heart Disease	—	—	—	—	—	—
Circulatory Assistance	4	738	—	—	4	738
Cardiomyopathies and Infections	2	108	—	—	2	108
Multiprogram Areas	50	14,704	1	176	51	14,880
Total	237	51,530	59	36,126	296	87,656
Blood Diseases						
Bleeding and Clotting Disorders	143	12,524	—	—	143	12,524
Red Blood Cell Disorders	73	7,088	—	—	73	7,088
Sickle Cell Disease	26	6,803	—	—	26	6,803
Blood Resources	14	1,239	2	331	16	1,570
Total	256	27,654	2	331	258	27,985
Lung Diseases						
Structure and Function	1	86	—	—	1	86
Chronic Obstructive Lung Diseases	6	966	1	24	7	990
Pediatric Pulmonary Diseases	11	643	6	407	17	1,050
Fibrotic and Immunologic Diseases	1	30	—	—	1	30
Pulmonary Vascular Diseases	1	14	—	—	1	14
Multiprogram Areas	1	853	—	—	1	853
Total	21	2 [illegible]2	7	431	28	3,023
Total Grants and Contracts	514	$81,776	68	$36,888	582	$118,664
Intramural Research					27	$ 2,042
Total					609	$120,706

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NHLBI (continued)

Research Area	FY 1982 Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Total No.	Total Dollars
Heart Diseases						
Arteriosclerosis	31	$ 8,556	23	$11,132	49	$ 18,706
Hypertension	86	12,941	10	7,126	96	20,068
Cerebrovascular Disease	1	77	—	—	1	77
Coronary Heart Disease	47	19,924	1	52	48	19,976
Peripheral Vascular Disease	1	256	—	—	1	256
Arrhythmias	7	624	10	338	17	962
Heart Failure and Shock	4	267	—	—	4	267
Congenital and Rheumatic Heart Disease	1	30	—	—	1	30
Circulatory Assistance	—	—	—	—	—	—
Cardiomyopathies and Infections	1	86	—	—	1	86
Multiprogram Areas	76	20,661	6	541	87	22,184
Total	255	63,422	50	19,189	305	82,611
Blood Diseases						
Bleeding and Clotting Disorders	95	13,937	—	—	95	13,937
Red Blood Cell Disorders	59	5,936	—	—	59	5,936
Sickle Cell Disease	25	12 156	—	—	25	12,156
Blood Resources	7	2,348	—	—	7	2,348
Total	186	34,377	—	—	186	34,377
Lung Diseases						
Structure and Function	1	85	—	—	1	85
Chronic Obstructive Lung Diseases	9	999	—	—	9	999
Pediatric Pulmonary Diseases	4	649	4	437	8	1,086
Fibrotic and Immunologic Diseases	—	—	—	—	—	—
Pulmonary Vascular Diseases	—	—	—	—	—	—
Multiprogram Areas	1	654	—	—	1	654
Total	15	2,387	4	437	19	2,824
Total Grants and Contracts	456	$100,186	54	$19,626	510	$119,812
Intramural Research					26	$ 1,890
Total					536	$121,702

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Research Area	FY 1983 Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Total No.	Total Dollars
Heart Diseases						
Arteriosclerosis	25	$ 8,848	19	$16,518	44	$ 25,366
Hypertension	47	8,428	8	4,086	55	12,514
Cerebrovascular Disease	1	63	—	—	1	63
Coronary Heart Disease	36	20,418	2	249	38	20,667
Peripheral Vascular Disease	1	277	—	—	1	277
Arrhythmias	5	608	11	1,903	16	2,511
Heart Failure and Shock	2	168	—	—	2	168
Congenital and Rheumatic Heart Disease	1	30	—	—	1	30
Circulatory Assistance	—	—	—	—	—	—
Cardiomyopathies and Infections	1	107	—	—	1	107
Multiprogram Areas	69	15,522	9	3,008	78	18,530
Total	188	54,469	49	25,764	237	80,233
Blood Diseases						
Bleeding and Clotting Disorders	79	6,190	—	—	79	6,190
Red Blood Cell Disorders	51	7,455	—	—	51	7,455
Sickle Cell Disease	21	12,043	—	—	21	12,043
Blood Resources	4	2,425	—	—	4	2,425
Multiprogram Areas	7	8,690	—	—	7	8,690
Total	162	36,803	—	—	162	36,803
Lung Diseases						
Structure and Function	1	98	—	—	1	98
Chronic Obstructive Lung Diseases	12	1,905	—	—	12	1,905
Pediatric Pulmonary Diseases	3	353	—	—	3	353
Fibrotic and Immunologic Diseases	—	—	—	—	—	—
Pulmonary Vascular Diseases	2	143	—	—	2	143
Respiratory Failure	—	—	—	—	—	—
Multiprogram Areas	—	—	—	—	—	—
Total	18	2,499	—	—	18	2,499
Total Grants and Contracts	368	$93,771	49	$25,764	417	$119,535
(FY 1984 Total Grants & Contracts (estimated))		($98,900)		($27,000)		($125,900)
Intramural Research					27	$ 1,984
(FY 1984 Intramural Research (estimated))						($ 2,100)
Total					444	$121,519
(FY 1984 Total (estimated))						($128,000)

