Greetings and Introductions

Provost Jonathan Cole

COLE: Good morning. I'm Jonathan Cole, Provost and Dean of Faculties at Columbia. It is truly a great pleasure for me to welcome you to Columbia University and to the first of our three-part conference entitled "Science, the Endless Frontier 1945-1995: Learning from the Past, Designing for the Future."

Science and technology policy would appear to be in a state of crisis. There are many indicators that a crisis does exist in the partnership between the federal government and the American research universities, that the terms of the partnership are increasingly being questioned and re-examined. If it should turn out that this is not, in fact, the case, so many knowledgeable and informed members of the academic and scientific community believe that it is so, that the perception of crisis is real in its consequences.

It is probably not a matter of hyperbole to suggest that we are witnessing a number of fundamental changes in the relationship between the federal government and the scientific and technology research community. There are apt to be material changes in the national system as innovation proceeds in the years ahead. To many observers, this moment of crisis appears ironical, after all, each of us is aware of the extraordinary half-century of scientific and technological growth and achievement that we have witnessed in the United States.

This 50 years of exceptional growth in knowledge in terms of its diversity, sheer volume, and unquestionable quality is perhaps unmatched since the glorious period of scientific and technological development in 17th Century England. It happens that the past half-century of America's emergence as the pre-eminent nation in the development of science and technology coincides with the period since the publication of *Science: The Endless Frontier*.

Even if Vannevar Bush is as much a symbol of this period of dramatic progress as its putative architect, it seems fitting that in the 50th anniversary year of the publication of *Science: The Endless Frontier*, we celebrate the work and the period: reflect on the origins of the Bush paradigm for scientific excellence, take stock of where we currently are in the relationship between science and government, consider whether we are in a period of crisis, and do some serious work on the future shape of the national system of scientific and technological innovation.

Historically, Columbia has played a formidable role in the development and elaboration of post-war science policy. It also has been a major beneficiary of that policy. In examining our own history, I thought it fitting for Columbia to host the major working conference to address critical issues of American science policy.

To that end, roughly a year ago I brought together a group of Columbia faculty deeply interested in these issues, with the goal of formulating plans for this conference. The members of the conference planning committee are listed at the end of your agenda for today's meeting.

Professors Richard Nelson and Michael Crow, and an extraordinarily able graduate student, Chris Tucker, who spent scores of hours developing ideas for the conference series, merit special recognition. They consulted with some extraordinary colleagues from around the nation. Together we developed the central themes for this series.

Now the intent of this conference is more than a celebration of Vannevar Bush or a celebration of the 50th anniversary of the publication of Bush's pioneering report. It is our intent to have this series serve as a forum for the thought analysis of the current and historic policy environment and as a forum for the presentation of new concepts regarding the design of a new science and technology policy model.

We are, of course, aware of the ongoing debates in Washington regarding science and technology policy matters. While cognizant of these debates, which are apt to become still more intense with the recent changes in Congress and the possible prospect of an implemented "Contract With America," we intend for these meetings to be driven by analytical rigor and concept development.

The support of science by the federal government should concern us and we must, I believe, work to educate members of Congress about the returns on the public's investment in science and technology. We must make the effort to prevent damage to the organization of productive science and technologies at our universities and national laboratories.

But we should not mistake symptoms of crisis for its causes. We must concern ourselves with both the origins of the Bush model and how it facilitated the growth of knowledge, and the causes of stress and breakdown of some features of the model.

By design, therefore, these meetings are intended to serve not as another forum for debating the latest Washington policy option or issue, but rather as a forum for historic analysis and new concept development. That is why we have brought together this extraordinary group of knowledgeable experts, the speakers, panelists and active audience participants.

In terms of structure of support for scientific and technological innovation, where have we come from, where are we today, and where are we going?

This meeting will provide analysis and context for the two meetings to follow. We believe that a formal historical analysis of Vannevar Bush, his path-finding report, and the science policy that later evolved is in order.

The second conference in May 1995 will focus on reviewing the institutions for research that grew out of the Bush model, how the national system of innovation has evolved, and what the fundamental issues facing each group in the system are.

The last of the three conferences, to be held in the fall of 1995, will be two days in length and will focus on designing options for the future. We hope that each of you will be able to participate in all three of these meetings.

Many of you know that we are not alone in celebrating the half-century since the publication of *Science: The Endless Frontier*. Other sessions on the subject have been organized by the American Association for the Advancement of Science and Sigma Xi. The National Academy of Sciences is also undertaking a project associated with the Vannevar Bush model for the support of science in the public interest.

For now, let us return at least momentarily to the November 17th, 1944, letter from President Roosevelt to Vannevar Bush. Roosevelt asked Bush to answer four questions.

One, "What can be done, consistent with military security and with the prior approval of the military authorities, to make known to the world as soon as possible, the contributions which have been made during our war effort to scientific knowledge?"

Two, "With particular reference to the war of science against disease, what can be done now to organize a program for continuing in the future the work which has been done in medicine and related sciences?"

Three, "What can the government do now and in the future to aid research activities by public and private organizations?"

And fourth, "Can an effective program be proposed for discovering and developing scientific talent in American youth so that the continuing future of scientific research in this country may be assured on a level comparable to what has been done during the war?"

Vannevar Bush answered these questions in his report, aided by many members of the scientific community who joined committees to consider each question. From the debate that followed the publication of the report, came the evolution of the National Science Foundation and a national system of innovation that linked the federal government with research and graduate Ph.D. education at research universities.

The model and its implementation led to the unprecedented partnership between the people of a nation, their elected representatives, and the producers of scientific and technical knowledge and human capital.

After 50 years, however, serious questions confront us in light of the erosion of some terms of the partnership. How can the partnership between research universities and the federal government be redefined and new sources of research support be acquired without entering in Faustian bargains?

When all is said and done about changes in the Bush paradigm, the federal government must and will continue to be the basic supporter of basic research in the nation and at universities. But it is not apt to invest on the same terms that existed during the period of extraordinary growth and knowledge over the past 50 years.

Consequently, the dilemmas facing research universities in particular are nothing less than how to sustain the world's most creative science and technology enterprise without the rate of increases in federal support that would appear to be needed to do so.

But these dilemmas are not simply about new resources, they are about the types of changes the university scientific community will have to undergo and the bargains it will have to strike in the effort to preserve and expand the research enterprise while ensuring continued quality. The drama in the situation lies in the nature of the bargains. What is being given up, at what cost, to achieve what goals?

In the post-Cold War era, the military rationale for government investments in science – which we undoubtedly will hear more about today – has to be replaced with a new rationale, one that builds more from the social and economic benefits for continued investments in American science and technology.

At today's session, we will analyze the original report and its consequences for science and technology over the past 50 years. The initial discussion will be placed in historical and political context and analyzed by distinguished members of the community of scientists and science policy analysts.

Now since this day's work is intended to produce substantial discussion and interaction, I want to apologize in advance for spending minimal time on the introductions of our speakers. Suffice it to say, that our speakers, respondents, and members of the audience were invited because each has produced an extraordinary record of achievement.

Science: the Endless Frontier as a Treatise

Professor Donald E. Stokes

Panelist Responses

Professor I. Bernard Cohen
Professor Gerald Holton

Moderator

Provost Jonathan Cole

DONALD STOKES: Vannevar Bush looms so large in our historical memory of the transformation of American science over the period of the Second World War, it is small wonder that we mark the half-century of the publication *Science: the Endless Frontier*, the illustrious report that helped usher in a golden age of American science.

Rather than probe the background and drafting of that report, I will deal with the significance of the argument that Vannevar Bush set out for the making of science policy in the post-war years and the legacy of that argument for the debates over science and technology policy in our own time – the increasingly troubled dialogue between science and government today.

It would be difficult to exaggerate the degree to which the relationship between government and science was transformed by the Second World War. The federal government had been involved in scientific activities from the beginning of the republic, and by the late 19th Century, a good deal of science being done in this country was in federal establishments such as the Smithsonian Institution, the Geological Survey, and the agricultural experiment stations that were started with federal support.

However, the current model of advanced scientific studies was not spread through the country by federal establishments. It was promoted by the nascent research universities, which laid the groundwork for their preeminence in science in the 20th Century with resources gathered largely from private donors, philanthropic foundations, state legislatures, and fee-paying students.

Indeed, by the period between the world wars, there was active hostility on the part of the scientific community to the acceptance of federal support, stemming from unease about the control that such support might bring. But this hostility was dramatically transformed by the war. It was a scientific war in large part, and that effort was led by enlightened scientists, with Vannevar Bush in the vanguard.

Bush recruited a small army of gifted colleagues for the scientific tasks of the war, with full backing of the strongest president of the 20th century. The Office of Scientific Research and Development (OSRD), as Hunter Dupree has noted, became as close to a General

Ministry of Research as this country has ever had. And the flow of resources for scientific purposes – including basic nuclear science research that produced the weapons that decisively altered the course of the Pacific War – showed the scientific community, as it showed the nation, what might be done.

As the war drew to a close, there was agreement between the scientific and policy communities that support should continue into peacetime, but the perspective of the scientific community was based on radically different grounds. When Franklin Roosevelt requested that Vannevar Bush develop a post-war science plan, the scientific community was determined that if this flow of resources continued, the direct governmental control of the content of research should be drastically cut back. That, in the broadest terms, was the aim of the report that Vannevar Bush produced.

The means that were used to try to achieve the dual effect of continued governmental resources with reduced governmental control were partly organizational. Four background advisory panels that went to work on the problem. The most important of these was chaired by Isaiah Bowman, the President of Johns Hopkins University. That panel developed the plan of a national research foundation with the responsibility, essentially as broad as that of OSRD during the war, of channeling most of the federal grants for the support of research.

They wanted to insulate the funding from the political process by making the foundation self-governing, with a board that was drawn from the scientific community, and that would choose its own director rather than having a director appointed by the President and confirmed by the Senate. They even sought to withdraw funding from the annual budget cycle by establishing a long-term, expendable endowment that would need to be replenished only at widely-spaced intervals.

Bush revised the organizational proposals to restore the foundation to the budgetary process, but he retained the idea of the director chosen by the board. If that plan had been implemented, it would have insulated the funding of science from the political process. However, much of the significance of *Science: The Endless Frontier* lay in the fact that the means by which this dual pair of objectives was sought was not left to organization alone.

Bush also included in his report a general way of thinking about the nature of basic science and its relationship to technological innovation. This turned out to be profoundly important in the longer run, so that as the proposed organizational plan foundered, the skillful use of Bush's ideological view of those basic relationships – what we might call a "paradigm view" – was employed more and more by those who wanted to achieve the objectives that were being sought.

A great deal of the vision of the nature of basic science and its relationship to technological innovation is contained in two aphorisms in the Bush report, both worthy of Francis Bacon.

Each was cast in the form of a statement about basic research – a term that was given currency by the Bush report.

The first of those aphorisms is that basic science is performed without thought of practical ends. That sounds like a definition, and a great many people have subsequently wanted to take it to be a definition, but Bush made it quite clear that the defining characteristic of basic research is its attempt to find more general physical and natural laws to push back the frontiers of fundamental understanding.

What that aphorism came to mean, instead, was that there is an inherent tension between the drive toward fundamental understanding on the one hand, considerations of use on the other, and by extension, a radical separation between the categories of basic and applied science. Bush went on to endorse a kind of Gresham's Law in which an attempt to mix the applied and pure in research was sure to result in the applied driving out the pure.

Having written that canon of basic research, Bush wrote down a second. It was that basic research is the pacemaker of technological improvement. If you insulate basic science from short-circuiting by premature thoughts of practical use, it will turn out to be a remote but powerful dynamo of technological innovation – the advances of basic science will be converted into technology by the processes of technology transfer, moving from basic to applied research, to development, to production or operations, according to whether the innovation is a new product or a process.

It is interesting to note that both those canons came to be captured by very simple, one-dimensional graphics. The first was represented by the ever-popular idea of a spectrum of research from basic to applied. The dynamic version, the second canon of basic research, was represented by the equally popular idea of the linear model that moves from basic research to applied research via the processes of technology transfer.

There was a third element in Bush's argument that has turned out to be one of great importance, that is very closely associated to the second canon of basic research. It is the notion that the nation will recapture the technological benefit of its investment in basic science.

This idea appears most clearly in the Bush report in the obverse form, in his statement that, "A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill." I will return to this additional element, the third part of a triad of fundamental assertions that turned out to be tremendously important in the Bush argument.

The reception of *Science: The Endless Frontier* was full of irony: the organizational plan was defeated, while the ideological view prevailed. In the five-year gap between the publication of that report in 1945 and the creation of the National Science Foundation

(NSF) in 1950, the authority of the NSF, which Bush had wanted to keep whole, was shattered by the policy process.

First of all, in 1946, responsibility for nuclear science went to the newly organized Atomic Energy Commission (AEC). In 1947, responsibility for basic science bearing on the military went out to the newly organized Department of Defense (DOD).

Perhaps most tellingly of all, the responsibility for biomedical and health research which had been part of OSRD during the war, went to the National Institutes of Health (NIH), as what had been a small in-house laboratory was reorganized into a much larger in-house complex and the huge flourishing external grant agency that we know today. So that when the NSF was created in 1950, it had the much narrower mission of supporting largely pure scientific research, largely in the university sector.

The irony is deepened by the fact that the defeat of the organizational plan made it more likely that the ideological view would triumph. Indeed it is likely that the cluster of ideas Bush outlined would have been only partially noticed in that report had it not been needed for the purpose the scientific community and its allies in the policy community wanted to achieve – independence from federal control – and this could not be achieved by the organizational plan.

Indeed, only when the organizational responsibilities for science were shattered and fragmented could the DOD use the Bush outlook to cement its relationship with the universities. In 1948, an enterprising reporter for Fortune Magazine went to a meeting of the American Physical Society and found that 80 percent of the papers being presented at the meeting were supported by the Office of Naval Research. At the onset of the Cold War, it was deemed essential to restore the status-quo ante of the second world war for a wide part of the basic scientific community. And when the NSF was created in 1950, it could happily endorse the view that pure research is the ultimate font of new technology, a view that was very congenial to an agency whose narrow limited function was to support basic research.

Indeed if Bush's National Research Foundation – with responsibilities almost as broad as OSRD's – had been created in the immediate aftermath of the war, the first of Vannevar Bush's canons, that basic research is performed without thought of practical ends, would almost certainly have come under intolerable pressure as the agency attempted to build and fund research agendas that met all of the scientific needs of the federal government.

There is very little doubt that the vision that was set out in *Science: The Endless Frontier* soaked into the scientific community very deeply, and into the policy community as well. If you want evidence of that, it might be clearest in the country's response to the launching of Sputnik in 1957. One might have imagined that our response to that technological surprise by the Soviets would be largely technological – that we would build bigger booster rockets and all the rest and, as we did ultimately, put a man on the moon.

But what is really significant about the country's response is that we regarded it not just as a challenge to a piece of our technology, but as a general scientific challenge. The years after Sputnik were years of soaring budgets for almost all branches of science, so that the technology coming out of the other end of the pipeline, according to the linear model, would be our technological surprises and not theirs.

Admiring as we all can be of the success of the paradigm view set out in *Science: The Endless Frontier* and its ushering in of the Golden Age of American science, the incompleteness of this view of the nature of basic science and its relationship to technological innovation has been increasingly clear.

Let's first of all return to the first of Bush's canons, that basic research is performed without thought of practical use. The rise of microbiology in the late 19th Century is a conspicuous example of the development of a whole new branch of inquiry because of considerations of use, not only the quest of fundamental understanding.

There is no doubt that Pasteur wanted to understand the process of disease at the most fundamental level as well as the other microbiological processes that he discovered, but he wanted that to deal with silk worms, anthrax in sheep and cattle, cholera in chickens, spoilage in milk, wine and vinegar, and rabies in people.

The melding of those motives in the work of the mature Pasteur is so complete that you could not understand his science without knowing the extent to which he had considerations of use in mind. The mature Pasteur – not the crystallographer at the dawn of his career, the man who took on the enigma of recemic acid at the *Ecole Normale* – embarked on a pure voyage of discovery. But the mature Pasteur never did a study that was not applied while he laid out a whole fresh branch of science.

And that example is not a solitary one. Lord Kelvin's view of physics was profoundly industrial and inspired in substantial part by the needs of empire. The work of the synthetic organic chemists, German and then American, over the turn of the century as they laid the basis of the chemical dye industry, and later, pharmaceuticals, was equally a melding of those two motives. Keynes sought an understanding of economies and their dynamics at the most fundamental level, but he sought that to lift the grinding misery of depression.

The creators of modern analytical demography have always regarded population change not only as a process that challenged understanding on a fundamental level, but as a problem with immense human consequences. Both the molecular and non-molecular ends of modern biology are profoundly influenced by scientific and applied objectives at once. And the earth sciences have always been influenced by natural disaster and economic gain.

Indeed, every one of the basic scientific disciplines has its modern form, in part, as the result of use-inspired basic research. We should no longer allow the post-war vision to conceal the importance of this fact.

Since that post-war vision has been kept in place, in part by very simple graphic images, I have created a little bit of graphic reasoning to try to move one step in a more realistic direction. This array presents a new model of scientific research, which provides a more accurate depiction than Bush's linear model. I call it "Pasteur's Quadrant."

Research is inspired by:

		Considerations of use?	
		No	Yes
Quest for fundamental understanding?	Yes	Pure basic research (Bohr)	Use-inspired basic research (Pasteur)
	No		Pure applied research (Edison)

(adapted from *Pasteur's Quadrant: Basic Science and Technological Innovation*, Stokes 1997).

If we were to return to the spectrum of basic to applied and ask ourselves where Louis Pasteur is on that spectrum, you might think initially that he is somewhere near the middle because he cared about both those goals at once. But that would be clearly mistaken.

You might conclude that he belongs way out toward the basic end of that spectrum, but he also belongs way out toward the applied end of the spectrum. Thus the anomaly of the mature Pasteur as two Cartesian points in this Euclidean one-space. If we want to stay with the Euclidean framework and eliminate this anomaly, we must grasp that spectrum in the midpoint and fold the left-hand end of it through an arc of ninety degrees. This restores Pasteur to the status of a single-Cartesian point in what is now a two-dimensional conceptual plane, with the vertical dimension representing the degree to which a given body of research is motivated by the quest of fundamental understanding, and the horizontal dimension the extent to which it's motivated by considerations of use.

