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ABSTRACT

A transcript of the hearing that discusses the findings and recommendations that are part of the Office of Technology Assessment's (OTA's) report on federally funded research is presented. The report evaluates the nature and distribution of research funding and decision-making and suggests alternative approaches that the Federal Government can take in funding research. The witnesses include: (1) Dr. Daryl Chubin (Program Director, Education and Transportation Programs, OTA); (2) Dr. Roland Schmitt (President, Rensselaer Polytechnic Institute); (3) Dr. Leon Lederman (President, American Association for the Advancement of Science--AAAS); (4) Dr. Rustum Roy (Evan Pugh Professor of the Solid State, Pennsylvania State University); and (5) Dr. Douglas A. Lauffenburger (Alumni Professor of Chemical Engineering, University of Illinois). (KR)

OTA REPORT, "FEDERALLY FUNDED RESEARCH: DECISIONS FOR A DECADE"

HEARING

BEFORE THE

SUBCOMMITTEE ON SCIENCE

OF THE

COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY

U.S. HOUSE OF REPRESENTATIVES

ONE HUNDRED SECOND CONGRESS

FIRST SESSION

MARCH 20, 1991

[No. 7]

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*Ranking Republican Member.

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WITNESSES

OTA REPORT, "FEDERALLY FUNDED RESEARCH: DECISIONS FOR A DECADE"

WEDNESDAY, MARCH 20, 1991

U.S. HOUSE OF REPRESENTATIVES,
COMMITTEE ON SCIENCE, SPACE, AND TECHNOLOGY,
SUBCOMMITTEE ON SCIENCE,
Washington, D.C.

The subcommittee met, pursuant to notice, at 9:37 a.m. in room 2318, Rayburn House Office Building, Hon. Rick Boucher [chairman of the subcommittee] presiding.

Mr. BOUCHER. The subcommittee will come to order.

In the closing days of World War II, President Roosevelt sought ways to turn a technologically formidable United States war effort to peacetime ends. During the war, the Office of Scientific Research and Development had been created to capitalize on recent scientific discoveries such as radar and penicillin.

The President commissioned the Director of the Office of Scientific Research and Development, Dr. Vannevar Bush, to report to him on the best means to harness scientific and technological knowledge to drive an economy at peace.

In his letter to Dr. Bush, the President wrote, "The research experience developed by the Office of Scientific Research and Development and by the thousands of scientists in the universities and in private industry, should be used in the days of peace ahead for the improvement of the national health, the creation of new enterprises bringing new jobs, and the betterment of the national standard of living. New frontiers of the mind are before us, and if they are pioneered with the same vision, boldness and drive with which we have waged this war, we can create a fuller and more fruitful employment and a fuller and more fruitful life."

The report that ensued, entitled "Science—the Endless Frontier," embodied Dr. Bush's vision of science and engineering as the pillars of a technologically advanced society, and became the blueprint for our present Federal research enterprise. The priorities set by President Roosevelt in his commission to Dr. Bush—national defense, public health, and Government support of research at universities and private organizations—are still recognizable today as the guiding principles of our national science policy.

The success of our national research system, established now just over 40 years ago, is today manifest. The United States supports a scientific enterprise whose excellence and diversity is without peer. We have uniquely tied scientific inquiry to education through broad-based Federal support of research at universities, attracting eminent scientists and promising students from around the world.

Yet, as our system now enters its fifth decade, it is beginning to show some signs of strain. Part of that strain is the product of the system's success: in spite of record high funding levels, even when adjusted for inflation, there are today more well-qualified researchers and more worthwhile projects than we as a Nation can afford to fund.

Part of that strain is also caused by the need to adjust priorities to better reflect the Nation that we have become at the end of the 20th century. And, part of the strain is caused by tensions within the system itself: big science versus little science; support for research projects versus support for research facilities; and support for established investigators versus support for young scientists. These are only a few of the issues with which we as policymakers are charged with grappling.

One conclusion, I think, is inescapable: no matter what level of research funding we are able to achieve in the coming years, we are going to have to continue to address the overall national goals, scientific priorities, research infrastructure, and management concerns.

To assist this committee in that task, we have asked the Office of Technology Assessment to report on the health of the research system. OTA has completed a summary report entitled, "Federally Funded Research: Decisions for a Decade," and today will release the summary and recommendations of that report at this hearing.

Following the OTA representatives, we will hear from a distinguished panel of scientists and engineers who will comment on the OTA findings, on priority setting, on research expenditures, education of the research work force, and ongoing analysis of the research system.

We will welcome our panel of witnesses momentarily. Before doing that, I would like to call on the Ranking Republican Member of the Subcommittee, the gentleman from California, Mr. Packard.

Mr. PACKARD. Thank you, Mr. Chairman.

I would like to welcome all of the witnesses who have come to testify today. We are particularly interested in the second panel who will provide an assessment of the report.

The United States currently maintains one of the finest research communities in the world. Six major Federal agencies — the NIH, NSF, DOE, DOE, NASA and the USDA—support more than 90 percent of the university basic and applied research. This funding has provided the American university system with the financial base from which to make advances in scientific knowledge which, in turn, spurs economic productivity and international competitiveness.

In order for the American research community to grow and continue to tackle complex scientific problems, we must assess the current federally-funded system to pinpoint weaknesses so that we as a Nation will be able to embrace the challenges that we will be faced with in the 1990s. That is why this OTA report is so important. It evaluates the nature and distribution of research funding and decision-making and suggests alternative approaches that the Federal Government can take in funding research.

One of the most significant points made in the OTA report was the fact that there will always be more opportunity than can be funded. There will always be more researchers competing than we can sustain and there will also always be more institutions seeking to expand their programs than the prime sponsor, the Federal Government, can fund.

I agree with the objectives listed in the OTA report. We must insure that funding remains available for the best research; that a full portfolio of research is maintained and that there is a highly competent work force to do the job.

Again, I want to thank you, Mr. Chairman, for scheduling this important hearing, and I certainly look forward to the information that will come out of the hearing through our witnesses.

Mr. BOUCHER. The Chair thanks the gentleman and welcomes now our first panel of witnesses from the Office of Technology Assessment to discuss the findings and the recommendations that are a part of the OTA report.

We welcome Dr. John Andelin, the Assistant Director for Science, Information and Natural Resources; Dr. Daryl Chubin, Project Director; Ms. Nancy Carson, Manager, Science, Education and Transportation Program for OTA; and Dr. Elizabeth Robinson, an Analyst, Science, Education and Transportation Program. We welcome all of you here today.

We will, without objection, make your prepared statements a part of the record. We have a 5 minute rule in terms of oral summaries of the prepared statements and would ask that you adhere to that.

Dr. Chubin, if you would like to make a presentation, we'd be happy to hear from you.

STATEMENT OF DR. DARYL CHUBIN, PROJECT DIRECTOR, SCIENCE, EDUCATION AND TRANSPORTATION PROGRAM, OFFICE OF TECHNOLOGY ASSESSMENT; ACCOMPANIED BY: DR. JOHN ANDELIN, ASSISTANT DIRECTOR, SCIENCE INFORMATION AND NATURAL RESOURCES DIVISION; NANCY CARSON, MANAGER, SCIENCE, EDUCATION AND TRANSPORTATION PROGRAM; AND DR. ELIZABETH ROBINSON, ANALYST, SCIENCE, EDUCATION AND TRANSPORTATION PROGRAM

Dr. CHUBIN. Thank you, Mr. Chairman. Good morning. Good morning, Mr. Packard. Thank you for the opportunity to appear before you today. I will summarize my written statement.

Sixteen months ago, this committee asked the Congressional Office of Technology Assessment to study the federally-funded research system, the state of information used to characterize it, and the challenges that system faces in the 1990s. A summary of the report, "Federally Funded Research: Decisions for a Decade," is being released today. The full report will be issued late next month.

Throughout this study, we have worked with the committee to assure a thorough examination of federally-funded research and to address the committee's special role in supporting and overseeing it.

The summary offers a broad brush look at the goals and processes that undergird Federal funding decisions and the issues that Congress must grapple with in the decade ahead. The full report, in contrast, looks in-depth at a number of individual issues and we hope that it will be useful to the committee over time.

Our testimony today thus only begins the transfer of information to the committee. We look forward to being of continuing assistance.

Recently, increasing calls for more funding for research have been heard from the scientific community. Similar calls have been heard throughout the history of Federal sponsorship of research and undoubtedly some of the witnesses to follow me will echo those concerns.

These calls reflect the tremendous excitement that we all share in the findings that result from scientific research. How much is enough for research, however, depends on the goals of the Federal Government in funding it. If the goal is to fund every good idea, then the demand for funding could be without limit. Our research system is so robust that it can produce more good ideas than can be funded.

If the goal is to produce a strong work force skilled in science and engineering, then the requirements for research funding may be closer to the educational pipeline, grade school to grad school. In practice, however, funding the best ideas, producing a skilled work force, contributing to economic competitiveness and other goals are all part of the research system. Once goals and needs have been identified, choices can be made.

The research system is feeling the stress of internal competition for funds and of the demands placed upon it by pressing national needs such as a search for a cure for AIDS. While the Federal funding of research has increased in constant 1990 dollars from $8 billion in 1960 to over $21 billion in 1990, the number of researchers has grown steadily, more than doubling during this same period.

OTA finds that stress is a natural part of the competitive research system and we question whether researcher stress per se indicates problems in the research enterprise. There is also debate over whether additional funds would relieve the stresses presently felt by researchers. OTA finds that additional funding would indeed allow the pursuit of more scientific opportunities and yield fruitful gains. It would also enlarge the system, create more deserving competitors for support, and increase future demands for funding.

The symptoms of stress that we hear and see—for example, investigators having to compete harder for funds while young researchers find it more difficult to launch their careers, would persist.

The Nation's academic system has the capacity to train many new researchers and tackle many new problems, but OTA is not here to advise you on what is an appropriate funding level for academic or basic research. Rather, we seek to provide a balanced prospective on this vibrant, pluralistic enterprise, and on the activities inside and outside of Government that make it so successful.

Regardless of the level of funding, OTA has identified four issues that need to be addressed and that are central to producing a stronger research system.

The first issue is to increase attention to setting priorities in federally-funded research. Research priorities are currently set throughout the Federal Government at many levels. However, these efforts fall short in three ways.

One, criteria used in selecting various areas of research and megaprojects are not made explicit and appear to vary widely from area to area. Two, there is currently no formal or explicit mechanism for evaluating the total research portfolio of the Federal Government in terms of national objectives. Three, the development of human resources and of regional and institutional capacity must be taken into account. These criteria build future research capability without compromising the quality of today's research. While not every project or agency will or should attend to these criteria equally, the total Federal research portfolio should explicitly reflect these concerns.

Priority-setting mechanisms that cut across research fields and agencies and that make selection criteria more transparent must be strengthened. Congress should insist, at a minimum, that the Executive Branch present and compare the criteria or the rationale underlying budget choices. Other criteria may be considered and comparisons made in congressional decisions.

Also, since megaproject costs certainly affect the initiation of new projects within an agency's budget, and perhaps those of other agencies as well, megaprojects are chief candidates for cross-cutting priority setting.

A second issue is to understand research expenditures. Many in the research community claim that increases in the costs of doing research exceed increases in Federal funding. However, the numerous and sometimes inconsistent meanings of cost and the lack of a suitable measure of research make this claim all but impossible to evaluate.

Specific research activities generally become cheaper to complete with time due to increasing productivity, for example, of computers and other technologies. However, advances in technology and knowledge also allow deeper probing of more complex scientific problems and create demand for greater resources.

Because success in the research environment depends heavily on getting there first, there is great advantage to having the financial support to acquire additional staff and cutting edge technology. Thus apart from the intrinsic joy of research, competition drives up demand for funding. In this sense, the cost of research will continue to match or outpace any increases in Federal funding.

On a less philosophical note, greater cost accountability could be encouraged. In particular, the Federal Government should seek to eliminate the confusion around allowable indirect costs, a topic of special concern these days to research universities, and develop better estimates of future expenditures, especially for megaprojects where final costs tend to be well above initial estimates.

A third issue of Congressional concern addresses education and human resources for the research work force. Recent projections of shortages for Ph.D. researchers in the mid-1990s have spurred urgent calls to increase Ph.D. production in the United States. OTA believes that the likelihood of these projections being realized is overstated and that these projections are poor grounds on which to base public policy.

In both this and previous OTA work, however, OTA has indicated the value to the Nation, regardless of employment prospects in the research sector, of expanding the number and diversity of such students in the educational pipeline, precollege through undergraduate, and preparing graduate students for career paths in or outside of research.

Participation in science and engineering at all levels can be enhanced if the opportunities and motivations of presently underparticipating groups, such as women and United States minorities, are confronted. Both set aside and mainstream programs could help to address these issues.

Not just the number of scientists and engineers, or their characteristics is at issue. Research in many fields of science and engineering is also moving toward a more industrial model, with larger and often multidisciplinary teams, specialized responsibilities, and the sharing of infrastructure.

The Federal Government has acknowledged these changes with funding for centers and through block grants. Perhaps it should also encourage universities to provide opportunities and rewards for young investigators and nontenure track researchers.

A final issue of congressional concern is to refine the data collected on the research system. Better information is needed to inform congressional decision-making. While data collected on the health of the research system in some areas are extensive, in other areas data are scarce. In addition, most of the research agencies, with the exception of NSF and NIH, devote few resources to internal collection.

Without comprehensive and relevant information, Congress cannot make well-informed decisions. OTA suggests additional information that could be collected for different levels of decision-making and accountability.

How then can the Nation meet these four challenges? As we look beyond this year's budget to the research opportunities that will emerge throughout the 1990s, OTA believes that Congress, the Executive Branch and research performers must begin to converge. Congressional hearings, legislation and oversight should address cross-cutting and within agency priority-setting, cost accountability, and the state of data on the research system.

This committee could take the lead for such hearings. Similar efforts should be initiated in the Executive Branch, especially OSTP, OMB and the research agencies. Not all of these problems, however, can be addressed by the Federal Government. Many policies are dictated by the practices of universities and laboratories, both Federal and industrial, especially in the areas of cost containment and expanding the educational pipeline.

This committee's leadership in overseeing programs at NSF has fortified the connections among research, education and human resources, and represents a foundation on which to increase these efforts.

In the decade ahead, the Federal Government must make tough choices in guiding the research system, even beyond issues of merit and constricted budgets. How do today's objectives and funding commitments bear on the Nation's future capability to do research?

OTA concludes that sustaining the research system will require more than funding. It will require new ways to manage the diversity and creativity that have distinguished United States contributions to scientific research.

Thank you, Mr. Chairman. My colleagues and I would be pleased to respond to the committee's questions.

[The prepared statement of Dr. Chubin follows:]

FEDERALLY FUNDED RESEARCH: DECISIONS FOR A DECADE

STATEMENT OF DR. DARYL E. CHUBIN
Project Director

Office of Technology Assessment
Congress of the United States

Testimony Before
Subcommittee on Science
Committee on Science, Space and Technology
U.S. House of Representatives

March 20, 1991

FEDERALLY FUNDED RESEARCH: DECISIONS FOR A DECADE

Statement of Dr. Daryl E. Chubin
Project Director
Office of Technology Assessment
U.S. Congress

Before the Subcommittee on Science
Committee on Science, Space, and Technology
U.S. House of Representatives

Mr. Chairman, 18 months ago, this Committee asked the congressional Office of Technology Assessment to study the federally funded research system, the state of information used to characterize it, and the challenges that system faces in the 1990s. The summary of the report, Federally Funded Research: Decisions for a Decade, is being released today. The full report will be issued late next month.

Throughout this study, we have worked with the Committee to assure a thorough examination of federally funded research and to address the Committee's special role in supporting and overseeing it. The summary offers a broad-brush look at the goals and processes that undergird Federal research funding decisions and the issues that Congress must grapple with in the decade ahead. The full report, in contrast, looks in depth at a number of individual issues, and we hope that it will be useful to the Committee over time. Our testimony today only begins a transfer of information to the Committee. We look forward to continuing to assist the Committee in this and related endeavors.

In preparing this report, in addition to analyzing various documents and data (which are summarized in attachment 1), we talked with many participants in the research system. We interviewed more than 125 staff of the six major research agencies (who were most cooperative and enlightening); we consulted with numerous researchers and research administrators--indeed our advisory panel was composed primarily of working scientists and engineers; and we conducted several site visits of research universities and Federal laboratories. Talking with all of these people helped sensitize us even further (since most of the staff on this project have been working scientists) to the complexity and the multitude of perspectives on the research system. With the release of this summary, we are expanding the dialogue to include the Committee, the executive branch, and the scientific community on several overarching concerns.

Recently, increasing calls for more funding for research have been heard from the s... ntific community. (For a history of Federal funding, see attachment 2.) Similar calls have been heard throughout the history of Federal sponsorship of research and, undoubtedly, some of the witnesses to follow me will echo these concerns. These calls reflect the tremendous excitement that we all share in the findings that result from scientific research.

"How much is enough" for research, however, depends on the goals of the Federal Government in funding research. If the goal is to fund every good idea, then the demand for funding could be without limit; our research system is so robust that it can produce more good ideas than can be funded. If the goal is to produce a strong work force skilled in science and engineering, then the requirements for research funding may be linked closer to the educational pipeline--grade school to grad school. In practice, however, funding the best ideas, producing a skilled work force, contributing to economic competitiveness, and other goals are *all* part of the research system. Once goals and needs have been identified, choices can be made and funding levels set.

The research system is feeling the stress of internal competition for funds and of the demands placed upon it by pressing national needs, such as the search for a cure for AIDS. While the Federal funding of research has increased in constant 1990 dollars from $8 billion in 1960 to over $21 billion in 1990, the number of researchers has grown steadily, at least doubling during the same period. OTA finds that stress is a natural part of the competitive research system, and we question whether researcher stress, per se, indicates problems in the research enterprise. There is also debate over whether additional funds would relieve the stresses presently felt by researchers. (For a summary of tensions in the research system, see attachment 3.)

OTA finds that additional funding would allow the pursuit of more scientific opportunities and yield fruitful gains; it would also enlarge the system, create more deserving competitors for support, and increase future demands for funding. The Nation's academic research system has the capacity to train many new researchers and to tackle many new problems. The symptoms of stress that we hear and see--for example, investigators having to compete harder for funds while young researchers find it more difficult to launch their careers--would persist.

OTA, thus, is not here to advise you on what is an "appropriate" funding level for "academic" or "basic" research. Rather, we seek to provide a balanced perspective on this vibrant, pluralistic enterprise, and on the activities inside and outside of government that make it so successful. **Regardless of the level of funding, OTA has identified four issues that need to be addressed and that are central to producing a stronger research system: setting priorities, understanding costs, developing human resources, and refining data on the system.** (See attachment 4.)

The first issue is to increase attention to setting priorities in federally funded research. Research priorities are currently set throughout the Federal Government at many levels in the congressional and executive branches. However, these efforts fall short in three ways: 1) Criteria and the "decision rules" used in selecting various areas of research and megaprojects are not made explicit, and appear to vary widely from area to area. This is a problem in the President's budget and many parts of the congressional decision process. 2) There is currently no formal or explicit mechanism for evaluating the total research portfolio of the Federal Government in terms of national objectives, such as a strong science base. 3) The development of human resources and regional and institutional capacity must be taken into account. These criteria strengthen future research capability without compromising the quality of today's research. This is now done in some agency programs, but is not widespread. While not every project or agency will or should attend to these criteria equally, the *total* Federal research portfolio should explicitly reflect these concerns.

Priority-setting mechanisms that cut across research fields and agencies, and that make selection criteria more transparent, must be strengthened in both Congress and the executive branch. Congress should insist, at a minimum, that the executive branch present and compare the criteria or rationale underlying budget choices. Other criteria may be considered and comparisons made in congressional decisions. Also, since megaproject costs certainly affect the initiation of new projects within an agency's budget (and perhaps those of other agencies), megaprojects, like the Superconducting Super Collider and the Earth Observing System, are chief candidates for crosscutting priority setting.

A second issue is to understand research expenditures. Many in the research community claim that increases in the "costs of doing research" exceed increases in Federal research funding. However, the numerous and sometimes inconsistent meanings of "costs," and the lack of a suitable measure of "research," make this claim all but impossible to evaluate.

Specific research activities generally become cheaper to complete with time, due to increasing productivity, for example, of computers and other technologies. However, advances in technology and knowledge also allow deeper probing of more complex scientific problems and create demand for greater resources. Because success in the research environment depends heavily on "getting there first," there is clear advantage to having the financial support to acquire additional staff and cutting-edge technology. Thus, apart from the intrinsic joy of research, competition drives up demand for funding. **In this sense, the "costs of research" will continue to match or outpace any increases in Federal funding.**

On a less philosophical note, greater cost-accountability could be encouraged by the executive branch and Congress. In particular, the Federal Government should seek to eliminate the confusion around allowable indirect costs (a visible topic these days of special concern to research universities), and develop better estimates of future expenditures, especially for megaprojects where final costs tend to be well above initial estimates. (For cost scenarios of the science base and select megaprojects, see attachment 5.)

A third issue of congressional concern addresses education and human resources for the research work force. (For a summary of the number of Ph.D. scientists and engineers in academia, and degrees awarded--BSs, Masters, and Ph.D.s--see attachment 6.) Recent projections of shortages for Ph D. researchers in the mid-1990s have spurred urgent calls to increase Ph.D. production in the United States. OTA believes that the likelihood of these projections being realized is overstated, and that these projections are poor grounds on which to base public policy. **In both this and previous OTA work, however, OTA has indicated the value to the Nation--regardless of employment opportunities in the research sector--of expanding the number and diversity of students in the educational pipeline (K-12 through undergraduate), and preparing graduate students for career paths in or outside of research.**

Participation in science and engineering at all levels can be enhanced if the opportunities and motivations of presently underparticipating groups, such as women and U.S. minorities, are confronted. Both "set-aside" and mainstream programs could help to address these issues.

Not just the number of scientists and engineers, or their characteristics, is at issue. Research in many fields of science and engineering is also moving toward a more "industrial" model, with team efforts, specialized responsibilities within research groups, and the sharing of infrastructure. The Federal Government has acknowledged these changes with funding for centers and through block grants. Perhaps it should also provide incentives for universities to experiment with policies concerning the opportunities and rewards for young investigators, postdoctorates, and nontenure track researchers.

A final issue of congressional concern is to refine the data collected on the research system. Better data are needed to inform congressional decisionmaking. While data collected on the health of the research system in some areas are extensive, in other areas, data are scarce. In addition, most of the research agencies, with the exception of the National Science Foundation and the National Institutes of Health, devote few resources to internal data collection. Without comprehensive and relevant information, Congress cannot make well-informed decisions.

OTA suggests additional information that could be collected for different levels of decisionmaking and accountability, concentrating on areas of policy relevance for Congress and the executive branch. (See attachment 7.) Refined inhouse and extramural data collection, analysis, and interpretation would be instructive for decisionmaking and managing research performance in the 1990s.

How can the Nation meet these four challenges? Congressional hearings, legislation, and oversight should address:

- crosscutting and within-agency priority setting (with emphasis on criteria to expand the future capabilities of the research system, such as strengthening education and human resources) in the six major agencies that fund research (the Department of Health and Human Services, Department of Defense, National Aeronautics and Space Administration, Department of Energy, National Science Foundation, and Department of Agriculture);
- cost-accountability efforts throughout the research system; and
- the state of data on the research system to inform decisionmaking.

This Committee could take the lead for such hearings.

Similar efforts should be initiated in the executive branch (especially the Office of Science and Technology Policy, the Office of Management and Budget, and the research agencies). Not all of these problems, however, can be addressed by the Federal Government. Many policies are dictated by the practices within universities and laboratories (both Federal and industrial), especially in the areas of cost-containment and expanding the educational pipeline. This Committee's leadership in overseeing programs at NSF has strengthened the connections among research, education, and human resources, and represents a foundation on which to increase these efforts. OTA believes that Congress, the executive branch, and the research performers must converge on these challenges. (See attachment 8.)

In the decade ahead, the Federal Government must make tough choices, in guiding the research system, even beyond issues of merit and constricted budgets. How do today's objectives and funding commitments bear on the Nation's future capability to do research? OTA concludes that sustaining the research system will require more than funding. It will require new ways to manage the diversity and creativity that have distinguished U.S. contributions to scientific knowledge.

Attachment 1

Summary of OTA Data Collection and Analysis on Federally Funded Research

Original data collection and analysis:

Description	Collection	Subject
Federal Agency Analysis	Interviews, site visits, & document review	Priority setting & funding allocation
University Case Studies	Interviews & site visits	Research costs & responsiveness to changing priorities
Bibliometrics	Citation Analyses	"Hot" fields, related fields, university comparisons, & other indicators
Analysis of SEI	Interviews & document review	Evolution of SEI volumes, data presentation, & future analysis
Researchers' Views	Surveys	Sigma XI members' perceptions of Federal research funding issues

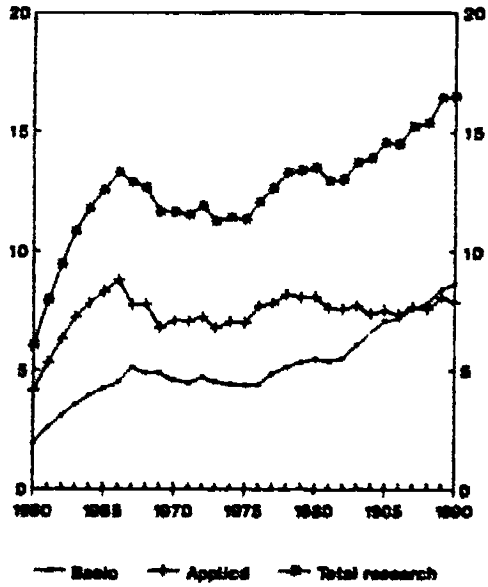
Secondary data analysis:

Description	Collection	Subject
Research Cost Comparisons	NSF, NIH & other datasets	Costs of research
Country Surveys	Interviews & document review	Priority-setting, funding allocation, & research evaluation in other countries
Congressional Earmarking	Budget analysis & document review	Budget information on congressional funding & definitions of "earmarks"
Rhetorical Analyses	Document review	Historical analysis of research decisionmaking by different branches of government & goals of different ideological groups
Research Evaluation	Interviews & document review	Post-1985 developments in research evaluation in the U.S. & abroad
Analysis of Science Policy Task Force Hearings	Document review	Analysis of House hearings on science policy, 1985-1987

SOURCE: Office of Technology Assessment, 1991.

Attachment 2

Figure 1—Federally Funded Research (Basic and Applied): Fiscal Years 1960-90
(In billions of 1982 dollars)

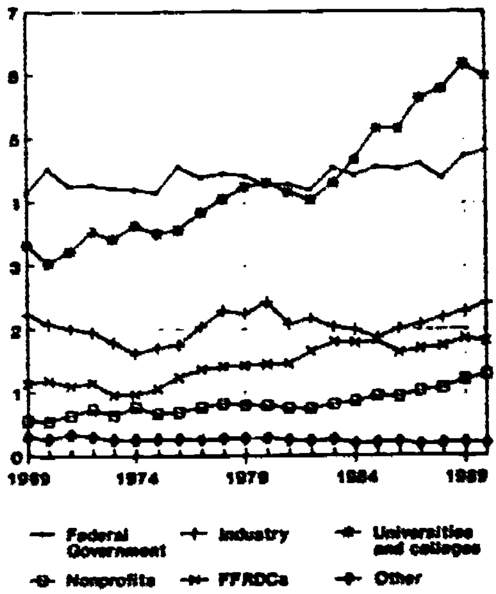


NOTE: Figures were converted into constant 1982 dollars using the GNP Implicit Price Deflator. For 1990 (current dollars), basic research = $11.3 billion, applied research = [illegible] billion, and total research = $21.7 billion. 1990 figures are estimates.

SOURCE: National Science Foundation, *Federal Funds for Research and Development, Detailed Historical Tables: Fiscal Years 1955-1989* (Washington, DC: 1989), table A; and National Science Foundation, *Selected Data on Federal Funds for Research and Development: Fiscal Years 1989, 1990 and 1991* (Washington, DC: December 1990), table 1.