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National Institute on Aging (NIA)

- Prospective and followup studies examining the effects of smoking, drug and alcohol use, weight and weight change, exercise, and blood pressure on illness, hospitalization, and death among elderly people
- Identification of risk factors associated with osteoporosis, hip fractures, and falls
- Investigations of the effects of specific lifestyles, stress, technological and occupational change and other biopsychological behavioral, and societal factors on age-related changes in health and functioning
- Studies of the etiology and management of age-related endocrine disorders, such as noninsulin-dependent diabetes, to delay onset or minimize primary and secondary effects
- Investigation of the etiology, management, and natural history of age-related nervous system disorders, such as Alzheimer's disease, and the effects of aging on the nervous system
- Investigation of the fundamental basis and preventive and therapeutic effects of exercise, nutrition, and medication on the patophysiological correlates of aging
- Studies to delay, retard, or reverse age related deficits in taste, smell, hearing, sight, memory, and mobility that have relevance for accident prevention and occupational safety and health
- Research to determine ways to characterize and minimize the adverse effects of medication and alcohol use in the aged
- Research to understand the basis of age-related changes of the immune system and their relationship to the onset of diseases, and to develop methods to delay, retard, or reverse such changes
- Studies of the basis of skin aging and measures to delay or reverse its effects on skin structure and function
- Investigations related to the etiology, control, and effects of systolic hypertension in the elderly
- Studies related to late-life health and sickness among Hispanic and black populations
- Investigations of disease prevention issues in the context of Teaching Nursing Home Awards
- Investigations relevant to geriatric medicine such as incontinence, osteoporosis, infectious diseases, and benign prostatic hypertrophy in terms of prevention and treatment

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FY 1981

Grant Research	No.	Dollars
Biomedical Research and Clinical Medicine		
Immunology	10	$ 1,560
Pharmacology	11	775
Nutrition	5	560
Neuroscience	14	1,713
Endocrinology	10	802
Physiology	16	2,290
Total	66	7,700
Social and Behavioral Aging		
Social/Psychological Aging	6	632
Cognitive Aging	9	972
Aging and Social Structure	3	516
Biopsychological Aging	11	880
Total	29	3,000
Total Grants	95	$10,700

Intramural Research	No.	Dollars
Gerontology Research Center		
Physiology	6	$ 300
Neurosciences	2	126
Longitudinal Study	2	789
Behavior	4	553
Endocrinology	2	137
Nutrition	2	232
Pharmacology	4	163
Total Intramural	22	2,300
Contract Research		
Epidemiology, Demography, and Biometry	6	3,340
Grant Research Total*	95	10,700
Total	123	$16,340

* From previous column

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NIA (continued)

FY 1982

Grant Research	No.	Dollars
Biomedical Research and Clinical Medicine		
Immunology	22	$ 2,020
Pharmacology	7	641
Nutrition	6	624
Neuroscience	15	1,820
Endocrinology	12	1,481
Exercise Physiology	13	1,230
Geriatric Research	7	876
Geriatric Training	3	155
Cell Biology	6	608
Dermatology	3	287
Genetics	1	72
Total	95	9,814
Behavioral Sciences Research		
Cognitive and Biopsychological Aging	10	1,279
Social/Psychological Aging	7	836
Older People in a Changing Society	7	725
Total	24	2,840
Total Grants	119	$12,655

Intramural Research	No.	Dollars
Gerontology Research Center		
Physiology	7	$ 474
Neurosciences	2	101
Longitudinal Study	2	109
Behavior	4	473
Endocrinology	2	213
Nutrition	2	70
Pharmacology	4	225
Total	23	1,665
Epidemiology, Demography, and Biometry	10	63
Total Intramural	33	1,728

Contract Research	No.	Dollars
Epidemiology, Demography, and Biometry	9	2,740
Gerontology Research Center	2	527
Total Contracts	11	3,267
Grant Research Total*	119	12,655
Total	163	$17,650

* From previous column

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FY 1983

Grant Research	No.	Dollars
Biomedical Research and Clinical Medicine		
Immunology	18	$ 2,998
Pharmacology	9	1,063
Nutrition	12	1,120
Neuroscience	58	7,022
Endocrinology	23	2,553
Exercise Physiology	12	1,248
Geriatric Research	32	6,952
Geriatric Training	38	2,287
Dermatology	1	271
Animal Models	2	46
Total	205	25,561
Behavioral Sciences Research		
Cognitive and Biopsychological Aging	23	1,933
Social/Psychological Aging	28	3,093
Older People in a Changing Society	18	1,867
Total	69	6,894
Total Grants	274	$32,454
[FY 1984 Total Grants (estimated)]		[$41,499]