There is not the slightest reason why these questions should be treated in dichotomous terms, but since the whole world loves to think in terms of dichotomies, then it's plain we have a double dichotomy.

Take a moment to consider the quadrants that are presented. The one at the upper left is for the pure voyages of discovery, the voyages of Newton. Let me call it Bohr's Quadrant, since there were no immediate considerations of use in mind as Niels Bohr groped toward an adequate model of the structure of the atom; although note that when he found it, his ideas remade the world.

The quadrant at the lower right might be called Edison's Quadrant since Edison never allowed himself or those working with him in Menlo Park five minutes to consider the underlying side of the significance of what they were discovering in their headlong rush toward commercial illumination.

Edison himself one night heated up a filament in a vacuum and observed what is now known in American physics as Edison's Effect because he wrote it down in his notebook. I owe to Nathan Rosenberg the observation that if he had tried to consider its more fundamental implications, he might have shared the Nobel prize with J.J. Thompson for discovering the electron, but he went right on.

But there certainly is "Pasteur's Quadrant," for work that is directly influenced in its course both by the quest of fundamental understanding and the quest of applied use – the sort of quadrant that supplies a home for what Gerald Holton has called, "work that locates the center of research in an area of basic scientific ignorance that lies at the heart of a social problem."

Now I will not comment on the fourth quadrant. Naming it is a growth industry, but I would just note in passing that it is not empty. And the fact that it is not empty helps to make the point that this is not a more elegant version of the traditional basic-to-applied spectrum, that we genuinely have a two-dimensional, conceptual plane.

Examples are equally plentiful that contradict the very simple dynamic linear model. One reason we can be sure that basic science is not simply exogenous to technological innovation is how often modern science is explaining phenomena that are found only in the technology.

An example of this process from earlier in the 20th Century is the work of Irving Langmuir, who became fascinated by the surfaces of the electronics components that were manufactured by General Electric and its other firms. It would not be right to say that the several billion-year history of the universe had not presented any analogs of those surfaces, but the human race had never seen them. The scientific community had never seen them until they appeared in the technology.

Langmuir, as he earned himself a Nobel Prize for working out their surface physics – a fundamental advance in physical chemistry – also laid the basis for patents by General Electric that secured its market position for years to come.

That example is one of an increasingly large number. Another would be the ongoing effort of the condensed-matter physicists to see whether semi-conductors can be built atomic layer by atomic layer – something that will require a fundamental advance of science to do – but focusing on phenomena that would not have been seen absent the miniaturization of semi-conductors with their astonishing increases in speed over several decades' time.

Indeed, we're going into the 21st Century with two closely interwoven trends: one, which is commonplace, is that more and more technology will be science-based. The other, which is still very widely under-appreciated, is that more and more science will be technology-based in just the sense that I've expressed and not merely in the sense of instrumentation, which has been important in Western science at least since the time of Galileo.

If we were to present a rival image for the one-dimensional linear model, it would be much more like the rise in fundamental scientific understanding and the rise in technological know-how as two loosely coupled trajectories. They are loosely coupled because the increase in scientific understanding is, at times, the result of pure science with very little intervention from technology, while the increase in technological capacity is often the result of engineering, design, or tinkering at the bench, in which there is no intervention by fresh advances of fundamental science. But at times, each of those trajectories profoundly influences the other. The influence can go in either direction with use-inspired basic research often cast in the linking role.

The experience of recent decades also has called into question the third of the elements of the vision in *Science: The Endless Frontier* to which I've referred, which is that the nation can expect to capture the technological return from its investment in basic science.

If we had been sitting at Vannevar Bush's elbow when he wrote, "A nation which depends upon others for its new basic scientific knowledge will be slow in its industrial progress and weak in its competitive position in world trade, regardless of its mechanical skill," we might have said, "Now just a moment, Dr. Bush, elsewhere in your report you've noted that the Yankee ingenuity borrowed the science of Europe to make great industrial strides – indeed the greatest in our economic history." But in the post-war world, with the U.S. so much in the ascendance both in science and technology, no one asked that question.

It has been asked increasingly insistently since, as the Japanese have repeated that historical lesson, making the greatest industrial strides while they continued to be substantially behind this country and Europe collectively in basic science. It has been an increasingly skeptical point in the policy community as to whether the investment that they are asked to make in pure science will bring a technological return that will be ours and not someone else's.

However much we may admire the foundation for post-war science that was laid by *Science: The Endless Frontier*, the bargain that was struck at that period between science and government was bound in the longer run to be a Faustian one.

If the society was told that a heavy investment in pure science would produce the technology to handle a full spectrum of society's needs, it was bound several decades later to stop and say, "Now just a moment, we have some unmet technological needs. Indeed, we have some that have been created by the technology spun off of your science – the deal is off."

Echoes of that view can be heard in the speeches of even such a great friend of basic science as George Brown, the former Chair of the House Science, Space and Technology Committee. Echoes can be heard in what Senators Mikulski and Rockefeller have said to the Forum on Science in the National Interest convened by the Office of Science and Technology Policy (OSTP), and in the white paper released by the British government.

The time has come to cut into an increasingly troubled dialogue between the communities of science and government with a fresh, more realistic formulation of the actual nature of basic science and its relationship to technological innovation. This would very much accent the importance of work in "Pasteur's Quadrant."

This more realistic vision is profoundly in line with Vannevar Bush's actual career. One of the lasting ironies about *Science: The Endless Frontier* is that the vision set out in it was so different from the genius of Bush's career as scientist-engineer and research administrator. From the beginning of his career, Bush showed his skill in bringing together judgements of societal need and of considerations of use and scientific promise.

That was certainly the key to how creative he was in national government, from the time, in the late pre-war years, when he became Chair of the National Advisory Committee on Aeronautics, to the dusk of his career when he Chaired the joint Research and Development Board for the Secretaries of War and the Navy.

In terms of our present experience, we have got to learn how to bring together authoritative judgements of societal need. In a representative democracy, those have to relate to the centers of legitimate authority in the White House, the Congress, and the nation, with absolutely rigorous and first-class judgements of scientific promise. That will require a set of institutional arrangements and processes.

The savage budgetary pressures we will have at least into the 21st Century are part of the reason why we must attempt to develop a fresh contract between science and government. It must make the case for continued societal investment in realistic terms of the problem-solving capacity of science, terms that command the support and enthusiasm of the policy community and the country behind it.

While I believe it's time to depart from some of the vision that was crafted in *Science: The Endless Frontier*, this does not represent any sort of wholesale rejection of the legacy of Vannevar Bush.

BARBER: Thank you very much for an exceptionally instructive discussion. I was hoping you were going to say one more thing about the report: that it proposed this insulation from the political process – a problem that festered for quite a while – and that President Truman, of course, refused to accept the National Science Foundation legislation until the condition was set up that it should, of course, be part of the general political process. Would you say something about that, please?

STOKES: I'm not sure whether you want me to comment historically or contemporaneously. Certainly, we can understand why Harry Truman and a great many others were not prepared to accept a set of organization arrangements that were so out of keeping with what was ordinarily true in American science.

But my own view, and this is the bias of a political scientist, is that if you had had that translated into legislation, the pressures on the National Science Foundation would have been intense from the stakeholders in the post-war system. And the experience of Vannevar Bush and others during the war would not have prepared them for it, even though Bush was an extraordinarily skillful political soul.

The authority of the war-time White House was simply matchless, and the transition to peace was going to substitute a cacophony of power centers, many of them on Capitol Hill, many of them in the departments and agencies. Indeed, some of those power centers ultimately broke Bush's health when he tried to deal with them in his last experimental assignment – some of them in industry, the sort of thing that he knew a great deal about, what came to be known as the "military industrial complex." So that simple organizational plan was just inherently unworkable.

Now in terms of our present experience, we have got to learn how to bring together authoritative judgments of societal need – and in a representative democracy those have to have some reference to the centers of legitimate authority in the White House and the

Congress and the country behind them – with judgments of scientific promise that are absolutely rigorous and first-class.

And that will require a set of institutional arrangements and processes that, as I say, are the subject of a whole additional lecture.

But I think that we are better able to do that today, we are more knowledgeable. The problem is that we will have budgetary pressures at least into the 21st century that are absolutely savage.

But those very pressures are why it seems so important that we develop a fresh contract between science and government in realistic terms that make the case for this continued societal investment – in terms the problem-solving capacity of science, which does command the support and enthusiasm of the policy community and the country behind it.

BARBER: It was another irony of the report that Bush's whole experience was that government was very successful in helping science. Here he was expressing an ideological opposition to government, which was very extreme, not reflective, I think, of widespread opinion among scientists at the time. Do you think that ideological view persists today and will cause trouble?

STOKES: Well, I think even those who had been most closely involved with him and the war effort were just ready to bring that to a close. The enormously distinguished group of scientists who did the Manhattan Project detested – and personalized their detestation of – Leslie Groves and all that he represented: the secrecy, the arbitrary intervention, the control. They wanted to get back to their campus laboratories. They wanted to drastically cut back the degree of direct governmental authority over the content of research.

And Bush undoubtedly shared that to a major extent, but also was responding to a constituency. In an immediate process sense, he was responding to the Bowman panel, which had expressed that view to an ultimate extent.

TRAUB: My question will be contemporary. As you pointed out, an argument for the funding of scientific research is long-term national advantage. What will be the effect of globalization on that argument?

STOKES: It will be massive. There are two related reasons why I think that an accent on what I'm calling "Pasteur's Quadrant" can be helpful. One is that, by a reverse twist, it does help to make the case for the support of pure science because of the unity of science.

If you attract the sympathy of the policy community of the country, in terms of the problem-solving capacity of science in a given area, you will also lay the basis for the investment in pure science in related areas. And that has happened over and over again.

The breakthroughs of condensed-matter physics have brought an explosion of pure science in condensed-matter physics as well. The invention of recombinant DNA techniques has brought an explosion of pure science in molecular biology as well. The advances in polymer chemistry have brought an explosion of pure science in related areas of chemistry as well, so that's an additional argument that I think ought to be persuasive with the scientific community.

But I also believe that the problem of who captures the technological return is somewhat easier to deal with when you are speaking of work in "Pasteur's Quadrant" than when you're speaking of work in "Bohr's Quadrant."

TRAUB: There's a tradition of funding basic research in high-tech manufacturing – Bell Labs, IBM, Boeing, GE, etc. If you ask people in the service sector about basic research, the answer is: we don't get a long-term competitive advantage, six months perhaps.

It seems to me that, as we get more global, the same argument might be used against funding of basic research where we don't achieve any long-term competitive advantage.

STOKES: Well, we do have globalization of knowledge and the industrial implications of knowledge. What is underway is the lessened capacity of countries to do economic stabilization in isolation. And therefore we need to seriously consider collectivizing some of the costs of fundamental science.

Indeed, the success of the Japanese shows – as we showed earlier in this century – that you can extend the benefit of knowledge that is the free product of fundamental science. Yet, we’re in some danger of producing a prisoner's dilemma, with countries wanting to move resources toward industrial-technology investment rather than fundamental science because they cannot be sure they'll capture the benefit.

The only way of dealing with a prisoner's-dilemma problem is collusion. And collusion in this case is not having us build an SSC in Texas but investing in CERN on the understanding that the next facility beyond that may be ours. Now that, also, is the subject of a whole additional lecture because that's very hard to do, and those investments, once made, have a life of their own. But, plainly, that's part of what we must do.

SANET: As you had discussed, in Dr. Bush's era, there was no strong distinction between basic science and technology, which had its great benefits. But the dichotomy – strong and serious dichotomy – between the two ends of the spectrum is very recent.

That points to the great need for educating the politicians and public both to the concept that basic science, applied science, engineering, and technology form a continuum, and the totality as an enterprise needs to be nourished. And if you don't nourish the whole enterprise, the enterprise and all its parts fall together.

You may have noticed that I put several more points than the two points in your spectrum. There is a cascade from one end to the other, as well as the reverse, as you had mentioned. But there is a cascade from both ends, and they hold together, they are coupled together, very closely. Now the question is, how do we accomplish that?

STOKES: I think I would dissent, at least mildly, from your premise. The strong distinction between science and technology achieved by the Germans in the late 19th Century, with the Germans so marvelously successful in both, led an admiring world to suppose that that was the natural order of things – including thousands of American students who flocked to the German universities and brought back into this country a vision of pure science that was really quite false to earlier American experience. American science was the science of Franklin.

And in the 20th Century, in academic life, the division of labor between the pure physical-science departments and the engineering departments has been thought by people who have seen the world in terms of the *Science: The Endless Frontier* paradigm as reinstitutionalizing that natural distinction, missing the fact that some of the most important Pasteur's Quadrant research has been done in the engineering departments.

If I had a single example, it might be the heroic advances in physical chemistry that would lay the basis of modern chemical engineering, work mainly at M.I.T. after the first World War. In fact, my colleague Charles Gillespie is prepared to say that one of three areas in which this country first became world-class in science was chemical engineering. So the apparent institutionalization of the pure / applied split in the physical-science departments and engineering is false.

But the perception was there, even in Vannevar Bush's time, and even despite the fact that his own career shows very clearly how wrong that sharp division was.

ANDERSON: When we mention the Japanese and their apparent ability to turn our basic science into their profit, my feeling is that occasionally we go a little bit too far, and we under-emphasize the extent to which we succeed still in holding onto the leadership in basic science and technology.

There are fields in which we have an overwhelming advantage in the technology, and it's because we have the overwhelming advantage in basic science as well – I could name several others, and I'm sure people here can. So, I wondered if you agreed with this. We have a danger in weeping into our beer a little bit about this situation. We have a danger in giving our politicians the impression that we are not succeeding in many cases as well.

STOKES: I not only agree, I very much welcome the example because the work in software is very much a Pasteur's Quadrant sort of field. I also would not want to be misunderstood, either, as to the scale of the Japanese success or as to the detailed nature of that. In many cases, what they licensed was a more finished technology that already had been the result of technology transfer in this country.

Now, they reverse-engineered on a massive scale, and learned a great deal from that, and Japanese science still probably is undervalued in North America and in Europe. It's coming on strongly, although the Nobel Prizes have yet to appear in any real frequency. But I very much accept your comment.

COLE: Thank you very much. It is now a great pleasure for me to bring to you Professor I.B. Cohen of Harvard University.

COHEN: Thank you very much, Dr. Cole. I take it that my appearance here among many of you who have been very important as policymakers is primarily as historian and witness. In order to understand the attitude of most American scientists in the days of the Bush report, and their zeal to advance and even to protect basic science, I believe we need to consider both the historical tradition and an actual situation.

When our modern science came into being in the 17th Century, a large number of founders were convinced of a dual role for the new science: one, to advance understanding of nature, and two, to use science in practical innovations to change every aspect of the conduct of life.

Two of the founders, two primary codifiers of the method, Bacon and Descartes, preached independently that the new science would yield important applications. It was Descartes, however, and not Bacon, who expressed a viewpoint like that of many 20th Century American scientists.

Bacon wrote that applications were of importance chiefly to prove that science was dealing with reality, and not to improve the comfort and well-being of mankind. But take heart, he argued, that if he could get financial support for scientific research, there would be benefits for artisans of all sorts, doctors, and so on.

And it was on this practical basis that the French government supported the new science. By mid-18th Century, however, despite continual promises, there was no delivery of the great practical benefits that had been promised, at least on a major scale. This happened for the first time as a result of Benjamin Franklin's research in electricity. No one then imagined that electricity was a practical subject. But Franklin, doing basic research led by curiosity, studied spark and glow discharges, electrical induction, and the significance of grounding, and concluded theoretically that lightning is an electrical phenomenon.

He devised several experiments to test this, and then invented the lightning rod. The reason he invented the lightning rod is not that he was practically minded, but that he had made the fundamental, scientific discoveries on which the invention could be based.

In the 19th Century, as everyone knows, science began at last to show its promise, its prowess as a fount of technology, medicine, and the world of practice. The most spectacular example has been mentioned, the field of the aniline-dye industry.

And until well into the 20th Century, the larger part, by far, of applied or industrial-oriented research continued to be chemistry. And with the success of applied science and the growing importance in the national economy, there was popular acclaim, and the public image of science was so closely associated with practice and invention that those concerned with the abstract pursuit of knowledge worried.

At the turn of the century, almost everyone in America had heard of Edison, but only a select few would have heard of the physicist Rowland. And so we may understand the complaints of Rowland and others about the low state of pure science in relation to the practical realm.

During the years between the two World Wars, many American scientists continued to worry about the lowest state of basic science and the over-emphasis on applications. They were hampered by the paucity of funds to support basic research and the lack of appreciation of pure science.

Because of the difficulty in funding basic science, the National Academy of Sciences in 1937 set up a task force, which had the result of constituting a new organization called the National Science Fund. The official constitution declared, "The object of the fund shall be the promotion of human welfare through the advancement of science."

There were other groups concerned with problems of science in the nation, the short-lived Science Advisory Board, the National Resources Planning Board, and others.

Now, the mission of this first NSF – as the National Science Fund was called – was twofold: one, to obtain funds for basic research, and two, to be the advocate of the benefits of investing in basic science. The chairman was William Robbins, director of the New York Botanical Gardens.

They decided that a useful propaganda tool would be a book for the general public demonstrating the ways in which disinterested, pure, or basic scientific research had yielded practical benefits. In 1941, I was chosen for this assignment, and the eventual book, *Science: Servant of Man*, centered on a collection of case histories, with an extended analysis to show the different ways in which applications followed from knowledge.

I introduce this episode, not to give you a bit of my autobiography, but as evidence that long before the Bush report, there was a strong conviction in the American scientific community that pure science was a major fount of applications, which justified financial support. Robbins and others in the National Academy of Sciences were well aware that business and taxpayers would never fully support the advance of knowledge for its own sake.

By 1944, World War II had effectively proved that academic science could produce astonishing practical applications. Theoretical and experimental physicists had been active in the well-known developments of radar, the atom bomb, and the proximity fuse. Many scientists came to consider it axiomatic that there was a simple chain from basic science to applied science, and development and production. In wartime, there had even been some direct transitions from the research laboratory into production. In peacetime, many envisaged a similar easy slide from pure science into technology.

Many historical retrospects on the thinking of scientists during the 1940s omit one or two aspects of their beliefs. One was the general impossibility of successfully predicting which particular subject of research would provide a desired, sought-for application. Another was that the person who discovers new truths may not be the best person to guide or even make the application.