Attachment 2, cont.

Figure 5—Federally Funded Research by Performer: Fiscal Years 1969-90 (in billions of 1982 dollars)



KEY: FFRDCs include all Federally Funded Research and Development Centers that are not administered by the Federal Government. Other includes Federal funds distributed to State and local governments and foreign performers.

NOTE: Research includes both basic and applied. Figures were converted to constant 1982 dollars using the GNP Implicit Price Deflator. 1990 figures are estimates.

SOURCE: National Science Foundation, *Federal Funds for Research and Development, Detailed Historical Tables: Fiscal Years 1955-1990* (Washington, DC: 1990), table 17; and National Science Foundation, *Selected Data on Federal Funds for Research and Development: Fiscal Years 1989, 1990 and 1991* (Washington, DC: December 1990), table 1.

Attachment 2, cont.

Table 2—Federally Funded Research in the 1980s and 1990s (in percent)

		Fiscal year 1980	Fiscal year 1991 (est.)
R&D as percent of total Federal budget		5.0	4.7
Total research as percent of Federal R&D		38.9	36.3
Basic research as percent of Federal R&D		15.7	18.1
Basic research as percent of total Federal budget		0.8	0.9
	Agency	**Fiscal year 1980**	**Fiscal year 1991 (est.)**
Percent of total (basic) research funds distributed, by agency	HHS/NIH	29/24 (38/35)	34/29 (40/37)
	DOD	20 (15)	15 (8)
	NASA	14 (12)	18 (15)
	DOE	11 (11)	12 (14)
	NSF	8 (17)	9 (18)
	USDA	9 (8)	5 (5)
	Other	7 (4)	10 (4)
	Performer	**Fiscal year 1980**	**Fiscal year 1991 (est.)**
Percent of total (basic) research funds, by performer	Universities	32 (50)	38 (47)
	Federal	32 (25)	30 (23)
	Industry	18 (7)	15 (9)
	Nonprofits	8 (6)	8 (9)
	FFRDCs[a]	11 (11)	11 (12)
	Ranking	**Fiscal year 1980**	**Fiscal year 1989**
Percent distribution of Federal R&D funds at academic institutions	Top 10	25	25
	Top 20	40	38
	Top 50	68	65
	Top 100	84	85

KEY: DOD—U.S. Department of Defense; DOE—U.S. Department of Energy; FFRDC—Federally Funded Research and Development Center; USDA—U.S. Department of Agriculture; NSF—National Science Foundation; HHS/NIH—U.S. Department of Health and Human Services/National Institutes of Health; NASA—National Aeronautics and Space Administration.

[a]The category of FFRDCs includes all Federally Funded Research and Development Centers that are *not* administered by the Federal Government.

NOTE: R&D data are based on Federal obligations; calculations involving the total Federal budget are based on outlays. Columns may not sum to 100 percent due to rounding.

SOURCES: Office of Technology Assessment, 1991, based on National Science Foundation data; U.S. General Accounting Office data; *Economic Report of the President* (Washington, DC: U.S. Government Printing Office, 1990); and *Budget of the United States Government: Fiscal Year 1992* (Washington, DC: U.S. Government Printing Office, 1990).

Attachment 3

Table 1—Tensions in the Federal Research System

Centralization of Federal research planning	←→	Pluralistic, decentralized agencies
Concentrated excellence	←→	Regional and institutional development (to enlarge capacity)
"Market" forces to determine the shape of the system	←→	Political intervention (targeted by goal, agency, program, institution)
Continuity in funding of senior investigators	←→	Provisions for young investigators
Peer review-based allocation	←→	Other funding decision mechanisms (agency manager discretion, congressional earmarking)
Set-aside programs	←→	Mainstreaming criteria in addition to scientific merit (e.g., race/ethnicity, gender, principal investigator age, geographic region)
Conservatism in funding allocation	←→	Risk-taking
Perception of a "total research budget"	←→	Reality of disaggregated funding decisions
Dollars for facilities or training	←→	Dollars for research projects
Large-scale, multiyear, capital-intensive, high-cost, per-investigator initiatives	←→	Individual investigator and small-team, 1-5 year projects
Training more researchers and creating more competition for funds	←→	Training fewer researchers and easing competition for funds
Emulating mentors' career paths	←→	Encouraging a diversity of career paths
Relying on historic methods to build the research work force	←→	Broadening the participation of traditionally underrepresented groups

SOURCE: Office of Technology Assessment, 1991.

Attachment 4

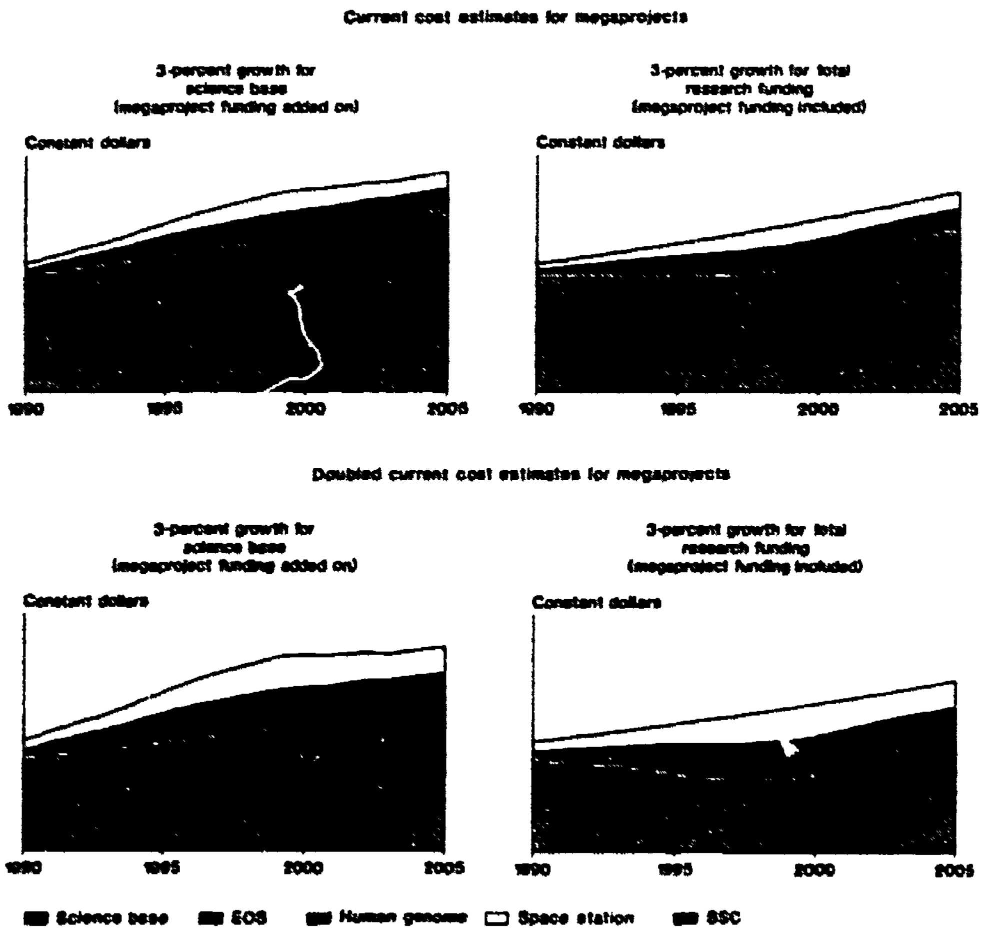
Table 3—Summar / of Issues and Possible Congressional Responses

Issue	Possible congressional responses
Setting priorities for research	Hearings on crosscutting priorities and congressional designation of a body of the Federal Government to initiate priority setting. Application of criteria to: a) promote education and human resources, b) build regional and institutional capacity in merit-based research decisionmaking, and c) balance little science and megaproject initiatives. Oversight of agency research programs that focuses on strategies to fulfill the above criteria, and on responses to priority setting.
Coping with changing expenditures for research	Encouragement of greater cost-accountability by the research agencies and research performers (especially for indirect costs, megaprojects, and other multiyear initiatives). Allowance for the agencies to pursue direct cost containment measures for specific items of research budgets and to evaluate the effectiveness of each measure.
Adapting education and human resources to meet future needs	Programs that focus investment on the educational pipeline at the K-12 and undergraduate levels. Attention to d-ersity in the human resource base for research, especially to the contributions of underparticipating groups. Incentives for adapting agency programs and proposal requirements to a changing model of research (where teams are larger, more specialized, and share research equipment and facilities).
Refining data collection and analysis to improve research decisionmaking	Funding to: a) augment within-agency data collection and analysis on the Federal research system, and b) increase use of research program evaluation at the research agencies. Encou-agement of data presentation and interpretation for use in policymaking, e.g., employing indicators and other techniques that measure outcomes and progress toward stated objectives.

SOURCE: Office of Technology Assessment, 1991.

Attachment 5

Figure 7—Cost Scenarios for the Science Base and Select Megaprojects: Fiscal Years 1990-2005



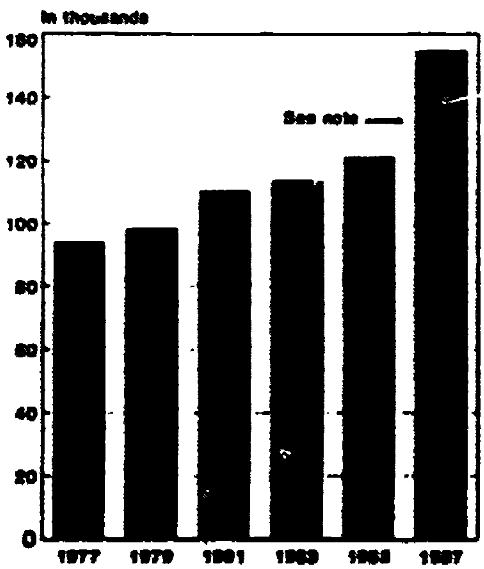
KEY: SSC—Superconducting Super Collider; EOS—Earth Observing System.

NOTE: These figures are schematic representations of projected costs for science projects. In the figures on the left, the science base is projected to grow at an annual rate of 3 percent above inflation. In the figures on the right, total research funding is projected to grow 3 percent above inflation. The cost estimates for the megaprojects are based on data from "The Outlook in Congress for 7 Major Big Science Projects," *The Chronicle of Higher Education*, Sept. 12, 1990, p. A24, and Genevieve J. Knezo, Congressional Research Service, Science Policy Research Division, "Science Megaprojects: Status and Funding, February 1991," unpublished document, Feb. 21, 1991.

SOURCE: Office of Technology Assessment, 1991.

Attachment 6

Figure 2—Doctoral Scientists and Engineers in Academic R&D: 1977-87

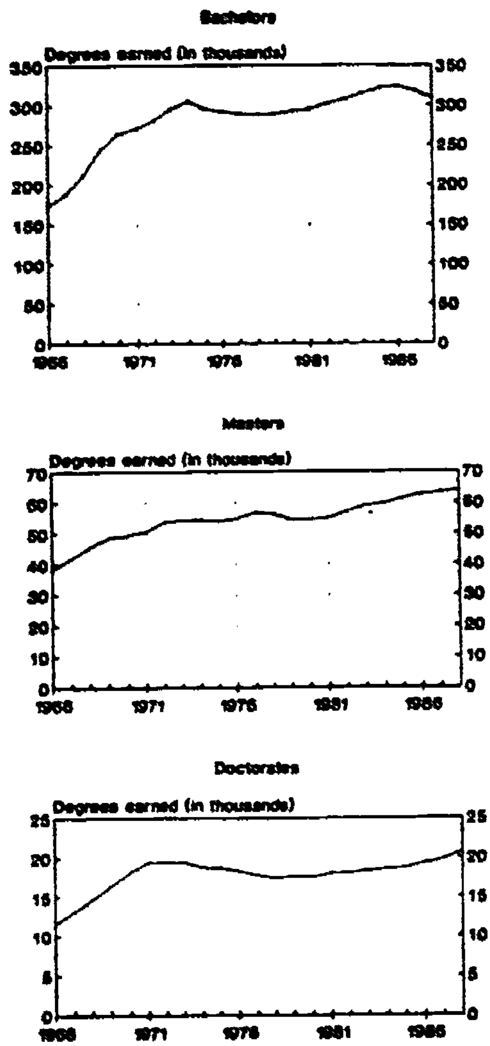


NOTE: There was a change in the wording of the National Science Foundation survey questionnaire of academic Ph.D.s in 1987: respondents were asked to identify whether "research" was their primary or secondary work activity. This change may have resulted in an artificially large increase from 1985 to 1987 in "academic researchers." Prior to 1987, Ph.D.s in academia were only asked to identify their primary work activity.

SOURCE: National Science Board, *Science & Engineering Indicators—1989*, NSB 89-1 (Washington, DC: U.S. Government Printing Office, 1989), appendix table 5-17 and p. 115.

Attachment 6, cont.

Figure 11—Science and Engineering Degrees: 1966-88 (by level)



SOURCE: National Science Foundation, *Science and Engineering Degrees: 1966-88, A Source Book*, NSF 90-312, (Washington, DC: 1990), detailed statistical tables, table 1.

Atta_hment 7

Table 5—Desired Data and Indicators on the Federal Research System

Category	Description	Method	Primary users: Congress	Agencies	OMB	OSTP
Agency funding allocation method	Funding within and across fields and agencies Cross-agency information on proposal submissions and awards, research costs, and the size and distribution of the research work force supported	Agency data collection (and FCCSET)	X		X	X
Research expenditures	Research expenditures in academia, Federal and industrial laboratories, centers, and university/industry collaborations Agency allocations of costs within research project budgets, by field Megaproject expenditures: their components, evolution over time, and construction and operating costs	Agency data collection	X	X	X	
Research work force	Size and how much is federally funded Size and composition of research groups	Lead agency survey	X	X		X
Research process	Time commitments of researchers Patterns of communication among researchers Equipment needs across fields (including the fate of old equipment) Requirements for new hires in research positions	Lead agency survey; onsite studies		X		
Outcome measures	Citation impacts for institutions and sets of institutions International collaborations in research areas Research-technology interface, e.g., university/industry collaboration New production functions and quantitative project selection measures Comparison between earmarked and peer-reviewed project outcomes Evaluation of research projects/programs	Bibliometrics; surveys of industry and academia	X	X		X
Indicators	Proposal success rate, PI success rate, proposal pressure rates, flexibility and continuity of support rates, project award and duration rate, active research community and production unit indices	Agency analysis	X	X		X

KEY: FCCSET–Federal Coordinating Council on Science, Engineering, and Technology; OMB–Office of Management and Budget; OSTP–Office of Science and Technology Policy; PI–principal investigator.

SOURCE: Office of Technology Assessment, 1991.

Attachment 8

Table 6—Summary of Possible Congressional, Executive Branch, and Research Performer Actions

Congressional hearings, legislative efforts, and oversight to:	• Set priorities across and within agencies, and develop appropriate agency missions. • Evaluate the total portfolio to see if it fulfills national research goals, human resources needs, scientific infrastructure development, and balance. • Initiate greater cost-accountability throughout the Federal research system. • Expand programs that fortify the educational pipeline for science and engineering, and monitor the combined contributions of agency programs to achieve education and human resources goals. • Augment data and analysis on the Federal research system for congressional decisionmaking.
Executive branch actions to:	• Enhance cross-agency priority setting in the Federal budget and increase research agency flexibility to address new priorities. • Institute better cost-accountability and cost-containment measures by agencies and research performers. • Expand agency programs to promote participation in the educational pipeline for science and engineering, and require agencies to report progress toward these goals. • Monitor and analyze policy-relevant trends on the research system, especially as related to the changing organization and productivity of research groups and institutions.
Research performer actions to:	• Contain and account for research expenditures. • Revise education and research policies as they affect: a) recruitment and retention in the educational pipeline for science and engineering, and b) faculty promotion, tenure, and laboratory practices.

SOURCE: Office of Technology Assessment, 1991.

Mr. BOUCHER. Thank you very much, Dr. Chubin. We extend our thanks to you and those who have worked with you in the preparation of this very excellent report which gives us a sense of some of the immediate needs that we have in terms of managing the Federal research enterprise today.

I notice that you recommend very strongly, and I certainly endorse your recommendation, that we have a high degree of priority-setting in terms of those projects in basic science research which should receive funding and in what amounts.

We have an inherent difficulty within the Congress in accomplishing that task given the diversity of jurisdiction that our committees have and the obvious necessity for having that kind of priority-setting be a cross-cut among the various agencies that perform research, a very difficult task for us to accomplish within the Congress. That's not to say that we won't try, but there are some structural limitations to our ability, at least at the outset, to accomplish that.

I have some questions for you in a minute about the role that you see the Office of Science and Technology Policy playing in helping to establish that kind of prioritization, but let me begin with any recommendations that you might have for how we, in the Congress, can play a more affirmative role than we have in the past in establishing that system of priority. What recommendations do you have for us beginning with the work, perhaps, of this committee?

Dr. CHUBIN. As we point out in the summary, there is really nobody who is minding the full research system and beyond committee jurisdictions, with which every committee is saddled. Somebody needs to take responsibility to look across fields and performers and agencies to get a sense of what is being proposed; what kinds of commitments are being made, and how particularly long-term commitments may inhibit the ability of the system to be flexible over longer periods of time.

We think that principal criteria that are now used to fund research should retain scientific merit and program-relevance. In other words, goals need to be coupled to the means by which they are to be achieved. OTA, in this report, suggests that two additional criteria should be taken into account, again not in every program or agency, but in some programs and for some purposes.

These two general criteria are: strengthening education and human resources, which of course builds for the research work force of the future—people are the most important component of the system; and then also, trying to develop institutional and regional capacity. There are many researchers that are spread throughout the Nation in many universities and other research institutions that do not receive large sums of Federal funds for research.

While on the one hand, we argue that the research universities represent a precious resource for this Nation and should be preserved and strengthened, it is becoming increasingly difficult for them to maintain excellence across the board. At the same time, other universities which have been emulating what we might call a research university model, might beware and also be careful about trying to build excellence across the board.

Instead, what we suggest is to try to target areas of research where they might be able to develop a critical mass, perhaps acquire special infrastructure and attract the appropriate personnel, and build on it.

There have been some awards in the last couple of years which I think have taken this kind of approach explicitly into account. Certainly the awarding of the Magnet Lab to Florida State is an indication here that in this area of research, the Florida consortium can grow and contribute greatly.

Mr. BOUCHER. Let me ask you this. We are very interested—at least I can speak for myself—in implementing to the greatest extent that we can, your recommendations with regard to establishing priorities. Can you give me just a real clean, concise statement of recommendations that you have for ways that we, as a committee, can go about doing that?

Dr. CHUBIN. There are the following suggestions made, and you might want to make reference, as well, to Attachment 8.

One thing that we suggest is that the whole process of setting priorities needs to go on apart from the annual budget process, the annual authorizations process, and we suggest that within each Congress, once each Congress, that this committee hold a series of priority-setting hearings to give an opportunity both to the science advisor and to the representatives of various agencies to come forward with their ideas about what kinds of initiatives should be put forward and how they seem to balance or change the portfolio of their particular agencies.

I might add that OSTP has already begun to do this in targeted areas through the FCCSET mechanism. They've done, we think, a particularly important job in the area of science and math education where they've done cross-cuts across all Federal agencies at all levels of education, specifying objectives in programs and dollar amounts that need to be spent in order to achieve the President's science and math goals by the year 2000. This is a model.

Mr. BOUCHER. So your recommendation to us would be a little more involvement in terms of hearings in listening to the agencies' recommendations with regard to their research priorities and then perhaps working with the Administrative Branch, with OSTP, in a collaborative effort to establish priorities.

At the risk of embarking on what some may term heresy, let me ask you this question. The fundamental problem we have here—and you'll find this virtually with any committee in the Congress—is getting their arms around the research enterprise. The Agriculture Committee has jurisdiction over the research that's funded by the Department of Agriculture; other committees have jurisdiction over research that's funded by the agencies over which they have authority. We have a broad sweep of it within the general jurisdiction of the Science, Space, and Technology Committee, but no single committee in the Congress really gets its arm around the entire Federal research enterprise, and that, of course, is the structural limitation that we face.

Even if we were to have hearings within this committee of all of the agencies under the umbrella of the Science, Space and Technology Committee, we still would not have the entire enterprise represented at the table before us, so we do have some problems in that respect.

Would it make sense to encompass within the jurisdiction of a committee, the entire Federal research enterprise? Would that kind of restructuring be a sensible step to take in view of the need that you place before us for priority-setting in terms of Federal research?

Dr. CHUBIN. Given the charter of this Committee, I believe that you can do that. I may be mistaken. Somebody should have responsibility for doing that. As soon as somebody suggests that, of course, there are cries that we are trying to manage the system centrally. We're trying to have an over-administered system.

OTA was very careful in this report, on the one hand, to recognize the pluralism of the system, its decentralized nature, but at the same time, to say that there is a need to make this process of setting priorities more transparent. In other words, there is comparative information that needs to be developed. How does OSTP develop a budget along with OMB? So when we look at that chapter in the budget and we are told that this represents the priorities of the Administration, fine, we don't know what went into those priorities.

Once those things would be presented, it would allow this committee an opportunity to respond to them. In other words, I think it expands the dialogue.

Mr. BOUCHER. We, just to correct one matter, do not have legislative jurisdiction in a number of research-related fields. One example, as I mentioned before, is the Agriculture Committee with authorizing jurisdiction over research that is administered through the U.S. Department of Agriculture and there are other examples.

I'm confident that if we invited those agencies to come and testify that they would. We could have a dialogue on the question of priorities but this Committee really doesn't have the authority through the authorizing process to establish across agency lines that broad priority mechanism. We can't do that.

I guess that leads me then to the next question which is this. Do you see an enhanced role for the Office of Science and Technology Policy in doing precisely that? They have a unique ability to set priorities because that is the Administration. They have an active director of that office at the present time. They have had a role in the recent past in setting priorities in areas such as global climate change and high performance computing. We see those recommendations before us at the present time.

Are you recommending a more aggressive role on their part in cross-cutting for priority-setting and if so, would you like to elaborate on that?

Dr. CHUBIN. We are not suggesting that OSTP do this instead of Congress. We are suggesting that OSTP, particularly through a reinvigorated FCCSET mechanism, has the ability and apparently the cooperation of the agencies and OMB to do just this sort of thing.

The problem is that it is a small operation and given their resources and given the fact that a large portion of their staff are on detail from other agencies, I think it would require an increase in their resources and some direct charge for them to do this.

I think that a concern would be that you can do agency cross-cuts in targeted areas. If you start doing it for everything, then what you're saying is that we have no priority among these targeted areas, that you can do this for high performance computing and you can do it for math and science education and for global change, but it may be much more difficult to add 15 or 20 other areas in which to do it.

I think the agencies do strategic planning all the time and I think that there is a need to try to get some of their more future-oriented planning out on the table.

Mr. BOUCHER. Are you recommending a resource increase for the Office of Science and Technology Policy in order to do more priority-setting?

Dr. CHUBIN. I don't believe that saddling any executive agency and OSTP with more responsibility and expecting them to do a better job with the same amount of resources makes much sense.

Mr. BOUCHER. So the answer is yes, then. Let me ask one final question, then I'll yield to my colleagues.

We are probably going to hear from some of the witnesses on the second panel today a suggestion that priority-setting really is not that important, that all we need to do is spend more money and that if we spend more money, the proposal pressure at the various agencies will lessen. We can fund more science projects, and that solves the problem.

I have the sense that it may solve the problem in the short term, but in the long term, as more scientists come into the system that proposal pressure is simply going to build up again and we'll be right back two or three years from now where we are today. Am I right about that?

Dr. CHUBIN. We share your concern, Mr. Chairman. Let me make it very clear that we have said in this report that the Federal Government could invest more in research. What we do not believe is that an increase in funding, which would relieve some of the stress in the short run, would not create some new problems in the long run. In other words, it would perhaps stem some of the stress, but that it would be visited upon the system once again. In other words, there are some strings attached.

Mr. BOUCHER. Thank you, Dr. Chubin.

The gentleman from California?

Mr. PACKARD. Mr. Chairman, thank you very much. Let me yield to the Ranking Member of the Full Committee, Bob Walker for any statement or questions that he might wish to have.

Mr. WALKER. Thank you very much.

I do have a couple of questions, if I could. The whole business of priority-setting is, of course, a major concern of this committee and others in the Congress, but what I fail to see in your report or I'm told in your testimony, is any addressing of the priority-setting that Congress does do through essentially pork-barreling in science. That we have now developed a pattern where the authorizing committees are frozen out of much of the priority-setting as the Appropriations Committees simply set priorities by allocating funding based upon who happens to have a clout in the conference committee that's meeting on any given day.

Could you comment a little on that and suggest any remedies that we might have for this kind of almost informal priority-setting that goes on at the present time?

Dr. CHUBIN. Mr. Walker, in the full report—which I'm afraid we don't have yet—we develop a discussion with some data and analysis of trends and earmarking over the decade of the 1980s.

Mr. WALKER. That's a nice term for it.

Dr. CHUBIN. To distill what we say in that little discussion is that earmarking is used for all sorts of purposes and that it is very clear from the information that we have gathered and also from speaking with many people in the research agencies, that earmarks are disruptive to agencies and agency planning. In other words, very often, almost always, the money that goes for an earmarked project does not go for something that on grounds of merit and mission, the agency would have preferred to have supported.

Also, for purposes of redressing inequities in the distribution of Federal research funds, earmarking doesn't work very well in that there are some States that are major recipients of R&D funds through the agencies, through peer review, who also gather large amounts of earmarked funds.

We, of course, given the scope of this study, would not be in favor of endorsing earmarking. We also know, given the —

Mr. WALKER. So you are not in favor of earmarking?

Dr. CHUBIN. Yes.

By the same token, there are programs that have been developed—and we should particularly credit NSF here, the EPSCoR program, the Experimental Program to Stimulate Competitive Research which is oriented to those States which receive the least amount of Federal R&D funds—use a merit- based or scientific peer review-based system for allocating funds as a way to build capacity in the system and it has worked quite well.

Indeed the 101st Congress mandated that DOD, DOE and EPA, themselves, develop EPSCoR programs. I believe USDA also has one. So there are ways to build what we are calling institutional and regional capacity into the system.

Mr. WALKER. I think your answer is very helpful because the main argument for earmarking has been that somehow we are overcoming the problem of all the major research institutions dominating the peer review panels and so, therefore, all the money gets allocated to a handful of universities, and that somehow, with earmarks, the Congress is overcoming that.

I will be interested in your data to indicate that isn't the case because I think the problem is that what we tend to get are projects that do not reflect any kind of scientific input but, rather, are political creations.

We had a situation last year where we earmarked money for a project in one State and when the press went to the university involved and asked them what the money was going to be used for, they said they weren't quite sure. They were glad to get the money and they were sure that they would use it somehow. That is not exactly priority research then that gets done. I think we've got to be very careful.

Let me ask you this about well-organized research. I think that is probably an area where this committee does have some ability to be helpful, but is there any merit in looking at some reorganization of the Executive Branch that would also move us in that direction?

The Chairman of this committee and myself have, over a period of years, talked at least peripherally about putting together a department of Government that would focus on civilian research. Maybe a Department of Science, Space, Energy and Technology, something that would pull some of the disparate elements together so that there is a consistent organization so that their presentations to OMB reflect a consistent pattern and so that there is somebody at the Cabinet table who consistently talks about the research agenda of the country.

Would that be at all helpful in terms of addressing some of the issues that you have raised in the report?

Dr. CHUBIN. We don't think so. Sorry.

Mr. WALKER. It's important to know.

Dr. CHUBIN. We think the system has worked quite well actually for a long time and the ability of researchers to go to different agencies and the ability of different agencies in some ways to divide the labor in supporting different research areas, is the strength of the system.

The problem is we don't always know which agencies are supporting which research, and which are overlapping and which things are falling between the cracks.

Science and engineering research are so embedded now or intertwined with the investments that this Nation makes, that trying to pull them altogether into one agency doesn't seem to make much sense to us.

The down side is, this is a more difficult system to manage because there are so many agencies that support research.