Intramural Research	No.	Dollars
Gerontology Research Center		
Physiology	17	$ 1,608
Neurosciences	6	2,148
Behavior	9	1,868
Endocrinology	1	693
Nutrition	1	171
Pharmacology	1	565
Total	35	7,054
Epidemiology, Demography, and Biometry	9	109
Total Intramural	44	7,163
[FY 1984 Total Intramural (estimated)]		[$8,469]
Contract Research		
Epidemiology, Demography, and Biometry	8	3,039
Gerontology Research Center	2	281
Behavioral Sciences Research	2	170
Office of the Director	1	3
Total Contracts	13	3,493
[FY 1984 Total Contracts (estimated)]		[$ 4,835]
Grant Research Total*	274	32,454
Total	331	$43,110
[FY 1984 Total (estimated)]		[$54,803]

* From previous column

National Institute of Allergy and Infectious Diseases (NIAID)

- Development of new and improved viral vaccines for diseases such as croup and pneumonia in infants, influenza, hepatitis, and viral diarrhea
- Development of new and improved bacterial vaccines for diseases such as meningitis, whooping cough, pneumonia, and streptococcal infections
- Development of vaccines for sexually transmitted diseases such as gonorrhea, genital herpes, and chlamydial infections
- Control of vectors of infectious diseases by biological means
- Screening techniques for prevention of severe allergic reactions
- Mechanisms for manipulation of the immune system to prevent allergic and other immunologic diseases

FY 1981

	Grants		Contracts		Intramural		Total	
Research Area	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Allergic Diseases	20	$ 2,917	—	$ —	—	$ —	20	$ 2,917
Bacterial Vaccines	53	6,009	13	1,842	8	2,177	74	10,028
Viral Vaccines	15	1.590	17	3,515	13	8,094	45	13,199
Prevention of Vector-Transmitted Diseases	57	6,136	—	—	5	668	62	6,804
Total	145	$16,652	30	$5,357	26	$10.939	201	$32,948

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Research Area	FY 1982 Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Intramural No.	Intramural Dollars	Total No.	Total Dollars
Allergic Diseases	23	$ 3,277	—	$ —	5	$ 1,725	28	$ 5,002
Bacterial Vaccines	54	6,265	7	1,324	9	2,225	70	9,814
Viral Vaccines	21	2,175	13	3,304	21	3,072	55	8,551
Prevention of Vector-Transmitted Diseases	52	4,952	—	—	11	2,026	63	6,978
Total	150	$16,669	20	$4,628	46	$ 9,048	216	$30,345

Research Area	FY 1983 Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Intramural No.	Intramural Dollars	Total No.	Total Dollars
Allergic Diseases	36	$ 4,256	—	$ —	5	$ 3,287	41	$ 7,543
Bacterial Vaccines	75	7,461	10	3,242	9	2,548	94	13,251
Viral Vaccines	54	5,472	11	3,926	20	3,165	85	12,563
Prevention of Vector-Transmitted Diseases	69	6,479	—	—	15	3,636	84	10,115
Total	234	$23,668	21	$7,168	49	$12,636	304	$43,472
[FY 1984 Total (estimated)]		[$26,547]		[$7,802]		[$13,303]		[$47,652]

National Institute of Arthritis, Diabetes, and Digestive and Kidney Diseases (NIADDK)

- Promotion of research in basic and clinical nutrition to advance knowledge about the functions and requirements of nutrients in the body, and the relationship between diet and nutrients to health and disease
- Investigation of the value of hormonal and dietary mineral supplementation in prevention of osteoporosis
- Studies aimed at prevention of benign prostatic hyperplasia, urolithiasis (kidney stones), and recurrence of urolithiasis
- Research geared toward gaining sufficient understanding of the mechanisms underlying the causative diseases of chronic renal failure to facilitate prevention
- Basic, clinical, and epidemiological studies of the etiology and pathology of arthritis, diabetes, musculoskeletal, skin, endocrinologic, metabolic, digestive, kidney, and hematologic diseases, with emphasis on causative, genetic, and environmental factors, studies to identify markers that characterize individuals predisposed to these disorders
- Investigation of the role of dietary behavior, satiety, and exercise in the development of obesity and the effectiveness of various treatments of obesity in preventing complications of obesity
- Studies of preventive aspects of obesity in relation to diabetes and arthritis
- Investigation of the effectiveness of intensive blood glucose control in patients with insulin-dependent diabetes mellitus in preventing or reversing its complications
- Research and development of insulin infusion pumps
- Studies on the prevention of recurrences of peptic ulcers and gallstones
- Research to discover a means for detecting carriers of cystic fibrosis and of other hereditary metabolic and blood diseases
- Research on new iron-chelating agents to help prevent fatal iron overload in patients treated with repeated blood transfusion for diseases such as Cooley's anemia