This first point was dramatized in those days by a story told by Carl Compton. Suppose, he said, that in the 19th Century there was a goal to increase the efficiency of lighthouses. Research would be undertaken on the efficacy of fuels, the design of wicks and chimneys, the shapes of mirrors, and the forms of lenses.

But no one would have sponsored research on the twitching of frogs' legs or the waving of wires in front of magnets. Research that we know was motivated by chance and curiosity led to the electric current. A supporting example, in my book, particularly pleased the sponsors and that was the development of hybrid corn. This innovation would never – although I worry about the word "never" – have been produced if the motive had been primarily to improve the corn crop, and not the study of the evolutionary history of maize.

The reason is that the method of research consisted of in-breeding, producing so-called pure lines. After several generations, in-breeding produces scrawny plants, with very few ears, or small ears, or ears with a few kernels. These features made it appear that the line of research was not the way to increase the corn crop. In the end, this research did provide the basis for producing a useful product, but it was a wholly different group of scientists, chiefly Donald F. Jones, who figured out the method of the double-cross to convert these ideas into a useful practice.

This case history also serves to illustrate how academic scientists like George Shell of Princeton, who did the basic research, couldn't necessarily do the applications. Clearly, if you look back at this period, there was a tension between the zeal of scientists to preserve the freedom of basic research and to ensure its support, and their insistence that basic research is useful because of applications.

As has been remarked by our first speaker, there is a tension that appears in *Science: The Endless Frontier*, a difference between the point-of-view expressed by Vannevar Bush in his own presentation, and the accompanying report of the Bowman committee, whose mission it was to explore the needs in support of basic research. Bush was aware of the complex stages between a discovery by scientists and its eventual application. And he appreciated the dignity of applied research. But academic scientists generally belittled the activities of the applied domain, considering that this was a low intellectual activity and that the people who did it were not on the same high level that they were.

As far as I know or remember, during these years there was no thought that a major part of innovation in industry in the post-war years might really depend more on mechanical innovation or new methods of management or production than on applications of new basic science.

My own recollections of these issues stems from my involvement as witness in the Bush report, at least with that part of it known as the "Report of the Bowman Committee" dealing with basic research. Isaiah Bowman, president of Johns Hopkins, was not the most active participant in the deliberations. Much of the actual research, the assembling of ideas, was the result of a group of young men, some of whom were associated with the radiation laboratory. They included Henry Gerlack, the lab's official historian, Paul Samuelson, then a fledgling economist working as a mathematician, John Edsel, a promising young biochemist, and Rob Morrison, associated with the Rockefeller Foundation.

Henry called this group his "secretariat." I agreed to serve in a less formal and much less important position since I was then busy teaching war-time physics, trying to complete the book for the NSF, and revising a book I had written with Bernard Barber on the history of American science policy and the organization of science for war.

The position papers and memoranda contributed by this secretariat were, of course, discussed by the main committee, which determined the lines of policy. Several meetings of the secretariat were held in Washington with the full committee. These, as John Edsel recalls, were chaired by Isaiah Bowman, who was not otherwise in much evidence. Neither Edsel nor Samuelson recall that Bush was ever present at a meeting in Washington and elsewhere. I myself did not attend the Washington meetings, and I only wrote a small part of the final report.

Now, Henry Gerlack – an old friend and former fellow graduate student – would regularly call me to try out certain ideas that he was developing and to elicit information. He had a great gift of style. It was he, and not Vannevar Bush, who coined the oft-cited phrase that "applied science drives out pure science" with its ivory-tower implications from Gresham's Law that something bad drives out something good.

One topic on everybody's mind during the preparation of that report was the problem of the post-war years. Europe, clearly, for many reasons, could no longer be counted on as the major fount of basic science. America, everyone was convinced, would have to fill this gap. This may help to explain my own special assignment for the secretariat and my contribution to the final text: a report on the organization and support of science in Europe, notably in France and the U.K., and the history of the support of science and aspects of science policy in the United States.

As members of the secretariat, we talked to many scientists, not just members of the committee, in order to get their opinions and their points of view. There were several fundamental fears expressed by various scientists, worries about a government foundation for government-funded support of science. Some of those which I particularly remember, and which illuminate the problems, are these –

One, first and foremost, that a government-funded foundation might tend to support only projects related to practical problems, research with an apparently predictable, practical outcome.

Two, that a government-supported foundation might be subject to political interference, that the agenda for science would be determined by politicians, and not by scientists.

Three, that politicians might object to granting research funds on the basis of merit, rather on a system of geographical or population distribution.

Four, there was a real fear of the monolithic pressure of scientific orthodoxy, a worry that the scientific community would support only research of a recognized kind in established fields. What, then, would happen to the mavericks, the oddballs, those brilliant creative but unorthodox scientists who did not follow accepted modes of research, or work in accepted fields? If all the support of science were vested in a single foundation, what would happen to someone whose project was turned down? Where could he turn? It was even suggested, therefore, on a serious basis, that maybe the government should establish two foundations, not just one.

Five, with almost all the financial support for basic science vested in a single government-supported foundation, what would happen in a time of depression or a revolt of taxpayers? Also, might not the existence of such a huge federal foundation cause private funding to dry up, or even an end of state funding for basic science?

Six, and in some ways an overriding concern of scientists in those days, especially those connected with the radiation lab, was the possibly inhibiting restriction of national security, a fear of a straightjacket of military control of basic science.

Let me conclude this eyewitness report with a final observation. *Science: The Endless Frontier* was produced in response to a letter of request addressed by Franklin D. Roosevelt to Vannevar Bush. Whose idea was it? Who wrote the letter?

I conducted an oral-history interview with Vannevar Bush a few years before his death. I asked him straight out, just as we were leaving, whose idea was it to commission such a report? Who had written the letter? He looked me in the eye, and without a moment's hesitation said the idea was his. He turned his head a little bit to one side as was his habit, smiled, stated unequivocally, "I wrote the letter."

There was no occasion for further discussion.

COLE: Thank you very much, Professor Cohen. Now we will hear from Professor Gerald Holton, and we will then have a series of questions for all three of our speakers. Jerry?

HOLTON: Dr. Cole, ladies and gentlemen, throughout history there have occurred moments when a public statement crystallized some aspect of the opinion of the time in such a way as to define the debate and the action for and against, for a considerable period. Such a defining statement, often in eloquent and memorable form, is a manifesto, whether the term is used or not. We have seen this phenomenon appear in every field, from political science to philosophy, from arts to education.

The first thing to say about this so-called Vannevar Bush report, dated July, 1945, is that it was meant to be and did become a remarkable example of this genre – a manifesto of its own time, and much beyond its time. My own definition of a "classic" is that it has survived both its imitations and its reputations. And despite all the internal contradictions and other flaws, many of which Bush was aware of as he released his report, the total impact over this half-century on science, on technology, on our universities, on other institutions, on life in our society, has hardly begun to be estimated. That task is long overdue, and I believe that later today one of the sessions of this conference will attend to it.

It is not a mere celebratory remark to say that without the report, or some equivalent at that time, America and the world would now be a very different and a very much reduced kind of thing. On a personal level, let me suggest that many of us in this room would have had a quite different and less satisfying career.

My assigned task in the brief time available is to comment on this morning's announced theme: *Science: The Endless Frontier* as a treatise. I shall barely touch on the large amount of scholarship that has been done on this report, and will confine myself to comments on four points.

First is the spirit behind the report and some of the historical settings.

The implied theory of the relationship between basic science, technology, and society behind the report, a second point. And incidentally, I use "basic" instead of "fundamental" in recognition of Bush's own remark in his autobiography that he found, on talking to some on Capitol Hill, that he'd better avoid the word "fundamental."

Third, I will touch on the recent critique from some of those who have declared that Bush's vision was a failure in its own terms.

And fourth, a new manifesto – I will remark on that which was unveiled four months ago by our government as the declared successor of the Bush report, and as guide for the next decade.

Now, what kind of a document did Vannevar Bush launch? What was the rhetorical structure that helped to make it so effective? Reading it, one quickly realizes that it is really two books in one. Up-front is Bush's own summary for the President, and through him, to Congress and the American people. It consists of a mere 34 pages, an excellent model for any major document intended to get serious attention, particularly if it comes from Washington.

The language is clear. Its sentences are short and simple, in line with Bush's own pragmatic Yankee style. Earnest and insistent, and with almost hypnotic effect, he presents and repeats again and again a few major ideas, organized under such headings as "The War Against Disease," "Science and the Public Welfare," "Renewal of Our Scientific Talents," "Scientific Progress is Essential," "Science is the Proper Concern of Government," and so on.

The rest of the 182-page booklet, as originally printed, is called "Appendix." That constitutes the second book that consists of the reports of the four main committees: the Medical Advisory Committee, Committee on Science and Public Welfare, Committee on Discovery and Development of Scientific Talent, the Committee on Publication of Scientific Information. While they are the raw material from which Bush drew his own part of the report, and they are full of ingenious inventions. We can believe Bush's later comment that few in Congress would have actually read those appendices with care, if at all.

Now, writing during the period between late November '44 and June '45, Bush and his colleagues knew the war was ending. The document, therefore, is imbued with the optimism of a victorious people that had gone through a hellish war to rescue Western civilization from its sworn enemy, thereby being thrust by fate to become, at least for a time, the masters of world affairs. The psychology showing through the prose is, therefore, rather utopian, the more so as the Cold War was not yet clearly in the offing.

The 40 people distributed over those separate committees did remarkably effective work in a very short time, but as Bush stressed later, many of them had already worked with one another during the war. They knew and respected one another, even if they disagreed on certain points. They worked pretty much in secret, with even the head of the National Academy of Science complaining to Bush that he didn't know who the members were.

In a recent critique of the Bush report, published in *Physics Today*, we find the sentence as follows, "Unfortunately, most of Bush's collaborators in writing *Science: The Endless Frontier* were professors who were not necessarily pioneers." The implication there is that Bush, despite being at heart an engineer, was deflected by his colleagues from insisting on including the federal support of technology along with science. I think that image will need correction.

There is no doubt that the scientists were eager to get back to basic research. Bush himself wrote later, quote, "I was as anxious to get out of government as were nearly all of those who manned the laboratories." They had to make up for lost time, and many were chaffing under the threat of the continuation in peacetime of military control of research, as we have already heard. But to illustrate briefly what I mean here, we need only look at the editorial page of *The New York Times* of Tuesday, August 7, 1945, the day after the release of the atomic bomb over Hiroshima, and shortly after the publication of the Bush report.

That whole day's paper, of course, fascinating, is full of the glories of that achievement as seen from the vantage point of the Times. Not until the next day's Times was there a negative comment under the heading that the *Observatore Romano* said the Holy Father thought the event had made a bad impression.

And it followed – the editorial page called the bomb, "The most stupendous military and scientific achievement of our time. It may even be the most stupendous ever made in the history of science and technology." And then it followed with a significant paragraph that spelled out sternly a model by which all future science progress would be achieved –

> University professors who are opposed to organizing, planning and directing research after the manner of industrial laboratories because in their opinion, fundamental research is based on 'curiosity,' because great scientific minds must be left to themselves. They have something to think about now. A most important piece of research was conducted on behalf of the Army in precisely the means adopted in industrial laboratories. End result: an invention was given to the world in three years, which it would have taken perhaps half-a-century to develop if we had to rely on prima-donna research

> scientists who work alone. The internal logical necessities of atomic physics and the war led to the bomb. A problem was stated, it was solved by teamwork, by planning, by competent direction, and not by the mere desire to satisfy curiosity.

In 1945, this opinion of how scientists should be directed was widespread. And we know that half-a-century later, the same kind of battle is still being fought.

Now, finally among the positive, general remarks about the Bush report as a treatise, we must say that it has had a long life, despite the many changes in its implementation. In terms of visionary ideas, the report remains a standard against which to measure its would-be successors, the subsequent reports that specifically claim to be the new-policy documents in the spirit of, or in reaction to, the Bush report for our time.

One such recent effort was that of the National Academy of Science – the Committee on Science, Engineering and Public Policy of the Academy, which issued its report called "Science, Technology and the Federal Government: National Goals for a New Era" in 1993. It invoked the Bush report in its first paragraph, and amplified it later in the text.

I have the impression that among the responses to that publication, the most forceful was the famous edict of September, 1993, from Senator Barbara Mikulski of the Committee on Appropriations, which used that Academy report explicitly in suggesting that, "performance milestone, greater accountability, and an ability to provide a strategic focus on basic research must occur."

The committee report, therefore, directed the foundation, "to revise its strategic plan," i.e., that, "not less than 60% of the agency's annual program, research activities, should be strategic in nature," and it added as a warning the phrase that's familiar to all of you, "not to shroud curiosity-driven activities under the rubric of strategic activities." And as we learned last month, the National Science Foundation has dutifully restructured itself accordingly.

Now, as to the more negative sides of the Bush report itself, seen in overall view, there was only minimal interaction with the White House. Bush, a master politician, had worked with Congress efficiently to make it possible that on the very day that the report was released, legislation – which he had helped to arrange to be drafted – was introduced in the House by Wilbur Mills and in the Senate by Warren Magnuson.

There were in the Bush report also glaring omissions, such as the social sciences and the humanities. Another negative aspect was the concept of a single national research foundation for all fields.

But one must here remember that it was in line with Bush's own suspicion of governmental interference in science. In his autobiography, he pays homage to many heroes, but it is only of Herbert Hoover that Bush says, "He [Hoover] created in me a devotion which never left me."

There was also, inevitably, ignorance of the way the future would turn out, which challenged the report's assumptions. There was no conception of environmental dangers owing to such crimes as DDT, which was singled out in the Bush report as one of the greatest advances, or for that matter, owing to the waste piling up quietly in the wake of the bomb program. There was no inkling of the exponentiation of science, with the corresponding exponentiation of cost.

Now let me turn to relations between the basic sciences, technology, and society in this report. The famous omission of federal support for basic technological progress, except, as the report stressed, in a proposed non-profit technology clinic, was based in part on a wrong idea current at that time about how basic science relates to technology. One recent commentator, George Wise, has written that in 1945, and even later, there just was no good history of science and technology available in large enough measure to make good judgments there.

The idea in the report is that of an assembly line. The beginning of the line is that an idea is in the head of the scientist, subsequent work stations along that line have labels such as "applied research," "invention development," and "engineering," and so on. A society seeking innovation should therefore put money into pure science at the front end. In due time, innovations will come out at the other end. It's a bit of a caricature, but one must remember that at the time there was only a handful of young practitioners working on these problems.

Today such a misunderstanding is no longer excusable. After all, we are meeting here at Columbia, the very center for excellence in the social study of science. And there are now 70 higher-degree programs in the United States in this field alone, and many more programs for science policy as such. So, in principle, such mischief might now be avoidable, or if it is not avoided, will be less forgivable.

To comment on Professor Stokes' point of an added new mode for research that is perhaps emerging, I have for some years been proposing a combined mode of research of a similar sort. I've called it "the Jeffersonian style of research" because Jefferson, while an admirer of Newton and Bacon, had both their problems and their projects in mind, when he launched the Lewis and Clark Expedition.

It is quite clear, particularly in the medical sciences, that it is possible to perceive an area of basic scientific ignorance that seems to lie at the heart of a social problem. It is basic research, located intentionally in uncharted areas on the map of basic science, but motivated by a credible perception that the findings will have a probability of being brought to bear upon persistent international or national problems. This sort of research, in fact, was the subject of an experiment in 1978 by Frank Press, then director of the OSTP and Science and Technology Advisor to the President. Dr. Press described the science-policy planning that went into the budget for the federal funding of research for '79.

In addition to the Office of Management and Budget, the heads of NASA and NSF and leaders in science and engineering from universities and government were brought together to consult with members of Cabinet. And now I am quoting –

> During the course of our interactions on research with the departments and agencies, the President queried the Cabinet members on what they thought some of the important research questions of national interest were. Here are a few examples by the Cabinet officers. Can simple chemical reactions be discovered that will generate visible radiation? How does the material pervading the universe collect to form complex, organic molecules? What are the physical processes that govern climate?

And on, and on.

These are, of course, questions of basic research for the purist Ph.D. theses of the best academic departments, and yet they are precisely targeted in areas of perceived national need.

Now, a few words on recent critiques of the Bush legacy as failure. Writing in 1945, Bush could still claim, "Scientific progress is one essential key to our security as a nation, to our better health, to more jobs, to higher standards of living, and to cultural progress." But in the last few years, the judgment in some high places has gone all the other way, thus, the distinguished Chairman of the Committee on Science, Space and Technology at the time, George Brown, turned the Bush dictum on its head, writing in *Science* in 1993 –

> Global leadership in science and technology has not translated into leadership in infant health, life expectancy, rates of literacy, equality of opportunity, productivity of workers, or efficiency of resource consumption. Neither has it overcome failing educational systems, decaying cities, environmental degradation, unaffordable health care, and the largest national debt in history.

The implication was that it was chiefly science which failed to cure all of those ills. And in the same vein, a few scientists have also called science today as making merely "toys for the rich" – I am quoting one of the articles.

And, of course, the October, 1993, report on the future of NSF by the Senate Appropriations Committee emphasized a view quite contrary to Bush's report. It's clear that the model now was NASA, with semi-annual reports on how the strategic research is obeying its proposed timelines.

Perhaps in part as an answer to these voices, a new intended manifesto was unveiled in early August of this year as the proposed successor of the Bush report. It is entitled "Science in the National Interest." As befits our more visual age, it indicates its authority by the Seal of the President of the United States on its cover, in full color, and the names of the President and Vice President on the cover, as well as on the covering letter inside.

The connection with the Bush report is made clear in the very first paragraph, in which Bush is credited with setting forth the investment strategy by which, quote, "government should accept new responsibilities for promoting the flow of new scientific knowledge and the development of talent in our youth." This is called "the bedrock wisdom."

But unlike Bush's report, which was shaped in a crash program involving experts of various sorts, the new document cites as its sources chiefly the two-day forum on "Science in the National Interest" at the National Academy, January 31 to February 1, 1994, and the input of these 250 invited persons, plus some documents from NAS and NSF, and a few other agencies.

In fact, the difference between these two reports is acknowledged in only two places. The theory for the way benefits are achieved for society is revised from the old linear-progress model to one that allows a more complex relationship.