Mr. WALKER. The only argument I would make to you is that foreign policy also dominates the agenda of virtually every aspect of our national life, and yet we have a Department of State that has a job to do. Business overlaps nearly every jurisdiction we have in the country, yet we have a Department of Commerce. I mean, somewhere along the line, just having the focus is important, but I think your point is a good one and I appreciate your making it.

Finally, is there going to be any comment in the report with regard to some of the investigations going on elsewhere on the Hill that is also going to dominate the priority- setting, namely the fact that universities have seen fit to off-load onto the research budgets a lot of things that are distinctly not research-oriented? Is that not going to undercut our ability to set priorities in the future and is it not going to undercut the ability to come up with sufficient monies to do the research agenda? I'm talking about the Stanford problems and those kinds of things.

Dr. CHUBIN. In the full report, Mr. Walker, we do have a chapter devoted to costs of research. We have not done the kind of financial audit that either GAO or Mr. Dingell's committee has undertaken.

Indirect costs, in our mind, have really been a black box in that every university has a different cost accounting scheme making it very difficult to compare what is defined as an indirect cost, what that university decides to ask the Federal Government for reimbursement, and because of that, we are just now, I think, starting to see how different universities deal with this.

Now, the short answer is that universities will probably start transferring more indirect costs to the direct cost line. The longer answer is that I believe there's more than just financial accountability going on here. In other words, we're talking about the quality of life on campus and universities have got to devise ways of dealing with that.

There's as much tension probably between the Stanfords and the other universities that will be investigated and Congress as there is between the academic administrators on those campuses and their own faculties about monies that go into the indirect cost line.

Finally, I would say that what we're really talking about here is how research bears on the education mission of universities. They are very much intertwined and universities are going to have to decide how they then account for those costs. So I think we are just starting to understand what's going on.

Mr. WALKER. Again, I think that's useful. I will tell you I think that it's going to be very difficult for this committee or any committees in the Congress to deal with research and development money if we think the money is going to pay for furniture for the president's living quarters. That is not going to enhance the ability to deal in a serious way with research projects on Capitol Hill. I think it's very important that we begin to understand the indirect cost issue and understand it in ways that help us focus on how we want our money to be used once we apportion it to the universities.

Ms. ROBINSON. We agree with you, Mr. Walker. We believe that any efforts that can be put forward to make this indirect cost more transparent so we understand what goes in there, would be a definite help for this problem.

Mr. WALKER. I thank the gentleman from California for yielding. Thank you, Mr. Chairman.

Mr. BOUCHER. The Chair thanks the gentleman and would note at this point that this subcommittee does intend to begin hearings rather shortly in the month of April on the question of indirect costs for university-based research and your participation in that would be most welcome.

The gentleman from Florida, Mr. Bacchus.

Mr. BACCHUS. Thank you, Mr. Chairman.

I'd like to begin by identifying myself with the remarks just made by Mr. Walker. Those of us who serve on this Committee and on the Subcommittee do so, in part, because of our belief in the importance of scientific research in the future of this country.

The President has recommended a significant increase in scientific research for this year. I support that recommendation as I think many of the members do, but given the budget agreement, we're going to have a hard fight ahead of us in the House and in the Senate to try to get those increases through.

Headlines about Stanford University and other problems we're facing in terms of accountability do not help us at all. I think we need to rid our Government of waste wherever it exists and if it exists in research spending, we need to do precisely that.

I was struck, sir, by something that you said just a moment ago that I think echoed something in your comments in testimony about the quality of life on campus. I represent Central Florida. On Monday of this week, I met for several hours on the campus of the

University of Central Florida with the department chairs and some other professors in the scientific fields.

Professors from chemistry, biology, statistics, physics, and other departments shared with me their concern about the quality of life on the campus. They are concerned about instrumentation needs. They are concerned about facilities needs. But most of all to a person, they were concerned about quality of life and they were especially concerned about the lack of incentives to encourage young people to go into science and into mathematics, and to make those fields their career.

I noticed in your testimony that on page five you express some doubt about the projected shortages that are frequently talked about in terms of science and math professionals. People at the University of Central Florida on Monday believe those shortages are real. My guess is you're merely saying they've been overstated.

Also, you talk about incentives as well and you say, "Perhaps it should also provide incentives for universities to experiment with policies concerning the opportunities and rewards for young investigators, postdoctorates and nontenured track researchers." This is precisely what concerns those people that I met with on Monday.

Could you perhaps elaborate on these incentives, on the shortages and on what kinds of incentives you would recommend we pursue?

Dr. CHUBIN. The reference to both opportunities and rewards for those categories of researchers who are not principal investigators, that is really the referent of that comment. What we have among the ranks of postdoctoral researchers and nontenured track researchers are people who were trained in research under a mentor; they were socialized to believe that they will have the opportunity to become principal investigators; that they will in fact be able to emulate, if they choose to go into the university as an employment setting, to emulate the career pattern of their mentor.

What we are seeing now is that because of funding stresses, because of competition, that many of those people are not being given those opportunities. They, in some ways, are trapped in a support position, working for a major professor.

At some universities nonfaculty are not allowed to submit proposals. They are not given the status that allows them to become principal investigators, so one thing that we suggest is, perhaps that policy should be reexamined and universities could easily do that.

Another opportunity that we think the market will bring is that if enrollments, as predicted, increase in the mid- 1990s, there should be more demand for faculty. In other words, there should be more opportunity for people to get faculty positions and maybe this problem will take care of itself. It's not clear.

With regard to shortages, OTA's position is really twofold and we've done two other pieces of work on this, so let me just briefly restate our position. We think that at the doctoral level, market signals and market forces will take care of the need for Ph.D.s. If there is going to be a shortage, it will occur at the baccalaureate level. The baccalaureate level means that the support personnel for research would be a factor, not researchers, because most researchers are going to get a Ph.D.

The concern we have at the same time is that there are some segments of the student population, and we name those— United States minorities, women, the physically disabled— for whom without some policy intervention, they will not get an opportunity to participate in these careers. They will not be recruited. They will not be retained without some programs in place. In other words, they have been chronically underrepresented.

Mr. BACCHUS. So you're recommending set aside programs for women and minorities?

Dr. CHUBIN. Not necessarily set aside programs. They have been viewed as set aside programs. If the criterion of strengthening education and human resources is taken as a criterion for funding in some programs, then it could be used in mainstream programs as well after scientific merit of the proposals has been identified.

Mr. BACCHUS. Educate me, sir. Is there any element of affirmative action in our current distribution of these Federal research dollars?

Dr. CHUBIN. I'm sorry?

Mr. BACCHUS. Is there any element of affirmative action in our current distribution of these dollars?

Dr. CHUBIN. In other words, are there programs that—

Mr. BACCHUS. Are there programs that specifically provide incentives for women and minorities to participate?

Dr. CHUBIN. Yes. There are many programs at NSF and NIH particularly.

Mr. BACCHUS. And they recommended increasing them, I believe?

Dr. CHUBIN. Yes. The problem here, Mr. Bacchus, is that the money for those set aside programs is usually quite modest. So there are a great number of programs that are targeted to women and minorities which are just not very well-funded to reach a large number of students at various levels by way of the education system. But their impact is less than they might be if those funds could be increased.

The problem is that in tight financial times, people view this as more of a luxury and that it's detracting from mainstream programming.

Mr. BACCHUS. As I hear you, you would like for us to increase that funding but also based on your earlier testimony, you would like us to spread the money around a little more as well?

Dr. CHUBIN. As we point out in the report, there are many competing "goods". There are many things that need to be done and it depends upon—

Mr. BACCHUS. But they don't all have to be done at Stanford?

Dr. CHUBIN. It depends upon what the objectives are and whose priorities they are. I guess what we're saying is you can't optimize on all of these things simultaneously.

Mr. BACCHUS. Thank you and thank you for your comments about the Magnet Lab at Florida State University.

Dr. CHUBIN. Thank you.

Mr. BACCHUS. The gentleman from California?

Mr. PACKARD. Thank you, Mr. Chairman.

I would like to have you discuss in more detail what the report says and what your reviews show on big science versus small science. And also on large scientific projects, what steps are taken to determine and what are some of the criteria that ought to be used in determining where big science goes?

I think there is a general feeling that they tend to congregate into specific geographic areas or certain universities capture more of them and so forth, and I'd be interested in further comments in those areas, please.

Dr. CHUBIN. Attachment 5, Mr. Packard, tries to show the crystal ball that we looked into in trying to estimate the relative impacts of megaprojects, the funding of megaprojects and also the funding of what we call the science base.

It's hard to get a handle on this one, we think, because the estimates on megaprojects seem to be inconsistent. Some of them include just construction costs, some of them start factoring in operating costs. We do not have a good sense of what goes into those cost estimates or what the criteria are for developing them.

Let's go back to what we put into this attachment. Starting with 1990 dollars, we estimated a 3 percent growth in the science base which is listed in the lefthand column with megaprojects piled on top of them, so you can see the contour of that curve.

If you look to the right, if you include the megaproject funding, there seems to be an adverse impact on the amount that would go into the science base and because there is a history of cost increases in megaprojects over time, what we did for good measure on the bottom two charts is just doubled this 3 percent growth, again with just funding for megaprojects piled on top of the science base on the left, and then what happens if you have 6 percent growth on the right. Again, a bigger chunk is taken out of the science base.

This is OTA's way of saying that although there is no place—this really gets back to Mr. Boucher's point about no committee can get its arms around the whole system—but if you tabulate all of the megaprojects and look at them this way, versus what often is identified as individual investigator or smaller science, there is a tradeoff here.

So what we're suggesting is that these outyear mortgages, as they are sometimes called, that are incurred by megaprojects can have long-term adverse impacts on what can be done in the science base. In other words, it will force some other choices for particularly the NSFs and the NIHs of the world.

Would you like to add something?

Dr. ANDELIN. Let me make a comment about I guess tying together what one might do to oversee the agencies and the question of megaprojects.

In bringing any agencies before the Subcommittee or Committee, whether they are those directly in your jurisdiction or those coming out of courtesy, the Committee could ask issues of how did you establish the merit for the package of proposals you have brought to the Congress; to what extent will this package of activities, if approved, affect the development of future scientific human resources; to what extent will it affect today's institutions and facilities and how will it affect those in the future—those just relate to setting today's priorities, if you will, and beginning to build towards the future.

To bring in megaprojects for those agencies that have such megaprojects now or where you might suspect they would in the future, you can ask questions not unlike how does this project's obligations change over the future if the total funding available in the future were to increase dramatically, not increase so much, be held rather tightly, saved by the budget reconciliation agreement; how would the acceptance of this megaproject today affect your flexibility in the future; what other major megaprojects, if you will, might you bring in over the next 5 or 10 years, and again, how might the rate of change of total budget over the next 5 or 10 years, the acceptance of today's megaprojects and the accuracy of the estimates of today's megaprojects affect whether or not you can bring something new into the future?

Without listing that list again, and I recognize it was a long one with some parenthetic comments, the point is that the decision today about any given megaproject or any package of them across agencies, depends almost as much on what your estimate is of the future funding capabilities of the Committee and the Congress because otherwise you don't know how big a bite this will take out of the base research, if you will, the smaller investigator, bread and butter projects, and you don't know without knowing what new major projects are coming on line—this attachment assumes no megaprojects anywhere for the next decade, otherwise either the funding envelope has to go up faster or there's a bigger bite out of the base research.

Many agencies, if you talk to them, will be talking about new plans. There's new space plans. There's new Department of Energy research plans, and those are not in this. So today's decisions depend upon your best judgment of what you'll be able to do in the future and the agencies' remarks and your judgment of the accuracy of those remarks as to the costs of what they are now proposing over that same future.

Mr. PACKARD. It appears to me that we need to consider a prioritizing system for big projects or megaprojects independent from small projects because they simply are not the same and there needs to be also, it appears to me, a prioritizing system between megaprojects and small projects because again the funding often is significantly affected in both areas by the other.

Then there needs to be an overlay of geographics because again, there is the perception that some States are getting most or many of the big projects, and I think that may have an influence. Then, of course, you have the interagency problem with megaprojects, all of which is so disjointed that often, at least from my perception, megaprojects are now being and have been in the past, in at least the 8 years that I've served on this committee, have been evaluated and prioritized on a case-by-case basis rather than having some fundamental or underlying prioritizing system or steps through which we could go to evaluate whether the project is worthy or not and ought to compete or not.

A good case in point is the SSC, the super conducting supercollider, which we evaluated almost totally oblivious of other big projects or small projects for that matter, and it was simply evaluated on its own merits without any real fundamental system of evaluation. I suspect we do that with other projects or megaprojects and I can think of two or three others that in my judgment have been handled much the same way over the many years.

Are you suggesting, or do you have anything in your report that suggests a different or more improved way of evaluation of megaprojects?

Dr. CHUBIN. I think you stated it well, Mr. Packard. There is a need for some systematic look across these projects and across the agencies which end up housing them. Then there needs to be some way, particularly as the megaprojects themselves are still under construction, there needs to be some interim measures of how they are satisfying the various objectives that have been set out for them.

If the SSC has an educational function, then by the mid- 1990s, we should have some interim measure of that.

Mr. PACKARD. I believe that would certainly lend itself, if we had such a policy and such a procedure, to addressing Mr. Walker's concern and that is that politics often drives the big projects rather than the merits and other facts. I think that's something this committee could probably best address and perhaps OTA also.

Thank you, Mr. Chairman.

Mr. BOUCHER. The Chair thanks the gentleman and recognizes the gentleman from Maryland, Mr. Gilchrest.

Mr. GILCHREST. Thank you, Mr. Chairman.

I guess anybody up there can answer this question. I'm trying to grapple and understand what Mr. Packard is saying and everybody up here about the priority-setting for megaprojects. Small projects seem to be a little bit political. It depends on region. It depends on who has the most clout, I suppose, to get the limited dollars.

Could I talk in an ideal sense for just a minute? Speaking ideally, considering the state of the planet—and I consider the state of the planet to be different in 1990 than it was in 1890, and it certainly will be different in 2090 than it is right now due to population, global warming, a whole range of things. So when all of these dollars are spent on all of these research projects, what, in your opinion, would be—maybe what should be the criteria used for prioritizing research in the United States?

Let's just for a second take everything else away, where would our money best be spent for research as far as global warming is concerned, alternative fuels, population growth, the ozone layer, stimulating curiosity among public school children to go into the scientific and math fields, those kinds of things? What should be America's priorities for research?

Dr. CHUBIN. Mr. Gilchrest, I think the short answer is that this is Congress' job to decide because Congress is trying to serve the public interest and is trying to attain national goals. Scientists will always want to advance knowledge, as well they should, and in the process, if they also happen to improve the technological innovativeness of the country, if they happen to improve the state of economic development in a State or region, all the better.

Those are objectives now that have been attached to basic research and the pursuit of scientific knowledge. So criteria have got to correspond with objectives.

John?

Dr. ANDELIN. I might add that again one of the reasons we suggest serious congressional hearings and perhaps removed from the budget process is to talk about the issue of what the Nation, the Congress representing the Nation, wishes those national goals to be.

What we note is that once the budget is effectively parceled out by agency, the research priorities become rather straightforward, whether they meet those agencies' goals and not whether something in one agency is better or worse than the project last funded or not quite funded in some other agency. It is already divided up by agencies, by Committee and Subcommittee jurisdiction. The same thing occurs in the appropriations process once the full Committee makes its rough cut to Subcommittees. This cross-cutting priority setting, which I suppose we could have called "thinking about the Nation's goals", its been done.

The issue is finished and you're beginning now to argue within an agency and presumably if the agency has done its job well, and many do. The priorities are pretty well lined up with what they've been told to accomplish.

What you can explore by higher level, broad hearings is just the series of questions you addressed to us, which is what are the Nation's best priorities for late in the 20th century as we approach a 21st century with different kinds of global pressures—Mr. Walker suggested that clearly; different demographics of the United States; different economic conditions. It is very much what Congress is here to help think about.

We're suggesting the kinds of questions you can ask even individual agencies to get a picture of how their agency or agency's present mission starts to meet what you might think of as broader national goals.

It isn't that in one year any one authorization or appropriation process or even a Congress, this all gets straightened out, but if we can just learn to ask the right questions, begin to get some sense of the credibility of the agencies in responding to those questions, we begin to build a basis on which we can judge national priorities better and whether the agency's individual missions add up to that national priority.

As Daryl said, the FCCSET cross-cut on science and math education looks pretty good but we're saying that megaprojects by being so large, by having such a large present and future claim on the budget, in a sense ought to be considered cross-cutting by themselves, even though any one of them fits rather nicely in the agency budget.

That, I believe, was alluded to in the testimony and was certainly discussed in the summary and the main report.

Mr. GILCHREST. Thank you. I know I need to do this and all of us to a degree I suppose, is to keep our eyes and ears open and the best thing we can do up here, I guess, is to ask the right questions. Thank you.

Thank you, Mr. Chairman.

Mr. BOUCHER. The Chair thanks the gentleman and just has one further question of this panel.

It seems to me that one of the reasons that we have made so many scientific advances and then flowing from that, advances in technology over the years, is the willingness that our researcher enterprise has had to invest in long-term and high-risk research.

I wonder in the course of your study if you have found any adverse effect on long-term and high-risk research that flows from the kind of proposal pressure that the agencies are having today, and if you have detected any such adverse effect. Do you find it more in the peer review project agencies such as the NSF, or do you find it more in an office like the Office of Naval Research where essentially a single project manager makes the decisions on which projects will get awards? Dr. Chubin?

Dr. CHUBIN. Let me say something about NSF and then Dr. Robinson will say something about ONR.

One problem that has been identified certainly is that researchers are having to spend more and more time in writing proposals as opposed to doing research. That's almost an operational definition of proposal pressure, that more are being dumped into the system in order to get funding.

Some people say—and I've heard this said more about NIH, though I haven't seen anything with data to support this — that proposals are actually getting better. Whether that means they are doing more research that's being reflected in the proposal itself remains to be seen.

Other researchers claim that the system is getting risk averse and that there's mundane or mainstream ideas that are being put into proposals in order to satisfy expert peer panels.

NSF, in part, has acknowledged this by developing yet another program, to their credit, which gives up to $50,000 for a small grant for what they call experimental research, which allows the program manager at his or her discretion to decide whether this is a good idea. And in a sense, it's seed money for that investigator to pursue that idea and then to reenter the system, or attempt to reenter the system by submitting a full-fledged proposal that would be reviewed competitively.

I guess that's one way of saying that even NSF, which funds cutting edge research all the time, is saying that in order to counter this tendency that may be out there, we'll have this small grants for experimental research program.

It's clear that at the two agencies where peer review has been the primary input to decision-making, NSF and NIH, that program relevance—another way of saying "manager discretion"—is more and more being exercised. In other words, peer panels, peer evaluations are certainly the primary input but they are not automatically the basis for deciding who gets and who doesn't.

Mr. BOUCHER. Are you suggesting by that that the proposal pressure itself is having an effect at the peer review agencies of increasing manager discretion?

Dr. CHUBIN. Well, NSF, itself, did in an in-house study that they released last August saying that they were being besieged with proposals and they have to find some ways to streamline their paperwork in order to continue to be selective of the most meritorious research. So proposal pressure doesn't refer only to what the researchers are feeling, it's also what the agencies are experiencing.

Mr. BOUCHER. By the answer is yes?

Dr. CHUBIN. Yes.

Mr. BOUCHER. Let me go back to my first question which is this, do you detect an adverse effect on high-risk research, getting those proposals funded because of the proposal pressure that exists today?

Dr. CHUBIN. If you're asking whether I think that high risk proposals are being declined funding more than they were in the past?

Mr. BOUCHER. Right.

Dr. CHUBIN. Members of the agencies who work in the agencies at various staff levels claim absolutely not.

Mr. BOUCHER. Do you believe that?

Dr. CHUBIN. I never believe entirely what the agencies tell me. That's my job, not to.

Mr. BOUCHER. What is your opinion based on the work that you've done and the research that you've done?

Dr. CHUBIN. I think there's a lot of latitude within programs, within agencies. The high-risk agencies are still high-risk. Everyone says DARPA is doing it the way they always did it, and the lower-risk agencies—I'm not sure who I'd want to put into that category—I think there's a lot of variation within those agencies as to the kinds of proposals that they fund, that they see as high risk and others that they do not.

Mr. BOUCHER. I don't want to dwell on this at great length. It's a relatively minor point in the great mix of issues we're talking about today, but I would like to get just one clear answer to this. In those agencies that fund both high risk and for the sake of discussion, low risk kinds of projects, do you detect any bias now against the higher risk kinds of projects as the results of proposal pressure?

Dr. CHUBIN. I didn't mean to be evasive before, but I don't think there is a bias in the agencies on this particular dimension. I think the agencies have got to balance various kinds of criteria and I think that they do a fine job at that.

Mr. BOUCHER. All right. Thank you.

The gentleman from New York, Mr. Boehlert?

Mr. BOEHLERT. Thank you, Mr. Chairman.

I must confess to all of you that I am as frustrated a member of Congress as you can find because I agree that we have to set priorities. We just can't do all things for all disciplines and simultaneously, but the problem is we rely on the advice of experts—as you might expect, we're generalists—to help us set the priorities. The frustration element gets at a very high level when we talk to the experts and very few of them are willing to discuss the merits of any project outside of their discipline.

I could address my favorite topic, the superconducting super collider, and I get a narrow discipline in the physics community that will tell me it's the greatest thing since sliced bread. Privately, I get people all over the scientific community saying, we think you're on the right track, we don't think this is a high priority. We think there are others that deserve a much higher priority. Privately, they say that. Publicly, it's something quite different. That's the frustration. Can you help me? Can you calm me down a little bit? Can you guide me in the right direction?

Dr. CHUBIN. I'm afraid I can disillusion you further. Do you want to say something?

Dr. ANDELIN. Let me give Daryl a chance to decide what he wants to say and make a comment of my own.

I think you're asking—no, you're asking what looks to you to be the same question of those different groups. I think, in fact, they are answering different questions. When you ask—it doesn't matter which project—the discipline about it, they're telling you about the narrow, scientific merit of that project and they're telling you a bit about how it ties into the future of that discipline. They have that in mind.

When you ask those in another discipline, they may be saying, well for the development of scientists and engineers for the future, for the breadth of the field, for the way in which we would build facilities that have uses beyond the discipline, for our interests, then this project—whatever the projects may be—will be evaluated differently.

So within the discipline, they are answering a disciplinary question. When you cross disciplines, they're talking about what I guess we'd describe as the health of the whole scientific establishment. It doesn't mean that either answer is wrong. They are just, I think, the same question means different things to different people and so you would expect to get different answers.

One of the reasons we suggest that Congress can decide what you'd like scientific research to accomplish, you might then—and there's a list of goals we mentioned earlier and we can talk about again—then you have a better idea how any given project or field fits into those, or activities within a field then you might get a somewhat more consistent set of answers.

You could ask the proponents of the SSC or any of the other megaprojects what they intend to accomplish on a number of national goals—pick your national goals—and if one of them is the advance of science or energy physics, I suspect you'll get a very positive answer. As you go into other fields, you may get, well we haven't thought about that or it too is very positive.

Mr. BOEHLERT. Well, let me ask you this. Why do you think there is such great reluctance—believe me on this subject I've talked to some of the most distinguished scientists in America —to say publicly what they are willing to say to me privately?

Dr. ANDELIN. I would be surprised if that weren't true, I suppose.

Mr. BOEHLERT. But why? I'm trying to get the why.

Dr. ANDELIN. Well, we're saying gentleman's agreement not person's agreement for science—

Mr. BOEHLERT. Are they afraid of getting verbally spanked by their colleagues and their discipline?

Dr. ANDELIN. But also if you're not in a given discipline, you can't easily judge the leading edge in that other field and so it's hard to make a statement about whether or not some project or area or funding increase is or isn't valuable to that other field.

Again, I think if you can ask a question that's constrained enough in words that everybody knows what it means and it means the same thing to all parties, you may get more consistent answers.

Mr. BOEHLERT. The Industrial Research Institute, for example, was asked to prioritize five megabucks science projects, for want of a better description, in terms of their contribution to the competitiveness of the United States. Human genome came out number one. Space station came out two. Number three was the national aerospace plane. Number four—surprise—SDI, and number five was the SSC, but that's sort of a broad base, anonymously answered survey of 200 or so professionals in the private sector.

Dr. ANDELIN. But that question is tightly enough phrased that you might get roughly that same answer, or a mix depending on the individual. It may be shuffled differently, but you'd get individuals to answer that question because you're saying judged on a competitiveness criterion, a how do these rank. If you ask, judged on the development of scientists and engineers over the next decade, how do these rank; if you ask, judged on the utility of whatever it is you're spending money on to build institutional capability or maintain institutional capability, how do these rank; or ask about the development of new material or new technologies, it depends on your question—if you ask about Nobel prizes—

Mr. BOEHLERT. So we have to be more precise and more narrow in our phrasing of the question?

Dr. ANDELIN. Yes. There's ten questions you could ask, not one. If you ask one, you'll get the answer that fits the person depending on where he or she is sitting and what they think is the question. If you ask it very specifically, they'll say I don't know, which is a legitimate answer or they'll help you sort it out.

Dr. ROBINSON. We also believe in this report that there should be recognition of what the role should be of the different communities. We don't put much stock in the science community being able to generate cross-cutting priorities. They don't have any mechanism to do it. They don't know how to do it, and they've shown in the past that it is very difficult, just like you've mentioned.

We feel, though, that it is the role of Congress and the role of others to apply additional criteria beyond just strict scientific merit and disciplines to order these priorities. So instead of relying on the science community to come up with the broad priority-setting structure, which they have shown to be unable to do and which we don't think they can do, Congress needs to take this on itself and also ask the Executive Branch to come up with a cross-cutting priority set.

Mr. BOEHLERT. Well, I accept your challenge and this year on this full committee, Subcommittee on Investigations and Oversight, we're going to be working the problem, as they say in this town, and we'll look to you for some guidance as we very carefully craft very specific questions because quite frankly, the only reason I'm on this committee is when I came to Congress as a freshman in 1982, the wizards of the back room looked at my background and said, Boehlert got a D in high school physics, he belongs on the Science Committee.

[Laughter.]

Mr. BOEHLERT. I say that, and it's true, but we are generalists up here and we're constantly reaching out for the best advice we can get. We're talking about billions and billions of dollars—they're not my dollars, I contribute a few to the Treasury but they're your dollars.

Yes, Doctor?

Dr. CHUBIN. Mr. Boehlert, we'd be happy to help you craft some of those questions.

Mr. BOEHLERT. Thank you so much. You've got a deal.

Thank you, Mr. Chairman.

Mr. BOUCHER. The Chair thanks the gentleman and thanks this panel for its presentation this morning. We may have some follow up questions for you in which case, we'll submit those in writing.

We now welcome our second panel of witnesses for the day, Dr. Rolard Schmitt, the President of Rensselaer Polytechnic Institute; Dr. Rustum Roy, Professor of Physics at Pennsylvania State University; Dr. Leon Lederman, President of the American Association for the Advancement of Science; and Dr. Douglas Lauffenburger, Alumni Professor of Chemical Engineering at the University of Illinois.

We welcome our second panel. Without objection, your written statements will be made a part of the record and we would ask for a 5 minute summary of your statements orally, and we will be happy to recognize the first gentleman to be seated who I believe is Dr. Schmitt. We'll be happy to begin with you, sir.

STATEMENT OF DR. ROLAND SCHMITT, PRESIDENT, RENSSELAER POLYTECHNIC INSTITUTE

Dr. SCHMITT. Thank you, Mr. Chairman.

I have learned from reading the papers and the comments in this room that university presidents, when they appear before congressional committees these days, have to establish some credibility. I want to spend a minute doing that. I think that with Congressman Boehlert, I have already have it, I hope.

I've spent 3 years as a university president, 37 years before that in industry, in a position that turned out many, many innovations that have produced billions of dollars of commerce for the United States, cat scanners, magnetic resonance imagers, high performance polymers, jet engines, lighting and so on, a lab which pioneered in many of these areas and won in many of them against the Japanese as well as others.