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	FY 1981							
	Grants		Contracts		Intramural		Total	
Research Area	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Arthritis	26	$ 2,349	1	$ 225	2	$ 261	29	$ 2,835
Musculoskeletal Diseases	5	589	1	25	—	—	6	614
Skin Diseases	1	509	—	—	—	—	1	509
Diabetes	22	3,119	1	109	4	977	27	4,205
Endocrinology	3	232	—	—	—	—	3	232
Metabolic Diseases	3	141	—	—	2	200	5	341
Digestive Diseases	9	797	1	188	1	75	11	1,060
Nutrition	64	5,829	1	101	—	—	65	5,930
Kidney Diseases	12	2,923	—	—	—	—	12	2,923
Hematology	10	749	—	—	1	40	11	789
Total	155	$17,237	5	$ 648	10	$1,553	170	$19,438

NIADDK (continued)

	FY 1982							
	Grants		Contracts		Intramural		Total	
Research Area	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Arthritis	57	$ 5,631	1	$ 429	5	$1,265	63	$ 7,325
Musculoskeletal Diseases	16	1,640	1	25	1	15	18	1,680
Skin Diseases	15	1,032	—	—	—	—	15	1,032
Diabetes	45	4,217	1	334	9	991	55	5,542
Endocrinology	8	570	—	—	1	57	9	627
Metabolic Diseases	15	1,345	—	—	7	571	22	1,916
Digestive Diseases	32	2,670	1	325	2	82	35	3,077
Nutrition	24	1,583	1	80	1	107	26	1,770
Kidney Diseases	33	3,539	—	—	—	—	33	3,539
Hematology	26	1,881	—	—	2	85	28	1,966
Total	271	$24,108	5	$1,193	28	$3,173	304	$28,474

	FY 1983							
	Grants		Contracts		Intramural		Total	
Research Area	**No.**	**Dollars**	**No.**	**Dollars**	**No.**	**Dollars**	**No.**	**Dollars**
Arthritis	113	$14,248	3½	$ 977	15	$4,101	131½	$19,326
Musculoskeletal Diseases	51	5,699	—	—	—	—	51	5,699
Skin Diseases	42	4,533	1	207	—	—	43	4,740
Diabetes	156	23,120	6½	2,002	19	3,829	181½	28,951
Endocrinology	18	2,012	—	—	10	2,635	28	4,647
Metabolic Diseases	31	3,513	—	—	11	3,323	42	6,836
Digestive Diseases	98	9,915	2	297	12	2,531	112	12,743
Nutrition	55	5,679	1	121	3	281	59	6,081
Kidney Diseases	93	13,404	—	—	—	—	93	13,404
Hematology	41	4,736	5	453	7	1,532	53	6,721
Total	698	$86,859	19	$4,057	77	$18,232	794	$109,148*
[FY 1984 Total (estimated)]	[698]	[$97,716]	[19]	[$4,564]	[77]	[$19,545]	[794]	[$121,825]

* Increase over FY 1981 and 1982 reflects a re-evaluation of reportable prevention efforts, which has resulted in an increase of funds reportable for prevention research

National Institute of Child Health and Human Development (NICHD)

- Prevention of death and disability associated with high-risk pregnancies
- Causes and prevention of prematurity and low birthweight
- Behavioral antecedents and prevention of habits harmful to health in childhood
- Metabolic, genetic, nutritional, and immunologic antecedents to disease and disability
- Effects of smoking, over-the-counter drug use, and other environmental substances on fetal development
- Causes, prevention, and amelioration of mental retardation
- Causes, prevention, and amelioration of birth defects
- Research on new and improved contraceptives and on contraceptive safety
- Determinants and consequences of adolescent childbearing
- Prevention of reproductive disorders

Research Area	FY 1981							
	Grants		Contracts		Intramural		Total	
	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Center for Population Research								
Social and Behavioral	131	$ 9,020	39	$ 3,073	—	$ —	170	$12,093
Contraceptive Evaluation	—	—	16	4,292	3	78	19	4,370
Reproductive Medicine	32	3,045	—	—	1	216	33	3,261
Contraceptive Development	—	—	42	8,254	—	—	42	8,254
Total	163	12,066	97	15,618	4	294	264	27,978
Center for Research for Mothers and Children								
Mental Retardation	296	14 682	1	79	1	846	298	15 607
Behavioral Pediatrics	10	757	—	—	—	—	10	757
Nutrition	106	8,594	—	—	5	1,049	111	9,643
Congenital Malformations	25	2,213	—	—	2	877	27	3,090
Neonatal Infection	—	—	—	—	—	—	—	—
High-Risk Pregnancy	85	8,174	7	1,049	—	—	92	9,223
Fetal Pathology	67	6,439	—	—	6	506	73	6,945
Prematurity	47	4,160	—	—	—	—	47	4,160
Disorders of the Newborn	52	4,490	5	279	8	365	65	5,134
Sudden Infant Death Syndrome	20	2,551	8	816	—	—	28	3,367
Total	708	52,060	21	2,223	22	3,643	751	57,926
Epidemiology and Biometry	—	—	—	—	20	806	20	806
Total	871	$64,126	118	$17,841	46	$4,743	1,035	$86,710