They say, "We depart here from the Vannevar Bush canon, which suggests a competition between basic and applied research. Instead, we acknowledge an intimate relationship among these two." The new metaphor, in the words of that report, is "an eco-system" rather than a production line, or as Harvey Brooks observed at the time, we are now talking about a seamless web.

Further, there is another departure from the Bush canon, namely, the social and behavioral sciences are briefly mentioned. The real surprise is, of course, that at this time of shrinking monies, there is a substantial increase for science proposed, from a total of 2.6% to 3% of the GNP for all science and associated research. But there is no analogy in these documents with respect to new organizational apparatus for implementing the recommendations. The existing NSTC and PCAST are going to be used for all the discussion to come.

Let me finish by saying that whether that document, the new one, will really become a manifesto in the traditional sense, and rally opposing forces in a common cause, remains very much to be seen. We are only beginning a long period during which the operational meaning of the intentions will become clear, just as was the case for the Bush report.

For the time being, one can expect a continuing battle to shape the outcome behind the scenes. Perhaps the best advice here for our more and more contentious age is another wise observation of Bush: "The question before us today is whether men and women in power can be reasonable before they become exterminated."

COLE: There are one or two comments and questions that I will direct to Professor Stokes and a few others. This comes from Dan Fallon, directed to Professor Stokes –

> Although Bush tried to design a framework for federal support of science in a post-war, peace-time economy, his ideas were shaped by the war-time experience. Furthermore, when the ideas were implemented, the national agenda was dominated by an unforeseen Cold War that lasted 50 years. Therefore, we have not yet seen a federal science policy appropriate for peace. The Bush Era of the past 50 years can be characterized as unusually dominated by hard science and technology, which have particular purpose in war-time competition. Let us assume that the future may be a more peaceful global environment. Won't that push federal support of science more towards the social sciences, and even the humanities, as science is asked to improve our economic competitiveness by making our society more productive?
>
> Isn't this what Mikulski, Dingle, Brown, and even Rivlin have been saying? In other words, can an historical analysis of the past 50 years really help us determine a sound policy for the future? Don't we need more than an adjustment – don't we need a fundamental re-thinking about who shall write An Endless Frontier for the next 50 years?"

STOKES: Well, that's a very eloquent speech masquerading as a question. [laughter] I think my own sense is that while the vanishing Soviet threat and the release of at least part of those billions impounded by the Cold War will undoubtedly produce some additional support for social and behavioral science, it will also produce a great deal of support for things other than military.

Certainly while economists would like to think that they are the utter key to economic productiveness, indeed, there's a great deal of physical, natural, biological scientific research that is extremely important for our economic competitiveness that will also flourish as our goals become somewhat more diversified.

Let me add that, as a social scientist working in this vineyard, I have never been put off by the hostility that clearly Vannevar Bush and his colleagues felt toward the social sciences. They, after all, had flourished in the inter-war period, when much more federal and private philanthropic foundation largesse was showered on them and was being invested in basic science.

It was a very human reaction to use the pivot of the war to turn the tables somewhat, and plainly, Dr. Bush did not want the social scientists to be mucking up his National Research Foundation, although Henry Moe was a very close friend, and he has some very admiring comments about Moe's view. Moe utterly disagreed with him on that.

COLE: Thanks, Don. The next question comes from Sam Silverstein, and he says–

> It seems to me that the present tension between science and society is not whether to invest in fundamental and applied science in universities and for-profit research institutes, but how much to invest. What measures do we have to guide the scale of public investment?

STOKES: The answer is none. [laughter] And we never have, and never will. And the brave, heroic attempt of the document that Gerald Holton has just cited to link that to 3% of gross domestic product is just a fresh example of how these things are quite arbitrary.

Nevertheless, to say that is not at all to undercut the importance of the sort of discussion we have underway now because in the period of really savage pressure on federal discretionary expenditures, it is tremendously important that the most persuasive and realistic case be put forward for national public investment in scientific research.

COLE: Donald Hornig has a comment. He says–

> My reading of Bush does not suggest that he proposed the linear model. Surely his war-time experience led him to understand the interchange between basic and applied research. What he says is that applied research will eventually stagnate unless the pool of knowledge, the intellectual capital, is replenished and enlarged. I think he would have agreed with most of Dr. Stokes' excellent points.

And one final question is from Patrick Hamlett, who says–

> Given that deciding what uses should drive research implies questions of access, what is your opinion about the populist approach to research organization and funding propounded by Harley Kilgore?

STOKES: Well, clearly the alternative visions that were held by the Harley Kilgores – and I make it plural – at the end of the war were part of what produced the Bush report. And I'm very sympathetic to that. Indeed, the scientific effort of the war had been largely screened off from the country. And when the screen was removed, and it was seen just exactly what had been done by the Manhattan Project, you had as highly sentient commentators as the editorial writers for The New York Times saying the sorts of things that they did.

And there was profound uncertainty as to what might happen if it was really left to, as I've said earlier, this cacophony of power centers to shape a science policy. And that is why, if Vannevar Bush did not write that letter, certainly he and others had thoroughly endorsed the idea of trying to put a way of thinking on this that really would be deeply influential, and largely succeeded in that.

Now before I sit down, let me just comment on Dr. Hornig's remark. Bush's whole career made clear that he really did understand the interactive effect that Dr. Hornig is pointing to. But the report itself, while it did not endorse anything as simplesse as the linear model – that came afterwards. If you were to look at the second annual report of the National Science Board, you would see the most flatfooted, simple-minded statement of the linear model that anyone has ever put in print.

That was not in the report itself, but it would be very difficult to read that report as not saying that science – the fundamental advance of science – is exogenous to technological development by pathways that are very multiple, circuitous, unevenly paced. But it is basically a recursive system – that is the nature of that analysis, however much Vannevar Bush in his actual career may have known that that, too, was too simple-minded.

COLE: Thank you, Don.

Reflection on the History of Science Policy in the US

Professor Harvey Brooks

Panelist Responses

Mr. William Golden
Professor Harvey Sapolsky

Moderator

Professor Jonathan Cole

COLE: As I said at the outset, I will be extraordinarily brief because the people that I am about to introduce really need no introduction. Our speaker who will reflect on the history of science policy in the United States will be Professor Harvey Brooks, the Benjamin Pierce Professor Emeritus of Technology and Public Policy, and Professor of Applied Physics at the John F. Kennedy School of Government at Harvard. Simply put, Harvey Brooks has been, and continues to be, one of the most knowledgeable and influential scientists in this country, and a major figure in developing this nation's science policy.

Professor Harvey Sapolsky is Director of the Defense and Arms Control Studies Program at the Massachusetts Institute of Technology. He has published extensively on aspects of science and the military, health planning, the telecommunications revolution.

And we are joined – I'm very, very pleased to say this morning – by Mr. William Golden. All of these monitors that you see up here may, at the moment, have my image on it, but Bill Golden has now appeared, and Bill, we want to welcome you to our conference. We are delighted that you will be a participant.

As most of those here know, Bill Golden has been one of the most influential Americans in the development of post-war science policy. He designed the first presidential science-advisory organization for President Truman in 1950. He is currently Chairman of the American Museum of Natural History, and the past Chairman of the New York Academy of Sciences, and Co-chairman, with Joshua Lederberg, of the Carnegie Commission on Science, Technology and Government. Bill is a member of the American Philosophical Society and the American Academy of Arts and Sciences, and one of the most knowledgeable people that I know in the area of science policy. I'm delighted to have you with us, Bill.

BROOKS: The debate that was launched by the original Bush report and its rival report, the Kilgore Plan, has roots that go back to the debate between J. D. Bernal and Michael Polanyi in Britain from the 1930s until the late 1950s. This is perhaps a somewhat over-simplified analogy, but nevertheless worth mentioning.

Then as now, the debate concerned the degree to which it is feasible and desirable to plan the agenda for the national science and technology enterprise in terms of explicit societal or economic goals. Polanyi stressed the need for autonomy and self-governance of the scientific community if it were to contribute most efficiently to societal goals in the long run. His view may be most succinctly summarized in the following quotation from the sociologist of science, Bernard Barber, in something he wrote in the 1960s.

> "However much pure science may eventually be applied to some other social purpose and the construction of conceptual schemes for their own sake, its autonomy in whatever run of time is required for this latter purpose, is the essential condition of any long-run applied effects it may have."
>
> (Barber 1962)

In contrast, Bernal, who was strongly influenced by Marxist thought, was impressed with what he saw as the tremendous inefficiencies of autonomous science. He believed that its enormous potential benefits for humanity could only be realized through a publicly discussed and debated flexible plan involving government and many representative elements of society. This same debate essentially has been reflected in all the subsequent debates about national science policy.

It is by now a truism that World War II was a watershed, particularly in the U.S. and, to a lesser extent, in Britain and Europe. For example, in 1935 the U.S. federal government contributed only 13 percent of total national expenditures for research and development, which constituted only 0.35 percent of the national income. By 1962, the federal contribution to this total had risen to nearly 70 percent, with the aggregate being more than 3.3 percent of the national income, an approximately 10 order-of-magnitude increase.

In the 1930s, federally-supported research and development was mostly conducted at in-house, civil-service laboratories, which accounted for about 0.25 percent of the federal budget. This figure rose to 11 percent by 1962, and represented probably more than 35 percent of the federal government's discretionary expenditures.

The imminence of World War II mobilized leaders of American science in advance of American participation in the war. And whereas technical advances in World War I had been generated largely from existing military needs as defined by the military, many of the World War II advances were born in the laboratory, almost as solutions looking for problems. Their military application evolved as military strategy and technology were developed in tandem, with scientists and the military in equal partnership, but with the civilian agency Office of Scientific Research and Development (OSRD) – headed by Vannevar Bush – able to make decisions independent of previously specified military needs. Scientists eventually were able to persuade soldiers to inform them of the general military problems involved, so that the scientists might reach their own conclusions about the kinds of weapons and devices the military would need to meet those problems.

Unlike the situation in World War I, science in World War II was mobilized under civilian tutelage, with the leaders of the scientific community having direct access to the President and to the Congressional Appropriations committees – if necessary, over the heads of the military, although in practice this privilege was seldom exercised.

The experience of World War II had a profound impact on both the political and scientific leadership, and crucially influenced the position of science relative to government after the war. The war-time experience convinced Bush of the importance of an independent role for scientists in an equal partnership with government. It was the fountainhead of his report, *Science: the Endless Frontier* (1945).

The essence of that report was contained in the following eight recommendations and five general principles.

The first recommendation: "Science, by itself, provides no panacea for individual, social, and economic ills. It can be effective in the national welfare only as a member of a team, whether the conditions be peace or war. But without scientific progress no amount of achievement in other directions can insure our health, prosperity, and security as a nation in the modern world."

Second, "It is clear that if we are to maintain the progress in medicine which has marked the last 25 years, the Government should extend financial support to basic medical research" – that is, the 25 years before the report was written.

Third, "Military preparedness requires a permanent independent, civilian-controlled organization, having close liaison with the Army and Navy, but with funds directly from Congress and with the clear power to initiate military research which will supplement and strengthen that carried on directly under the control of the Army and Navy." It is sometimes said that Bush envisioned that all military research would be conducted under a kind of a overarching Department of Science. That was never envisioned, as this recommendation makes clear.

Fourth: "Basic scientific research is scientific capital. Moreover, we cannot any longer depend upon Europe as a major source of this scientific capital. Clearly, more and better scientific research is one essential to the achievement of our goal of full employment."

That fourth principle most clearly embodies the idea of basic research as the prerequisite for technological innovation. There are two rather different views of this. One is that specific ideas emerging from basic research are the inspiration and source of technological innovation. The other is that the cumulative output of basic research is essentially a resource that can be mined by applied scientists and engineers for the purposes of innovation. It's my view that Bush held much more of the latter view than the direct-event connection.[1]

The fifth recommendation in the Bush report was, "If the colleges, universities, and research institutes are to meet the rapidly increasing demands of industry and Government for new scientific knowledge, their basic research should be strengthened by use of public funds."

Sixth: "To provide coordination of the common scientific activities of these governmental agencies as to policies and budgets, a permanent Science Advisory Board should be created to advise the executive and legislative branches of Government on these matters." This function apparently was originally envisioned for the National Science Board. However, it became unrealistic so long as the National Science Foundation budget constituted such a tiny faction of the total federal support of scientific research, as it did through most of its early history.

The seventh recommendation: "The Government should provide a reasonable number of undergraduate scholarships and graduate fellowships in order to develop scientific talent in American youth. The plans should be designed to attract into science only that proportion of youthful talent appropriate to the needs of science in relation to the other needs of the nation for high abilities." This was a sort of foretaste of the G.I. Bill and was perhaps the most significant and practical initial outcome of the Bush report.

And the final recommendation: "A new agency should be established, therefore, by the Congress, devoted to the support of scientific research and advanced scientific education alone….The agency to administer such funds should be composed of citizens selected only on the basis of their interest in and capacity to promote the work of the agency. They should be persons of broad interest in and understanding of the peculiarities of scientific research and education." This last phrase recurs throughout both the Bush report and through many of the subsequent discussions.

Those were the eight recommendations of the Bush report. There were also five principles which must underlie the program of support for scientific research and education. Bush set these down in the following terms:

First, the new agency "should have a stability of funds so that long-range programs may be undertaken." Second: "The agency to administer such funds should be composed of citizens selected only on the basis of their interest in and capacity to promote the work of

[1] This was used in a very controversial study called "Project Hindsight." It essentially showed that basic research contributed very little to the development of new weapons systems; however, the study used an event-tree analysis, which I think was a methodology inappropriate to the question.

the agency. They should be persons of broad interest in and understanding of the peculiarities of scientific research and education."

Third: "The agency should promote research through contracts or grants to organizations outside the Federal Government. It should not operate any laboratories of its own." This was a pretty flat-footed recommendation, which was followed both in the implementation of the National Science Foundation, and also in the implementation of the Atomic Energy Commission. It was followed to a considerable extent also in the early days of the Defense Department, at least for the support of basic research.

Fourth: "Support of basic research in the public and private colleges, universities, and research institutes must leave the internal control of policy, personnel, and the method and scope of the research to the institutions themselves. This is of the utmost importance."

And fifth: "While assuring complete independence and freedom for the nature, scope, and methodology of research carried on in the institutions receiving public funds, and while retaining discretion in the allocation of funds among such institutions, the Foundation proposed herein must be responsible to the President and the Congress. Only through such responsibility can we maintain the proper relationship between science and other aspects of a democratic system. The usual controls of audits, reports, budgeting, and the like, should, of course, apply to the administrative and fiscal operations of the Foundation, subject, however, to such adjustments in procedure as are necessary to meet the special requirements of research."

I would like to also to add two other quotes from the Bush report, because I think they explain why he laid such emphasis on universities and independent research institutes.

First, from page 19:

> It is chiefly in these institutions that scientists may work in an atmosphere which is relatively free from the adverse pressure of convention, prejudice, or commercial necessity. At their best they provide the scientific worker with a strong sense of solidarity and security, as well as a substantial degree of personal intellectual freedom. All of these factors are of great importance in the development of new knowledge, since much of new knowledge is certain to arouse opposition because of its tendency to challenge current beliefs or practice.

And then,

> Industry is generally inhibited by preconceived goals, by its own clearly defined standards, and by the constant pressure of commercial necessity. Satisfactory progress in basic science seldom occurs under conditions

> prevailing in the normal industrial laboratory. There are some notable exceptions, it is true, but even in such cases it is rarely possible to match the universities in respect to the freedom which is so important to scientific discovery.

Bush's observation in this quotation seems even to be supported by the phenomenon which we have seen occurring in the last many years, of the gradual migration to academia of some of the most creative and productive scientists from those exceptional industrial laboratories that Bush apparently had in mind in that statement, such as the Bell Laboratories, the General Electric Research Laboratory, IBM Corporate Laboratory, and several other examples. It's not that these laboratories have not continued to make very important contributions, but apparently, there has been a tendency for a certain amount of migration out of these laboratories, which supports his observation.

Vannevar Bush wrote another report, which is not anywhere near as well-known as *Science: the Endless Frontier*, but is at least as enlightening with respect to Bush's personal view of the relationship between engineering and science, and between pure and applied science. It is called "The Report of the Panel on the McKay Bequest to the President Fellows of Harvard College" (Harvard College 1950). The following two quotes are taken from Section 4, entitled "Present Day Engineering and Applied Science." They clearly express that Bush's views were not quite as purist as has often been implied in recent interpretations:

> The borderline between the engineer and the applied scientist is becoming dim. It has never been clean-cut. An applied scientist is one who renders science useful. An engineer is one who utilizes science in an economic manner for man's benefit...The difference has, in the past, been mainly that the former starts as a scientist and seeks to apply, while the latter begins with the appreciation of a human need and searches out the science by which it can be met...Yet even this difference has been modified. Engineers, those who are really in the forefront of advance, are becoming more entitled to be recognized as scientists in their own right... Applied scientists, under the pressure of war and its aftermath, have often become accomplished engineers as well.

You can see the influence of Bush's war-time experience in that statement.

There was an interesting phenomenon in the World War II scientific effort. It occurred in the radiation lab and the proximity-fuse lab, and was particularly obvious in the Manhattan Project: the leaders of those civilian efforts came, by and large, from backgrounds in nuclear physics. Nuclear physics at that particular time was a subject which involved very much of a cross between science and engineering, since the engineering and apparatus of nuclear physics was a very important part of the whole enterprise. Contrary to the popular wisdom about theoretical scientists, many of the people who led the effort in the radiation lab, the radio-research lab at Harvard, and the Manhattan Project were people who, in their practice of basic science, had experience in many ways quite typical of engineers. That was particularly true at that time in the history of the development of physics.

The second quote provides quite a contrast to some of the statements that have been made about *Science: the Endless Frontier*:

> A science such as physics, or chemistry, or mathematics is not the sum of two discreet parts – one pure, and the other applied. It is an organic whole, with complete interrelationships throughout. There should be no divorcing of applied science from its parent systems...Certainly whatever the organization, there should be a community of interest, a vigorous interchange of ideas and students within the department of mathematics and the applied mathematicians, and the applied mathematicians of whatever stamp who are operating directly in the field of applied science and engineering.