I also come from a university in which our administrative costs are going down. Our overhead rate is going down. There are no flowers in the president's house paid for by taxpayers' money and where a minuscule fraction of our research support comes from the Japanese, and that only with the concurrence of our American partners, whereas a larger fraction of our research is supported by industry than that of any other significant research university. So that, I hope, will establish the grounds on which sit, stand or speak.

The OTA report, "Federally Funded Research: Decisions for a Decade," is indeed a very, very timely and important report. In reading it, I found a number of parallels to a talk I gave on February 15, 1991 to a joint meeting of the Councils of the National Academies of Science and Engineering, and the National Science Board. Thus my written testimony which has been submitted is based on that earlier talk. What I would like to do is just summarize it very, very briefly.

The perspective of my remarks is that of an academic research university, rather than that of the Federal Government so it differs slightly from that of OTA. So I took a look at the total funding of academic research, not just the Federal funding.

This is important because Federal funding has been the slowest growing source of support for academic R&D over the last two decades. Moreover, today Federal funding accounts for only about 60 percent of total academic R&D funding.

What is perhaps most striking to one that looks at that is that the level of discontent in the academic research community has become more and more intense during the 1980s when the growth of total support has been strongest.

Recently this was dramatized especially strongly by an interesting little survey that appeared in the February 1991 issue of Physics Today. There it was reported that young physics faculty were surveyed in 1977 and were again surveyed in 1990. In 1977, 63 percent of those surveyed thought that funding was adequate in 1990, only 11 percent thought so— a dramatic drop.

Mr. BOEHLERT. Excuse me. That's within the physics community?

Dr. SCHMITT. That's young physics faculty, that's correct.

If you look at the per capita support, the actual per capita support in constant dollars in the physical sciences, which admittedly includes physics, chemistry and a little bit of astronomy, that per capita support in constant dollars hit a minimum in 1977 when 62 percent of the people thought funding was adequate and the per capita support has been growing steadily at about 6 percent per year ever since as the number who thought there was adequate support plummeted, a very curious phenomena, and one of the most dramatic ones that I can imagine to show one of the things that seems to be happening.

During the 1980s, the constant dollar per capita support of academic research in total has been growing substantially at the same time that the level of discontent seems also to have been growing. So I have had to come to believe that there are structural issues in the system as it exists today that are as important as the money issues and my submitted testimony examines this curious situation in more detail.

Just to mention a few of the changes that have occurred during the last two decades, there has been a very significant growth in the academic research system consisting of two elements—one in the number of institutions engaged in research and second, in the relative amount of effort devoted to research over education and teaching.

The academic research system has quite admirably, in my opinion, spread to a larger number of institutions and Federal funding has contributed to that spread. We have many more research universities today than we had in the early 1970s. At the same time, the research intensity on those campuses has grown. A larger and larger fraction of academic doctorals have become dedicated to the research, the number of academic doctorates with R&D as their primary activity has more than doubled in the last 15 years. Furthermore, the new doctorals going into academia have increasingly gone into research and development, not teaching positions. Finally, among the academic doctorals primarily devoted to the research, nonfaculty employees are growing the fastest. So every sign indicates the increasing research intensity of the system.

If you look at the deployment of post doctoral students and of graduate students, we find changes in those that are all consistent with this increasing research intensity on the campus, more and more research assistance devoted to particular research goals, less and less resources into more general things like traineeships and fellowships.

What do you conclude from all of this? In my view, academic research has three essential functions. One, the first one is to contribute to knowledge of value to our society. The second is to educate graduate students via the composite system of advanced, specialized courses and research apprenticeships. Third, is to enrich the education of undergraduate students via teaching by faculty who are in touch with advancing knowledge.

All of the trends I remark on indicate a significant shift to that first function, namely contributing to knowledge and the significant growth in its size, especially during the last decade. Moreover, that trend is one that has tended to reduce local flexibility. Virtually all the support is by targeted research grants. There is little local flexibility left to reallocate resources.

I think that one of the problems of support for young, beginning researchers today may come from this limited flexibility. It used to be that department heads and deans were able to assign trainees and some of the fellows to young faculty members to help them get started in research. That flexibility is gone today. The Federal support of traineeships has dropped significantly.

So I believe that the discontent today and the stresses expressed in many quarters has led many to believe that it is simply a shortage of money that is a problem but as you can tell, I think that the problems are deeper than that and the pressures to continue evolving further in the same direction may not be the thing to do. It may be time to pause and ask is this the best way to go.

I think OTA has articulated some of those issues very well, at least for the Federal Government. I believe the academic community must also join in this reexamination. Solving this, there is no single magic bullet or master plan that can solve these problems. There are many specific actions that need to be strengthened or initiated and they generally fall into three groups.

The first that I would put forward is making better use of our present resources. In my view there are significant opportunities for doing that and I can go into some detail on that if you like. The second is linking research more strongly to education and to human resource development. The OTA report does acknowledge human resource development can be an important component of academic research. I think it is a high priority. Finally, is devising innovative new initiatives to attract new resources.

In the testimony that I submitted, I have details of several specific suggestions on that. I might just mention two or three of those. One is the spread of research to more and more universities. I agree with the OTA comment that in doing that, we should not encourage every new research university to become a comprehensive research university. We should rather adopt policies and practices which will encourage some of these universities to specialize in certain areas, others to specialize in other areas. I think some changes in the policies and practices of agencies could help to produce that result.

Another dimension of research utilization quite frankly is the local management of resources. By that I mean equipment, space, services, as well as the direct efforts of the research team itself. I am sure that on most campuses, there are great opportunities for more sharing of equipment, more effective utilization of space, and especially less expensive ways of procuring needed services. I could give you examples of that, if you wish.

In research and education today there is a strong bias towards using nonfaculty, doctoral research people on research grants rather than using graduate students, which I think is a very bad thing. Why is that so? The reason is very simple. The sponsoring agencies are all oriented towards seeing what research results are produced and to get nonfaculty doctoral people working on those research contracts rather than graduate students produces results more quickly, leading to publications earlier and a better chance of renewal—again, a situation that could be changed and affected by agency policies.

I believe that the Administration's program of traineeships and fellowships across all of the principal agencies supporting academic resources, a very desirable initiative and strongly support that. In addition, an organization with which I am associated, CORETECH, is also advocating the establishment of fellowship and traineeship programs by each of the Federal megascience and megatech programs. We talked about those programs earlier. If we're going to have such large megascience, megatech programs, they ought to be asked to contribute to the development of the human resources that they use.

Finally, we need a lot of new ideas. I emphasized local flexibility. I think some of the ideas in NSF illustrate what I'm talking, the Engineering Research Centers, the Science and Technology Research Centers, were very fine initiatives that brought millions of additional dollars into the science budgets, but we need more new ideas.

One that's been suggested which would help to alleviate a problem I mentioned earlier. That is this decreasing local flexibility would be something equivalent to DOD's independent research and development funds, the R&D funds provided to academic institutions doing federal research, funds equal to a certain percent of the Federal R&D contracts and awards that could be used by the institutions themselves for unrelated programs and especially to help solve this problem of getting young, new investigators started.

Well, there are other ideas like that. My point simply is that while I believe it is important to continue the growth rates in R&D, in academic R&D, at the rates of the 1980s—I don't want to say that there are no money problems, far from it—I also believe that we need to make some structural changes in the way that growth is used.

As things stand, I don't know that just pumping another $10 billion into the system won't simply intensify these structural problems that have caused the stresses that are curiously out of sync with the growth rates, as I pointed out, and won't simply precipitate the call for another $10 to $15 billion increment down the road.

That's a summary of the written testimony I submitted, Mr. Chairman.

[The prepared statement of Dr. Schmitt follows:]

Academic Research and Development-
How Can We Improve Its Condition?

Roland W. Schmitt
President, Rensselaer Polytechnic Institute, Troy, NY

Testimony before the Subcommittee on Science of the Committee on Science, Space and Technology of the U.S. House of Representatives
March 20, 1991

In reading the draft of the Office of Technology Assessment's report on "Federally Funded Research: Decisions for a Decade", I was struck by the many similarities with a talk I gave on February 15, 1991 to a joint meeting of the Councils of the National Academies of Science and Engineering and the National Science Board at the Beckman Center in Irvine, CA. A principal difference is that the OTA report examines the issues from the perspective of the federal government whereas I had looked from the perspective of academic institutions. The following testimony is based on that earlier talk because I believe it is relevant to the issues being addressed by this committee today. I have added material developed since mid-February and have omitted a lot of comments about political realities that I conveyed to that earlier audience of scientists and engineers.

There is a curious phenomenon occuring in the academic research community today, a phenomenon that the OTA report alludes to but does not fully address. It is this: after a decade of some of the best growth in support of academic research that has ever been experienced, the discontent among academic researchers is at an all time high. For example, a recent survey[1] reports that among young physics faculty there has been a dramatic drop from 63% in 1977 to 11% in 1990 in the fraction who believe that research funding is adequate. This report joins a rising tide of complaint about the plight of academic research in the U.S today, a tide that was dramatized by Leon Lederman's survey[2] of the views of 250 academic researchers.

While I could not readily find data on physics alone, Figure 1 shows the support trends per academic doctoral in the physical sciences (mainly physics,

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[1]Roman Czujko, Daniel Kleppner and Stuart Rice, Physics Today, Feb. 1991, p.37
[2]Leon M. Lederman, Science: The End of the Frontier?, A report to the Board of Directors of the American Association for the Advancement of Science, Jan. 31, 1991.

chemistry and astronomy) from 1973 through 1987. The curious fact is that young physics faculty felt best in 1977, after several years of diminishing support and felt worst after the sustained growth in per capita support during the '80s.

Whatever explanation one wants to offer for this phenomenon, it dramatizes the fact that we need to understand much better than we do today what is happening on the academic research scene.

So let's look at the condition of academic research today. I'll begin by correcting, in part, the impression left by the AAAS report: academic research is not quite as bad off as implied. First, looking only at basic and applied research, the report omits the development effort at academic institutions amounting to $870M, or a little over 6% of campus research expenditures in 1989. But, even more important, it omits the non-federal sources of support which were $5.8B or 40% of the total in 1989.

Moreover, as Figure 2 shows (along the bottom row) during the 20-year period, 1969 to 1989, federal support grew more slowly than any other source, averaging 8.8% per year compared to 15.1% from industrial sources and 11.4% from other sources. If you look at the two ten year periods, 1969 to 1979 and 1979 to 1989, you will notice that in the decade of the seventies, the growth rates were lower and the inflation rates higher than they were in the eighties. So, again, the eighties, when discontent has been growing on campuses, has been a decade of considerable real growth in the support of academic research!

But, we need to examine support in constant, not current dollars and we, therefore, need a price deflator. The AAAS report uses the OMB R&D price deflator. In spite of its name, it was not designed to measure the price of R&D inputs. The GNP price deflator is more commonly used. They differ from one another: during the two decades in question the OMB R&D index grew at 7.2%/year while the GNP index grew at only 6%/year. (The differences between the two deflators, again, were larger in the 70's than the 80's. So growth during the 80's is not very different, whichever deflator is used.)

Putting these two things together--total academic R&D expenditures, not just federal expenditures for basic and applied, and the GNP price deflator rather than the OMB R&D deflator--you get quite a different picture from that of the AAAS report. Figure 3 shows a comparison of the two views. One yields an increase of only 33% in constant dollars, the other yields 101%, or double between '69 and '89.

An item of considerable interest is the rate at which institutions have been increasing the use of their own funds for R&D. As Figure 4 shows, it has been much faster than the increases in state supported R&D even though the latter has received more public attention. Considering the financial problems that many institutions of higher education are facing today, this may be one of the sources of the troubles of the research community.

Figure 5 shows that the academic funding per doctoral looks very different when you look at total funding, using the GNP deflator, than when looking at federal funding only, using the OMB R&D deflator.

Just to be sure that there are no prominent anomalous subsets of this data, Figure 6 shows the expenditures per academic doctoral in several areas. The per capita amounts are quite different but the patterns are all similar.

What conclusions can we draw from these data? First, while the total amount of money going into the academic research system is a constraint, there seems to be other problems as well. We are back to per capita expenditures near those of the "golden era" of the late 60's but complaints are at a fever pitch. Now, it will be said that the per capita expenditures need to be higher today because of the more sophisticated, more expensive equipment and facilities that are needed. I agree. But, while the higher capital intensity of research today should imply higher per capita costs of the inputs to research, it ought also lead to higher productivity and thus lower costs per unit of research output and these outputs are hard to measure.

In any event, if one is to judge by the level of discontent, the stresses in the academic research system are high. If total money alone can't explain this phenomenon, what other factors might also be contributing? Maybe the system has some structural problems that also need correction. And, if there are structural problems, how can we be sure that just pouring more money into the system will solve them?

Let's look at some of the changes within the academic research system. During the last two decades, there has been significant growth of this system. It has consisted of two expanding elements: first, the number of institutions engaged in research and second the relative amount of effort devoted to research over education and teaching. Let's look at each.

The academic research system has spread to a larger number of institutions and federal funding has contributed to this spread. From the '71-'73 period until the '87-89 period, the number of academic departments receiving federal research money grew about 22% in engineering and almost 50% in the life and physical sciences as Figure 7 shows. So, today, academic research is thriving not just on the East Coast, the West Coast and a few selected spots between. It is also pursued on campuses in the Southeast, South, Southwest, Midwest, Northwest and many more.

Meanwhile, the research intensity on these campuses has grown. A larger and larger fraction of academic doctorals have become dedicated to research. Figure 8 illustrates this shift. The number of academic doctorates in the natural sciences and engineering[3] with R&D as their primary activity has more than doubled in the last 16 years. Further, new doctorals going into academia have increasingly gone into R&D, not teaching positions - especially in doctoral institutions. Moreover, among academic doctorals primarily devoted to research, non-faculty employees are growing the fastest. There is also a set of S&E academics who are approaching retirement in this decade who came into the system during its period of rapid growth; they have significantly occupied the tenured faculty positions in academic institutions, leaving only limited opportunities for entry into a normal academic career by junior people. This, too, has undoubtedly contributed to the younger peoples' movement into research-oriented positions. Meanwhile, the faculty itself is aging, the average age having grown from 42 to 47 in the last 15 years.

But, if a larger and larger fraction of academic doctorals are engaged in research today, then expenditures per doctoral in total - the measure we used earlier - may not be the right measure. We need to know the so-called "full time equivalents" engaged in research and see how the expenditures per FTE have varied over time. Maybe this is what has deteriorated. The data is a little sparse, some applying to academic institutions without the Federally Funded R&D Centers (FFRDCs), some applying with them. Figure 9 shows what is available. Frankly, it suprises me a bit because I believe it should have shown some decrease to be consistent with the other data I've presented, data that I think is more trustworthy. Nevertheless, for completeness I show it with the caveat that it is a facet that needs a deeper look.

Looking at the deployment of postdoctoral students and of graduate students, we find the changes are all consistent with the increasing research intensity of campuses. About 80% of all postdocs are used in R&D and these have

[3] In this paper "natural science and engineering" (NS&E) refers to all of "science and engineering" except psychology and social sciences.

grown from ~4,300 in '73 to ~10,300 in '89, a growth of ~8% per year. Also, Figure 10 shows how the utilization of graduate students has changed. The fastest growing segment is Research Assistants, where federal support has contributed to the growth but not by any means caused it all. Teaching Assistants, almost totally funded by non-federal sources have also grown but not as rapidly as Research Assistants. Meanwhile, Fellowships and Traineeships have languished with federally supported ones having dropped by about one-third. Figure 11 further shows that the number of full time NS&E graduate students supported by federal funds languished during the 80's while those supported by higher educational institutions own funds grew. It is a trend that cannot continue, given the fiscal constraints faced by those institutions today.

A final question that might be asked is whether or not a few people on each campus get a high proportion of the research money while many others struggle for it. This possibility was suggested to me by the Vice President for Research at a major research university. I have not had time to get data on any campus but my own. Figure 12 illustrates that his suggestion may have merit, at least enough to investigate it further.

What do we conclude from all of this data? Academic research has three essential functions:

- to contribute to knowledge of value to our society,
- to educate graduate students via the composite system of advanced, specialized courses and research apprenticeships,
- to enrich the education of undergraduate students via teaching by faculty in touch with advancing knowledge.

All of the trends we've noted indicate a significant shift to the first function and a significant growth in its size, especially during the last decade. The increase of academic staff devoted to research, the increase of research assistantships which are attached to specific research grants or contracts, the lag of the more flexible fellowships and traineeships which are more closely linked to the educational function, the increase of teaching assistantships that generally relieve faculty of teaching burdens - are all consistent with this trend.

Moreover, the trend is one that tends to reduce local flexibility. If the distribution of research funds on each campus is highly skewed - as I've shown it to be at Rensselaer and suspect it to be elsewhere - we may have a fraction of the faculty on each campus who are very well off mixed with many who are struggling. And if virtually all of the support is by targetted research grants, there is little local flexibility to reallocate resources, whatever the merits.

One of the problems of support for young, beginning researchers today may stem from this limited flexibility. Department heads and deans used to be able to assign trainees and some fellows to young faculty members to help them get started in their research. In fact, today, because of the lag in the number of such positions, it is teaching assistants who may be assigned to younger faculty to help with research, thus turning some of these positions into a subsidy of research, supported largely by the institutions themselves.

One of the issues is whether or not the key criteria for federal support of academic research should be "scientific merit and mission relevance" as the OTA report suggests and which would say that the first of the three essential functions that I stated above is, indeed, the correct one for emphasis. Or, should human resource development to strengthen future research capability share equal status as a federal objective. I happen to think that it should, thus giving equal weight to the second and third of my essential functions, above, of academic research. It is an important issue because it bears directly on whether the attitude of the federal government is that of procurement or that of resource development. An attitude of "procurement", the dominant one today, leads directly to systems of limited local flexibility.

The discontent today and the stresses expressed in many quarters has led many to believe that it is simply a shortage of money that is the problem. As you can tell, I think the problems may be deeper than that and that the pressure to continue evolving further in the same direction may not be the thing to do. It may be time to pause and ask, is this the best way to go?

During the last two decades money did not grow as fast as researchers during the 70's. Only in the last decade has that begun to turn around - ironically the unhappiness of the community seems to have erupted during the very period when the change has become favorable. Meanwhile, though, academic institutions have had to get increasing amounts of support from industry and from state governments and, especially, have had to put more and more of their own resources into it. And stresses are appearing, today, in all three of those quarters: academic institutions, states, and industries.

So, what do we do about our condition? First, there are several things that we can't or shouldn't do. Let me list them.

•Reduce the number and distribution of research universities and, like Britain, try to concentrate on a relatively few elite schools. This won't work and can't be done. It would be a political minefield; first, because of continual Congressional interest - and, in NSF's case, a statutory requirement - in paying attention to geographical distribution. And second, because we've now, quite rightly, convinced many politicians that research universities can contribute significantly to regional economic development.

•Reduce the number of Ph.Ds awarded to U.S. citizens so as to reduce the alleged "surplus" of researchers. I fear that this is the most likely outcome of the kind of campaign launched the AAAS report. It would be bad for two reasons. First, we will need an influx of new, teaching faculty before the end of the decade. And second, academic research is not a surplus commodity. It still has much untapped potential for contributing to solutions of the nation's and the world's problems.

•Place a tax on high-tech products as suggested by the AAAS report. This would be a disastrous policy, a quick way to make the U.S. even less competitive in world markets. For the past several years a consortium of universities and industries, CORETECH - has been struggling to get the R&D tax credit made permanent - as a means of stimulating industrial R&D and industrial support of academic R&D. The idea of taxing these goods instead of giving a tax incentive to the R&D behind them is a sure prescription for further damage to U.S. competitiveness.

There is no single magic bullet or master plan that can solve our problems. Rather, there are many specific actions that need to be strengthened or initiated. They fall into several groups that should be of interest to this committee today:

•Making better use of our present resources.

•Linking research more strongly to education and human resource development.

•Devising innovative new initiatives to attract new resources.

Let's take them one by one.

My suggestions are not wholly original. I have borrowed liberally from others, especially from Frank Press, President of the National Academy of Sciences, from Robert White, President of the National Academy of Engineering, and from working groups of CORETECH, a lobbying organization for science and technology, which I chair.

Resources:

We need to make better use of the resources that we do get. There are several dimensions to this. The first is a policy and program management issue.

I said earlier that we should not try to reverse the spread of research to more and more campuses, as some have implied if not suggested explicitly. But, that doesn't mean that every campus aspiring to eminence in research has to be comprehensive in its research. We ought to be encouraging different clusters of excellence at different campuses. And we can do this if proposals were to be judged not on intrinsic merit alone, but also on context - is the work linked or synergistic to other work on the campus? Will it help to build a steeple of excellence on the campus? It would be a policy conducive to a strong, dispersed academic research system that yet had only a healthy degree of redundancy across all institutions.

Another dimension of resource utilization is the local management of resources - and by that I mean equipment, space, services, etc., as well as the direct efforts of the research team itself. I am sure that on most campuses there are great opportunities for more sharing of equipment, more effective utilization of space and less expensive ways of procuring needed services. For example, when granting money for new equipment, agencies might ask what the plans are for downstream use of the equipment and for plans to share it. Addressing these issues wisely can help individual researchers, yet there are virtually no forces in the academic environment - short of crises -that motivate either the administration or the faculty to address them. I believe that some sort of task force to visit many campuses, to identify the best practices on any of these campuses and to widely disseminate this information would be valuable.

In many instances, reorganization of academic education and research could substantially improve the use of resources and the cost of overhead. Frank Press made this suggestion and pointed out that it might be particularly timely because a generation of faculty is being replaced by a new one.

Research & Education:

Turning now to the second group of actions: linking research more strongly to education and human resource development. One of the arguments of the AAAS document is that the austere level of funding will discourage graduate students from pursuing research careers in academia. Yet, ironically, as I've pointed out, some of our problems arise from the separation of an increasing portion of academic research from teaching.

Today, there is a strong bias toward using non-faculty, doctoral research people on research grants rather than graduate students. Why? It produces results more quickly, leading to more publications, earlier, and a better chance of getting future renewals and new grants. The bias is against using research grant money on graduate students, let alone against involving undergraduates. This is a bias that could be corrected by policies of the granting agencies - a shift back to fellowships and traineeships, for example. Or, changes like the recent NSF requirement to submit only the ten top publications plus a list of graduate degrees produced by principal investigators instead of the usual list of dozens or even hundreds of publications. The purpose of this change is to let reviewers see the educational as well as research output of the principal investigator.

CORETECH, the lobbying consortium of industrial and academic institutions, has adopted a platform in support of the administration's program of traineeships and fellowships across all of the principal agencies supporting academic research. In addition, it advocates the establishment of a fellowship and traineeship program by each of the federal megascience and megatech programs.

There are a lot of other policies of funding agencies that constitute both implicit and explicit biases against the educational function of research in academe; we should institute a study to identify all of them and then decide on appropriate changes.

But most important of all is the change in federal attitude and policies toward the funding of research on campuses. The view that it is the procurement of a commodity, however meritorious, should be abandoned and the human resource devlopment dimension should become prominent. This, again, would support strengthened programs of traineeships. Other ideas that would move us in this direction are noted in the next section.

<u>New Ideas</u>:

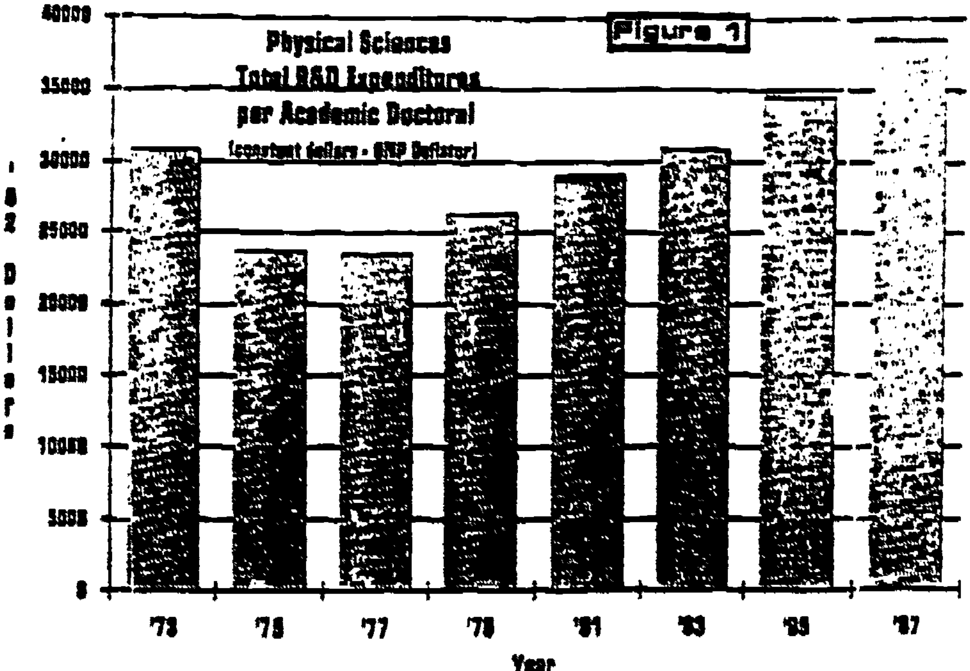
We need some new ideas. What we must do is to generate new ideas that can be promulgated effectively to OMB,to DOD, to NIH, etc. and that will be persuasive, congressional committee by congressional committee in Congress.

At NSF, the invention of the interdisciplinary research centers, the Engineering Research Centers and the Science and Technology Research Centers brought millions and millions of dollars into NSF budgets that would not otherwise be there.

What are some new ideas for the future? One is to provide the equivalent of DOD's Independent R&D funds to academic institutions doing federal research - funds equal to a certain percent of federal R&D contracts and awards could be used by the institution for unrelated programs - a stimulus for the exploration of new fields. Or, institutional grants linked to broad goals - not specific research areas - could be awarded competitively, again permitting local flexibility. Both of these approaches are directly in line with my plea for systems permitting greater local flexibility and more devoted to human resource development.

In the end, one must conclude that money alone in amounts much greater than growth during the 80's may not solve the problems of the academic research system. While I believe it important to continue growth at the rates of the 80's, I also believe that we need to make some structural changes in the way that growth is used. As things stand, we don't know that just pumping another $10B into the system won't simply intensify the structural problems that have caused the stresses and precipitate a call for another $10-15B increment down the road. Unfortunately, also, the general call for another $10B doesn't give anyone, anywhere in positions of responsibility anything to do. It works wonderfully on the billboards and marquees and headlines. It is appealing to the media. Even though the value of that appeal shouldn't be neglected, the real, effective warfare has to be fought elsewhere.

So what do we do? There have been many talks, many news articles, even a few studies, but no one has yet faced up to reexamining the system we have, asking how it can be improved, and coming up with specific recommendations, agency by agency, congressional committee by congressional committee. We should do so and the Office of Technology Assesment's report is a good beginning.