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NICHD (continued)

	FY 1982							
	Grants		Contracts		Intramural		Total	
Research Area	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Center for Population Research								
Social and Behavioral	76	$ 9,382	17	$ 2,466	–	$ –	93	$11,848
Contraceptive Evaluation	–	–	13	3,446	–	–	13	3,446
Reproductive Medicine	65	7,162	–	–	12	3,171	77	10,333
Contraceptive Development	–	–	35	8,432	–	–	35	8,432
Total	141	16,544	65	14,344	12	3,171	218	34,059
Center for Research for Mothers and Children								
Mental Retardation	273	15,857	1	60	5	1,659	279	17,576
Behavioral Pediatrics	12	1,531	–	–	1	56	13	1,587
Nutrition	98	8,454	9	1,081	4	241	111	9,776
Congenital Malformations	23	2,387	–	–	1	992	24	3,379
Neonatal Infection	14	904	-	–	–	–	14	904
High-Risk Pregnancy	60	7,594	5	797	2	73	67	8,464
Fetal Pathology	51	6,543	2	89	4	496	57	7,128
Prematurity	37	4,141	–	–	–	–	37	4,141
Disorders of the Newborn	32	3,614	1	35	8	462	41	4,111
Sudden Infant Death Syndrome	16	2,411	2	352	–	–	18	2,763
Total	616	53,435	20	2,414	25	3,979	661	59,828
Epidemiology and Biometry	–	–	–	–	23	1,071	23	1,071
Total	757	$69 979	85	$16,758	60	$8,221	902	$94,958

	FY 1983							
	Grants		Contracts		Intramural		Total	
Research Area	**No.**	**Dollars**	**No.**	**Dollars**	**No.**	**Dollars**	**No.**	**Dollars**
Center for Population Research								
Demographic and Behavioral Science	131	$ 9,897	13	$ 2,077	—	$ —	144	$11,973
Reproductive Medicine	68	6,791	—	—	11	3,030	79	9,821
Contraceptive Development	—	—	29	9,525	—	--	29	9,525
Contraceptive Evaluation	—	—	10	2,168	—	—	10	2,168
Total	199	16,687	52	13,770	11	3,030	262	33,487
Center for Research for Mothers and Children								
Mental Retardation	235	16,911	6	402	3	418	244	17,731
Behavioral Pediatrics	16	1,722	—	—	—	—	16	1,722
Nutrition	106	10,212	11	1,719	6	482	123	12,413
Congenital Malformations	25	3,022	—	—	3	1,264	28	4,286
Neonatal Infection	17	1,345	—	—	6	655	23	2,000
High-Risk Pregnancy	76	7,726	7	1,394	3	124	86	9,244
Fetal Pathology	55	5,804	1	72	5	590	61	6,466
Prematurity	39	4,014	5	645	—	—	44	4,659
Disorders of the Newborn	57	4,667	1	169	8	484	66	5,320
Sudden Infant Death Syndrome	12	1,196	3	584	—	—	15	1,780
Total	638	56,618	34	4,986	34	4,017	706	65 621
Epidemiology and Biometry	—	—	--	—	30	1,359	30	1,359
Total	837	$73,305	86	$18,756	75	$8,406	998	$100 467
(FY 1984 Total (estimated))		($79,965)		($20,000)		($9,235)		($109,200)

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National Institute of Dental Research (NIDR)

- Studies of antiplaque and antimicrobial compounds to prevent periodontal diseases and caries
- Development and testing of caries preventive measures, such as adhesive sealants and fluoride agents and vehicles
- Elucidation of the role of genetics and teratogens in congenital craniofacial anomalies
- Development of methods to diagnose early oral cancer lesions
- Development of vaccines against dental caries and herpes simplex virus
- Studies of adhesive bonding between tooth structure and composites
- Development of improved methods to assess the nutritional status of individuals
- Investigation of factors affecting the diffusion and adoption of effective oral disease prevention methods
- Development of strategies to influence acceptance and use of disease preventive methods

FY 1981

Research Area	Grants		Contracts		Intramural		Total	
	No.*	Dollars	No.*	Dollars	No.*	Dollars	No.*	Dollars
Dental Caries	60	$3,120	15	$1,464	45	$ 437	120	$5,021
Periodontal Diseases	8	526	1	63	9	461	18	1,050
Soft Tissue Stomatology and Nutrition	1	69	—	—	2	72	3	141
Restorative Materials	1	55	—	—	—	—	1	55
Total	70	$3,770	16	$1,527	56	$ 970	142	$6,267