This same principle should apply elsewhere. My view of the relationship between engineering, science, and the research enterprise is that it is divided the into two parts: not science and technology, or pure and applied, but rather opportunity-oriented research and need-oriented research, where "need" refers to social need and "opportunity" refers to both scientific and technological opportunity. These are generally identified with science and technology respectively, but that's not a complete identification. These relations have been profoundly transformed. However, they still represent two parallel streams of intellectual evolution, but with increasingly frequent and more profound cross-fertilization and interdependence. Both agendas have severe limitations when pursued single-mindedly, and these limitations can only be overcome by pursuing both types of agenda in parallel with ever-increasing opportunities for cross-fertilization.

The limitation of the opportunity-oriented approach is that the potential applications of the resulting knowledge are usually spread over a very wide spectrum of societal problems, and highly dispersed in time. Many applications and their timing are unforeseeable when the research is first undertaken. On the other hand, the limitation of focusing too narrowly on the presently formulated or foreseen societal problems lies in the fact that the very definition of these problems may often depend on knowledge not yet discovered.

Also, the knowledge produced by the opportunity-oriented approach tends to be cumulative and can only be created if pursued in the right logical sequence, making it impossible to produce needed knowledge on demand just at the time the need for it first becomes apparent in connection with the solution of the societal problem.

Because of these issues of timing and problem-specificity, the two types of knowledge are most sufficiently pursued in parallel, in an appropriate mix and with continual but deep interchange between the two knowledge streams, each of which is cumulative in its own terms. And, of course, the technological branch is cumulative to just as large an extent as the science branch.

I suspect that the tighter and more frequent the interaction between the two streams of knowledge, the greater the importance of the opportunity-oriented agenda relative to the society-oriented one, even while the latter absorbs and will continue to absorb the far largest fraction of resources.

Not only does the opportunity-oriented agenda more frequently enrich and make more cost-effective the pursuit of the need-oriented agenda, but also the societal agenda will more frequently spin off new intellectual challenges worth pursuing in the opportunity-oriented mode, beyond the needs of the immediate problem, for the sake of their contribution to the conceptual structure of knowledge.

Each of the parallel agendas will increasingly serve as triggering sources for the other in a more symmetrical fashion than has often been appreciated by the inhabitants of either branch of the scientific agenda.

And I might add, the inhabitants of the two branches of the technical agenda are not necessarily distinct classes of people, although they often may be. You find some people, like Edwin Land, who shift back and forth between one agenda and the other.

It is important to make note of the fact that the Bush report did not really recognize the extent to which the scientific agenda – that is to say, the opportunity-oriented research agenda – was often initially triggered by an applied problem, sometimes one that was very narrow initially. This is a legitimate criticism of the Bush report.

It is still important to look at the way such an applied problem is pursued. That is to say, it should be pursued, and ought to be pursued in much greater depth, with much larger ramifications than just the solution of the immediate problem.

An examination of the R&D budget in the U.S. since World War II shows the evolution of science policy during that time. Essentially, it can be divided into three eras. The first era is the Cold War era, which extends and rather abruptly ends around 1966 or '67 so far as R&D is concerned, even though this was the period of the build-up of the Vietnam War. In fact, there was a big de-emphasis on strategic weapon systems during that time.

From the period from 1966 to about 1975, there was an actual fall-off in federal R&D which amounted to about 17 percent in real terms. At the same time, there was a fall-off in university research in the physical sciences, which declined by about 14 percent. And even in the biomedical sciences there was no fall-off, but there was a level-off during that period.

For reasons which are not entirely self-evident, in about 1975 or 1976, there was a resumption of growth in the federal R&D budget, and it was spread over a considerably larger domain. There was also a dramatic increase in energy-oriented R&D from about 1974 to the early 1980s. But the most striking aspect is the rapid rise and continuous rise of privately supported industrial R&D, which continued right through the deep recession of the 1980s.

So, there were really three periods here. The first period was the Cold War period. The second, the period of the dip, might be termed the social-priorities period. During this time, there was an almost doubling of the amount of support for research in the social and behavioral sciences, although it never reached the extent it did in other fields. This was the period of the Great Society program.

It was followed, in the mid-1970s, by considerable disillusionment with the power of the social sciences to attack social problems, and by the gradual resumption of the Cold War military build-up, which began in the second half of the Carter Administration and accelerated during the subsequent Republican administrations.

It is interesting to note that the combined expenditures on defense, space, and nuclear energy never reached the peak, in terms of percentage of GNP, that they had reached in the 1960s. In fact, the build-up was much less rapid than the build-up that had taken place in the early part of the 1960s.

The other characteristic of the period after 1975, although it began considerably earlier and there were even signs of it in the late 1960s, was the increase in interest in economic performance. This was a change from the 1966 to 1975 period, where the priorities were public-sector needs, as formulated in the Great Society program.

After 1975, there was a rapid build-up of public concern about the declining international economic competitiveness of the U.S. especially vis a vis Japan, which became pronounced in the Carter Administration. That period ended in about 1986, and there has been a gradual shift whose exact nature I think we still cannot foresee, but is clearly a part of what is being debated now.

With the surge of the relative private investment in R&D accompanying the unprecedented prosperity of the late-1990's, combined with the growing public and political skepticism about the relative cost-effectiveness of "big government" and tight limits on government spending, a dominant issue of science policy has become the criteria that justify public investment in R&D as opposed to relying on the private sector, if necessary by restructuring incentives so as to induce more private R&D investment. It is generally agreed that there must be some pubic or common good arising out of federal R&D, which cannot be captured by individual firms or even by voluntary associations of individual firms, but just how this public good can be measured, and what is the relative efficiency of private and public spending is a matter of increasingly intense debate. That the economic returns to R&D are large, especially in the longer term, is less and less called into question by the public and politicians, but there is a paradox here. Aggregate returns alone are insufficient to justify public investment in the absence of any showing of a common good that can be quantified sufficiently well to show that it exceeds the sum of the private returns to individual firms. The more tangible and measurable the returns, the more they are likely to be labeled as "corporate welfare" and left to the private sector to support. The more elusive and diffuse they are, the more likely they are to be questioned by skeptics. Closely related to this issue is the optimal allocation of federal R&D spending among universities, non-profit research institutions, and industry.

COLE: We do have one question we'll take before we move to Dr. Sapolsky. This is a comment and a question received from Lilli Hornig, and it says–

> With respect to the encouraging of training of scientific talent, Bush's recommendation of fellowship support cannot be shown to have very direct connects with the actual numbers of students in a field. Thus in the physical sciences, where federal fellowships and other student support is most concentrated, there has been almost steadily declining student interest, while many other areas – notably life and behavioral sciences – have attracted growing numbers, even in the absence of federal support, like psychology.
>
> How can one re-think the issue of attracting students to fields of national interest so as to use federal funds most effectively in educating college and advance students? Should we try to manipulate fields in this way?

BROOKS: I think that's a very interesting question. In fact, the fellowship programs that were undertaken during the '60s certainly did not very much influence the distribution of people in the field. But you have to ask the question: compared to what?

If it had not been for those fellowship programs, one may wonder whether, in fact, there would have been a much more precipitous drop in the physical sciences than there actually was. I think if you look at the more dramatic example of the G.I. Bill in the 1950s, it's somewhat harder to make the case that there was not growth in the physical sciences and engineering. And, of course, that was a much more broadly distributed program, and was big enough to have a real impact on numbers, not only in the universities, but even outside the university.

So you may have to separate the period of the G.I. Bill in the late '40s and most of the '50s from the period of the build-up of fellowships in the 1960s, which were more motivated by a feeling that with the combined military and space-program build-up, a real shortage of scientists and engineers was developing.

And, in fact, that's the only period in the whole post-war history when there was good evidence of shortage, an actual shortage, of scientists and engineers needed for the combination of federal and private programs, indicated by the salaries of scientists and engineers relative to the labor force.

Furthermore, during the space-program build-up particularly, there were about 100,000 non-degree people in the aerospace industry who were converted into the equivalent of graduate engineers within the industry – another piece of evidence of the shortage.

But the broader question – the broader point raised by your question – I don't really know the answer to, except that you have to look at it in a "compared to what" business. And I think there would have been no reason, really, to expect necessarily an absolute increased response in this particular case.

COLE: Thank you, Harvey. We will connect now with Bill Golden and reverse the order because I know that Bill is constrained by a meeting that he's attending out in California. So, Bill, can you hear me?

GOLDEN: Yes, I hear you very well, Jonathan.

COLE: Well, it's good to have you again.

GOLDEN: Well, I'm glad to be able to connect, and I've been edified, as the audience directly there in the rotunda has been, to hear Harvey's comprehensive history of science and technology in our country, with emphasis on the practicalities.

Van Bush – or Vannevar Bush, none of us called him "Van" to his face [laughter] – was a very practical man. He had a somewhat formal exterior, but he had a very warm inside.

I remember him very well as being very helpful and kind to me, in spite of being rather very formal and not entirely in agreement with my boss when I was at the Atomic Energy Commission as assistant to Louis Straws. Louis Straws and Van Bush were both very talented men. They were formal and cool, at best, to each other. But he was very good to me. I'm just impelled to reminisce about that.

Now we're here concerned, surely all of us, with the future. And Harvey has brought us up to date, and has enunciated principles from Van Bush and others that certainly are, in many respects, equally applicable now.

There are some different emphases, certainly. Going back to World War II and post-World War II, during which scientific and technological research and support grew so dramatically in our country, indeed, throughout the world, the stimulus was – and always has been in history – concerns with military matters. That goes back at least to, well, at least to, let's say, David and Goliath [laughter], when David threw the best of modern technology at the time, a super slingshot, which won the battle for him.

But things have changed very greatly, as we all know, the Cold War being over, fortunately. The emphasis is on global-economic competition – the key word through the world is "jobs" – and modern communications, of which my being able to talk with you in this way is a very minor example.

The United States has to be concerned with its economic standing in relation to global economic competition – a kind of competition which heavily relies on matters of technology, and before that, of science; or perhaps I should say, the interaction between science and technology.

In fact, technology greatly influences science, and there is certainly a circular effect there; one instance would be where instrumentation, made possible by technology, enables advances in science.

I do want to touch on certain points that Harvey brought up, one of them being the need that Vannevar Bush pointed to – the need for dependable, or rather, the need for a “stability" of funding for science and research, research and technology I should say, projects over a period of years.

Our country suffers from the short-term approach of funding, although funding has grown greatly and been very generous. I think the taxpayers generally do not object to what has been done and is being done. The Congress has a very short-term view of committing funds. This is less of a problem in many other countries.

I don't have a prescription to alleviate the situation, but I think it's important to mention that, as many of you know, the United States increasingly is regarded in other parts of the world as not a reliable partner in megascience projects that require many years of funding. Or in projects which are not megascience in the sense of major instrumentation, such as the superconducting supercollider or others of that major ilk, but rather studies going on over a period of years and over areas of geography in other countries, where cooperative efforts among different countries enables a much better result. These would be, for example, in areas of environmental issues – in recording data and experimental tests, such as underwater sound tests – and cosmological issues involving telescopes in many parts of the world. These require stability in funding.

This opens up the question of a role for the Department of State in science and technology policy formulation, where science and technology have never, except for a brief period after World War II, had the attention that I believe, and many others believe, they should have. There is not a real career opportunity in the State Department for scientists and engineers. And I mention it here, in part, because I hope to stimulate interest among all of you in encouraging State Department action to create a more favorable climate for the consideration of science and technological issues in the formulation of policies on issues that are not directly science and technology issues but in which science and technology are woven into the fabric. This, of course, increasingly includes many economic issues. I would hope that all of us will be thinking of how to encourage the State Department to improve the status of science and technology in the organization chart.

The Carnegie Commission, which some of you know about, and which has issued many publications, copies of which will be available to any of you who want them, has been very much concerned with the United States' science and technology in world affairs. Among the practical outcomes of the Carnegie Commission's studies over a five-year period, has been the creation of something that calls itself the Carnegie Group. The Carnegie Group is an informal organization – the most general term – an informal organization of the ministers of science or their equivalents in the G7 countries. Of course, for the United States, we have our Science Advisor to the President as our approximate equivalent of the ministers of science in the other countries.

This group invited these ministers to a meeting some years ago, to discuss whether they would like to have an informal get-together in which they would get to know each other without any staff being present, without any minutes being kept. Getting into a position where they, having common concerns on matters related to science and technology that cross all country borders, would be able to discuss them with their neckties off.

This first meeting was so successful that they asked us invite them a second time over a weekend. And from that time on, every six months they meet, they discuss common issues of science and technology and their relation to world affairs and to economic affairs and indeed to the affairs that concern all of us as homo sapiens. They just recently held their eighth semi-annual meeting. Now this group is much concerned with the attitudes of the equivalents of our State Department in these countries. I think it worth mentioning for that reason, because international affairs are increasingly matters that concern science and technology and the welfare of all of us.

The welfare issue brings me to the last point I want to make in this response, and that is to call attention to the so-called underclass in the United States. It may not seem directly a matter of science and technology, but it is very much a matter that concerns all of us, in which science and technology may be able to be helpful.

A substantial fraction of our population is classed as underclass, and no one in this room is in that class. And we are fortunate. But the underclass in the United States is I would say – should be – a concern of all of us who are not in the underclass. Not just for reasons for compassion, not for reasons of gratitude that we're not in such a state, but as a matter of enlightened self-interest.

Unless we can improve the status of the underclass, which is growing more rapidly than the rest of the population, it is going to impair our economic status by giving us a burden to support and by giving us a growing fraction of the population that will find it difficult or impossible to obtain jobs in an increasingly technological society.

I bring this matter up now because technology, which is so helpful to most of us, is impairing the opportunity of jobs for those who are not adequately educated. It's easy to say that all they need is education and that would be so, but there is a need for the motivation in order to seek the education, to feel that jobs are available, and indeed to find them available.

To go further, the question of what creates the motivation gets into issues concerning early childhood, into family issues, into nutrition, and this is not the place to go into them. I hope to bring these issues to your attention and to put the pebbles in your shoe regarding this problem, which I think is a major one. I believe it would have concerned Vannevar Bush greatly, if it prevailed when he was there, and would if he were here now.

I want to bring up, in closing, the name of Leonardo – Leonardo DaVinci. Leonardo was a very practical quasi-engineer, technician, scientist of sort in his time. He was very much concerned with primarily advancing his own economic status through military devices that he would prepare to sell to any ruler who would seek his services. He was driven very largely by economic issues, and I think we might keep in mind that many of our scientists and engineers today are also encouraged by the opportunities for personal financial advancement as well as the glory of advancing learning and the prestige that comes from seeking Nobels and similar prizes.

COLE: Thank you, Bill. Let me pose one question to you – that is posed, in fact, by Ann Griffin – and it is as follows: "You mentioned the demise of the SSC, what lessons do you derive from this experience? Do you foresee a shift to international cooperation in big science projects? What are the implications for American scientific autonomy then?"

GOLDEN: Well, I would comment that the Carnegie Group to which I referred, at its recent meeting – which was held in Brussels, the center of European union, the European community – paid much attention to just such matters as to how cooperative ventures in megascience could be most effective. I have no answer to the question, but I think the fact that there is increasing awareness of the need to find practical solutions – practical palliatives at least to what has been the unreliability of the United States for making long-term financial commitments – gives me some feeling of encouragement that ways will be worked out to bring about effective practical arrangements between the governments involved in megascience and cross-boundary projects.

COLE: Thank you very much, Bill. We're going to move on at this point to hear from the comments and responses of Professor Harvey Sapolsky.

SAPOLSKY: Science policy is the scientist's struggle for a public patron, the scientist's search for a winning rationale for financial support. There are three main rationales for public patronage, two of which have just been derailed by events that I never expected to see in my lifetime.

The first is national security as a rationale. Under this banner, science in the service of defense – a banner that Vannevar Bush strongly believed in – flourished over the last 40 years.

It's my estimate that we spent approximately $13 trillion on the Cold War. The R&D portion of that was perhaps $2 trillion. That means hundreds of billions of dollars were given to basic research, largely in the name of national security. I include under the national security banner the Department of Defense, the National Aeronautics and Space Administration, and what was the Atomic Energy Commission and is now the Department of Energy. Those three agencies have dominated science in the last 40 years. Even the National Science Foundation, to some extent, because its initial resources came largely under the Sputnik era, was created under a defense rationale.

Defense has been quite good to science over the last 40 years. There was no better, more protective patron of science than national security. National security kept the democratic wolves away from the door.

Just as Bush wanted, science had autonomy under the defense rationale. Scientists were largely free from management oversight, they were given resources to acquire the fanciest equipment, and they were free from geographic and other constraints on distribution that affect so much else of what the federal government does.

Defense, in a real sense, protected science from politics, just as some said Bush might have wanted. Congressmen left military appropriations, or what they thought were military appropriations, largely alone.

I could elaborate on the reasons why the defense agencies were so generous to science, but suffice it to say that I'm not a believer in the argument that the military were converts to science after World War II, that they became believers in the beauty of science and the utility of basic research. For me, the success of this rationale was a combination of bureaucratic accident, bureaucratic politics, and bureaucratic inertia. But it's been very helpful.

In any event, that has all come to an end with the end of the Cold War. It marks the end of a very long gravy train. I believe that the military, much-hated on some campuses, will be sorely missed.

Industrial competitiveness is the second rationale. It has sparked great interest in recent years as a potential replacement rationale. And it even has generated a fair amount of funding for science, though not a tremendous amount.

Industrial competitiveness is a more difficult rationale for the scientist, though, than defense, in the sense that some politicians actually believe in it. Because of this, they find it hard to resist distributional instincts.

Under a national security rationale, politicians may be less prescriptive. If they think a bomb is being made, they want the best bomb, and they are willing to allow the bomb to be made wherever the military might think it can best be done.

But with industrial competitiveness as the rationale, then they will start to think that maybe this belongs in their district, because if it is good for the industrial competitors of the United States, it can not be bad for their districts, can it? So, what is good for the United States could be good for Florida, and that's what's going to happen with that kind of rationale.

Moreover, the contribution that science makes to industrial competitiveness can be better measured than it can be for defense. Wars that you do not fight are hard to link together with the equipment or science that you invested in.

I think this competitiveness rationale would force the allocation to be subject to much more managerial direction, and you see that in the expressions of the committees of the Congress, where they start to say what they think about how this money should be funded.