Sources: NSF [illegible], NSF [illegible], NSF [illegible], NSF [illegible]

Growth Rates of Academic R&D (& Inflation Rates)

	Total	Federal	Indus-trial	Other	GNP Deflator	OMB Deflator
'68-'78	9.2%	8.4%	12.5%	10.7%	7.1%	8.1%
'78-'88	10.4%	9.1%	17.8%	12.0%	5.0%	5.5%
'88-'89	9.8%	8.8%	15.1%	11.4%	8.0%	7.2%

Figure 2

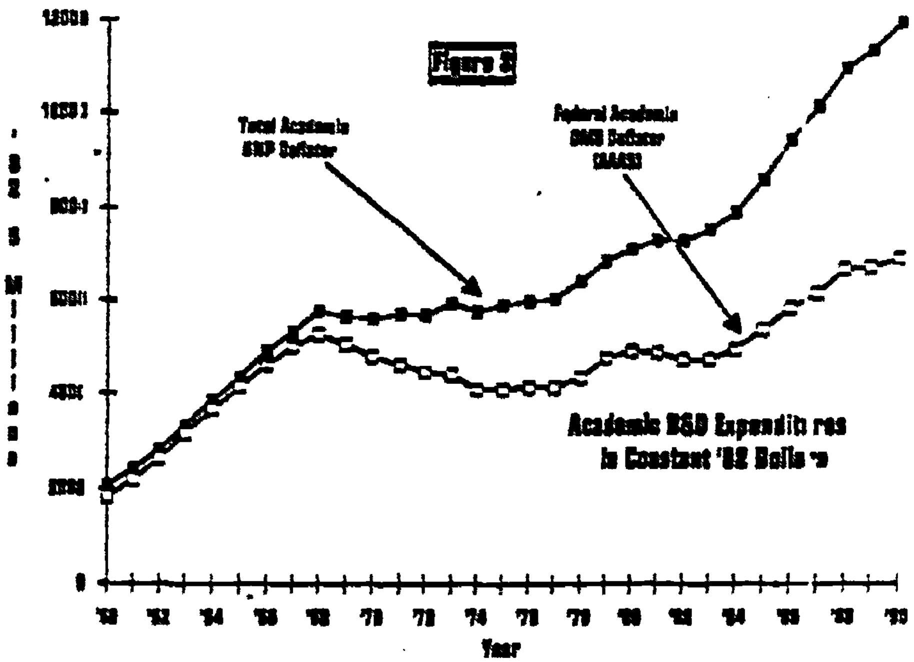


Figure 3
Year

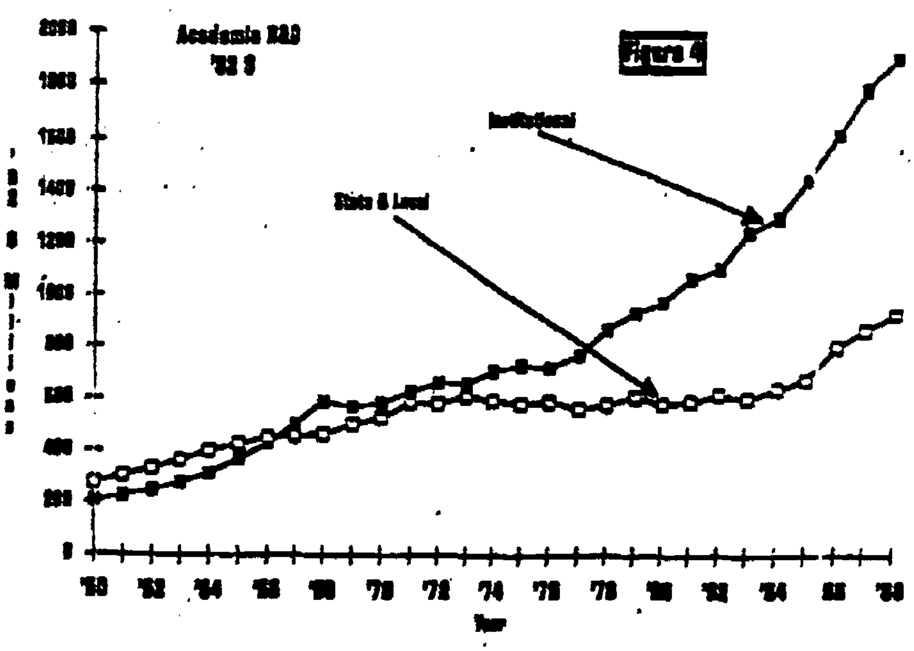
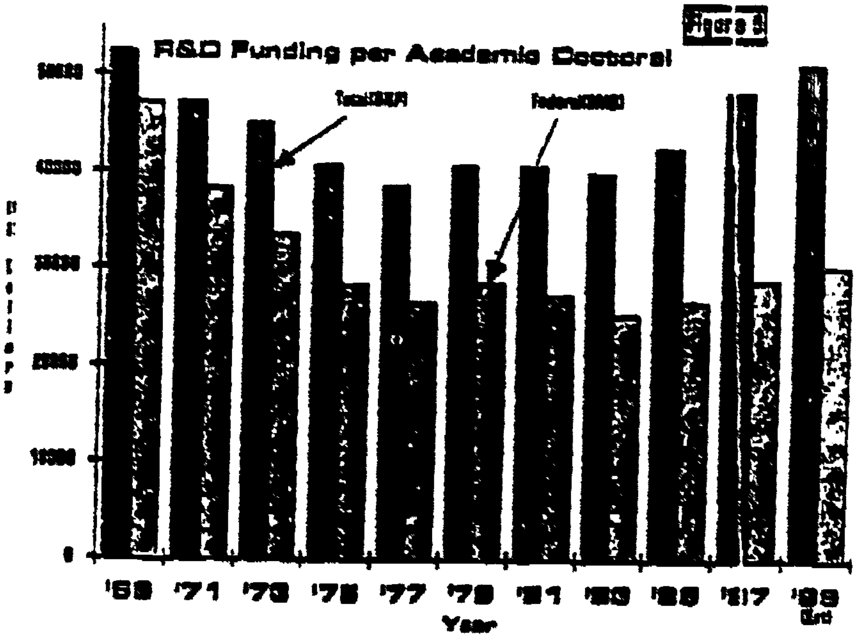
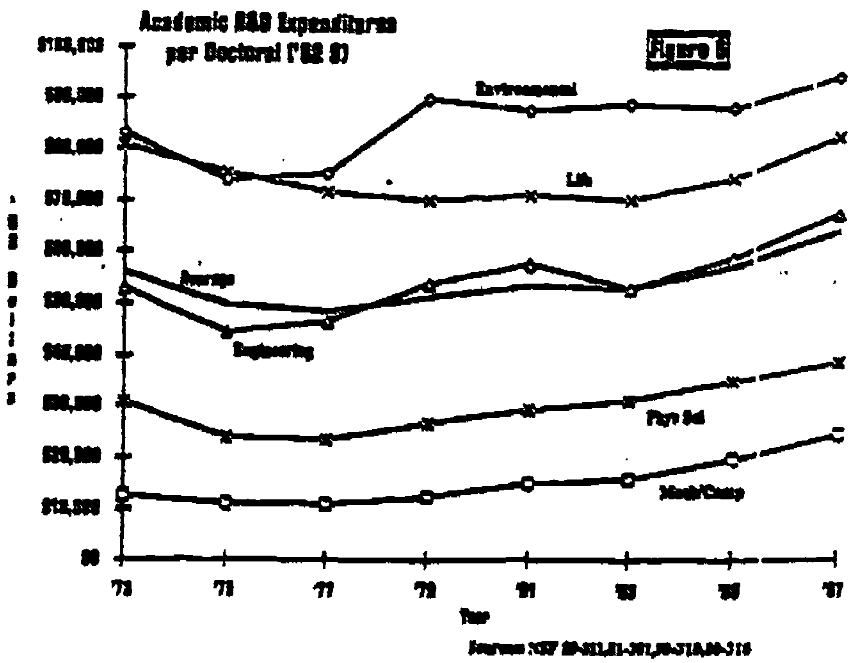


Figure 4
Institutional
State & Local
Year


R&D Funding per Academic Doctoral
Total
Federal
'69
'71
'73
'75
'77
'79
'81
'83
'85
'87
'89
Est
Year


Academic R&D Expenditures
per Doctoral ('82 $)
Environmental
Life
Average
Engineering
Phys Sci
Math/Comp
Year

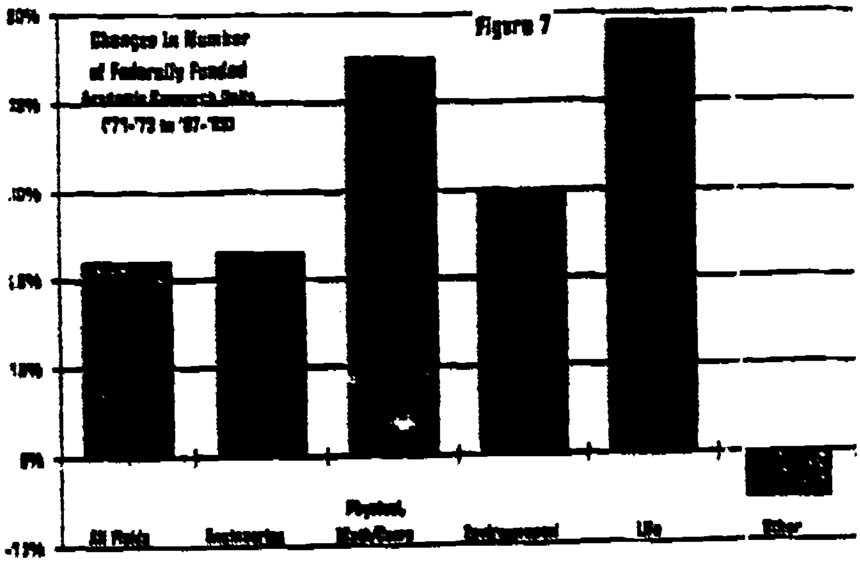
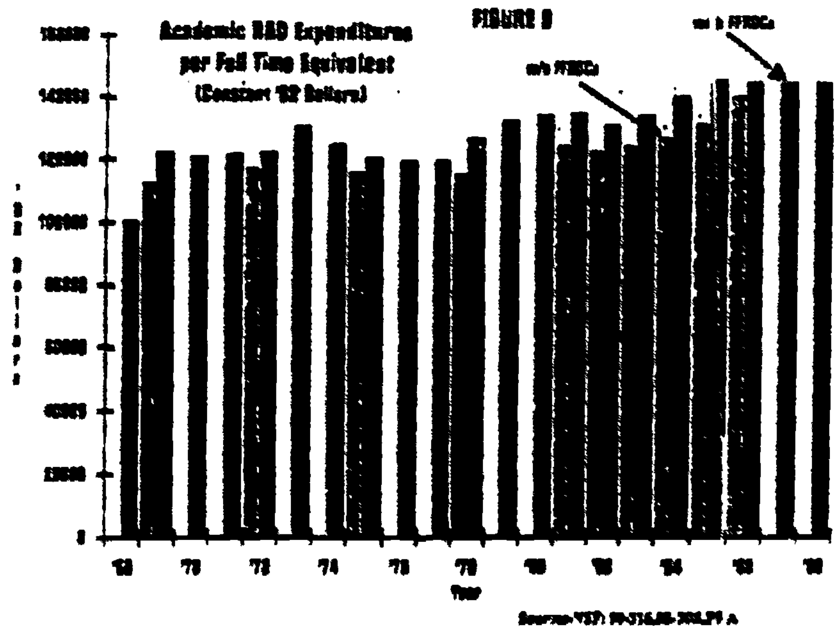


Figure 7
Life
Other


Academic R&D Expenditures
per Full Time Equivalent
(Constant '82 Dollars)
FIGURE 9
Year

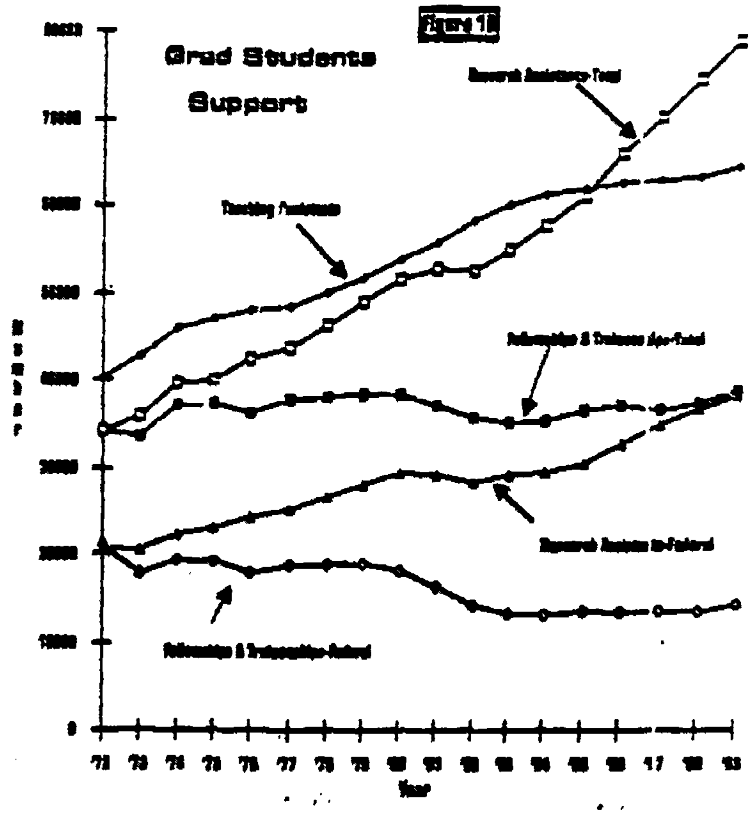
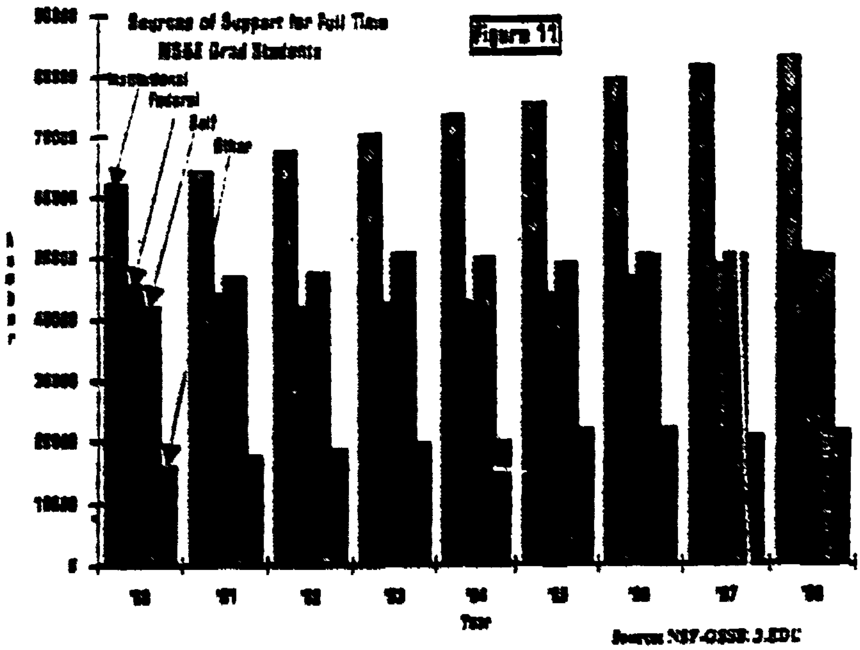


Figure 10
Grad Students
Support
Teaching Assistants
Year


Sources of Support for Full Time
NS&E Grad Students
Figure 11
Institutional
Federal
Self
Other
Year

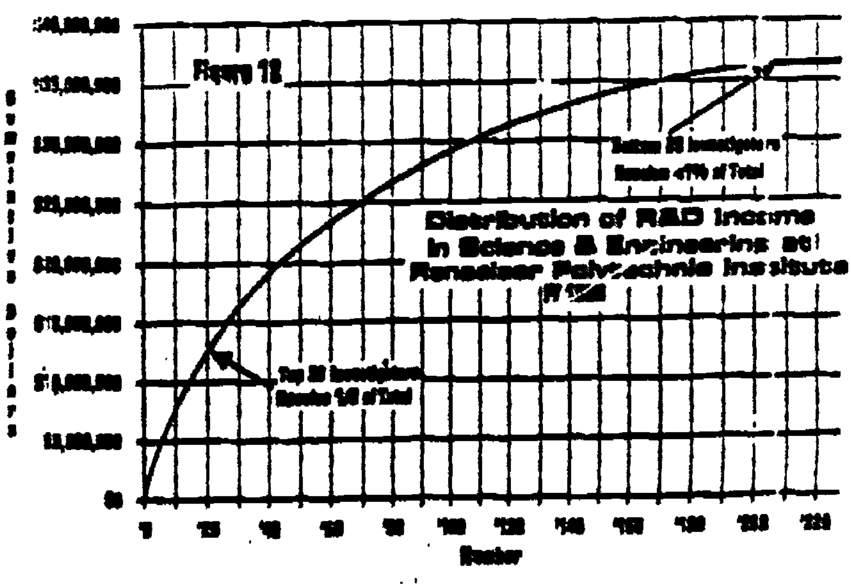


Figure 12
Distribution of R&D Income
in Science & Engineering at
Rensselaer Polytechnic Institute
Number

Mr. BOUCHER. Thank you, Dr. Schmitt. We'll withhold questions until the other witnesses have presented their oral summaries.

Dr. Lederman, we'll be happy to hear from you.

STATEMENT OF DR. LEON LEDERMAN, PRESIDENT, AMERICAN ASSOCIATION FOR ADVANCEMENT OF SCIENCE

Dr. LEDERMAN. Thank you, Mr. Chairman.

I'll try to stick to the charge that we were given in this and concentrate really on the OTA report where I find, from my dominant impression, one omission and two places where I thought the discussion was weak.

The omission has to do with evaluating the health of the research system. In several places, we are reassured that "This structure has sustained the largest and most productive research capability in the world," and yet a 1960s assessment of United States leadership in autos, tires, steels, machine tools, consumer electronics, and so on would have generated similarly encouraging bravos but look where we are now.

In our research enterprise, there have been over the years—and it's referenced in the OTA report—symptoms of trouble. The failure to pay attention to those symptoms is, in my view, an omission of the report.

The weaknesses are related to discussions of the scientific manpower and to the understanding of the cost of research.

In my critique, I'd like to discuss research and education together. Including education is important because the intimate connection between the two is useful as a metaphor or historical lesson that this subcommittee might find interesting.

My mindset in discussing the OTA study is a rather long range one as befits any discussion of research or education. Both enterprises, interwoven one into the other, involve complex infrastructures which have enormous inertia with respect to change.

I confess to a vision which is driven by what I think are logical arguments to predict that by the end of the decade, there will be a dramatic change in the level of federally- funded research and federally-funded science and math education. My prediction is that whereas research science, as in the OTA graph, is now at .8 percent of the Federal budget. I predict that sometime by the end of this decade it will exceed 2 percent or 3 percent and be climbing rapidly. I know this and I believe that many committee members know this because it is becoming increasingly clear that we, as a Nation, have been underinvesting in human resources—human resources, the national stock of brains and brawn that will be needed to maintain and enhance our economic well-being address the science and technology parts of environmental degradation, clean and inexpensive energy, affordable health care, unmet social needs and our decaying cities and poor rural areas, and our responsibilities collectively with our industrial allies to address the so-called north/south inequities and standard of living.

I maintain the stock of basic knowledge to carry out this program and to simultaneously provide the ever-increasing level of fulfillment demanded by our citizens is insufficient in 1991.

If you think this vision requires general removal by white-coated attendants, let's recall that in 1983, the science/education budgets were hovering at the few tens of millions of dollars. I think at one point, almost zero. Today, the NSF alone is spending over $300 million on science education, not to mention the large Eisenhower Program at the Department of Education.

The confidence I have that the Federal Government will rearrange its priorities in favor of nurturing the human resources does not mean that we should be complacent. I confess to a complete uncertainty as to how we get from here, from our preoccupation with intense short-term problems, the deficit, the recession, the savings and loans, so-called limited resources, to the correct balance between operations and investment.

I worry if we don't address the issue omitted from the OTA report, we may find our science infrastructure going the way of our educational infrastructure. Somehow while so many of us were concerned about other things, our educational system began to crumble and we became a Nation at risk.

There is a metaphor and a lesson. For almost a decade now, with increasing investment, we are vainly trying to turn education around, but so far it is not at all clear that things are really moving. The only way to realize the President's worthy but romantic goal of being number one in math and science by the year 2000 is to find a way to sabotage the other nations.

The lesson is that these massive infrastructures are easy to destroy and incredibly difficult to repair. The symptoms of trouble in the research community are alarming enough to be given extremely serious attention like the canary in the mine. Signs indicate that there is a danger present.

I'm aware of the difficulties of obtaining a rational objective assessment of the health of the scientific enterprise and I understand why the OTA finessed this issue. On the other hand, it's a complicated problem but has to be done. Data on the state of unhappiness of scientists is admittedly one subjective indication which convinces me that there's a problem but it may not convince the Congress. There are other mechanisms, measurements of publications, citations, prizes, movement of scientists, the attraction to science of students, economic activity of our high tech industries and doubtless many other indicators can be assembled and monitored.

I believe this needs to be done if we are not to find ourselves totally unable to cope with the problems I listed earlier. Therefore, I would urge the Subcommittee to ask the OTA staff to devise a system for measuring and monitoring this very elusive quantity.

Let me go to the shortages. OTA expressed skepticism about the projections of huge shortages in Ph.D. scientists and engineers are expected within the early years of the next decade. The data come from demographics and from very modest projections of economic growth with more or less the same level of science and technology activity we have today.

Reviewing this, I find the projected shortage predictions fairly persuasive and the OTA rebuttal rather weak. I think that it's prudent to take these projections seriously. Suppose, in fact, the projections are correct and even possibly underestimated because none of the NSF projections include the new burden on science and technology that follow from a global economy and global obligations.

Today, we do not hear complaints that there are too many teachers. Let's discourage young people from going into teaching. Nor will the question of too many scientists or too many engineers ever be raised in Japan, for example. Once we have a crisis, it will take decades to restore the interest in science and engineering in the high schools and colleges.

Granted that scientists are incorrigibly unhappy. It has never spilled over to the young, best entrants and to the graduate students and this is what's happening now. Once the word gets out, the pipeline will be empty and we as a Nation will be at risk. I think here again the demographic problem, the pipeline issue, including immigration flow, require a system of in place assessments and monitoring.

If in fact our science infrastructure is showing signs of crumbling, then the effort of many of us to bring minorities and women into the science limited work force, which the OTA report strongly favors, may be compared to the famous Pied Piper of Hamlin.

Finally, the cost of doing research—and here to sort of abbreviate my comments, I'd like to say that the OTA discussion, in my view, just grazes the key point of what is called the complexity factor. Let's put aside the issues of the inflation and where we are now compared to say the good old days.

Having solved the problem of the 1970s in any scientific discipline such as properties of Type II superconductors or the properties of quarks inside nuclear matter, one finds in 1980, a new level of sophistication and much more powerful apparatus is essential to continue the research.

Because of the ever deeper level of subtlety that the problems present us with which still have to be solved, advances in technology and knowledge are enabling, yes, as the OTA pointed out, but what is missing is a simple choice, either you increase your power of observation with appar.. us and people, or you quit because you're incapable of making progress.

OTA's emphasis on the competition factor enters in the sense that if scientists are better supported elsewhere, the use of inadequate resources is even more pointless. I stress this because the Congress should understand that science must grow in real costs because of this complexity factor. The opening of new fields, which comes upon us, is an additional pressure to grow. The expansion and number of scientists is gratefully absorbed in larger groups to handle more complex apparatus in developing the new fields and expanding the geographical base of our research system.

Let me summarize my comments. The OTA report is an excellent beginning and cogently addresses numerous issues— priorities, data collection, accountability, criteria which must supplement scientific merit and so on. The Congress and the Nation must look beyond these to the issue of how much science and how many scientists does the Nation need, realizing that the structural inertia in the sciences residing in the Congress, the funding agencies, the universities themselves, and the concurrent inertia in our educational system must be taken into account in any long range policy decisions.

This leaves the questions of the health of science, how this influences the realistic mechanisms for assuring an adequate flow of science literates, and the continuous efforts to understand the dynamics of evolving research needs.

I see as a minimum full and continuous employment of the OTA staff.

Thank you.

[The prepared statement of Dr. Lederman follows:]

Testimony
Committee on Science, Space and Technology
Subcommittee on Science
Leon M. Lederman
March 20, 1991

Mr. Chairman,

Before I begin my remarks specific to the OTA report, I wish to emphasize that I speak only for myself as a private citizen, but as a private citizen who has spent 45 years in the business of doing science, of teaching science, of administering a university laboratory of 200 people and managing the Fermi National Accelerator Laboratory with 2000 people. More recently I am back in the university, teaching undergraduate liberal arts students, trying to do some research with one post-doc and some summer undergraduates, and also trying to do something about science and math education in the great urban center we call Chicago.

The task OTA set out is of extreme importance to the nation: to outline the prospects for scientific research in the next decade and to suggest Congressional actions which would be designed to strengthen the Federal role in guiding, sustaining and managing (in OTA words) the research system.

My critique of the excellent OTA report will concentrate on what I perceive to be an omission and on two places where the discussion, in my view, is weak. The omission has to do with evaluating the health of the research system. In several places we are reassured that "...this structure has sustained the largest and most productive research capability in the world" (page 50). A 1960's assessment of U.S. leadership in autos, tires, steels, machine tools, consumer electronics, etc. would have generated similarly encouraging bravos. Yet there have been, over the years and referenced in the OTA report, symptoms of trouble. The failure to pay attention to those symptoms is, in my view, an omission. The weaknesses are related to the discussion of scientific manpower and to understanding the costs of research.

By selecting the omissions and perceived weak points of the OTA draft report I do not mean to criticize the entire report, which contains many valuable analyses and recommendations. The OTA staff has suffered many "full and frank" discussions with its totally uninhibited advisory panel (of which I am a member), and many of the panel's criticisms have been included in the report. However, to be responsive to the subcommittee's charge, I will discuss the omission and the two weaknesses (at least as I perceive them) in the OTA draft.

In my critique, I would like to discuss research and education together. Including education is important because of the intimate connection between the two and is useful as a metaphor or historical lesson that this subcommittee may find interesting.

The Health of American Research

My mind-set in discussing the OTA study is a rather long-range one as befits any discussion of research and/or education. Both enterprises, interwoven one with the other, involve complex infrastructures which have enormous inertia with respect to change. I confess to a vision driven by logical arguments which convince me that we will see, probably towards the end of the decade, a dramatic change in the level of federally funded research and federally funded science and math education: I predict that science, now 0.8% of the federal budget according to OTA, will exceed 3% and be climbing rapidly by the year 2000 or so. I know this and I believe many of your Committee members know this because it is becoming increasingly clear that we, as a nation, have been underinvesting in our human resources. This is the national stock of brains and brawn that we will need to maintain and enhance our economic well-being, that will address the S & T issues of environmental degradation, of clean and inexpensive energy, of affordable health care, of the unmet social needs in our decaying cities and poor rural areas, and in our responsibilities, collectively with our industrial allies, to address the so-called North-South inequities in standard-of-living. The stock of basic knowledge to carry out this program and to simultaneously provide the ever-increasing level of fulfillment demanded by our citizens is obviously insufficient in 1991.

If you think this vision requires my gentle removal by white-coated attendants, let me remind you that in 1983, the science education budgets were hovering at a few tens of millions of dollars and today the NSF alone is spending over 300 million dollars, not to mention the Eisenhower program at the Department of Education.

The confidence I have that the Federal government will rearrange its priorities in favor of nurturing its human resources does not mean that we should be complacent. I confess to complete uncertainty as to how to get from here—from our preoccupation with intense short-term problems such as the recession, the deficit, the S & Ls—to there, the right balance between "operations" and "investment."

I worry that if we don't address the issue omitted from the OTA report, we may find our science infrastructure going the way of our educational infrastructure. Somehow, while so many of us were concerned about other things, our educational system began to crumble and we became a nation at risk. Here is the metaphor and the lesson. For almost a decade now, with increasing investment, we are vainly trying to turn things around but, so far, it is not at all clear things are really moving. The only way to realize the President's worthy but romantic goal of being Number 1 by the year 2000 is to find a way to sabotage the other nations! The lesson is that these massive infrastructures are easy to destroy and incredibly difficult to repair. And the symptoms of trouble in the research community are alarming enough to be given extremely serious attention. Like the canary in the mine, there is danger present. Now I am aware of the difficulty of obtaining a rational, objective assessment of the health of our scientific research enterprise. I fully understand why OTA finessed this issue by the sensible preamble: "OTA finds that under almost any plausible scenario for the level of research funding in the 1990's, there are issues of planning, management, and progress towards national goals to address" (Page 10). Data on the state of unhappiness of scientists is one subjective indication which convinces me that there is a problem with the health of science but, quite understandably, may not convince the Congress. Measurements of publications, citations, prizes, movement of scientists, the attraction to science of students and doubtlessly many other indicators can be assembled and monitored. I believe this needs to be done if we are not to find ourselves inexplicably unable to cope with the problems I have listed earlier. I would therefore urge the Subcommittee to ask the OTA staff to establish a system for measuring and monitoring this very elusive quantity.