* Project numbers may represent fractions of projects due to the NIDR multiple-coding scientific classification system

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FY 1982

Research Area	Grants		Contracts		Intramural		Total	
	No.*	Dollars	No.*	Dollars	No.*	Dollars	No.*	Dollars
Dental Caries	59	$2,827	9	$ 557	45	$ 616	113	$4,000
Periodontal Diseases	8	471	1	57	13	700	22	1,228
Soft Tissue Stomatology and Nutrition	8	682	—	—	9	724	17	1,406
Restorative Materials	1	66	—	—	—	—	1	66
Total	76	$4,046	11	$ 7[illegible]	67	$2,040	154	$6,878

FY 1983

Research Area	Grants		Contracts		Intramural		Total	
	No.*	Dollars	No.*	Dollars	No.*	Dollars	No.*	Dollars
Caries and Restorative Materials	142	$ 8,047	15	$1,516	70	$1,346	227	$10,909
Periodontal and Soft Tissue Diseases	45	3,468	2	31	13	786	60	4,285
Craniofacial Anomalies, Pain Control, and Behavioral Research	32	2,310	—	—	5	248	37	2,558
Total	219	$13,825	17	$1,547	88	$2,380	324	$17,752
(FY 1984 Total (estimated))		($14,793)		($1,655)		($2,547)		($18,995)

* Project numbers may represent fractions of projects due to the NIDR multiple coding scientific classification system

National Institute of Environmental Health Sciences (NIEHS)

- Coordination and expansion of government toxicologic testing programs
- Development of improved techniques for
  - — predicting the mutagenic, teratogenic, and carcinogenic hazards of chemicals to man
  - — detecting and quantifying low-level chronic effects of pollutants, including carcinogenesis, mutogenesis, teratogenesis, and organ toxicity
  - — understanding the mechanisms of toxicity of environmental and occupational pollutants and hazards
- Selected epidemiological studies such as case control of maternal exposure histories and effects on childhood development, reproductive toxicology, and the presence of mutagenic activity in biological fluids
- Investigation of the mechanisms of toxication and biological effects of numerous exposures to environmental substances, including asbestos, mercury, vinyl chloride, sulfuric acid mist, kepone, auto exhaust, pesticides, food additives, aerosols, nitroso compounds, and occupational chemicals
- Studies of physical factors, such as microwave radiation and noise

	FY 1981							
	Grants		Contracts		Intramural		Total	
Research Area	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Prediction, Detection, and Assessment of Environmentally Caused Diseases and Disorders	165	$16,594	31	$ 5,998	108	$ 6,828	304	$29,420
Mechanisms of Environmental Diseases and Disorders	147	11,048	11	2,626	55	3,518	213	17,192
Environmental Health Research and Manpower Development Resources	14	7,778	—	—	—	—	14	7,778
Total	326	$35,420	42	$ 8,624	163	$10,346	531	$54,390

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FY 1982

Research Area	Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Intramural (Dollars)	Total (Dollars)
Characterization of Environmental Health Hazards	99	$11,187	3	$ 76	$ 3,296	$ 14,559
Biological Response to Environmental Health Hazards	130	16,187	—	—	17,174	33,361
Applied Toxicological Research and Testing	—	—	31	9,938	6,571	16,509
Biometry and Risk Estimation	23	2,169	10	2,224	5,860	10,253
Resource and Manpower Development	11	7,900	—	—	—	7,900
Total	263	$37,443	44	$12,238	$32,901	$ 82,582

FY 1983

Research Area	Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Intramural (Dollars)	Total (Dollars)
Characterization of Environmental Health Hazards	123	$12,664	4	$ 209	$ 3,817	$ 16,690
Biological Response to Environmental Health Hazards	140	17,067	—	—	19,983	37,050
Applied Toxicological Research and Testing	14	2,083	122	49,686	12,670	64,439
Biometry and Risk Estimation	6	2,220	6	1,227	7,521	10,968
Resource and Manpower Development	10	9,650	—	—	—	9,650
Total	293	$43,684	132	$51,122	$43,991	$138,797
(FY 1984 Total (estimated))	(293)	($49,867)	(123)	($53,387)	($48,510)	($151,764)

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National Institute of General Medical Sciences (NIGMS)

- Investigation of hereditary factors that contribute to many major diseases
- Research into the cellular and molecular basis of disease
- Research to improve the safety and efficacy of drugs
- Prevention of death and disability due to injury, burns, shock, and trauma