This rationale was subject to a life-change with the demise of the Democratically-controlled Congress. Industrial policy under the Republican-controlled Congress is also being revised. I think it is an endangered species. The Republicans believe in supporting R&D as much as needed for things that the government buys – often largely military equipment. They have no problem with supporting whatever is deemed to be relevant to that kind of activity, because the government is the purchaser. They also believe in supporting basic research, because they believe that business firms, for their own self-interest, will under-invest in research.

But they do not support what they identify as applied research. They think that this is entirely the province of industry, and not at all part of a seamless web. They may see anything under the industrial competitiveness banner as applied research and say goodbye to the National Institute of Standards and Technology, the Technology Reinvestment Project, and all the other programs using that rationale.

Now, let me turn to the third rationale, one which I have not mentioned previously, and that is health. This one, I think, is a winner. It simultaneously serves both democratic and elite instincts that are mixed in with support of science. The geographic distribution of health dollars under the healthcare banner is assured by the population distribution, because the medical schools in the United States are distributed to be near what they call euphemistically "clinical material."

Boston, of course, has great medical schools and New York City has great medical schools, and San Francisco has great medical schools, just like they have great physics departments and whatnot. But so does Atlanta, and so does Salt Lake City, and so do St. Louis and Houston. These are all great medical centers in the United States.

There are 150 medical schools, they are all wealthy under this regime, and they can concentrate resources in the academic medical setting quite easily. They do it in 150 settings, not just five or 10, but it is easy to serve both democratic and elite instincts here.

Better yet, health as a rationale is pretty well protected from downturns: despite protestations by Americans that they have deep religious convictions, I don't see Americans eager to meet their maker. I see them quite willing to spend the entire GNP on healthcare of one kind or another.

Right now we're at the 14 percent mark, a trillion dollars a year that is being invested in health, and it goes up 10 percent per year. In fact, they have parties when it's under double-digit inflation in the healthcare industry. They think they had a successful year in controlling costs when inflation is only 9 percent. So this is on its way.

I think the rationale of science's contribution to health care of one form or another will generate great support and autonomy for scientists. It is the very combination that Bush wanted for science – and it is the permanent quest for health. That is truly the endless frontier.

COLE: Thank you Harvey. Let me now convey to you a number of comments that have come forward, and then speak to a couple of questions.

The first comment is from Fred Seitz of the Rockefeller University, and he says–

> The great danger the scientific community in the United States faces at present is not only that funds will become limited, a matter that was to be expected, since the rate of growth of such funding has been outrunning the rate of the growth of NDP, but that the core decisions concerning the areas of science which merit support will be determined by bureaucrats who are guided by forces outside the community itself.
>
> One very dangerous sign was the abrupt dismissal of William Happer from the Department of Energy when he expressed before a committee of the Senate his honest view that humanity is in no immediate danger of increased exposure to ultraviolet radiation. An all-powerful government responsible for most of the funding of basic science can readily create a domestic forum of Lysenkoism.

A second general comment made by Howard Gobstein goes as follows–

> *Science: The Endless Frontier* has great meaning to the science community in a sense similar to a bible. However, it has little meaning to the present political leadership or to the public, who in all likelihood have not even heard of it, nor would they care. A very cynical view would be that here we sit talking to ourselves on topics that are irrelevant, or at best not germane to present public interests. A key challenge, as I see it, will be how to use what we have learned from our study of science policy and the public reaction to craft a similar contemporary manifesto that would stimulate the same vision, imagination, and support for science by today's political leaders.

Those are two comments, let me then mention two questions. This one by Michael Salvato, is directed to Harvey Brooks. And he asks–

> What is the significance of the rise in private-sector funding of R&D expenditures, especially as a proportion of total R&D, and what are the implications for federal science policy?

BROOKS: Well, I think one of the major sources of the relative rise in private sector expenditures is really the decline in what I call a defense complex. That is, defense, space, and atomic energy. You have to separate out the "D" part of the federal R&D budget from the "R" part. And the big shift has really been due to the decline in the "D" part. And this, of course, relates to the competitiveness rationale of the support of science, which was brought up in one of the earlier comments.

You can make a table that classifies the major industrial countries of the world, not in terms of their total R&D expenditures but in terms of the total R&D expenditures of the private sector. And there's an almost perfect correlation, at least until recently, between economic performance and the percentage of total R&D expenditures, as a percentage of GNP by the private sector.

One country that's a little off the curve on this – which I don't entirely understand, because in terms of productivity, its performance hasn't been so good – the country which is strongest on this scale is Switzerland. Switzerland has by far the largest percentage of its R&D expenditures supported by the private sector. On the other hand, it hasn't shown a very good productivity performance, but I think most people would still argue that the Swiss economy is in pretty good shape.

COLE: Thanks, Harvey. This is directed to Bill Golden. This is from Professor Mischa Shwartz from Columbia. And he asks you–

> You raise the growing issue of the underclass in the United States, specifically. What can be done by scientists and engineers, in a period of expected reduction of federal support of programs targeted to this group? Do you have any specific proposals?

GOLDEN: Well, I wish I did have specific proposals. I think it's a very complex matter. It goes back to motivation and questions how motivation can be established, and how the underclass young people can be encouraged to want to learn to read and write and do arithmetic in the first year or two of school. Those who don't, with few exceptions, are doomed to an inferior status the rest of their lives.

There are students of the complex sociological issues involved, which go back to single-parent families, to families who have not been brought up in an environment in which education is recognized as desirable, and parents, who may themselves be underfed, want to feed the infants.

So, I wish I had a prescription. But I think we need to increase the public awareness of this issue, which is moving toward us inexorably, and is increasingly burdensome to all, as the population percentage of the underclass grows. I think that, in a way, the beginning of wisdom is an increasing awareness of the existence of a problem and a recognition that even though we don't see a difference from one day to another, if we look ahead 10 years, we're going to find that this tsunami of the burden of the underclass is approaching us very closely.

So I hope that all will be thinking about it more acutely than they, I should say we, have been doing so far.

BROOKS: If I may comment, this is something that I've been thinking about a great deal lately, trying to get a project started at the American Academy. I think the problem is much broader than the underclass. It's the problem of income distribution overall in the whole society. Since roughly 1979, inequality of income in American society has been steadily growing after roughly 20 years of decline, with a very abrupt decline in equality during World War II. There is no agreement as far as I can make out among economists or anybody else, as to what the source of this decline is.

This is a universal phenomenon in industrialized countries. It's largely concealed in most of the other countries because of the much more generous welfare programs.

Canada, for example, if you look at it in terms of earned income, actually has a more rapid rise in inequality in income than the United States. But nevertheless, that is almost completely offset – or has been in the past almost completely offset – by much more generous welfare programs.

I notice now that the Canadian budget deficit, as a percentage of GNP, is one of the largest in the world, and this is increasingly a problem that's going to face the other countries that have been able to suppress the problem, essentially by means of redistribution programs. But I think the cost of these redistribution programs is now getting so high that the problem is going to be more and more a universal problem of the industrialized world.

The U.S. has very low unemployment, particularly long-term unemployment, whereas the European countries and Canada have much higher and much longer-term unemployment. But we have much greater unequal distribution of income. So there seems to be a trade-off between the two.

COLE: Thank you. The next question goes to Harvey Sapolsky and it's from Michael A. Dennis, Cornell University. And Dr. Dennis asks–

> Isn't health also another third rail for science and technology? HMOs and insurance companies don't like to deal with university-based researchers, since they would prefer the government to pay for the research. But will a Republican Congress pay for such research when they might agree that the health insurance firm should pay for the research they use? Won't research become a hot potato that both the private and public sectors will attempt to avoid paying for? Health and insurance company profits may be incompatible.

SAPOLSKY: I think there's a bipartisan distaste for natural death, so I don't think it's going to be a problem of Republicans opposing this. In fact, historically, they've used health research as the alternative to universal insurance.

They didn't bring it up particularly in this last go-round, but they opposed, in the past, passage of universal insurance schemes by saying, why don't we invest in the best insurance scheme of them all, basic research in health care? And part of the rapid experience of the increases in NIH during the 1950s was largely on that rationale.

I think they'll support health-care research. I think the public is so much unanimous on this interest that they can't oppose it.

GOLDEN: I agree with Harvey Sapolsky and with Harvey Brooks, both Harveys, that funds will be effectively used. But I think it's important to point to a very real problem that relates to this. While research is being well-fed, the teaching hospitals are facing a leaner and leaner diet. And while it is very desirable to have universal coverage for health and medical needs, the pressure so far has been and is growing to reduce the payments that are made to teaching hospitals as distinguished from primary care hospitals and the like. And I think it very important that the teaching hospitals be nourished sufficiently so that they can utilize the advances in science.

Now, at Mt. Sinai Medical Center in New York, we have both a medical school and a teaching hospital, and while the medical school increasingly is faring well in obtaining grants for research, grants which I'm satisfied are very well expended, the hospital is being confronted with reduced payments – and this applies to all the teaching hospitals.

I referred to Mt. Sinai, but in New York there's Cornell and NYU – all the teaching hospitals in New York have the same problem. I think it worth mentioning. It's very important.

COLE: Thank you. It might be appropriate to take questions or comments from the floor at this time, if anyone has them. Please identify yourself.

LICHTER: My name is Bob Lichter. I'm from the Camille and Henry Dreyfus Foundation. The discussion has focused on drivers of science, science's role in defense and industrial competitiveness, national competitiveness, international competitiveness, health. The emphasis has been on scientific knowledge as the product.

I suggest that this emphasis is misapplied. Well, I don't know figures, but I would suggest that most scientific results will remain buried in the scientific literature in one or another of the leading or less-leading journals, perhaps available to be mined appropriately. But most unlikely to be so, most of it.

But for me this is not a reason to diminish support of science and for academic research, if you accept the premise that the product is not the knowledge, but the people who are producing that knowledge, and particularly the students, undergraduate, graduate students, post-doctorals, and others who are involved in that whole effort, who become the scientists who make the change and, equally importantly, become the advocates and the cheerleaders for science. That the process of doing science is the best way to learn about science.

And that brings one then to the question, the observation, that the people who are likely to be doing science in the future, most definitely, will not look like the people who are sitting here in this room.

And I would like to see some discussion directed toward, in fact, who are going to be the next generation of scientists and the implications of that question for developing science policy.

JONATHAN COLE: Thank you. I believe some of that may be touched on this afternoon, and some of it will probably be the subject of some extended discussion in the conferences that will follow later in 1995.

Roundtable Discussion (I)

G. Pascal Zachary
Professor David Hart
Professor Donald Hornig
Professor Lewis Branscomb
Professor Nathan Rosenberg

Moderator
Professor Richard R. Nelson

NELSON: I came to know Greg Zachary last summer when, as a reporter and staff writer with a nose for what's going on in various places, he somehow found out about this conference aborning and gave me a call. And two minutes into the conversation, I recognized how lucky we were that he had given me that call. Greg Zachary is, as he puts it, an independent historian. Right now he is working on a biography of Vannevar Bush and therefore his presence here is especially valuable. He also is a staff writer for *The Wall Street Journal*.

And as I talked with him, I thought to myself, gee, how could you do much better than to get someone here who would not only contribute to the conference by telling us some very interesting things about Vannevar Bush, but also get the whole enterprise written up and advertised in *The Wall Street Journal*. [laughter] Greg?

ZACHARY: To begin this review on Vannevar Bush, I want to look at how Bush was seen by his contemporaries. "Meet the man who may win the war" is how *Collier's* opened a profile of Bush in early 1942. "The general of physics" is what *Time* magazine called him in its 1944 cover story. These are military images.

I will get back to this sense of Bush as a military chief, but first I am going to outline a few things about him that are important to understanding and appreciating his outlook and goals for the report, *Science: the Endless Frontier*, and for post-war science and technology policy. To start with, Bush was not a scientist. He shrewdly deployed the term "scientist" when it suited his interest, but he was an electrical engineer and an academic entrepreneur. He counted among his credits the co-founding of the radio tube company Raytheon in the 1920s, and the creation in the 1930s of what were then the world's most powerful mechanical computers.

Respectful of the scientist and reverential towards science, Bush nevertheless viewed the engineer as the central actor in the extraordinary transformation of our material lives in this century. For Bush, the engineer was a new creature, the offspring of the revolutionary union between business and science. Applied research, when sponsored by entrepreneurial capitalists, promised to unlock vast creative energies for the good of humanity.

"Not many years ago," Bush wrote in 1921, "commercial research was looked down upon as undignified and mercenary and not to be mentioned in the same breath with the study of the swing of the planets in their orbits. To the businessman, on the other hand, the pure-research enthusiast was a dreamer and a solver of academic puzzles – ornamental perhaps, but useless and expensive."

"Today, all this has changed," he concluded. "We have learned that no science worthy of the name is so pure as to be entirely devoid of possibilities of service to the needs of a complex civilization."

In this, Bush was not pandering to the insistent desires of a commercial society. He relished learning for its own sake, and was curious about all aspects of the world – the stars, plants, even the nature of time. Yet for Bush, the pursuit of knowledge was an aspect of living, not the object of life. It nourished his ceaseless activity.

Bush fiddled with everything. His wife, Phoebe, once said, "He's got a shortcut for everything." He played with painting and photography, carved his own pipes, built boats, fishing rods, all manner of things. At the height of the war, he even wove baskets late into the night.

The mathematician Norbert Weiner was so impressed by Bush's dexterity with wires, wood and tools, that he described Bush as "one of the greatest apparatus men that America has ever seen. He thinks with his hands as well as his brain."

Bush's sense of knowledge as a physical encounter with a stubborn reality informed his passion for invention. "At bottom, I invent because I can't help myself," he once told James Killian. Yet despite his personal pragmatic bent, Bush made *Science: the Endless Frontier* a paean to pure research.

In celebrating science and seeking to win for its practitioners government money without strings attached, Bush sought to repay a debt he felt the nation owed its scientists. This was by no means an act of altruism, however. Bush held fast to the notion – now justly discredited – that scientific discoveries preceded technological innovations in a linear fashion. Inventors, in other words, fed on science like hogs on corn. In the hothouse of invention spawned by the war, Bush insisted that innovators had drawn down the country's storehouse of scientific knowledge to a dangerously low level.

So, in a sense, Bush's espousal of science for its own sake needs to be modified. He justified federal support for pure research by raising the specter that the nation would be left vulnerable unless science were reinvigorated. Despite this specter, Bush felt that federal support for science was a mere sideshow, compared to what he saw as the central task of government coming out of World War II: the unification and rationalization of the armed services, and the task of inventing and producing the most advanced weaponry in the world.

While recalled as a patron of pure research, Bush was actually consumed by a desire to reform the military, and privately dreamed not of winning a Nobel Prize, but becoming the nation's first Secretary of Defense. This desire was the culmination of many internal battles in World War II. Bush was Chairman of the Joint New Weapons Committee during the war. His presence was the first time a civilian had any direct relation with the Joint Chiefs of Staff. His purview, authority, and clout with the military, which stemmed from his direct relationship with Roosevelt, gave Bush a taste of the military's problems and a feeling that he and other civilians had to be concerned not just with developing new weapons, but with military strategy and tactics as well. Interestingly, in *Science: the Endless Frontier*, Bush says that the military can improve existing weapons, but can not be trusted to make new ones.

A full seven months before the release of *Science: the Endless Frontier*, Bush wrote a friend that, "The proper path is not for science to aim for a cabinet post of its own, but for scientists to qualify for cabinet posts generally. This has not occurred in this country and I do not understand the reason. I think that a few scientists sitting as chiefs of the regular departments [of the federal government] would make quite a difference. Quite frankly, I would not personally be interested in going down that path unless there were an opportunity of great magnitude, such as Secretary of Defense."

At a minimum, Bush felt military readiness was too important to leave to generals and admirals, who in the past had been all too willing to sacrifice long-term plans for short-term gain. "War and preparation for war are far too important to be entrusted to generals," he wrote, adding, "When the founding fathers placed our military organization underneath our political civilian system, they knew what they were doing and we had better keep it that way."

Again, I think all of this underscores why there is a provision in *Science: the Endless Frontier* calling for a portion of the nation's military research to be coordinated by the National Research Foundation (NRF) and for the NRF to coordinate, or to at least to monitor, all federally-funded research. Now you can see more clearly the reason for this recommendation.

However, even as he lobbied for legislation outlined in *Science: the Endless Frontier*, Bush spent his time and dwindling political capital largely on the more crucial task of opening the military to civilian expertise, both in terms of R&D and more generally in terms of the management of the sprawling defense establishment.

Indeed, he was appalled by the bitter inter-service rivalries, the wasteful duplication in research, and the poor process of making national-security decisions, both in the nuclear realm and on more conventional questions. “This kind of organization would not be tolerated one week in a manufacturing concern producing bobby pins,” Bush said in early 1946.

While he failed to convert the military establishment to his own technocratic outlook, Bush nevertheless created the beginnings – in the Department of Defense’s (DOD) research and development board and weapons-systems evaluation group – of the management structures whose existence made possible DOD chiefs such as Harold Brown and the William Perry, secretaries very much in the Bush mold. Yet in civilian science and technology policy, Bush's legacy is faint, despite the flattering talk of the so-called Bush era having extended into the 1990s.

His justly famous report, meanwhile, is more often read as a fable of high-minded purity, when in reality, *Science: the Endless Frontier* was partly the fruit of a carefully orchestrated political campaign to stop cold the allies of Senator Harley Kilgore, who presented an alternative ideal of tying government support for science to explicit economic and social needs.

Trading on the prestige of science in the public mind and downplaying the ties of lower-status engineers to the research process was a key element in Bush's campaign. The fact that Kilgore's philosophy and not Bush's has informed everything from the moon race to the war on cancer to synthetic fuels and the supercollider strongly suggests that the Bush era actually ended many, many years ago.

NELSON: As we all know so well, one of the key episodes in the evolution of the structure of science governance and financing that has shaped the post-war era up to the present time was the struggle over the formation and design of the National Science Foundation.

This is a topic that David Hart, who is now at the Kennedy School of Harvard University, has been studying and reflecting on. I understand, David, you're quite intrigued in looking at science policy, not only as a phenomena in its own right, but as a window into understanding the dynamics of politics, the debate about the role of government in the United States, and we'd love to hear from you on the struggle about the National Science Foundation.

HART: Thanks. That introduction and Greg's talk are both perfect set-ups for what I wanted to say. I think my talk will re-cast the earlier discussion a little bit. I thought for this audience it would be most useful to put the NSF debate – in which Vannevar Bush and Senator Harley Kilgore were the most prominent, but by no means the only participants – to put that debate in a larger political perspective.