Weakness: The Issue of Projected Shortages of Scientists

OTA expresses skepticism about the projection of huge shortages in Ph.D. scientists and in engineers that are expected within the early years of the next decade.

My own reading of these projections is different since the data come from demographics and from very modest projections of economic growth with more or less the same level of science and technology activity we have today. OTA then proposes that the famous pipeline which connects our K-12 students with college and graduate schools be kept robust so that, should shortages develop, market inducements will move college students into science and graduate students into Ph.D.'s. "OTA believes there are initiatives that maintain the readiness of the educational pipeline to respond to changing demands for researchers..." (page 34). I have a lot of trouble understanding this. Good students will not invest 5 or 7 years of their life without some assurance that they will be able to engage in the research they want to do. A pipeline that then conducts them to high school teaching may be good for high schools but, more likely, it will be a much diminished pipeline. Then there are the other skeptics out there who insist we already have too many scientists and that what is needed is birth control so that fewer scientists will be better supported. This is again a crucial issue that we must all understand better.

But suppose the projections are correct, even greatly underestimated. (None of the NSF projections include the new burdens on science and technology that follow from our global economy and our global obligations.) Today, we do not hear the complaint that "...there are too many teachers, let's discourage young people from going into teaching." Nor would the question of "too many scientists" or "too many engineers" ever be raised in Japan, for example. Once we have a crisis, it will take decades to restore the interest in science and engineering in the high schools and colleges.

Given that scientists are incorrigibly unhappy, it has never before, in my experience, spilled over to the best young entrants and to the graduate students; and this is what is happening now. Once the word gets out, the pipeline will empty (market forces) and we, as a nation, will be at risk. I think here again, the demographic problem, the pipeline issue, the immigration flow require a system in-place of assessment and monitoring.

If, in fact, our science infrastructure is showing signs of crumbling, then the efforts of many of us to bring minorities and women into the science-literate work force (which OTA strongly favors) may be compared to the Pied Piper of Hamelin.

Weakness: The Costs of Doing Research

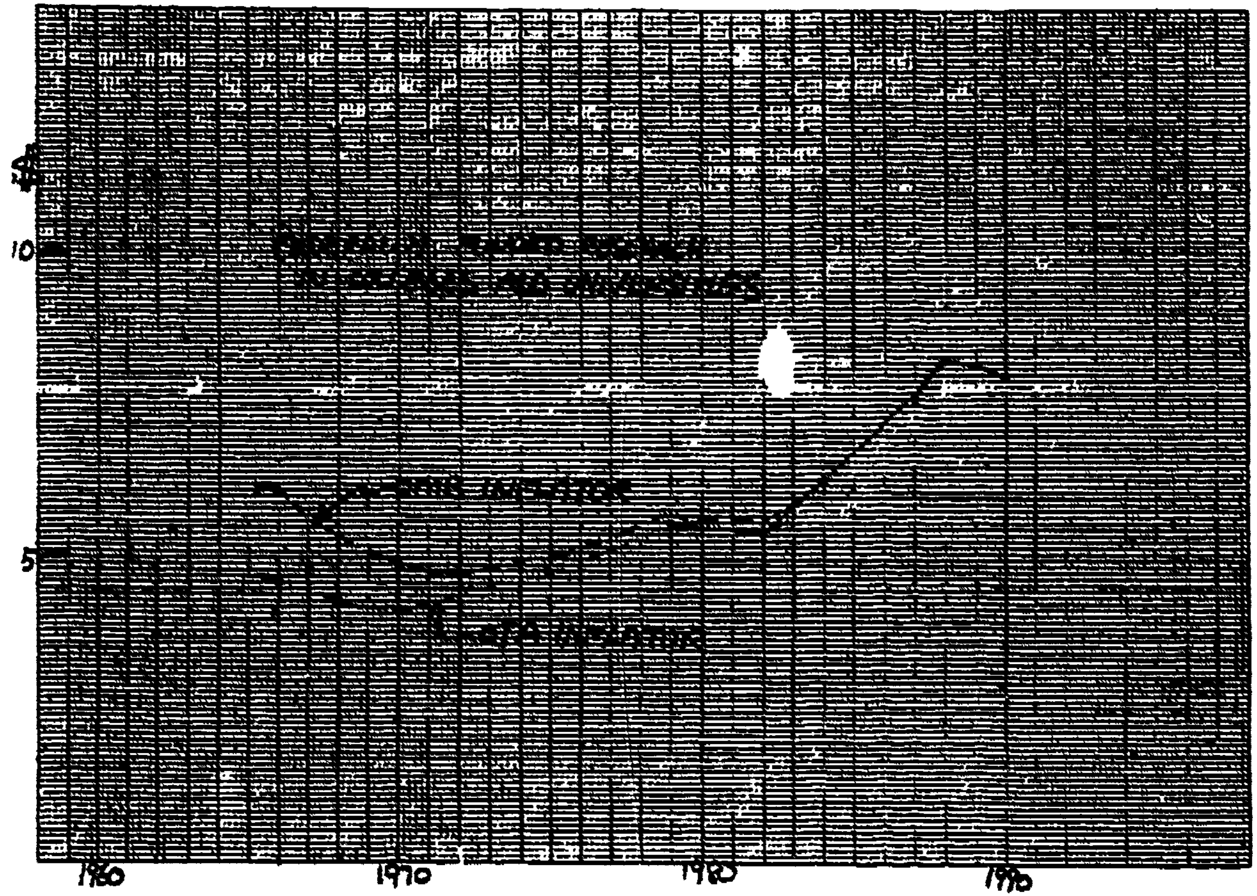
Here it would be useful to redo the OTA figures on the history of funding, using deflators that are more appropriate to the S & T enterprise, developed by OMB and published in Science and Engineering Indicators. I have appended a graph with two curves: the OTA Figure 1-5, and the same data using the OMB deflator. Although the differences seem small, they go a long way toward explaining why so many of our science policy leaders (Bromley, Bloch, Press...) say we are underinvesting. For example, OTA says "From 1969 to 1990, Federal funding for research at universities and colleges grew from over $4 billion to nearly $8 billion (in constant 1990 dollars)" (page 8).

My graph and my quote would read: From 1968 to 1990, the growth has been from $6.9 billion to $8.0 billion. This inflation difference is merely "market basket" and salary increases as experienced by institutions which buy electronics and hire scientists and engineers rather than the average "CPI" type of inflation.

OTA (page 20) points out that "...advances in technology and knowledge are 'enabling'; they allow deeper probing of more complex problems. This is an intrinsic challenge of research." OTA then discusses the role of competition in driving up the cost of research.

This discussion, in my view, misses the key point about what is often called the "complexity" factor. Having solved the "1970" problems in any scientific discipline, such as the properties of Type II superconductors or the properties of the quarks inside nuclear matter, one finds, in 1980, that a new level of sophistication and a much more powerful apparatus is essential to continue the research--in these two cases, for instance, a more intense die laser or a more powerful particle accelerator. The driving force for the increase in costs is the deeper level of subtlety of the problems still to be solved. Advances in technology and knowledge are "enabling" yes, but what is missing is the choice--either you increase your power of observation (with apparatus and people) or you quit because you are incapable of making progress. OTA's competition factor enters in the sense that if scientists are better supported elsewhere, the use of inadequate resources is more pointless. I stress this because the Congress should understand that science must grow in real costs because of this factor. The opening of new fields is additional pressure to grow. The expansion of the number of scientists is gratefully absorbed in larger groups to handle the more complex apparatus, in developing the new fields, and in expanding the geographic base of our research system. These pressures and cost increases are difficult to quantify but the experience of the past 20 years (1968-1988) indicates a growth pressure of something like 8% per year. If we believe this, we can understand the decline in our capability to do scientific research in the universities now as compared to the late 1960's. The real increases are at the 35% level, the needs more like 300-400%!

Let me summarize my comments. The OTA report is an excellent beginning and cogently addresses numerous issues: priorities, data collection, accountability, criteria which must supplement scientific merit, etc. The Congress and the nation must look beyond these to the issues of "how much science?" and "how many scientists?" does the nation need, realizing that the structural inertia in science (residing in the Congress, in the funding agencies, in the research universities) and the concurrent inertia in our educational system must be taken into account in any long-range policy decisions. This leads to questions of the health of science, how this influences the realistic mechanisms for assuring an adequate flow of science literates, and continuous efforts to understand the dynamics of evolving research needs. I see, as a minimum, full and continuous employment of OTA staff.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A BRIEF FOR FERMILAB

Leon M. Lederman
March 13, 1991

Fermilab is now the highest energy accelerator in the world and it will remain at this frontier until the supercolliders, the SSC in Texas and the LHC in Europe's CERN laboratory, come on the air by about the year 2000. Since proton machines traditionally take several years to begin to produce useful data, the hegemony of Fermilab will extend well into the next century.

The 25 year history of Fermilab is characterized by exuberance and success. The first phase (1972-82) achieved twice the energy specified in the design with twice the number of beam lines, on schedule and at a cost of $10M less than appropriated.

The second phase yielded the successful operation of the world's first superconducting synchrotron, raising the energy from 400 Billion-volts to 900 Billion-volts while reducing energy consumption from 60 megawatts to less than 20 MW. This phase saw the initiation of proton-antiproton colliding beams and the operation of the "CDF" particle detector, probably the most sophisticated system of accelerator and detector art ever achieved. Some 1400 experimenters from 100 U.S. universities and laboratories and institutions in 17 foreign countries have chosen Fermilab for their research facility.

The third phase, begun in 1989, involves a dramatic upgrading of the collision rates in order to fully exploit the Laboratory's leading role for the next decade (1991-2001). It is motivated by crises in the progress of our understanding of the nature of fundamental particles (e.g. the top quark) as well as the need to solidify the knowledge base. It is reasonable to expect that the scientific results, the technological advances and the experience of the Fermilab users will all be crucial to the successful exploitation of the SSC.

The third phase has the concomitant goal of providing the Laboratory with an important facility for addressing crucial issues in particle physics away from the high energy frontier which will pass to SSC and LHC. In particular, the Main Injector, designed to provide unequalled collision rates for the Fermilab collider, will also provide beams of superb properties for addressing two general categories of problems which are very unlikely to be addressed by SSC. One has to do with intense beams of neutral kaons which will enable studies of the enigma of "CP" symmetry-violating processes (addressed by cosmologists as the "origin of matter" problem) at a level of a hundred times greater sensitivity than is now possible. The other facility is super intense beams of neutrinos to elucidate both the long-standing puzzle of the behavior of neutrinos from the sun and the nature of the gravitational "dark matter" that permeates the universe.

Thus, one can easily foresee, a fifteen year period of leadership and productivity for a Laboratory that has achieved a spectacular scientific and technological track record. The history of machines is replete with surprises which could well provide Fermilab with additional scientific missions.

The advances to which Fermilab contributes is part of the investment in basic research, the human knowledge resource from which, if history is any guide, the nation will draw upon for decades to come. Fermilab's technology in such diverse areas as superconducting systems, medical accelerators and instrumentation has already paid back a significant part of the investment made here by the Federal Government. The Lab^^atory has advanced its science while, at the same time, setting ne tandards in its attention to educational outreach, to ecological concerns with its created wetlands and massive prairie restoration, to unique medical applications, to its world famous architecture and its concern for the blending of science with the visual and performing arts. Surely this absolutely unique institution is a place of pride for the nation.

Mr. BOUCHER. Thank you, Dr. Lederman.
Dr. Roy?

STATEMENT OF DR. RUSTUM ROY, EVAN PUGH PROFESSOR OF THE SOLID STATE, PENNSYLVANIA STATE UNIVERSITY

Dr. ROY. Mr. Chairman, first, as a working scientist, I write proposals. I wrote about 35 of them last year and I hustle bucks from all kinds of people.

I come here though as a citizen, not as a scientist. I'm concerned about the health of federally funded research, not about whether I can make a buck easier.

I want to really commend OTA for the usual solid work which they've done in this report. I mean, they always do it right. The trouble is they've got to take a politic attitude. You saw in the answers to your question that they have to hedge their statements. Well, witnesses don't have to hedge their statements.

Let me start with the first very good thing they did. On the issue number three, education and human resources, they really did a splendid job. I think they've put to rest this nonsense about shortages. You can't talk about the shortage until you know the demand side. There is an oversupply of scientists today. You can get a postdoc for $25,000 anytime you want them, gross oversupply.

Furthermore, their research has been supplemented by the American Association for Engineering Societies, the National Research Council, and the NAE President, all of them support the position—no shortage, no likely shortage—we've got the mechanisms to take care of it.

However, in science education, what OTA failed to do was to point to Congress' own priorities. The Kennedy-Hatfield Resolution says, first priority, making a technically literate citizenry; second priority, increasing more scientists and better ones. I believe that if you're serious about your priorities—and I believe that is the secret of education for the minorities, the women, the whole resource pile—is technological literacy, Mr. Chairman, you won't get any action unless you put it in the language of the bill. If it says 50 percent of the money has got to go for the technological literacy, you'll get it; otherwise, you won't.

Now I want to talk about the issues that OTA did not address. In your letter, you asked us to comment on whether the report provides a comprehensive discussion of the full range of issues. It didn't, and I think for good reason. They were not given that big a charge perhaps. I think the worse thing is that the context of research was ignored.

There is no such thing as research in academia. It is a system: science, technology society. I want them to put that thing up because I want to show you competition, Mr. Chairman. We don't live in a simple world. I'll show you the Prime Minister of Japan sitting next to his advisory group, the science technology agency, to which I am an advisor now and then, and see what they say.

They say the following things. "First, we must look at the harmony between science, technology and mankind and society." If you don't look at that system, it simply won't work. I think Mr. Boehlert was asking that same question, Mr. Packard was asking that question. We've got to take a look at the whole system. That's one thing they missed, a systems look.

Second one they missed is that our theory on science policy for 40 years has been wrong. If you say science leads to applied science leads to technology, I've got a lot of bridges to sell you. If people still believe that stuff, the white coats should take them out. It doesn't. It's technology that leads to science. It drives the system. It gives us the economic base on which it will go.

Again, I think that in your little overheads I handed you, this committee asked the CRS to do a study of the relationship between the Nobel prizewinning and prosperity. It was an inverse relationship. I'm a scientist. I believe in data, and I think it's very interesting to see that no scientist refers to the data when they try to connect science, abstract science, to winning prosperity.

Mr. BOUCHER. Dr. Roy, I'm going to interrupt you for just a second and advise some of the folks sitting at the table just over here that there's a screen about to come down and they might want to move.

Dr. ROY. I thought I'd inject a little competition again. There is the Prime Minister of Japan again facing us and you notice that in this report which is February 1991, no later than that, no earlier than that, it says, "Our two goals"—and I point specifically to the two goals— "achieve harmony, science, technology and society," if they are not embedded in each other, and the second one, "improve the efficiency of intellectual production activities."

Our godforsaken system is so inefficient in the universities that what Dr. Schmitt said, I associate myself with his remarks. We've got a lot of room for improving our efficiencies.

Now, I want to go back to what else they missed. First one, no one reads the literature. We had a meeting in Arizona last month on looking at the different national systems. Everyone agreed, no United States scientists anymore—not no—but most-people don't refer to the literature.

Mr. BOUCHER. Dr. Roy, is that the entire use of the screen that you'll need?

Dr. ROY. Well, since we've gone through some of them, that's the use of the screen. Thank you Mr. Chairman. I was going to go through them all. You know scientists always use overheads.

You could again improve the efficiency, Mr. Chairman, with one sentence. You can do more today by saying in the appropriations bill: "To assure that any recipient of Federal grants files a record of their having thoroughly read the literature, that's all, upon penalty of revocation of grant." Nobody is reading it. The biggest mine of knowledge is not with the sophisticated instruments; it's with simple instruments. It's the stuff which really makes the world go around. It has got to be brought to human scale. If you don't read the literature, you're missing it. Why don't you force us to read the literature. The science community won't do it without your forcing it.

The second one is what has been referred to, the proposal peer review system, as though God sent this damned thing down with Moses. He didn't, you know—or she didn't. The fact is that we've got many systems—the ONR system. Weinberg proposed an overhead system to free the Nation's scientists from big brother. I proposed detailed systems. The OTA didn't even comment on new options for funding. I think they should have.

The second thing they failed to do is to give you a handle between two kinds of science. "2S science", science for society and "3S science", science for self and the institution of science. They're both good things, but one is a consumption good, the second one 3S science, and the first one is an investment good. 3S science returns zero; I mean mathematical zero, to the nation that provides the money. Of course it provides it for everybody, not that it does either good, but it doesn't connect with the particular nation that supplies the money. Those 3S sciences should be justified as a vehicle for education or as a cultural activity. It should not become an entitlement.

I'm afraid that the national science community somehow thinks if you've got a Ph.D., you should send me money from Washington. I think that's got to be stopped quickly. Otherwise we'll be eaten up by that monster.

The OTA report also says our biggest failure is not to refer to the state of industrial research. Mr. Chairman, I am crying because the greatest institutions of American research are being shut down. Not one university is hurting that way, but Bell Labs, GE where this gentleman ran that lab. That was the greatest materials lab in the world, Bell Labs is the greatest research institution in the world. You'd better be concerned about that, not about universities. They can look after themselves. They are not doing too badly, thank you.

The fact is that we are not paying attention to the place where research counts, in long-term research in industry. Somehow we've got to get it there in some new system, maybe even divert it if necessary, if push comes to shove, from universities to industry.

What are my suggestions? Limit growth of S3 and S2 to COLA, cost of living; require agencies to make more efficient distribution systems; defer things like the supercollider for 10 years; and charge the OTA to come up with three alternatives uses for the same money.

What Mr. Boehlert was asking about, I tell my students in science policy, never compare something with nothing. Give us three options and have a peer review, Mr. Chairman. The scientist is always crying about peer review. The total National Academy should be the peers. Ask what IRI did, ask the total Academies to rank them, where of these three or four options would you put your money for the health of the Nation and its science? I think you'll come up with a very useful way of getting some guidance.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Roy follows:]

FEDERALLY FUNDED RESEARCH
DECISIONS FOR A DECADE

Testimony before the

U.S. House of Representatives

Committee on Science Research and Technology

by

Professor Rustum Roy
The Pennsylvania State University

March 20, 1991

PERSPECTIVE OF THE WITNESS

Mr. Chairman,

I am honored to be invited to testify before this Committee which in my view has a better grasp on the key issues of U.S. policy in S/T and R/D matters than any other.

I am an atypical witness at such hearings because I remain a working scientist (the 9 a.m.-11 p.m. type) who writes proposals, gets support from six different agencies and twenty companies, visits Washington twice a month, and rides herd on a research group. At the same time I also do science policy analysis and give advice at the state, federal and international level. I lecture regularly in some the worlds best labs and have specialized in Japanese R&D, since in so many fields it is they who now do and they who will increasingly determine the future of the world's science and technology.

I am also atypical in that, my own field is interdisciplinary materials research—which happens to be the cynosure of R&D planners today. "Materials" is the kind of REAL science which 90% of the citizens think of when they hear the word "science," and they even got a reasonably accurately picture when it is described. It shares this with other REAL sciences—agriculture, earth, health, engineering—which humans can touch and feel. All citizens are deeply interested in, aware of, and can relate to these REAL sciences. Indeed, most R&D especially the more esoteric science is sold to the public by making connections, however flimsy to the results and products of these REAL sciences. These applied sciences are directly connectable to human scale and human concerns.

REAL sciences can be contrasted to typical ABSTRACT sciences—physics, chemistry, biology—which by a most unfortunate set of circumstances after WWII became equated to "science." Especially and almost uniquely in the U.S. (within the developed world) R&D policymaking has long been dominated by members of the abstract science community, who are technically the least qualified to do so.

The consequences of this highly selective application of the word science are a gross distortion of national perspective. While the value and significance to the economy, health, welfare of the country is dominated by the REAL sciences the R & D policy is dominated by abstract science. To take an analogy, from the nation's "sports-economy": these applied sciences represent the major sports: baseball, football, basketball, etc.. On that scale some abstract sciences such as particle physics and radio astronomy would lie somewhere between tiddlywinks and Chinese checkers in their contribution to the nation's well-being. Yet the policy apparatus has been diverted to concern about the U.S. tiddlywinks capacity.

It is the applied or real sciences which connect the world of federally funded research to the national economy to the environmental concerns of the citizens, and their myriad of health issues. My comments on the OTA report are made from this perspective of the real science community.

Summary Comments on OTA Document

I start by noting that the document is characterized by a thoroughness and attention to detail that has characterized OTA from the beginning. It is very reliable in what it says. My comments, as will be seen, are not criticisms of what is there, but a look at what is not there.

Mr. Chairman, your letter of invitation asks the witnesses to *"comment on whether the report provides a comprehensive discussion of the full range of issues which require public policy attention in the coming decade."* I take that as my first point of departure.

I come before you as a citizen of this nation concerned about the totality of its well-being—not as a scientist pleading a case for my community. I think you should be, as I am, more concerne about the health and welfare of 200 hospital workers in Philipsburg, PA, who have just lost their jobs, or the electronics plant in Buffalo, NY, where the production has been moved to Mexico, than about the marginally increased difficulty of getting research funds for young or old faculty. No one promised me or each of today's new Ph.D.'s a rose garden or a permanent federal entitlement.

The OTA report in substantial part focusses only on the academic community—which in my view is the segment of the national research community least important to the nation's well-being. All this may sound a little heretical to many scientists, and perhaps to many of the members. I mean quite literally "heretical." Because, as I said in a sermon in one of Washington's most prominent churches a month ago, "Science has become America's theology, technology its religion." That is very dangerous both to science and to the nation. Hence if someone, especially a prominent scientist tries to demystify science, take some of the hoopla and buncombe out of it, if he argues the case with data that academic science is neither particularly significant in the totality of our national life, listeners are shocked. When he claims, again showing data, that for the next decade or two most scientific advances will result in further economic setbacks to the U.S. economy, the listeners react as if one were blaspheming. I developed that case before this committee in the last Congress: it was picked up nationally because it was so new as an idea. The origin of this is that over the last decades some parts of the science community have seduced the powerful elite—including members of Congress—by offering magic to heal the nation's problems. And it has cowed the non-scientific intelligentsia into silence by its use of equations and technical jargon, into not challenging the argument.

Do I exaggerate? Is this mere rhetoric? You decide. Here is an excerpt from the pen of the editor of *Science*, the flagship journal of American science.

> A Department of Science could be useful if it is devoted to untidiness and evangelism. It could serve as a catalytic force for increasing scientific research and generating scientific approaches in all phases of our society and our governmental structures. It could send out its missionaries to bring the gospel of basic research to the heathen in the outer darkness. *Science 227* (8 February 1985).

My first point then is that the report, as well done as it is, stays wholly within the frame of reference of the academic science community. In other words, the report starts and ends within the traditional categories, language and methodologies of the R&D policy analysis of the sixties and seventies in the period of U.S. technological hegemony. The authors seem to be unaware—as is the vast majority of the U.S. citizenry and its science community—that the sun has set on that empire.

Mr. Chairman, in your letter to your fellow members you indicate that witnesses will be asked if the report "covers all of the subjects which should be given attention to in the public policy arena." It is that request which I address first.

The document simply does not think BIG enough, or BOLDLY enough. Just let me compare the tone of the document to a similar document which arrived from Japan this week. The Science and Technology Agency of Japan publishes a newsletter. Prime Minister Kaifu's Council for Science and Technology on January 22, 1991 used very different language in tackling R&D policy for the decade.

They write, *"Recently problems have arisen that are difficult to solve by traditional approaches. These include (a) achieving harmony between science and technology and mankind and society, and(b) improving the efficiency of intellectual production activities."* (a and b added.)

The context of research in the OTA study is confined to "research," not even development or technology, not the economy, and society is totally ignored.

Harmony, synergism within the team is not even mentioned. This is absurd. The Japanese have taken to heart (as they did with Deming's advice 40 years ago) what many of us American professors have been saying to them: "science can only be healthy in a healthy technology which in turn must fit comfortably into a healthy society."

The Bible says *"This ye should have done ... but not neglected the other also."* The solid, analytical work on R&D policy is all there in the OTA report, but the new ideas being discussed among the nation's leading S&T participant managers are significantly absent.

Mr. Chairman, the Chair of the whole SRT Committee, the Honorable George Brown, in the commentary appended to my book with Deborah Shapley (Lost at the Frontier, p. 171) makes the point that the OTA report ignored. He quoted from Vannevar Bush's Science: The Endless Frontier, what most scientists conveniently omit: *"It (science) can be effective in the national welfare only as a member of a team."*

Let me reinforce the absolutely vital point about the fact that science is only one member of a team by a very accurate football analogy—which befits someone who has spent 45 years at a great football school.

Science is like the quarterback on a football squad. By itself the QB cannot win football games, although he often gets the credit for the team's wins. But a team with one (or even two excellent backup) quarterback is totally ineffective if there are no good blockers, no pass receivers, no running backs. And a coach, having a losing season with such a team, who comes to the boss and asks for yet another quarterback would surely be laughed out of office. The OTA report has focussed on the best part of the American team, its basic science, the QB, when all his best passes are being intercepted by Japanese and German backs and run in for touchdowns, while the team's losses got worse every year.

I will take as my contribution to this debate the pointing up of some of these key areas where totally new critiques and new approaches are called upon in the federal funding of research, not in isolation but as a member of the national U.S. team for research receiving national funding.

SUGGESTIONS FOR IMPROVEMENT

1. The Demise of Economically Useful Research

I was astonished to find almost no mention of the dire straits of the U.S. industrial scientific/technological enterprise in a report on U.S. research.

In case Congress does not know it, the OTA should surely tell it that by far the most significant "research" in the U.S., measured by its impact on the economy, jobs, etc., is the long-term research in industry which is pulled by and linked to new/improved product development. The important fact the Congress must know is that this most important research is dead or dying in all but a half dozen U.S. companies. I must as a witness tell you that to hear from "experts" (who have not been inside the laboratories of the major Japanese corporations in the last year or two) about the health of U.S. research is to court disaster. This is no longer the sixties and seventies. The easy job of playing against no competition is over. It is now a competitive world of S&T out there, and to pretend that we can isolate "academic research" in national policy matters is ludicrous. I and a half dozen friends who are V.P.'s or senior managers of major U.S. companies occasionally meet around the breakfast table at the Tokyo Palace Hotel every few months and weep as we have seen, over the last five years, in front of our eyes, in lab visits to Japanese companies, the disappearance of the U.S. "nationally useful" research capability, and the amazing ascendancy of its Japanese counterpart. All this while academic research received an additional 30-50% (depending on details) over the decade 1978-88.

The Congress must surely be made fully aware that what we are doing in research today, the sum total of the U.S. system: universities + government + industry, is not working any more to help the economy or create stable basic sector jobs or keep us competitive in major industrial sectors.

2. The Negative Economic Value of Some Basic Research

The OTA omits a second key axiom beyond any dispute regarding the value of "basic research." This is that "basic research alone, especially in the abstract sciences has absolutely no economic value to the nation paying for it." (This excepts the very important but incidental function of training students.)

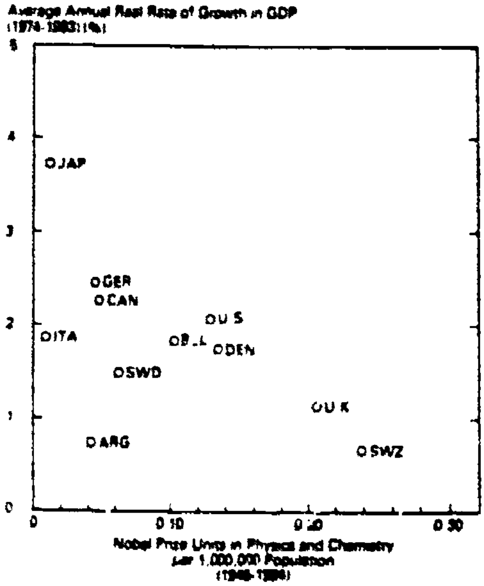
In 1985, Mr. G. A. Keyworth, Science Advisor to the President, made the following statement—the absurdity of which is self-evident on reading it: "*Basic research leads inevitably to technology and thence to prosperity*" (Physics Today, 1985). Of course, the exact opposite is true. Technology, as the great historian of science, de Solla Price of Yale observed, leads to science in most cases. Yet no committee of the National Academy pointed out the absurdity of such a statement, or had Mr. Keyworth issue a "misspeaking" demurrer. Let us look at sc ne data.