	Grants*					
	FY 1981		FY 1982		1983	
Research Area	No.	Dollars	No.	Dollars	No.	Dollars
Genetics	5	$ 983	5	$ 699	4	$ 695
Pharmacological Sciences	9	1,109	3	244	4	283
Physiology and Biomedical Engineering						
Anesthesiology	8	697	1	58	—	—
Trauma and Burns	16	2,916	11	1,726	11	1,483
Total	38	$5,705	20	$2,727**	19	$2,461
[FY 1984 Total (estimated)]					[23]	[$2,960]

* NIGMS has no contract or intramural programs in prevention

** Decrease from FY 1981 due to iatrogenic (treatment-induced) diseases no longer being included as prevention research

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National Institute of Neurological and Communicative Disorders and Stroke (NINCDS)

- Prospective studies of elderly persons to identify the role of prior heart disease, hypertension, age, race, and lifestyle as risk factors for stroke
- Study of the role of improved blood glucose control in the prevention of the neurological complications of diabetes
- Prevention of posttraumatic epilepsy following severe head injury and prevention of spinal cord degeneration after acute back injury
- Basic, clinical, and epidemiological studies of Alzheimer's disease to explore cause, risk factors, pathogenesis, and impact in several U S population groups
- Basic, clinical, and epidemiological studies of Huntington's disease, multiple sclerosis, and amyotrophic lateral sclerosis to improve early detection and intervention measures
- Basic research on acquired immune deficiency syndrome (AIDS) to delineate etiology and the underlying mechanisms of attack on the immune system
- Development of a new method of measles immunization especially useful in very young children
- Basic and clinical studies of lipid storage diseases such as Tay Sachs and Gaucher's to detect carriers and correct enzyme deficiencies
- Research to prevent or alleviate noise-induced hearing loss
- Research on the neurobiology and prevention of autism
- Studies of the physiologic mechanisms of regulation of food intake to control obesity
- Clinical and laboratory studies of neurotoxicity associated with exposure to heavy metals for the development of adequate screening tests
- Development of a tissue culture test for the rapid detection of active genital herpes infection in pregnant women
- Studies to determine whether preschool language impairment is a precursor of dyslexia and which communicative skills are effective during early school years in preventing such outcome

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NINCDS (continued)

FY 1981

Research Area	Grants		Contracts		Intramural		Total	
	No.	Dollars	No.	Dollars	No.	Dollars	No.	Dollars
Neurological Disorders Program	83	$ 9,451	5	$1,497	8	$ 258	96	$11,206
Communicative Disorders Program	34	5,015	1	45	1	10	36	5,070
Fundamental Neurosciences Program	6	427	1	54	—	—	7	481
Stroke and Trauma Program	27	2,818	4	850	—	—	31	3,668
Office of Biometry and Epidemiology	—	—	5	290	8	80	13	370
Intramural Research Program	—	—	1	83	68	6,786	69	6,869
Total	150	$17,711	17	$2,819	85	$7,134	252	$27,664

FY 1982

Research Area	Grants		Contracts		Intramural		Total	
	No.	Dollars	No	Dollars	No.	Dollars	No.	Dollars
Neurological Disorders Program	113	$13,219	8	$2,192	8	$ 296	129	$15,707
Communicative Disorders Program	35	5,403	1	42	1	12	37	5,457
Fundamental Neurosciences Program	6	460	1	50	—	—	7	510
Stroke and Trauma Program	—	—	—	—	—	—	—	—
Office of Biometry and Epidemiology	—	—	4	271	9	92	13	363
Intramural Research Program	—	—	1	78	74	7,792	75	7,870
Total	154	$19,082	15	$2,633	92	$8,192	261	$29,907

Research Area	FY 1983 Grants No.	Grants Dollars	Contracts No.	Contracts Dollars	Intramural No.	Intramural Dollars	Total No.	Total Dollars
Neurological Disorders Program	212	$22,494	9	$2,477	10	$ 111	231	$25,082
Communicative Disorders Program	84	6,600	1	50	25	43	110	6,693
Fundamental Neurosciences Program	6	168	1	54	—	—	7	222
Intramural Research Program	—	—	5	366	52	4,820	57	5,186
Total	302	$29,262	16	$2,947	87	$4,974	405	$37,183
[FY 1983 Total (estimated)]	[312]	[$33,871]	[17]	[$3,193]	[89]	[$5,353]	[418]	[$42,417]

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Division of Research Resources (DRR)

- Pregnancy and infant care among adolescents, low-income persons, and those at risk of genetic diseases; genetic counseling and followup of high-risk neonates; developmental disability screening of preschool and elementary school children
- Use of improved methods and techniques, especially urine, blood, and chromosomal analyses, for early detection and diagnosis
- Susceptibility of infection and vaccine efficacy evaluation
- Risk factors related to coronary heart disease, such as hyperlipoproteinemia, cholesterol, hypertension, salt consumption, and stress studied in conjunction with nutritional habits
- Toxic agent surveillance to determine the biological effects of toxic substances and methodologies to identify and detect them; studies of threshold differences to radiation, drugs, and chemicals
- Caries prevention research using topical fluoride agents, mouth rinsing, and quantification of fluoride update in the enamel
- Smoking cessation strategies in children, adolescents, and adults; social influence variables and network analysis of friendship ties as an approach to prevention
- Reliability of a toxic screen in drug overdose and behavioral approaches to promote responsible drinking
- Research to improve the nutritional status of mothers, infants, and pregnant teenagers; effects of diet, including dietary ratios of fat, carbohydrates, and proteins on health status
- Effects of exercise and stress on various health indicators in the elderly