I'll emphasize the dimensions of the debate that touch on economic policy and technological competitiveness in particular, partly because that's what my own research stresses – although I have to say I haven't looked at the primary documents on the NSF, I'm mostly relying on other people's work there, but my own research is in this area – and partly because other speakers today are going to take up a whole range of other issues that the NSF debate had implications for. I'm going to speak to three main subjects briefly – obviously I have just a few minutes.

First, the range of views on post-war science policy and their relationship to the more general economic policy debate that was occurring in the post-war period.

Second, the areas of agreement and disagreement between Bush and Kilgore, suggesting that there were more areas of overlap than is usually seen between them.

Third, the politics of the 79th, 80th, and 81st Congresses that prevented these agreements from being realized in legislation until 1950.

As to the first point, let me say it's important to recognize that all sides in this debate were committed to the market as the pre-eminent economic institution. Even Henry Wallace, who is one of the forgotten figures in this debate, but an interesting one, was far from the "pinko" that he was portrayed to be in the 1948 presidential campaign.

What the political combatants in the argument disagreed about was what the state ought to do to make markets more vibrant. The left side of the debate, so to speak, is Kilgore's side, although, as I'll get to in a minute, Kilgore's view was a much attenuated version of a leftist vision for science and technology that grew out of the Great Depression. His side of the debate should be viewed in the populist tradition, not as some kind of socialist alternative to Bush.

Kilgore, Wallace, and others were most exercised over the concentration of economic and political power that they thought the R&D capacity of a few large firms provided. And they thought the state ought to develop the capacity to serve as a scientific and technological counterweight to these interests in order to expand markets and competition. While this view made little impression in the end on the NSF itself, it had other adherents, notably in my work James Newman, who drafted the Atomic Energy Act in 1946. And that passed Congress, unlike the NSF act.

So Kilgore's side is to develop the state as a counterweight to the concentrated power of big business. Bush, obviously, was far less concerned about such market failures, although he did propose – not in *Science: The Endless Frontier* but elsewhere – modest changes in the patent law to protect small, innovative companies, which he recognized as an important element in the U.S. national innovation system. The only major market failure that Bush identified in the governance of technological innovation was the support of university-based research – a market failure that private endeavors like those of Herbert Hoover in the

1920s had failed to close. There are some interesting parallels between Hoover and Bush in their political thought.

So that's Bush's position. Now, the story is often told that there are simply these two camps, but there are at least two other positions that need to be mentioned. One is the hard-line laissez faire view that the state ought to stay out of anything that wasn't mission-oriented, including academic-research funding.

While Frank Jude, who was the president of the National Academy of Sciences and of Bell Labs at the time, was the only prominent member in the scientific community to adopt this view, he echoed the views of a powerful block in Congress Now this was a substantial permanent faction which both the Kilgore and Bush factions had to unite against in order to get their bill through. They failed to do so in 1946, leading to the bill dying in the House of Representatives.

The other position that I wanted to mention, this time on the left, relates to the emergence of Keynesian economics, during the war and immediately after – and the related fixation on economic growth as the solution to a whole host of woes, including those woes that concern the populace, that is, that concentrated economic power would stifle the market.

Growth, made possible by government spending, especially in support of consumers, but really regardless of its purpose, was seen to lubricate private markets and private innovation. As a result, many on the left bowed out of the science debate, and more importantly, the technology debate in favor of debates related to social welfare that could yield a bigger bang from a Keynesian point of view. They lost interest in the structure of the economy, including the development of new industries, that had concerned liberals earlier, especially in the 1930s.

So the point is that the Bush-Kilgore debate, as it is colloquially known, really should be seen in the ideological and historical context of the much more complicated, multi-faceted debate over the place of the state in the governance of the economy and in the governance of technological innovation. This was a debate that is permanent in American history, but it reached a peak in the immediate post-war period.

The second point, to turn to the more narrow issues in the Bush-Kilgore argument, is that they agreed, at least in 1946, more than they disagreed, and compromise should have been achieved at that time. In effect, it was – before it was spiked by the conservatives in the middle of 1946.

I don't think the characterization of pure research versus research targeted to social needs, as referred to in the original charge from the conference directors – I don't think this distinction captures the debate, at least at that point. Both sides agreed on the importance of NSF funding for defense and health-related research, to mention just two important social needs. They also agreed on the importance of university-based research. These three areas were to have the statutory claim on 90% of NSF's funds, at least in Kilgore's 1945 bill. This was before they started negotiating with Bush to create a compromise. Bush and Kilgore also agreed on the size of NSF, which was to be roughly $100 million to $150 million a year at full strength.

There were substantial differences over policy for government-owned patents and on the funding for non-defense industrial development, as opposed to academic research, in the summer of 1945. But these were either conceded by one side or other other or separated out from the NSF bill by the end of the year. The development end of funding, for instance, was put into the Fulbright bill to expand the capabilities of the Department of Commerce, of which Wallace was now secretary. This is another side tale that I can't pursue right now, but it's an interesting one. And I should also mention that Kilgore had backed away from the anti-trust agenda that he had pursued through much of the war, including the hearings from 1942 to '44 on science and technology mobilization.

Where the camps differed on NSF, obviously, were the areas of organizational control and the mechanisms for allocating funds. Kilgore preferred a more broadly representative National Science Board or Advisory Committee, as it was sometimes called, and a director who was directly accountable to the president. He also feared that a few elite institutions that were geographically concentrated would garner all the funds, as they had during World War II.

Bush wanted a board and director insulated from politics and more responsive to the scientific elite. Pre-existing research quality and peer review were to guide distribution. An argument can and was made on behalf of both of these approaches, that they served social needs. In any case, I want to say that not too much should be made of them, at least in 1946, because both sides showed a lot of give, and in fact they reached a compromise bill in February of '46.

What happened to the compromise, however, was that larger political and economic forces intruded upon it, and some of these things will ring true for those of you who have been watching politics this year. First, the Senate schedule in the second session of the 79th Congress – that is, in 1946 – was tied up with issues of extreme ideological controversy, such as price de-control. A vote on the NSF compromise, which should have gone to the Senate floor sometime in February or March, was held up until June on account of this.

The delay created opportunity for defection from the compromise. This occurred in the House of Representatives, partly because the House was more conservative than the Senate, and partly because the Republicans were blocking things toward the end of the 79th Congress, because they sensed that they were going to win in November. A lot of people have recalled this period, thinking about the 103rd Congress that just closed. As Wallace put it, "The opposition does not want the Administration to receive credit for the passage of sound and constructive legislation." Does that ring any bells?

A further incentive on the part of Congress was a desire to re-assert Congressional control over the federal government. After the unprecedented three-plus terms of Roosevelt, and after the war years in which it had waned, bills like the NSF seemed to permanently cede authority to the executive. In the 80th Congress, which the Republicans controlled, Truman vetoed a bill that was passed by both houses which was more in accordance with Bush's tastes, as many people have noted. And I'm sure, as he stated in his veto message, that he preferred accountable officials and objected to the procedures for appointing a director that were included in the 1947 bill.

But he was also jousting at the same time with Senator Robert Taft, who was a potential presidential opponent in 1948, and he was also setting himself up to do battle with the do-nothing Congress, which he did quite successfully in 1948.

In the 81st Congress, after united Democratic control over the presidency – Democratic with a capital "D" – and Congress had been re-established, the NSF bill was still bottled up in the Rules Committee by conservatives who were concerned about its cost and its "social tendencies." It took extraordinary parliamentary maneuvers to get it out and actually get it voted on in 1950. So the laissez faire-ists actually came close to squelching the whole thing, at least before the Korean War.

Finally, in addition to the support of ONR and other agencies, which alleviated the interest-group pressure from scientists for an NSF, there was a surge of economic growth of all kinds in the immediate post-war period, and this alleviated the pressure for state activism of all sorts.

As Paul Samuelson put it in the 1951 version of his classic economics text, "secular stagnation" had really concerned the liberals before the war and the wartime Keynesians as well. The problem in secular stagnation: too few investment opportunities. This view about secular stagnation had motivated interest in making public investments in science and technology. It was replaced by belief in the probability of "secular exhilaration," an economy in which there were too many investment opportunities to be realized. And hence, there was not much need for an act of state.

Let me leave with one final thought. The point of my comments, apart from their historical value, is that science and technology policy making is an integral part of the larger American political process. For much of the post-war period, some students of science policy and some practitioners, though it's harder for practitioners, have tried to insulate or divorce their work from the larger disciplines and processes in which it is imbedded, or to see it mainly as the province of scientists. This leads to, I think, such anomalies as taking *Science: The Endless Frontier* to be a treatise when it's really a political document.

The apolitical pose that this perspective reveals is very reminiscent of that of Bush. Bush, I think, at bottom was a very political animal, and he used this pose of being above politics to political ends. Times like the '40s and like the present, I think, show us that we ignore the larger political and economic context of science and technology policy making at our own peril. Thank you.

NELSON: Those two presentations form something of a pair. They fit together very well indeed and in the presentations that will follow, we're going to leave this particular topic. So I think this is a good opportunity for questions from the floor and further discussion of these things.

QUESTION: Well, I have one. And that's this: As you looked over, Greg, the Bush records, and in particular looked at a number of the materials that indicate what was in various people's minds leading up to the document *Science: The Endless Frontier*, what impressions have you – you began to talk about them – regarding the reasons why Bush staked out a view of the relationship between science and technology in at least the first part that, I think we all know very well, he himself really didn't adhere to? I take it this was a political strategy that he thought was essential at that time to further a particular objective.

ZACHARY: There are a couple of things that are puzzling, which is, he seems to have been very conflicted about the role of government in helping innovation. He accepted that private companies ought to pick up the ball and run with the scientific knowledge. On the other hand, he realized too, that most of the economic bang was from that activity. And so in removing the government from all that, the ability to shape innovation from a governmental level was almost nil.

He was not unhappy about that, but that's one of, I think, the big weaknesses in his structure. Today, it's common to talk about private/public partnerships. There was no talk of that for Bush, partly because industry felt hostile to government. Bush, in the midst of the debates over *Science The Endless Frontier*, joined the boards of AT&T and Merck and later became chairman of Merck. He began as a scrappy entrepreneur, but he had made many big company contacts as time went on.

GAZIS: Divas Gazis from IBM. I know that Greg has some views about the present state of affairs, so I would like to ask him the following question: Suppose Bush were here today, how would he address the problems that we are facing, the downsizing of the scientific establishment, the international competition, and so on? What is your guess?

ZACHARY: Some of it's a leap of faith, but one of the things that appeals to me about Bush is that his enlightened conservatism, I think, would be very much in vogue today, a magnet for people who are frustrated with the sort of liberal order that has been in decline.

Bush clearly understood the need for the engineer and for the manager to be central to the process of innovation, and he also understood the importance of markets. Even in military innovation, Bush stressed what is needed actually on the ground.

One of his big fights with the military was over the use of weapons – that it wasn't enough just to make them, you had to make sure they were used and then once they were used, you had to be able to tailor them to the actual conditions of battle.

He was very much immersed in a market-oriented approach to innovation, and I think that that's contemporary. I think he would also want to involve industry at this point more than academia. By the late '50s, Bush was disappointed at the extent to which universities and non-industry researchers were dependent on the government.

It was telling that the Carnegie Institution was eclipsed as an important player in federal research because Bush simply would not accept any federal funds for research in the entire time. He was there until 1955, and he insisted, and Carroll Haskins, his successor, wouldn't accept any either. I think Bush felt that these dependency relationships with the government had a corrosive effect on the entrepreneurial capacities of the private sector, and I think he was correct. He would probably propose a more radical downsizing of the direct contracting to non-profits and try to do something to stimulate the private sector, either through contractual means or through tax incentives.

HART: I just want to make one point related to that. I think he also would have been very interested in consortia, which was something that his, I don't want to say mentor but, colleague Herbert Hoover was very interested in – not necessarily government funded, but to facilitate industry-wide research, so that industry would help itself and to overcome some of the barriers to innovation that way.

ZACHARY: A good example of that was the way in which he divvied up the patent rights to penicillin among the many pharmaceutical companies that worked in concert during the war to mass produce penicillin. So he did actually have experience with that kind of thing.

HART: And you see the same thing in synthetic rubber, where Bush wasn't necessarily involved, but Compton and Conant were on the Baruch commission, set up the synthetic rubber research program.

QUESTION: Just a very brief question on a point that seems to have been glossed over. One of the eight recommendations that was described earlier today had to do with support for students, fellowships and scholarships for students. From a historical perspective, how important that really was to him? And also, in the context, something I found fascinating, that he felt scientists should take more leadership roles in government apart from science. Would a preparation for that be included in his vision of what the education of scientists would be about?

UNIDENTIFIED: Support for students – his position was also surprising and not very contemporary. He was quite glad that support was raised. He thought that was essential. However, he had a view that the degree of support that occurred by the late '50s was excessive. He talked in 1955 about what he felt was the existence of a natural aristocracy within the United States.

In other words, he believed that there were some natural limits on the number of people of merit and talent within the society. He felt that, in the case of science, that you could not create many more than that natural level through a brute force, throwing money at the problem. So again, by the late '50s, he feels that's overdone and what we're actually doing is producing large numbers of mediocre people to augment the talented people.

ZACHARY: That's very interesting, that's a very Schumpeterian view, if I might say so.

UNIDENTIFIED: And that's what makes him so contemporary in a lot of ways.

ZACHARY: It's also interesting to me that Wallace was a scientist. Harley Kilgore didn't know much about science or technology at all. But Wallace did know a lot about it, and he and Bush were [unintelligible] so I don't think he wanted just any scientist to be in the Cabinet, if that's fair to say. [laughter]

QUESTION: Maybe I didn't understand something, but it was under Vannevar Bush that the whole pyramid model of who rises to the top started, which then got translated into today's pipeline. And the way I remember it, is that you don't know who's going to get to the top.

ZACHARY: That's true.

QUESTION: So you better do as much as you can at that time.

ZACHARY: That's true, although I was thinking more in terms of higher education and postgraduate support. But yes, he supported the idea of, let's say, lower education broadly, and he did feel the pipeline needed to be replenished. But there were natural limits on, say, the doctoral level. The number of outstanding scientists that a generation could produce, he did not feel was elastic. He thought that you were pretty much stuck with a fairly small number. It was an elitist view, but it was one he never really changed.

NELSON: We'll come back to questions a little bit later. We have within this group a significant fraction of the very distinguished science policy makers who have been involved in the shaping of science policy in the United States over the last number of years, and our next two panelists are prominent members of that group.

Donald Hornig, you all know, served as President Johnson's Science Advisor from 1964 to 1969 and I think is characteristic of many of the people who played a prominent role in science policy making in the United States over this period. He is a triple threat. He's been in government, he has been Vice President/Director of Eastman Kodak, and for many years, he's been an academic, including his presidency of Brown University. And Donald Hornig is going to be reflecting on some aspects of that experience in Washington.

HORNIG: Well, I have to tell you it's a daunting task to be told to give your perspectives on science and the White House after Vannevar Bush. The White House was involved from 1958 until the present and quite a lot has happened. [laughter]

Even if I confine myself to the 10 years in which I was on the President's Science Advisory Committee under three presidents, there's a lot to say. So I will restrict myself to some somewhat random observations, particularly as they are connected with Bush.

It seems to me that despite all that's been said, Bush came out of the lore with – as reflected in *Science: The Endless Frontier* – a remarkably perspicacious sense of what the role of a government, and particularly our government, could be. It wasn't confined to basic research. He started off, of course, with a push on the importance of medical research.

And as Harvey Brooks has enumerated, he urges the continuation of the applied research work in all of federal agencies, including the Department of Defense. He brings in the idea that, in defense, external civilian agencies working on these problems would enhance the quality of what went on in the department. He advocates the encouraging of industrial research, not from funding in that case but through tax incentives and reform of the patent laws and such.

And so it's against the background of this whole spectrum of activities to strengthen science that he also comes to the one thing for which there's no other existing agencies at that time to deal with, and that's the whole basic research and student training aspect.

And it seems to me that this is really very central. It's not just that basic research needs to be done, but rather that it is a proper role of the government.

In fact, he makes two of his first points: the importance of scientific progress and science as a proper concern of the government. And from there on, this report had an impact. I think calling it a manifesto is right. What he said was deeply believed by a whole host of people who worked with him during the war at Radlab, Los Alamos, where I was – he pulled it together and stated it very clearly and simply.

What happened subsequently wasn't an acceptance or rejection of Bush – Bush planted a seed that started to grow right away, and I mean right away.

ONR was founded in 1946, largely because the Navy was convinced of what he said about basic research. As has been mentioned, the AEC isn't a rejection of Bush; it was already a government function. It hadn't had its own agency until that point, but at that point, the whole development of nuclear energy, nuclear power, as well as the support of nuclear physics, went into this new agency and took off.

In 1945, the National Institutes of Health began their growth, and I must say until late in the morning, I had the impression that national science consisted of physics. [laughter] But in fact, Bush perhaps saw medical research in his incipient national research foundation, which would be the NSF. But it didn't wait for the NSF. It began sooner.

The NIH is the only part of the government I know of, and my experience only begins under Eisenhower, in which the appropriations for their budget have exceeded the presidential request every single year.

Once the ideas of the Bush report took hold, all sorts of things began to happen. The first thing that happened is that the budgets grew faster than he ever anticipated, and the complexity of the organization grew. And hence, in some ways, it got more out of control.

And then came Sputnik in 1958, which of course Bush never foresaw. And that led to the involvement of the White House. And why? In a big way, the issues of science got swallowed in much bigger national issues than could be handled within the federal apparatus.

That led to the formation of the president's science advisory committee and Jim Killian's becoming the Special Assistant for Science and Technology. And ever since, in one form or another, there's been an office in the White House to attempt to, in some way, shape and coordinate this business.

But also with Sputnik, another thing happened. It became clear that the White House doesn't control things. To put it simply, things that have big economic and social impacts have other constituencies. And even in the case of space, for instance, right off the bat, PSAT wasn't even consulted about Apollo by President Kennedy. Its advice was rejected on how to get to the moon. The big action was directly between Mr. Webb and President Johnson, or Kennedy, in the first instance.

And other things happened like that. The SST, under Presidents Kennedy and Johnson and finally Nixon, was much opposed by the president's science advisors, but for the most part, it went merrily on its way with a, let's say, harassing action on the fringes.