The Congress commissioned the C. R. S. to do a study to show the connection between basic research and domestic "prosperity." The amazing result is shown below. Nobel prizes are one—albeit rather poor—index of basic research prowess. What the C. R. S. report shows beyond any shadow of doubt is that the better a nation is at winning Nobel prizes in physics and chemistry, the poorer its economic performance. If one of the justifications for academic research is winning international science prizes, surely it is essential to deal with such data.

No one has challenged the C. R. S. report. When I presented it before Japan's International Technology Forum it immediately became the subject of discussion. And within six months senior Japanese policymakers were not only showing it but building further on the idea. I commend it to the members of the Congress as the central graph to keep before its eyes whenever it is tempted to fund research like the

Figure 1?

Growth in Gross Domestic Product and Nobel Prize-Winning in Physics and Chemistry



supercollider and space astronomy to win Nobel prizes. Along with each such prize, another factory will probably disappear in your district.

Two Kinds of Science. Avoiding the Linguistic Trap

In recent presentations I believe I have found a way for nontechnical policymakers in the Administration or in Congress to avoid the trap of appearing to be against "science." It is quite effective and accurate to separate two kinds of science: "2S science" or "3S science." 2S stands for Science for Society. The kind of science—however long term—which is done to meet a need of the public or private sector. 3S stands for Science for Self and the institution of Science. This is the science which each practitioner does for self-gratification, chasing curiosity, or because she or he thinks it may advance the tiny part of the science world or discipline in which he or she works. It has no identifiable value to the culture or the nation. The argument that from this kind of research comes the great miracles of science, transistors, new medicines, strong materials, is not backed by any historian. Of course such knowledge is used to make certain technologies work, but if it wasn't one kind it would be another. Even more relevant here is the fact tnat even if someone did combine such an idea with a dozen other necessary ingredients of capital, labor, infrastructure, it could be any nation in the world that got the benefit—not the one that did the research. Such is the nature of 3S science: a consumption good, not an investment good. I propose to the Congress that they clearly separate 3S science from 2S science when debating funding of research.

Alternative Mechanisms for Funding 3S (and 2S) Research

One of the most obvious failures of the U.S. academic researc'. system :s the incredible waste of human talent in getting the money to the scientists. The CTA report failed to get across to Congress the magnitude of this problem.

The horror stories of the number of proposals received in response to a DOD, DOE or NSF proposal were insufficiently analyzed. The "success rates" on occasion approach 1 in 50, even 1 in 100 in some cases. That means that the total national effort by scientists wasted in writing the 49 or 99 or 9 unsuccessful proposals is often worth more than the total national expenditure for some years. No sane country can waste 30-40 percent of its best brains on writing essays and complain about a shortage of funds.

I respectfully call the Congress' attention to the second goal of the ST Agency in Japan: "improving the efficiency of intellectual production activities."

I am certain we can improve the efficiency of our system by a third in 3 years. That means that without adding one dollar to academic research we could get a 33% increase in "intellectual production" by devising new funding mechanisms. The Japanese captured the automobile industry because they increased the efficiency (in miles per gallon) of their cars. Does it not strike the Congress as totally incongruous that the entire S&T establishment has not made a single proposal to get rid of the "gas guzzler" funding system we have while they come to Congress yearly for more money. The report is therefore to be faulted in not analyzing the many alternatives which are available or have been proposed in more or less detailed form. I mention only three:

1. Convert to a DOD, strong manager system;
2. Elaborate on Dr. Alvin Weinberg's (founding Director of Oak Ridge) proposal to fund 3S science strictly as overhead on 2S science;
3. Work out my own much more explicit and detailed model for funding based on a peer review of performance "formula." Any agency could try an experiment with this formula in one division in one year. My formula has three great advantages: (a) It recognizes the reality of the multiple purposes of funding science; (b) it provides much greater continuity, and a maximum geographical distribution, both of which will add to efficiency; and (c) Congress can "fine tune" it, intelligently, annually to respond to changing national needs.

Peer Review of Inter-Field Funding

It is astonishing that the science community which claims that it lives by peer review has not asked that the total allocation of resources among fields be peer-reviewed by the total science community or their representatives. Alvin Weinberg, again, in a separate paper 25 years ago, pointed out that the most significant review of any field would come from scientists on the periphery of the field. It would, I believe, be absolutely essential data for Congress to commission a study which would give them the peer-reviews by the national community of scientists of the present allocation of funds among fields. This could be done by asking a national sample; or a sample of the National Academies' members, or whatever, to rate or review the present allocation: (a) among fields; (b) between Big Science and Little Science. This should be followed by parallel intrafield surveys: Thus surely the physics community as a set of peers should voice their opinion on what percentages of the funds should go to particle physics (including the SSC), solid state physics, atomic physics, etc. The Sigma Xi survey is a beginning in this direction which showed that only 2% of scientists would give priority to the SSC. If peer review is so significant in science, why is it not being applied here?

In fact, of course, the peer review system, as Deborah Shapley and I pointed out years ago, as practiced, bears no resemblance to a "jury of one's peers" in the halls of justice. In science we pointed out, "an axe murderer" would insist on a jury of only axe murderers to judge her or him. It is time for a change.

Thus a national peer review of the present allocation among fields would be an invaluable guideline for Congress.

SPECIFIC OVERVIEWS OF STUDY

My overall impression of what is contained in the study is one of solid achievement. The facts marshalled and presented, the organization around four key themes, the even-handed treatment of divergent policy viewpoints are all exemplary.

I want specifically to call attention to and commend the OTA authors for:

(a) Avoiding the "basic-applied" trap.

(b) Calling attention to the real, albeit mechanical, need for a system of data collection which will make national planning very much more reliable, but not politically easier.

(c) Tackling their issue #3, "education and human resources," and coming down forcefully on the side of those that believe that there is certainly no shortage now. Moreover, since it is impossible to attempt a responsible projection of the demand even 5-10 years out cries of shortfall are meaningless. Even more reassuring to the Congress is the analysis that shows that the nation has several mechanisms to cope with developing needs.

I would add to their references on p. 33, 34, two references which clearly make the case that not only is there no shortage of engineers (AAES, Engineering Manpower Bulletin #105, 1991), there may indeed be far too many scientists (R.M. White, Presidential address, NAE, Oct. 2, 1990).

Indeed here again the document could have added some marketplace data. If in the U.S. we believe in the marketplace test, how is it that no data were cited to show that postdocs in most science fields are available with 2 years of experience at a salary of about $25,000. To cry shortage in the light of that situation and an impending economic downturn, and increasing competition from Japanese and

Cerman establishments, is absurd. The OTA document is appropriately politic: but it could have compared the braindrain scenario which exists in England today, when that country is paying to train Ph.D.'s which promptly leave for many other countries in the world.

In the section on Research and Education in flux, the report correctly points to the thinking of our best minds in the field such as Ernest Boyer and the Carnegie Foundation report. But it fails in two respects. First, it does not squarely lay the blame for the sorry state of science education on the academic research establishment. which through its official proxy—the National Science Board—presided over the emasculation of the education directorate of the NSF from 30-40% of the total in the late fifties, to 2% in the early eighties. "Repentance" of earlier mistakes is the pre-condition for healthy living. Is there any evidence that the research science community has repented of its outrageous neglect of science education?

Secondly, the report fails to make recommendations whereby the Congress could legislate to attain the goals such as those outlined by Boyer.

Boyer's report advocates, for example, much greater emphasis on interdisciplinarity and applications (of science). The Kennedy-Hatfield resolution last year itself calls for "scientific and technical literacy" as the Congress' first goal for science education. Every member knows that these sentiments must be incorporated into the language of appropriation bills if they are to be effective. The academic research establishment has totally failed to INSTITUTIONALIZE interdisciplinarity on its campuses, technological literacy takes a backseat to making more and better scientists. But with a stroke of the pen Congress can achieve the goals, by, e.g. having NSF, NIH earmarking money only for institutions that have installed permanent interdisciplinary structures. Especially as the conclusion is confirmed that there is no impending shortage of scientists and engineers, surely as the Kennedy-Hatfield resolution suggests, language such as that which "Project 50:90" advocates propose, would be appropriate. That "50% of all NSF and DOEd funds be spent for technological literacy of 90% of the citizens who will not be scientists, but Congresspersons, CEO's, Presidents, labor leaders and voters."

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Vita of Witness

Rustum Roy is Evan Pugh Professor of the Solid State, Professor of Geochemistry and Professor of Science, Technology and Society at The Pennsylvania State University.

Professor Roy is one of the nation's leading materials scientists specializing in synthesis of new ceramic materials. He has published 500 papers and 5 books and received many awards. He is a member of the U.S. National Academy of Engineering, a foreign member of the Royal Swedish Academy of Engineering Sciences and the Indian National Science Academy.

Professor Roy's specialties include science policy, science education and the science-religion interface. He has been involved in science policy making and analysis for two decades at the State, Federal and international levels, and in the private sector as first chair of the National Council of Churches Committee on Science, Technology and the Church. He has written major books and dozens of articles in these fields and delivered the prestigious Hibbert Lectures in theology in London.

For the last decade he has become a national spokesperson for the cause of a radical redirection of math and science education towards the goal of the technological literacy of the majority of citizens, instead of making more or better "scientists."

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OTA Report

Especially valuable was the treatment of Their Issue #3, "Education and Human Resources"

No shortage of scientists now — data show too many now and no birth control.

- N.R.C., AAES, NAE statements confirm OTA view.

No way to predict demand.

U.S. has many mechanisms to accommodate fluctuations.

<u>In the light of this, Congress should act to achieve its own goals in science education:</u>

Congressionally mandated increases in science education, be devoted to explicit Congressional goals of Kennedy-Hatfield resolution, proportionately aimed at technological <u>literacy for all citizens</u>.

Congress must use explicit language in legislation:

E.G. <u>Project 50:90</u>. "50% of the budget for 90% of the people" *(via approaches such as Project 2061. STS programs dealing with nuclear waste, genetic engineers, job loss, etc. Issue oriented museum programs; new applied sciences in K-12, to recapture vocational and industrial arts streams, etc.).*

Otherwise science education programs focus on making more scientists.

OTA Report

Major Omissions

1. Dire straits of the most important component of U.S. research — long-term research in industry.

2. Absolute need to divert funds (if need be) not only to civilian industry-supporting research.

   And if push comes to shove, FROM universities to industry.

OTA Report

Failed to Think Big
Address Global Issues

1. The context of "research" was ignored. Science must fit within technology, which in turn must fit within society [cf. Japanese P.M.'s STA Committee, Feb. 1991.

2. That U.S. policy for 40 years has been based on a totally erroneous hypothesis. Science → applied science → technology.

"Science leads inevitably to innovative technology and thus to prosperity" (Keyworth, 1985).

- Every historian knows it is exactly opposite!
- Compare Japan and England and their science vs. their technology. 'Nuff said.

3. No reference to C. R. S. 1986 Study conclusively showing negative correlation between winning Nobel prizes and growth of GDP.

OTA Report

Failed to Point out the <u>Horrendous Inefficiencies</u> of Present System

1. No one in U.S. reads the literature (not true in Japan or USSR). Results in huge waste, and in misleading agencies. Congress could easily put language in appropriation bills to require agencies *"to assure that any recipient of federal grants file record of their having thoroughly read the literature under penalty of revocation of grant."*

2. No critique of proposal reviewing system. <u>Unbelievable</u>.

   - Wastes 35+% of total time of most valuable scientists.

   - Much more efficient systems <u>must</u> be experimented within this decade

     - *DOD — strong managers*
     - *Weinberg — overhead*
     - *Roy — peer-reviewed performance*

OTA REPORT

FAILED TO FIND USEFUL CATEGORIES OF SCIENCE W.R.T. VALUE TO PUBLIC

2S SCIENCE: SCIENCE FOR SOCIETY. SCIENCE SERVES PUBLIC AND PRIVATE SECTOR. SUPPORTED FOR NATIONAL OBJECTIVES BY ALL AGENCIES.

3S SCIENCE: SCIENCE FOR SELF AND INSTITUTION OF SCIENCE.

RETURNS ZERO TO THE NATION WHICH PROVIDES FUNDS.

EVERY NATION CAN, DOES AND WILL ALWAYS USE 3S SCIENCE FROM WHEREVER IT IS AVAILABLE.

3S SCIENCE CAN BE JUSTIFIED
- PARTLY AS VEHICLE FOR EDUCATION
- CULTURAL ACTIVITY AT MODEST LEVEL

ENTITLEMENT AND WELFARE ATTITUDE ON PART OF SCIENCE COMMUNITY MUST BE ELIMINATED.

INNOVATIVE DECISIONS FOR A DECADE

- LIMIT GROWTH OF 3S SCIENCE TO COLA.
- REQUIRE AGENCIES TO DEVELOP MORE "EFFICIENT" $ DISTRIBUTION SYSTEM. REMOVING THE BIG BROTHER CONSTRAINTS ON RESEARCH DIRECTIONS OF INDIVIDUAL SCIENTISTS.
- DEFER SSC FOR TEN YEARS WHILE INTERNATIONAL COLLABORATION IS WORKED OUT.
- CHARGE OTA TO COME UP WITH 3 ALTERNATIVE USES FOR SAME FUNDS FOR SCIENCE.
- USE SAVINGS TO RADICALLY CHANNEL NEW SCIENTISTS AND TEACHING.

Mr. BOUCHER. Thank you, Dr. Roy.

Dr. Lauffenburger, we'll be happy to hear from you, sir.

STATEMENT OF DR. DOUGLAS A. LAUFFENBURGER, ALUMNI PROFESSOR OF CHEMICAL ENGINEERING, UNIVERSITY OF ILLINOIS

Dr. LAUFFENBURGER. Mr. Chairman, my comments are really just those of a foot soldier in the trenches, though I may be the monster to which the previous speaker referred. We'll find out, I suppose. What I will present is not a brief for the health of American scientists but for the health of American science and engineering research and education.

It's not really stress on a faculty member that's the problem, but just that we must now spend so much time obtaining funds that it's difficult to do the job we're really supposed to be doing, that of teaching and of education, so it's not the stress, it's the effects of that stress on doing our job.

A number of trends have converged to lead to this current situation in which researchers perceive the system to be primarily driven by funding pressures. There is greater demand on faculty members to generate funding revenue than ever before, while at the same time the ratio of available funding to the amount sought is decreasing.

I see three main reasons for this. First and foremost, the range of potentially valuable research project directions is expanding rapidly because new ideas and technology increase vision and capabilities exponentially. The number of well- educated investigators interested in pursuing these directions with the appropriate methods and tools is increasing correspondingly.

Second, an increasing number of universities are asking for substantial research efforts from their teaching faculty. Third, historically research-based universities are requiring increased contributions from faculty toward salary coverage and other expenses from their grants.

All of these factors are working together so that the role of faculty members is now considered by many universities to be as fundraisers as well as scholars and teachers. In tenure and hiring decisions, the question: can he or she raise substantial levels of funding is a very important factor. This is having strongly detrimental effects on the scientific community by both distorting the research portfolio and by discouraging present and future participants.

The research portfolio is distorted toward a conservative bent because when only a small percentage of grant proposals can be funded, projects previously demonstrated to be productive are much safer bets than those in which hypotheses and approaches are innovative but risky.

Discouragement of researchers is especially tragic at the present moment for two reasons. First, so many scientific fields are now offering dramatic advances promising revolutions in technology with great economic benefit; and second, just when the need for greater participation by underrepresented demographic groups is being recognized and encouraged, the insecurity of an academic research career makes it a poor choice compared to most other professions.

Regarding the specific issues discussed in the report, my chief criticism is that the interplay between research and education, which is vital, is underemphasized in exploring the issues. This criticism will be fleshed out in my brief comments to come.

Issue one, setting priorities. I agree with the report's assessment that no matter what the level of absolute funding, priorities must be established. There is no question that megaprojects must be subject to cross-cutting priority- setting. These along with large "center" or "program" projects play a quite different, and, I believe, subordinate role in addressing the Nation's needs compared with small team or individual investigator projects. The latter contribute much more significantly to education and to unexpected scientific breakthrough even while the former may help solve specifically targeted tasks in the short term.

The fewer the ideas being supported, the greater the chance for scientific stagnation. It would be extremely unfortunate if scientists receive the message that the most successful way to gain research support is to emphasize participation in megaprojects or large centers. Macro-level policy decisions should be made regarding the amount of investment for large scale research quite separate from the foundational need to support small scale research in a way which does not compromise the future.

Issue two, understanding research expenditures. Just one small remark here that I'd like to make which concerns the possible trend mentioned for academic research to move toward an industrial model where project teams are larger and more specialized and responsibilities are more distinct.

I would submit that this industrial model has not served as the basis for the historical technological superiority of the United States. It is therefore extremely interesting to learn from this report that the overall costs of such a model are in fact greater than for the more traditional investigator or small team model because the educational benefits from the larger teams are also generally inferior.

Issue three, adapting education and human resources. I strongly believe that the key to education is research done with an educational emphasis. Knowledge is taught best when the student sees how new knowledge is created. Any weakening of this connection is primarily due to the growth of centers and large research teams in which education is not a major focus.

Increased support for individual investigator and small team research will go a long way toward strengthening that connection and generating more excitement about science among undergraduates.

Interdisciplinary research does not require centers or large teams. Indeed, to the extent that these promote intragroup specialization, they can actually be counterproductive to a good interdisciplinary educational experience.

I would strongly discourage increased emphasis on centers and large teams as a favored model for academic research.

Issue four, refining data collection and analysis. Measurement of research outcomes is difficult from a management point of view. Attempts to put it on a seemingly quantitative basis such as bibliometrics as mentioned in the report, are fraught with danger. Citation indices, for example, only measure a snapshot of current thought regarding a scientific topic but are of little value in projecting what will truly show up as a valuable contribution in the future.

Policy options. Significant improvement in the academic research environment is necessary to prevent the discouragement of a generation of scientists and the technological base for the United States that they would develop. For the sake of both education and scientific progress, priority-setting policy must favor increased support for the individual investigator and small team laboratory models.

This concludes a brief summary of my written testimony, Mr. Chairman.

[The prepared statement of Dr. Lauffenburger follows:]

COMMENTS ON OTA REPORT "FEDERALLY FUNDED RESEARCH: DECISIONS FOR A DECADE"

Douglas A. Lauffenburger
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I have been asked to comment on three major aspects of the OTA Report, "Federally Funded Research: Decisions for a Decade". These are: (1) whether the report provides a comprehensive discussion of the full range of issues which require public policy attention in the coming decade, and which issues are most important; (2) trends which have led to current stresses in the academic research system; and, (3) the policy options developed in the report.

My comments are not intended to be those of a spokesperson for the scientific community, but rather of merely a footsoldier in the trenches of academic research. Nonetheless, I will not present a brief for the health of American scientists, but instead for the health of American science and engineering (hereafter simply referred to as science) research and education. I will start by describing the trends which I believe had contributed to the current stresses in the academic research environment, then follow up on the issues requiring public policy attention and finally the policy options.

Trends Contributing to Current Stresses

A number of trends have converged during the past decade to lead to the current situation in academia, in which researchers perceive the system to now be primarily driven by funding pressures. There is greater demand on faculty members to generate funding revenue than ever before while at the same time the ratio of available funding to the amount sought is decreasing. Not all of the trends leading to this situation are necessarily negative, in fact some are highly positive, but the resulting situation is undeniably detrimental to the health of scientific research now and for the future.

1. Above all, the range of potentially valuable research project directions is expanding rapidly, because new ideas and technology increase vision and capabilities exponentially. The number of well-educated investigators interested in pursuing these directions, with the appropriate methods and tools, is increasing correspondingly.

2. An increasing number of universities are asking for substantial research efforts from their teaching faculty. Universities' motivation may arise from a number of driving forces, including a desire for higher caliber of faculty (because most PhD graduates wish to have a

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research component to their academic career), increasing competition for undergraduate students (with a research experience providing an added attraction to the normal educational experience), and, possibly, a need for alternative revenue sources (other than tuition, endowment giving, and taxation).

3. Historically research-based universities are requiring increased contributions toward salary coverage and other expenses from faculty grants, perhaps because of a need for alternative revenue sources.

All of these factors are working together so that the situation now exists in which the role of faculty members is considered by many universities to be as fundraisers as well as scholars and teachers. In both tenure and hiring decisions, the question: "Can he or she raise substantial levels of funding?" is an important factor. There seems to be almost as much emphasis on the "input" to research (that is, funding) as on the "output" (new knowledge and educated students). This is most certainly having detrimental effects on the scientific community, by both distorting the research portfolio and by discouraging present and future participants. The research portfolio is distorted toward a conservative bent because, when only a small percentage of grant proposals can be funded, projects previously demonstrated to be productive are much safer bets than those in which hypotheses and approaches are innovative but risky. Discouragement is manifest and easy to document in faculty colleagues leaving academia and students declining to enter. This is especially tragic at the present moment for two reasons: first, so many scientific fields are now offering dramatic advances, promising revolutions in technology with great economic benefit; and, second, just when the need for greater participation by underrepresented demographic groups is being recognized and encouraged, the tremendous insecurity of an academic research career makes it a poor choice compared to most other professions.

Issues Discussed in OTA Report

Given this background, I am reasonably satisfied with the issues raised in the report, though not with all the solutions offered. Table 1-1 in particular is extremely helpful in outlining the conflicts to be resolved in policy making. My chief criticism is that the interplay between research and education, which is vital, is underemphasized in exploring alternative sides of the issues. This criticism will be fleshed out in my specific comments which follow.

Issue 1 - Setting Priorities: I agree with the report's assessment that, no matter what the level of absolute funding, priorities must be established. There is no question that "megaprojects" must be subject to cross-cutting priority-setting. These, along with large "center" or "program" projects, play a quite different -- and I believe subordinate -- role in addressing the nation's needs compared with small-team or individual-investigator projects. The latter contribute much more significantly to education and to unexpected scientific breakthroughs, even while the former may help solve specifically-targeted tasks in the short term. The fewer the ideas being supported, the greater the chance for scientific stagnation. Macro-level policy decisions should be made regarding the amount of investment desired for large-scale ("targeted") research, followed by similar decisions choosing among candidates, quite separate from the foundational need to support small-scale ("educational") research in a way which does not compromise the future. Figure 1-7 is very instructive in demonstrating how megaprojects can indeed erode the fundamental scientific research base, but it does not go far enough in charting the similar effects of centers and program projects. It would be extremely unfortunate and counterproductive if scientists receive the message that the most successful way to gain research support, in these times of great pressure to do so, is to emphasize participation in megaprojects or large centers. How to involve scientists effectively in cross-disciplinary priority-setting is a crucial issue, unresolved in this report. Scientists are trained not to be consensus seekers and not to extrapolate their judgments beyond their areas of expertise. One possible approach would be to assemble an overlapping group of distinctly interdisplinary scientists on technical advisory panels for cross-cutting decision making. However, it would be dangerous to provide too much "top down", macro-level targeting of small-team and individual-investigator programs.

Issue 2 - Understanding Research Expenditures: Most aspects of this issue are addressed very well in the report. One remark worth making concerns the mention of a possible trend of academic research toward an "industrial model", where project teams are larger and responsibilities are more distinct within the group. I would submit that this "industrial model" has not served as the basis for the historical technological superiority of the U.S. It is therefore extremely interesting to learn from this report that overall costs of such a model are greater than for the more traditional individual-investigator or small-team model, because the educational benefits from the larger teams are also generally inferior. In sum, there appears to be little reason to encourage such a model for academic research.

Issue 3 - Adapting Education and Human Resources to Meet Changing Needs: This section contains some slightly misguided notions. There are really no compelling arguments for targeting center-type funds to "have-not" institutions. "Pork-barrel" research funding should be avoided completely. It is quite difficult to build "centers of excellence" in an isolated fashion, especially in the modern multidisciplinary scientific environment. The best way to drive diffusion of funding to "have-not" universities is to increase the percentage of individual-investigator or small-team grants funded. I also disagree that the connection between research progress and cultivation of human resources is necessarily growing more tenuous. I strongly believe that the key to education is research -- done with an educational emphasis. Knowledge is taught best when the student sees how new knowledge is created. Any weakening of this connection is primarily due to the growth of "centers" and large research teams in which education is not a major focus. Increased support for individual-investigator and small-team research will go a long way toward strengthening that connection, and generating more excitement about science among undergraduates. Further, interdisciplinary research does not require centers or large teams; indeed, to the extent that these promote intragroup specialization, they are actually counterproductive to an interdisciplinary research education experience. I would strongly discourage increased emphasis on centers and large teams as a favored model for academic research. Finally, the chief barrier to greater participation by traditionally underrepresented groups in research is the need for improved educational opportunities at the K-12 educational level. Agencies responsible for funding scientific research will do best to focus on scientific merit as the decision criterion. As I stated earlier, the best way to attract minorities into the scientific community is to alleviate at least some of the tremendous insecurity they see in it compared to alternative career possibilities, so that they can more easily see the exciting benefits of intellectual challenge and role-model service

Issue 4 - Refining Data Collection and Analysis to Improve Research Decisionmaking: Measurement of research outcomes must remain elusive from a quantitative, management point of view. Attempts to put it on a seemingly objective basis, such as "bibliometrics", is fraught with danger. Citation indices measure a snapshot of current dogma regarding a scientific topic, but are of little value in projecting what will truly show up as a valuable contribution in the future. They cannot anticipate new breakthroughs, nor the slow incubation of a significant innovative approach The importance of many ideas is not immediately recognized upon publication. I believe it is the case that many Nobel laureates cannot be found near the top of citation index rankings. Peer review remains the most reliable form of current evaluation, although only hindsight is truly 20-20. It is also doubtful that substantial funds will be best spent on increased agency ollection and analysis.

Policy Options

My comments here are short and simple. Significant improvement in the academic research environment is necessary to prevent the discouragement of a generation of scientists -- the current young faculty and the potential young faculty who are currently students -- and the technological base for the United States that they would develop. For the sake of both education and scientific progress, priority-setting policy must favor increased support for the individual-investigator and small-team laboratory models. The genius of American science has never been a result of "top down" idea management promoting a few favored propositions, but rather has derived from encouragement of creative individuals free to pursue innovative ideas in the scientific marketplace.

Douglas A. Lauffenburger, University of Illinois
Biographical Sketch (1991)

D.A. Lauffenburger is currently Alumni Professor of Chemical Engineering at the University of Illinois at Urbana-Champaign; he additionally holds secondary faculty appointments in the Department of Cell & Structural Biology, the Bioengineering Program, the Beckman Institute for Advanced Science & Technology, and the National Center for Supercomputer Applications at Illinois. Previously, he was on the faculty at the University of Pennsylvania during 1979-1990, serving as Chairman of the Department of Chemical Engineering from 1987-1990. Lauffenburger's teaching and research interests are in the field of molecular/cellular bioengineering, with primary focus on combined theoretical and experimental studies of receptor-mediated cell functions. He has coauthored approximately seventy refereed research publications, five invited review articles, and one research monograph; a text on Mathematical Models of Receptor-Mediated Cell Phenomena is close to completion for publication by Oxford University Press. Ten PhD students and four MS students have completed their degrees under his supervision, with six of the former now holding faculty positions in major academic chemical engineering departments. Notable honors and awards won by Lauffenburger include the AIChE Colburn Award (1988), a Guggenheim Fellowship (1989), the O.A. Hougen Visiting Professorship at Wisconsin (1989), the inaugural E.W. Thiele Lectureship at Notre Dame (1985), an NIH Research Career Development Award (1984), and an NSF Presidential Young Investigator Award (1984).

Mr. BOUCHER. Thank you very much and the Chair extends thanks to all of the panel members for their attendance this morning and sharing their ideas with us.