DRR* Program	FY 1981	FY 1982	FY 1983
	Grant Dollars**		
Animal Resources Program	$ 1,331	$ 2,256	$ 3,007
Biotechnology Resources Program	746	79	173
Biomedical Research Support Program	1,914	1,626	4,979
General Clinical Research Centers Program	4,899	9,663	18,193
Minority Biomedical Support Program	1,196	1,047	2,177
Total	$10,086	$14,670	$28,529
[FY 1984 Total (estimated)]			[$31,572]

Research Category	FY 1981	FY 1982	FY 1983
	Dollars		
Preventive Services, Research and Data			
Family Planning	$ 39	$ 107	$ 524
Pregnancy and Infant Care	1,807	2,237	3,019
Immunizations	320	887	843
Sexually Transmitted Diseases	33	—	3
High Blood Pressure Control	334	1,269	1,201
Health Protection, Research and Data			
Toxic Agent Control	828	361	1,059
Occupational Safety and Health	164	97	111
Accident Injury Control	11	9	32
Fluoridation of Water Supplies	31	40	57
Infectious Agent Control	500	640	992
Health Promotion, Research and Data			
Smoking Cessation	112	103	208
Alcohol and Drug Misuse	167	636	697
Improved Nutrition	2,934	6,126	13,950
Exercise and Fitness	207	268	661
Stress Control	163	338	963
Cross-Cutting Issues	2,436	1,552	4,211
Total	$10,086	$14,670	$28,529

* Prevention funding is categorized in the first chart according to DRR programs, and in the second chart according to cross cutting research categories

** DRR has no research contract or intramural programs in prevention

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Fogarty International Center (FIC)

- Collaborative research training by established senior biomedical investigators who conduct studies with foreign counterparts and by foreign postdoctoral biomedical scientists who work with distinguished U S. scientists

- Advanced studies including Fogarty Scholars-in-Residence, international components of grants for support of scientific meetings (on such topics as viral hepatitis, environmental mutagens, influenza viruses, biology of the interferon system, and genetics of insect disease vectors), and the conduct of special issues studies (on such topics as the control of poliomyelitis)

- Through the Gorgas Memorial Laboratory, Panama, resear : and research training on the p. evention of tropical and other diseases of concern to the United States and Central America, particularly viral and parasitic diseases

	FY 1981					
	Grants		Intramural		Total	
Research Area	No.	Dollars	No.	Dollars	No.	Dollars
Fellowships for Collaborative Research	47	$1,173	—	$ —	47	$1,173
Advanced Studies						
Scholars-in-Residence	7	366	—	—	7	366
Scientific Meetings*	3	35	11	155	14	190
International Iss es	—	—	5	15	5	15
Gorgas Memorial Institute**	1	1,700	—	—	1	1,700
Total	†	$3,274	16	$170	†	$3,444

* FIC provides partial support, along with other BIDs, for the international component of conference grants

** Core support for the Gorgas Memorial Laboratory (Panama) of the Gorgas Memorial Institute is provided through the annual appropriation to FIC

† Because of the varied nature of these activities, a total number would be an inappropriate agg egate

FY 1982

Research Area	Grants No.	Grants Dollars	Intramural No.	Intramural Dollars	Total No.	Total Dollars
Fellowships for Collaborative Research	29	$ 889	—	$ —	29	$ 889
Advanced Studies						
Scholars-in-Residence	9	461	—	—	9	461
Scientific Meetings*	25	149	—	—	25	149
International Issues	—	—	7	143	7	143
Gorgas Memorial Institute**	1	1,607	—	—	1	1,607
Total	†	$3,106	7	$143	†	$3,249

FY 1983

Research Area	Grants No.	Grants Dollars	Intramural No.	Intramural Dollars	Total No.	Total Dollars
Fellowships for Collaborative Research	106	$2,259	—	$ —	106	$2,259
Advanced Studies						
Scholars in-Residence	6	517	—	—	6	517
Scientific Meetings*	29	157	—	—	29	157
International Issues	—	—	4	302	4	302
Gorgas Memorial Institute**	1	1,710	—	—	1	1,710
Total	†	$4,643	4	$302	†	$4,945
(FY 1984 Total (estimated))		($4,904)		($586)		($5,490)

* FIC provides partial support along with other BIDs for the international component of conference grants

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