To begin a series of questions, we, I think now, are very much aware of the fact based on the OTA report, but also on other information that is currently available, that many projects today that are submitted to funding agencies for funding are not being funded because of the very large number of meritorious projects being submitted, and that while we do have more funds available today for these projects, that's enough to fund even those that truly have merit.

So the result is that many truly meritorious projects are in fact not being funded today That being the case, I wonder if there are other qualities that we should look to in addition to scientific merit to give us a guide for making decisions among those projects that do have merit and pass that basic test.

For example, I think Dr. Lauffenburger, you had suggested that the educational value that comes from having the project funded should be considered. Are there other factors that we ought to be looking at? Should we look at building geographic strength in conducting basic research? Should we be looking at building institutional strength among universities that perhaps have not participated as fully in the process in the past? Are these values that ought to be taken into account as well?

Dr. Lauffenburger, you had mentioned this point in your testimony, so let's begin with you.

Dr. LAUFFENBURGER. Yes, in my written testimony I address this briefly. What I attempted to say was that I believe geographic diversity and these other criteria are important but the way to go about them is to not target, focus large center type funds because that's very difficult to sort of unilaterally impose a center of excellence in a place without all sorts of supporting departments and so forth.

I still believe the best way to get a stronger distribution away from very small, focused, perhaps over-represented areas is just by increasing the number, the actual percentage of individual investigator projects that can be funded because in fact that will allow more of them to be distributed other places around the country, so I believe that's the most effective way to do it.

Mr. BOUCHER. All right. Dr. Roy?

Dr. ROY. I'm afraid you fall for the scientists' line that there are more meritorious projects out there. Forget it. Most of that stuff is junk. The fact is that the new citation studies are saying that a lot of the stuff is never referred to, so say there is a lot of meritorious projects out there, I challenge that assertion. There is no basis in fact. It's simply somebody wrote a damned fool essay and you're supposed to say that's a meritorious project? No evidence it's a more meritorious project.

I support Dr. Lauffenburger's position on the question of education linkage. That is an absolute gain and I think the best way to do it is to what the Ministry of Education does in Japan. Dr. Schmitt referred to NDEA projects in the 1950s and 1960s where we could give fellowships and assistantships directly to students.

That's the way portable traveling assistantships so that can education and research can be linked.

The second thing which I think we need is to really pay attention to the economic consequences. You asked for the second criteria. I would say you jolly well link it to a regional economic development. The States are where the action is. It's no longer at the Federal level, but at the State level if somebody can link to a State program for economic development, that graduate program should be supported.

Mr. BOUCHER. So you're saying there may be some difference of opinion as to whether there are in fact meritorious projects not being funded?

Dr. ROY. Absolutely.

Mr. BOUCHER. Do you agree that we ought to add as criteria for project selection building educational components and the value that is attached to that and also economic development and regional development of institutions?

Dr. ROY. But you ought to ask for a criterion which is so simple, Mr. Chairman. Every proposal now we send to six agencies, and they say there is a tremendous proposal pressure out there! Nonsense. Every paper is repeated six times. That doesn't mean there is good stuff out there. You've got to be very careful in saying, who said there is a meritorious project out there.

Mr. BOUCHER. Thank you for your thought-provoking contribution.

Dr. Schmitt?

Dr. SCHMITT. I perhaps don't go quite as far as Dr. Roy in some of his statements but I just want to point out there is a difference between a large fraction of proposals that don't get funded and the number of investigators that don't get funded. A piece of data we really don't have well in hand now is how many of the people out there submitting proposals that don't get funded are getting money from other areas?

This brings me back to another thing. If you look at the data, Dr. Roy mentioned that State support has been growing. Interesting enough, institutions own support, the academic institutions own support has been growing much more rapidly than State support in spite of the publicity that State support has been getting.

In fact, that may be one of the reasons for some of the stresses out there. I can tell you that institutions of higher education today are facing some fairly stringent financial challenges and yet, their own investment of their own resources into funding has been growing.

I can give you an example of that. My own institution, I now know what those costs are. We perform about $48 million worth of research per year; we receive about $41 million to do that research. We are putting $7 million of our own money which comes from endowment and gift income into the subsidy of the research we do on campus. I suspect that's true on many other campuses and in today's economic climate, it presents some problems of its own.

Mr. BOUCHER. Dr. Lederman?

Dr. LEDERMAN. I think the question you addressed is of scientific merit and I think the OTA—I didn't have too much problem with their way of doing it. They say scientific merit has to be the first criterion but there are other things you want to accomplish which is the preparation of future resources. I think the geographic distribution is a refined thing because there are all sorts of talent out there that you begin to discover if you have local institutions which are strong and research centers around the country. So there is a very good benefit to that.

Other preparations for the future like giving recognition to minorities, bringing women in as part of the research program are all beneficial. I thought the OTA discussion there was balanced and correct, but you have to remember that if you are supporting science, you've got to watch the merit of the science and not water down that quality too much because then it's a balance between the quality of the science and the preparation of the future. That's why science and education are so tightly bound.

Mr. BOUCHER. One of the trends that we are witnessing now is increased industrial support for university-based research. Is that a healthy trend? Are there problems associated with that? Are we achieving about the right balance do you think, between Federal funding, the university's own funding and funding from the private sector?

Dr. SCHMITT. Well, yes, I can speak to that from both sides of that equation. I'd have to tell you it's some of the practices I had that I'm unhappy with today, so I wish I could have anticipated where I was going to be when I was establishing some of the policies.

Levity aside, I think the industrial support of academic research is a very healthy thing, provided it is done in the right way. I do not believe it is healthy for industrial institutions to put a lot of additional restrictions on academic research, on publication policies, and the like.

My philosophy has always been, even when I was on the other side of that equation, that for an industry to put money into academic research, it ought to put it into areas that it's prepared to do something about rapidly.

The competitive advantage an industry can get out of supporting academic research is to themselves be well prepared to take up the results of that research more rapidly than any of their competitors. I think that is a philosophy and approach that ought to be promulgated widely among industrial supporters of academic research.

Frankly, the industrial support of academic research I would guess is not going to continue to increase as rapidly as it has been. If you look at Figure 2 of what I turned in, you will see that over the 20-year period 1969-1989, the industrial support of academic research has been growing about 15 percent per year. It was about 12-1/2 percent in the decade of the 1970s, about 18 percent in the decade of the 1980s. My guess is that's going to drop off some, so I think that is not going to be as rapidly a growing area as it has been in the past.

Mr. BOUCHER. Dr. Lederman?

Dr. LEDERMAN. I have no problem there. I think that's a question of the free enterprise system working. The industrial components want to get research done and they can get it done in the universities and the universities have a choice. They can take it or not take it, so I think that's a fine thing to do. I don't see any real problem unless there were constraints on the publication or constraints on which students could perform that work, but other than that, it's certainly a positive thing.

Mr. BOUCHER. Are you generally concerned though that as the private sector funds a greater percentage of the research taking place at universities that the research becomes too mission-directed?

Dr. LEDERMAN. Yes.

Mr. BOUCHER. Is that something we ought to—

Dr. LEDERMAN. I would worry about it if it came to that. On the other hand, that's why we have the Federal Government. I think clearly the obligation to support the abstract research, the useless research, as was referred to, is a part of the Federal Government. There is no guarantee of anything happening in that kind of research—high risk intellectual activity.

If the imposition of other more directed research harmed that, if people would say that's the only place I can do my research, that would be a problem. Again, it's a question of balance, balance between basic and applied and then a balance between the sources and the motivations.

Mr. BOUCHER. Dr. Roy?

Dr. ROY. I think that's one of the healthiest things which happens but I agree with Dr. Schmitt is that that's pretty well peaked. I don't think it's going to go up. In fact, this year we ran by the way the lab which has the highest percentage of industrial research of any similar lab in the Nation, and we are next to MIT but he's got a private institution which has the highest percentage for a university. I'm talking about a lab. So we are both experienced in that field.

I think moreover when we talk about peer review, let me tell you that the best peer review is when somebody puts $50,000 and says hey, this is good work, I'm willing to put $50,000 behind it. So I've suggested many years ago that the best system would be for the Federal Government to give an incentive to universities who do get industrial research, to have Federal matching funds. It eliminates all this "kerfuffle" in NSF and all that which we don't need at all.

The simplest and most precise peer review is industry putting in money. I'm not suggesting that money be given to industry. I'm saying it be given to the university to do the free, untrammelled, innovative work which today is difficult to fund because the peers are the worst guardians against innovation. Remember the threat against innovation is not from the agencies. It's from the peers. So our system is such that it militates against innovation. Industry, on the other hand, looks for innovation; that's all they pay for.

Mr. BOUCHER. But if we let industry do too much of the guiding, don't we really run the risk of our research taking on too much of a short term and mission-specific orientation?

Dr. ROY. Just the opposite. Industry only comes to me when I'm so far ahead of the pack, as Dr. Schmitt said, it's got to be connected to their short-term interests but I'm suggesting the feds now give me to say, hey, Roy, go do some more of that in any field you want to. That's the difference between writing a proposal which spells out what you're going to do and being rewarded for your performance that you've done something creative which industry is certifying.

Mr. BOUCHER. Dr. Lauffenburger?

Dr. LAUFFENBURGER. I think industrial funding is sort of a double-edged sword. It certainly is beneficial for bringing a stronger connection between real industrial problems and the scientific expertise that can be brought to bear on it by the universities and the conterminous problem is when it is too mission-directed, when it is too clearly tied to development of current technology instead of exploration of possible technology that you don't even envision yet.

Let me just say that one healthy program in this regard, I think, that's come out of NSF in the last so many years was this Presidential Young Investigator Award system where young investigators would be given a certain amount of money to be matched by industrial contributions and to the extent that those industrial contributions were not contract-oriented but were more toward the support of that person's laboratory and the development of some new approaches, I think that's been very valuable.

Mr. BOUCHER. We are seeing now, as some of you have suggested, more of a copying by university research of the industrial model where teams of researchers work together and share infrastructure and overhead. What do you generally think about that? What does that portend for the future? Is this a healthy trend; is this not a healthy trend? In view of its taking place, should we at the Federal level be changing our way of doing business? Should we make money in block grants, for example, to accommodate that?

Dr. SCHMITT. I think it depends on how you do that. It can be a very healthy thing and contribute immensely to the academic enterprise. I will give you an example. At my institution, we have a Center for Manufacturing Productivity that I think is one of the leading academic research enterprises and technologies.

The philosophy from the beginning in establishing interdisciplinary research centers on our campus, that one and many others in materials and computer aided design and the like, was those centers have to play an important role in the educational process. So in the Center for Manufacturing Productivity where a lot of graduate research as well as other research is going on, if you walk into that, you will also find undergraduate classes actually in those facilities doing work, laboratory work pertaining to their undergraduate courses.

So I would say that one of the criteria one wants to place on these things and which was placed on the engineering research centers is that it had to have a significant impact on undergraduate education. I think it's entirely possible and as a matter of fact, if you talk to students themselves, they will tell you that's some of the most exciting undergraduate experience they have.

Mr. BOUCHER. Dr. Lederman?

Dr. LEDERMAN. I don't know if your emphasis was on interdisciplinary versus large groups. Large groups are becoming a necessity because the problems get more complicated and whatever it is; whether it's a tabletop experiment or work on a distant telescope or a particle accelerator, the large group structure is needed to handle the complexity.

There's a misconception about how these groups work and in general, this gets to the question that was raised earlier about big science versus small science. The standard group in the university that's going to work at a distant facility might be a professor, an assistant professor, two post-docs and three graduate students. It's the same sort of structured group that works at a tabletop or a distant facility.

The difference is when it comes to a different facility, he joins with 20 or 30 other such groups in a joint effort, a jointly shared effort to create some sort of facility or to get data out of that facility. Then they go back and they wrestle with the data they have and in the same historically traditional, innovative creative enterprise.

I studied in fact the names on papers in a variety of different fields and found that the number of scientists per paper has been increasing. It's doubled in the last 20 years, all except for mathematics where it went from one to 1.01, so they are still individuals.

Mr. BOUCHER. Dr. Roy?

Dr. ROY. I think that there's no question that what we call a small team, which was always present, (I think Dr. Lauffenburger agreed, two or three faculty in different fields interactively working on a problem) is now the unit of science. There are fields as Dr. Lederman just said, mathematics can do it individually, but in most of the fields, that is necessary.

Let me caution you. The university world, I take Dr. Schmitt's institution as somewhat of an exception, the university world has not managed to institutionalize interdisciplinarity. I have pushed the directors of NSF to write in there that they must institutionalize it. Otherwise, what we get is ad hoc teams to get the money, then they don't see each other until June 30 when they write the report.

We've got to watch out against that. We can do something. Institutionalize it. Have some tenured professorships in manufacturing productivity or whatever. If we are going to be serious about the fact that the original carving up of knowledge into physics, chemistry, biology, material science is not a permanent thing. We must now pay attention to the national needs.

In Japan, for instance, precision engineering, that didn't exist. Now they have 17 departments. We have not done that. Materials research in the universities was a disaster. We fudged the data for years and years and we told NSF what we were supposed to be doing. They ate it up and in fact we didn't institutionalize it. So I'm suggesting small teams are important, I'm suggesting that we do move to institutionalize some of those areas.

Mr. BOUCHER. Dr. Lauffenburger?

Dr. LAUFFENBURGER. Well clearly, I addressed this at some length and maybe I'll just try to make the distinction that I wanted. Absolutely, groups of investigators from different fields and crossing disciplines are essential these days to really doing pioneering research. That's not what I was talking about.

I think the detriment comes when you develop large enough centers so that in any given field there can only be one, for instance, nationwide, only one center in the entire country, let's just say in biotechnology because what that means is that you've invested in the ideas of the particular people at that institution, some of whom may be the best researchers in the world and others not, and you've diluted the small number of ideas into a larger group of investigators at the expense of other investigators elsewhere that may in fact have ideas that are quite valuable as well. So that's what I'm talking about, when you're really bringing it down to massive support of one small set of ideas at one institution.

Mr. BOUCHER. Well, that distinction is very clear and duly noted.

Let me inquire about one other area and then I'll yield to my colleagues. We have had testimony before this subcommittee previously that there is a dire need on the part of most universities to have additional Federal support for facilities construction for research.

The National Science Foundation has a research facilities construction component which unfortunately the Administration is not recommending any funding for, for the upcoming fiscal year. While we have this distinguished panel here, I think I would be remiss if I didn't solicit your comments on the need that you see for additional Federal support for research facilities construction and therefore, funding for that program? Dr. Schmitt?

Dr. SCHMITT. You have to understand that as a member of the National Science Board, I've been in favor of a facilities program for a long time, so just make sure you have my bias. I think it is an extremely important need, but the problem is always the fact that the faculty, the scientific community get hung up over whether a single dollar that you're going to put into bricks and mortar is going to come out of laboratory research.

While Erich Bloch was still Director of NSF, he visited our campus once and I took him to a building in which we had a research program being sponsored by NSF in a laboratory room and whenever it rained, we had to put plastic over the room because the roof was leaking. That's the syndrome and I have to tell you having become familiar with the academic accounting systems, and academic financial systems, there is nothing in those systems that tends to make universities choose the right priorities when they have to choose between doing an experiment in a laboratory and repairing a roof. The whole accounting system is set up so that you don't have to face that issue ever.

We do have to make choices about whether we need to repair the roof or do an experiment under that roof. Those choices are unavoidable. The university accounting systems today avoid it totally and the Federal Government with some sort of a program that required matching funds of some sort could help to stimulate that in my opinion.

Mr. BOUCHER. Dr. Lederman?

Dr. LEDERMAN. Well, you're probably familiar with the Packard-Bromley Report in 1986 which identified some $10 billion worth of needs in replacing obsolete equipment in universities and follow up on that—I've forgotten who did it —said not so long ago, of that number, a very, very small part had been done but time went on and so the deterioration of equipment and obsolescence of equipment got worse.

I think I agree totally with Dr. Schmitt that there has to be a balance and you have to have a judgment as to what to put into facilities of that kind. I think the NSF program was a sound program. I'm sorry to see that it didn't get entered.

Mr. BOUCHER. We haven't had the last word on that yet, I might say.

Dr. Roy?

Dr. ROY. I think I want to separate two things. One is bricks and mortar and one is equipment. I think in bricks and mortar perhaps a program somewhat which is based on the old NIH program where you could get a certain percentage back for bricks and mortar are the kind of R&D you're doing and let it be done locally. Otherwise we have to write another set of proposals for 16 different buildings. It would be better to have a formula return for bricks and mortar.

On the equipment question, I think, again I would certainly give an incentive to the universities to make better use—I think the regional facility idea is working. I send all my students to Cambridge in England or to Arizona State. I don't want to put up another $600,000 damn fool machine. Why should I. But we don't give the money for travel which will make much more efficient use of high-priced facilities. Every professor likes to have something to show the visitors. Well, that game is over. I think. We don't have the money for that. So if we give facilities, we should make very sure that they are well-utilized and that means about 16 hours a day. If there is not a night shift, don't give them anymore money.

Mr. BOUCHER. Dr. Lauffenburger?

Dr. LAUFFENBURGER. I'd say yes, that's a valuable program. It's sort of a necessary corollary to what I would see as the favored support of small teams and individual investigators who couldn't support those facilities themselves. I would see that equipment and facilities as sort of the best kind of center in which they are operated and many different people can use them at one institution or even at neighboring institutions.

Mr. BOUCHER. It had been suggested that the NIH program had particular merit because it, like other NIH initiatives was peer-reviewed and that basically meant that the institution that had the best proposed use for the facility would get the funding for its construction, unlike the typical way the Congress tends to fund brick and mortar facilities which is through the appropriations process and line items. That calls in to play political influence to a greater extent oftentimes than scientific merit. The gentleman from Pennsylvania earlier was commenting on that.

Others had suggested that the research facilities program was very valuable because when the NIH put its imprimatur on a given facility, even though the total contribution to its cause trimmed from the NSF might be fairly small, that just the stamp of the NSF in saying this is a facility we think is worthwhile, tends to leverage a lot of additional support from the State, the university, and the private sector, so it has value from that standpoint as well.

I thank this panel very much for its thoughts this morning and recognize now the gentleman from California, Mr. Packard.

Mr. PACKARD. I'll be rather brief, Mr. Chairman.

The debate between which comes first, research or technology, and the debate on how to and who profits most from the application of the technology or the research, the individual researcher or the institution, or private sector is constantly going on and I think some of the written material that I've read—unfortunately I didn't get to listen to some of your testimony, Dr. Roy, but I know in some of your written testimony you've mentioned that in your judgment we have it reversed, that technology is the innovator and the author of research and applied science.

I fully recognize the procedures and the policies that Japan has had and its benefits to their economy versus that here in this country. I have to assume, though, that most universities would not like to see a flow of funds and the flow of the emphasis away from research to technology and to applications. Would you expand on that, and then I'd be interested in hearing from perhaps each of the others and then I'd be through, Mr. Chairman.

Dr. ROY. Mr. Packard, if you remember, I testified before you a couple of years ago and made the same point.

Mr. PACKARD. I sure do.

Dr. ROY. That it's not flow of money from research to technology, it is what drives the research. If Bell Labs didn't exist, the discovery of the transistor action which was a cooperative thing would never have been pulled into the field, it was a technology pull, get rid of the filament drain and that's the way we're going to move something.

Unless we are going to utilize that knowledge, it sits on the shelf. We built an atom bomb based on Germany's basic science, they discovered fission, we didn't, so science is just thrown out there and anybody can grab it and use it.

I said last time, the Japanese have invented a science eating tree, so we're out here growing science and they are eating it. We don't have the trees anymore, we're destroying our capacity. What I'm crying about is that the industrial capacity to make knowledge into products has been destroyed.

In the university, we can have two kinds of research, both are exactly long term basic research. One can be connected to our industrial infrastructure and the other can be for the good of science, writing papers and so on. I'm saying that the public sector, the Congress, using our tax money, has got a right to say if it helps the Nation, we've got to give a priority to that. We're not cutting back, zeroing out the others, but surely in these hard economic times, I'm suggesting that not only has technology driven most of the good science from Galileo onwards, but in fact today given the economic climate, we should use that as a major criterion in deciding where to put our emphasis. We should in fact use the utilizability of that knowledge as part of the criteria.

Mr. PACKARD. Dr. Schmitt?

Dr. SCHMITT. Yes, I have two comments to make.

I am not as alarmed or apprehensive about the tender nature of exploratory research as has been expressed otherwise today. I remind you that Pasteur looked at sour wine and invented bacteriology and Irving Langmuir looked at a blackened light bulb and launched surface chemistry and Jansky sat out in a corn field listening to static and discovered radio astronomy.

The fact is that people of genius will do things of genius whatever stimulates them. I think as Dr. Roy said, a lot of good fundamental science can come out of being stimulated by very practical problems provided the individual doing it, and the individual working on it is given the freedom to approach the problem in whatever way they want. That's the key to it. You've got to give people of genius elbow room, the scope to approach problems in whatever way they wish.

Second, I would differ with Dr. Roy on one little thing. The Japanese have yet to beat us in any pioneering discovery or invention or in the first introduction into the marketplace of a product based on such pioneering things. Where they have consistently beat us is in the second, third, fourth, fifth generation.

Mr. PACKARD. What needs to be changed in the United States to alter that? What are we not doing that they are doing?

Dr. SCHMITT. We have not been very good at the incremental type of innovation, the type of innovation that involves manufacturing process, involves getting quality up and so on. They are purported on the one hand to be quick to market and yet at the same time people call them persistent, they appear to be opposites.

In fact what they do is they get something on the market quickly when the volume may not be very high. They find out how the customer reacts to it, and then they very quickly keep changing it and learning—they use the market almost as an experimental tool, so some of the processes that they have used to succeed I think some United States firms are beginning to learn how to emulate now, but the only thing I would say is I would hate for the U.S. give up the one place where it has and still is preeminent. That is in the pioneering types of discoveries and in fact being the first to market with things based on those pioneering discoveries.

Mr. PACKARD. Dr. Lederman, do you have a comment?

Dr. LEDERMAN. Well, I'm clearly someone who has been active in fundamental research, far out research, useless research, if you call it, abstract stuff, and yet you would be blind to the lessons of history if you didn't realize that when the Greeks started us off on science with a notion of how the universe works, that's been the motivation if you like, you an easily follow the road from there to Waxahachie, Texas where some other large project, megaproject is going on, and the road is very clearly traced.

In the course of answering that question, we've changed the way people live on this planet and how they think on this planet, and you go through various highlights of that. I don't think I have to lecture you on the Newtonian revolution and on the discovery of electricity by Faraday, all out of curiosity-driven, how does the universe work questions. I think that goes on and on and all the way to before the transistor, there was the quantum mechanics.

When data came out of the atom, it was totally inexplicable and when finally it was solved in Europe in the 1920s and 1930s, we had made a total revolution in our understanding of what goes on inside of an atom, which now I once estimated accounts for about a third of our GNP. That's the understanding of the processes inside an atom.

If you ask the innovators of that, Mr. Schrodinger and Heisenberg, and so on, why did you work on this atom, they wouldn't have said anything about the GNP. They wouldn't have known what it meant. It was there. It was how the universe works.

I don't see any reason why this sequence of things doesn't continue on and on. Science begets technology, technology then enables science to make more powerful things. If you go to a particle accelerator, it's full of technology which comes out. Without that technology, we'd be helpless. Without the incredible advances in computing science, we'd be helpless nowadays, but the science also begets the technology, so it's an interactive thing which because it interacts accounts for the tremendous pace of change we have in modern society. It's because of this escalating spiral of science, technology, science and it's our job, it's your job to make the right balance and not to skew it too far in one direction or the other direction. That's the hard part.

Mr. PACKARD. It's obvious this dispute extends itself to the very panel. We'll have the last word from Dr. Lauffenburger.

Dr. LAUFFENBURGER. Well, my comments here would just echo and support those of Dr. Schmitt and Lederman. I would have nothing to add other than what those two have said already.

Mr. PACKARD. Do you want to have the last word, Dr. Roy?

Dr. ROY. Yes, of course we disagree with Dr. Lederman with the value of some of that science. I'm putting economics before us because I think we are in economically stringent times. If you look at the sports economy, in my testimony I said we can talk about science and we can talk about sports, baseball, football and so on, and there are some sciences, abstract sciences which are about as important as tiddly- winks, but it's a very interesting subject to those who play tiddly-winks, but do we spend our time on scientific tiddly- winks which do not connect to the business of the country, to the human needs, I say basic science is that which is closest to human needs.

I think the basic science which you should be supporting is that kind of science and I believe the definitions—I think we need to have a panel on really trying to answer that question, Mr. Packard, which way does the flux run and let's illustrate it because there is more than one kind of science. I've tried to use those expressions. Science for society which is publicly funded and science for self and for the institution which should be privately funded.

Dr. LAUFFENBURGER. I would like to add one thing. The difficulty with what was just said is that it's almost impossible for Congress or anybody else to decide which of today's tiddly-winks will actually end up being the building blocks for a technology tomorrow. That's the mistake you don't want to make.

Mr. PACKARD. Thank you very much.

Mr. BOUCHER. The gentleman from Oregon, Mr. Kopetski.

Mr. KOPETSKI. Thank you, Mr. Chairman.

I'm fascinated by this discussion. I represent one of the great research facilities in the world, Oregon State University, and we have both the partnerships, joint ventures and we have basic research going on in all kinds of areas. In fact, the Chair was able to take the Chair of the full committee up just this last Friday to show off some of the interesting activities going on.

I was curious about the whole debate going on with respect to the indirect costs and what amount and the proper role of the Federal Government and what kinds of reforms are needed to remove confusion, achieve maximum accountability for indirect costs.

Perhaps the panel could, if they like, comment on this area.

Dr. SCHMITT. I presume you were not in when I introduced my testimony saying when university presidents appear before a congressional committee today they have to establish their credibility

The fact is there are two things I think that are going on. One is there are some real and valid costs pressures on academic institutions, for example, the new animal facilities, for example microelectronic facilities. There are a lot of expensive kinds of facilities that are coming onstream today whose maintenance, upkeep, heating and everything else are more expensive. A lot of environmental regulations, the universities are subject to some real cost pressures just like every other institution in the country because of some of the things we've decided are important to do. That's going on and that's valid.

The second thing is that the question of what is it legitimate to charge to overhead of a research contract or not, is obviously up for debate. Frankly, academic institutions thinking about overheads have been faulty in my opinion. What I found is with a very modest overhead rate of 59 percent now coming down, we collect on our research contracts that what was happening is they were taking some of those "overhead dollars" and giving them back to researchers to use for direct costs to give them incentive to get more contracts.

It just blew my mind coming from industry that that illustrated a total misunderstanding of what overhead was or the recognition that overhead costs are real costs. I think there is that kind of an attitude

Mr. KOPETSKI. Do you think the community can get together and decide what is an indirect cost and what's not?

Dr. SCHMITT. It isn't all that clear in industry either. In the end, you have to establish the rules and the regulations as to what is legitimate charge and what isn't. Believe me those debates go on in industry as well. We just haven't paid as much attention to them in academic institutions.

Dr. ROY. I think that there was a solution, although Dr. Schmitt's indirect costs are going down, and you really should commend an institution which has done that because we should give them an incentive, give them an extra point or two. You've got to give an incentive to us to take our overhead costs down. At present there is no incentive, we want to pad it and make it look bigger.

One way that NSF used to run for many years was a fixed overhead, whatever that number is, and that's one way that we all will kind of drop off certain items from overhead and do it. It would simplify bookkeeping, it is a possible solution and I commend to you the reexamination of that.

The second one is something that Dr. Schmitt mentioned in his testimony which I had in my book about 10 years ago, an IR&D, an Independent Research and Development fund in order to make that legitimate. We really should give—universities, who do $200 million worth of research and if the feds are putting up $100 million of that, there could be an IR&D fund and a fixed overhead.

I think these two would separate those two items, and I have been advocating both of those for quite a long time.

Mr. KOPETSKI. Thank you, Mr. Chairman.

Mr. BOUCHER. The Chair thanks the gentleman and there being no further questions by members of the committee at this time, with the Chair's thanks, this panel is dismissed. The hearing is adjourned.

[Whereupon, at 12:26 p.m., the subcommittee adjourned, to reconvene at the call of the Chair.]

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