Now these are examples of a class of science that I don't know how to deal with yet and that Bush didn't even foresee. And that's the whole phenomenon with big science.

One of the early triumphs of the system was sorting out between the Muro accelerator – this was in '63 and '64 – and the 200 GEV machine, which was eventually built.

I think the need for a White House science apparatus is clear. How it effectively deals with the rest of the political community is not at all clear. By what standards, scientifically, one deals with things like big space missions is not clear.

There's another problem too with the Science Advisory Committee as I knew it, which is that the whole matter has become so complex. In the period between 1959 and Vietnam, it was true that the Science Advisory Committee could focus on a limited number questions – arms control, the ABM, a small finite number of things that could be dealt with.

Now, the whole enterprise is so big and there's so much expertise in so many other places, such as NIH, which the White House apparatus has never dared touch. I think the White House apparatus has to constrain itself to what properly concerns the president and trust the agencies, other than to make sure that the president knows what's going on elsewhere.

I'll close by saying there is one theme that all of the White House people have stuck with: that Vannevar Bush was right and it is necessary to support basic research. NSF, even up to the present, hasn't been able to really hack it on its own. And strong pressure from within the White House to rescue its budgets has been necessary in almost every year since the 1950s.

I'll leave it right there. Thank you.

NELSON: Lew Branscomb is another person who has for many, many years played a varied and influential role in the molding of science policy in the United States. For many years, he was at the National Bureau of Standards, toward the end as director. He's been Chief Scientist at IBM. And for the last number of years, he has contributed very effectively to the teaching on and research on science policy at Harvard.

LEWIS BRANSCOMB: Thank you, Dick. Earlier this week, David Hertz said, "Are you really going to discuss the institutional evolution for science and technology since the Bush years in 10 minutes?" So you're about to find out whether I am or not. [laughter]

The debate over the balance between scientific autonomy and public accountability for government-funded science is just as much a source of conflict today as it was during the Kilgore-Bush years. The Clinton Administration's science policy, Science in the National Interest – SNI as it's often called – issued last August, raises this balance in the clearest possible terms but provides little in the way of policy to resolve it.

SNI basically says these trade-offs will be made in the National Science and Technology Council on a case-by-case basis. Read my lips, in other words. The debate basically turns on the extent to which inputs and outcomes have greater weight in three functions: setting goals and allocating resources, determining the locus of both political and operational control, and providing for financial accountability.

The goal-setting issue has been joined by Senators Mikulski, Rockefeller, Harkin, and others. Do scientists decide what research capabilities the nation will have? Or does that decision have to depend on national goals in some way?

The political and operation control issue turns on this: to whom the governing body of the institution is most accountable. If the NSF director reports to a board of scientists rather than to the president, how does the agency protect itself from the perception of conflicts of interest as they make research awards to the very institutions they represent?

I can tell you, having been chairman of the National Science Board, the rules of who leaves the room when are draconian beyond belief to deal with that. The financial accountability issue is even simpler. If the president asked the Congress to entrust the executive with an appropriation of tax money to a national research or a science foundation, how can the president ensure the accountability of the foundation if the CEO cannot be removed by him?

As an aside, let me say that I continue to be astonished that in the 1990s, the National Research Council would issue a report recommending the creation of a civilian technology corporation to receive a one-time $5 billion appropriation from your and my taxes, under an institution whose CEO reports to the board and not to the president. They apparently didn't read the veto message that Don Price wrote for Harry Truman.

Then as now, the way this policy balance is struck is reflected through the institutional structures and policies of the responsible agencies. Thus, the legislative battle between Kilgore, with the support of the administration, and Vannevar Bush, with support from Senator Magnuson and others, turned on the government's structure of the NSF or the NRF and whether the CEO be accountable to the president or to a board of part-time scientists.

That this issue is resolved largely in Kilgore's favor and not in Bush's did not, however, put the issue of scientific autonomy and accountability to rest. A wide variety of institutional devices emerged that embody a great variety of alternative arrangements, each intended to strike a different compromise.

The most decentralized and autonomous are the NSF and NIH models, in which the dominant mode is agency selection by peer review from among unsolicited proposals from the scientific community.

At the other end of the spectrum is ARPA, which conducts no in-house research but rather selects projects by managerial prerogative to fill goals set by the agency. But TRP is something of an exception. Or NASA, which manages an agency mission agenda through directly-operated laboratories. And of course the directly operated government laboratories of the Department of Defense.

In between lie the contract national laboratories, with DOE being the principal subject of discussion these days since DOE spends approximately $6 billion in R&D through that full spectrum of laboratories – weapons and otherwise.

The establishment of not-for-profit research institutions that are not operated by the government but are funded by the government and work only for the government was intended to provide not only operational advantages unavailable to the civil service but also to get the creativity and the flexibility we associate with privatized science.

And I owe it to Harvey Brooks who taught me that the real important thing about the Bush report, or at least Bush's contribution, was that he privatized what had been government science. Not that he invented a particular institutional floor. Insulation from direct political control, then, was seen as an indispensable step to that creativity and flexibility.

So what do we see today?

Well, we see pressures rising at NIH and NSF to demonstrate that they can conform research allocation to national goals, even though at the same time they're certainly told by the Congress that they're to be allowed to continue to select projects by peer review and to delegate a lot of operational latitude to research performers.

Goal-setting has been strongly centralized in the DOD, except in ONR and its sister agencies, but may be moving somewhat in the other direction, at least to the extent that reform of the acquisition system through more exploitation of dual-use technology leads military agencies, particularly ARPA, to seek cooperative agreements with commercial firms and alliances to those firms with state universities and, indeed, national laboratories.

The DOD national laboratories were supposed to be the most conspicuous example of an institutional form that exists in some arms' length from the agencies that preserves managerial flexibility and embodies a lot of creativity. I don't deny the creativity, but the flexibility seems to have been almost entirely lost.

The directors of Los Alamos, Livermore, and Sandia have, in my opinion, less control of their laboratories than does the director of NIST or the National Cancer Institute. DOE headquarters officials are constantly accused of extreme forms of micro-management.

There have been countless commissions and White House panels that have been unable to substantially redirect or downsize the labs. It isn't clear to me that the Galvin Commission will be any more successful.

In fact, one of my great memories of the Science Advisory Committee that Don Hornig put together in the late '60s, where I was privileged to serve, was when we decided to do a study of the national labs and what could be done to modernize their missions and make them more effective. He asked Al Hill to head a panel at PSAT to do that. We assumed Al would go get 30 people together and work for six months and bring a big report. He showed up at the very next meeting at PSAT with a single file in his hand and he said, I'm finished.

We said, where's your panel? He said, I don't need a panel. He put the file on the projector. It was a matrix. And across the top were all of the things you might recommend about fixing national laboratories and down in the rows were 18 previous recommendations and studies about national labs. [laughter]

And he said, "Implement any of the previous reports." [laughter] I told that to the Galvin Commission, and they've got a problem. Now, the fact that the DOD labs are indeed managed not by the government but by the University of California or the Martin Marietta Corporation or the University of Chicago has not substantially insulated them from scrutiny. At the same time, those operating contractors are really in no position to take the initiative for the restructuring and redirection of the laboratories.

In our book *Empowering Technology*, we propose a solution to that problem, but I won't go into it here. We might come back to that in conference number two, which is more on this subject, I think. Don Price, in chapter two, "Government and Science," which he wrote in 1954 – and which I very strongly recommend to you because it discusses exactly the subject of this conference – does not make light of the hazards of political interference, which the contract lab form of organization is supposed to prevent.

But Price notes that freedom from political oversight is a two-edged sword. And I'm going to paraphrase a quote from Price by quoting him correctly, but I'm going to substitute the word "laboratory" where he used the word "agency." "How was the laboratory going to be given the political support it needed to get the necessary appropriations? Who was going to defend the laboratory against political interference and who was going represent it and defend it in the interdepartmental infighting that is so important in Washington?"

This is the political problem that in many ways is faced by the DOE national laboratories, not, at the present time at least, the fear of political interference in the content of their work.

Now, all this would be complicated enough, but we now have a set of new circumstances for which the structure of federal R&D institutions inherited from years of Cold War is no longer quite appropriate. The intellectual distinctions between science and engineering, between theory, modeling, and experiment, between design and analysis are all weakening.

Fundamental science is contributing to industrial technology not only as a source of new inventions and concepts but enters into processes at every level in the innovation process. Furthermore, there is increased recognition that complimentary assets play a growing role in determining the rate and risk of successful applications of knowledge. And finally, we are all recognizing now that the diffusion of government R&D to private innovation is far from automatic and free, as in some sense the paradigm described, we have been told improperly, to Bush, implies.

What, then, can we hope for the future?

First, we must think very hard whether we can separate two issues that are often confused as one. First, on what basis are scientific resources to be allocated? And second, how are projects to be selected and how much latitude will the performers have in carrying them out? As Harvey has pointed out, these are separate questions. There should be more thoughtful and more explicit strategies for rebalancing the allocation of research resources.

In the light of consensus judgments about where new knowledge and new talent are most likely to be needed as well as where the most exciting intellectual opportunities have appeared, I don't see how we can duck the dilemma that's posed for us in the SNI.

The members of the National Science Board's commission on the future of NSF, which was chartered to examine exactly that question, were prepared to go fairly far in trying to put forward a philosophy for answering that question, but the wording of the actual report obfuscated the views. And as Bruce Smith of Brookings pointed out in an interview in *The Scientist*, that report reads as though it were written by the authors of the Dead Sea Scrolls.

Second, I think there is universal recognition that privatization of the performance of science was indeed an important idea, and was Vannevar Bush's most enduring contribution.

Notice I said "privatization of performance," not "of investment." This comes down to the willingness to carry out most projects from megascience to individual investigators, wherever the talent is found – in universities, in firms, and even in government-funded laboratories.

But that leaves the necessity for the political protection and operational competence of the agencies that are accountable for the money and for its expenditure. To decide how this institutional protection from inappropriate political intrusion should be provided, we need to test the alternative policies and structures to determine which are most robust against corps de influences, are most open to scrutiny, and are most effective in assessing and maintaining quality.

Now the good news is, we just did the experiment. All you have to do is look at the structures that are vulnerable to earmarking and those that are not.

Regardless of the balance between top down and bottom up controlled investment strategies, the key to avoiding corrupting influences is a documented, competitive, publicly exposed process for project and performer evaluation, both before the monies are spent and after the work is done.

We find it alive and reasonably well in NSF and NIST and NIH. It is conspicuously weak in DOE and parts of DOD. But notice that those processes require standards by which merit must be evaluated. Purely subjective opinions of peer reviewers are necessary but not sufficient.

I put it to you that we cannot escape seeing what the goals were, what the motivation was for the work, if indeed we're to have an open objective and competitive means of assessing it and making the decisions.

I don't think Van Bush would have disagreed with the following conclusion. Be clear where the money is being spent, who is spending it, and how they will account to the taxpayers for what they do. Then entrust the task to the most talented people with the best ideas in a competitive environment, using a process that preserves fairness and objectivity and accountability, without which this delegation cannot be sustained for long, politically.

Now I have a two- or three-sentence epilogue, then I'm done. The epilogue – pointed out by Zachary but not mentioned by anybody else here – is this: it is striking to me that while the issue of goal-orientation or investigator initiative as the source of priority is the big debate that we carry over from *Science: The Endless Frontier*, that is not the big debate in Washington.

It's a debate in Washington. But if you look at the legislation the Congress has done in the last 10 years, starting in 1984, you will see what the Congress is focused on isn't that at all. It's what the Congress calls "accelerating the commercialization rate of government investments in research."

It is indeed the assumption that government investments in research have enormous value and if the public is to get back from it, we need to do something to ensure that that work finds its way into industry. Some would use the word tech transfer. I hate tech transfer; I don't think it's a good description of anything that actually happens in the real world, but there is a diffusion process here, an adaptation, and a market for knowledge, which is important.

That's not discussed in Bush's era or in his report. It's the dominant issue today, and if you were to address that issue alone and not the issue of creativity and competition and imagination and fundamental science, what you would do is just what Zachary says Bush would do today. And that is, we put the incentive structure in the industry.

You might have an ATP program that puts some money up to entice them to spend their own, but you'd ask them to take the initiative, for them to define the program, and you'd nudge them along.

So we would incorporate the process of the transition from the creation of knowledge to its utilization – internalize, that's the word I want – we'd internalize that process within the institution as best we could, recognizing we'll never do it terribly well because even in big corporations, it's hard to get work out of the research lab into the product divisions in the same damn company.

But if that's hard, how hard are quotas going to be to do any good?

So I suspect maybe in the next conference or two in this series, when you talk about institutions and current issues, we'll come back to that. I would remind you that solving that problem leads you down a different path, a path that is probably in conflict with the preservation of autonomy and independence and creativity for science.

NELSON: Just before this panel session began, I had some thoughts about briefing Lew Branscomb on how he should end up so as to feed in very nicely to Nathan Rosenberg's discussion, which will follow Lew's. I didn't have a chance to talk with him in person, but mental telepathy worked extremely well in this case. [laughter] I'm glad that you followed these vibes that sort of went across the cable that this is what you should be doing at the end of your discussion.

This morning, I was struck when Donald Stokes and then others observed that at the time that *Science: The Endless Frontier* was written, there had been very little scholarly work, at least in English, on the history of science, which was becoming a field but not strongly yet. I. Bernard Cohen was there, present at the creation. Nor was there very much in the way of research on history of technologies, and maybe *Science: The Endless Frontier* reflects that.

One of the quite interesting things that the developing expenditures on science and technology, research, and development have done over the last 50 years is to build quite an extensive intellectual community that's concerned with how science and technology evolve, and many of you here are members of that.

And Nathan Rosenberg, Professor of Economics at Stanford, has been a very major pioneer along these roads. I've learned a lot of what I know from Nate. And Nathan is going to be picking up by plan just where Lew Branscomb left off.

ROSENBERG: No matter how you look at it, coming to supportable conclusions about the impact of science and technology policy upon economic performance is remarkably difficult. For one thing, even coming to an agreement about what we mean by "technology policy" is far from straightforward. Does it include, for example, the regulatory activities of the Food and Drug Administration (FDA) or the Environmental Protection Agency (EPA)? There can be no doubt that the FDA's regulatory actions have a very powerful effect on the development of new technologies by pharmaceutical firms and medical device firms.

Similarly, many governmental activities exercise a powerful influence over the development and exploitation of new technologies, even though the primary purpose of those activities may have little or nothing to do explicitly with technology development. Technology policy may be primarily a matter of unintended consequences.

To make matters worse, economists are far from agreeing on the quantitative importance of technological change to American economic growth. Beginning in the mid-1950s there was a huge increase in interest in the subject and it would be fair to say that economists now set the contribution of technological change to economic growth higher than they once did. There has also been a growing awareness that the contribution can not be represented by some single abstract number because the impact of technological change on the economy is going to depend on what is going on simultaneously in other sectors of the economy – the rate of accumulation of tangible capital, the acquisition of skills on the part of the labor force, demographic changes, etc. In order to simplify and narrow my focus, I will confine my attention to federal R&D spending.

A budget is clearly a statement of policy. I'd like to make three observations concerning distinctive features of the post-World War II period that have been very important for their eventual economic impact.

First of all, the government became the dominant purchaser of R&D, but without at the same time becoming the primary performer. The unique institutional development has been the manner in which the federal government has accepted a vastly broadened financial responsibility for R&D without at the same time arranging for the in-house performance of R&D, with the exception of the federal labs.

Second, private industry has become the main performer of all R&D. And third, the university community has become the main performer of the basic research component of R&D, as Bush had advocated. In the post-war years, somewhere around two-thirds of basic research has been financed by the federal government but more than half of all basic research has been performed by universities. These observations help to clarify why it is easier to discuss the government's science policy than its technology policy. The government has emerged as the main source of financial support for science.

Technology, however, is a far different and much more complex matter, and yet technology, not science, directly affects the course of economic activity. And since technology is primarily incorporated in goods and services that eventually are sold in the marketplace, the ultimate responsibility for technology is in the hands of profit-maximizing firms in the private sector. So that, as I see it, technology policy presumably must refer to the actions of government that influence the decisions of firms as they consider the wisdom, or "unwisdom," of investing in new technologies.

In this sense, decisions to improve technology or purchase new technology are investment decisions. And investment decisions may be influenced by various activities of government, many of which are conducted with other criteria or goals in mind – such as regulation, taxation, and matters of national security. Or perhaps even more important, success or failure in the exploitation of new technology, in a certain sense the bottom line, goes far beyond the activities that are directly subject to government influence.

Success involves commercial skills; it involves and intimates understanding of the trade-offs between costs and performance, and the design of new technologies; and it involves the development of effective feedback mechanisms that permit quick adjustments and adaptations in response to new information from the marketplace about consumer preferences.

In addition, America's leadership in the high-tech sectors in the post World War II years has been vastly assisted by the easy entry of new small firms that frequently have served as the early carriers of new technology. This role was facilitated by the venture capital industry, an almost uniquely American institution. The venture-capital industry has been vital to the early American lead in new industries of precisely the kind that have tended to be spawned by university research – electronics, biotechnology, medical devices, etc.

It should be added that creativeness of the interface between university research and industrial research has been one of the most decisive determinants of American success in the high-tech world. Having said that, I'd also suggest that in the post-war years, American society has become excessively absorbed with the up-stream forces shaping the course of technological change, to the neglect of downstream forces that are much closer to the marketplace.

By any measure, we have done remarkably well at the research activities that occasionally win Nobel Prizes, but we've been a great deal weaker, especially in recent years, at the skills that are nourished by continuous information feedback from the market, and that involve improvements in efficiency in the manufacturing process. One relevant piece of evidence on this score is that American high-tech firms report that they devote about two-thirds of their R&D expenditures to product innovation, and only one-third to process innovation, whereas their Japanese counterparts do exactly the opposite – two-thirds to process improvement, and one-third to product innovation.

So the federal government's post-war largesse and support of research may have had one entirely unintended consequence. This nation has developed a strong comparative advantage in the early research-intensive stages of the innovation process – the kinds of research activities at which universities excel. But at the same time, we have neglected the later stages of the innovation process that become more important as an innovation moves closer to the marketplace, where sustained attention to incremental improvement, rapid response to information concerning consumer tastes, and the refining of process technologies come to determine commercial success. This neglect was reinforced during the first half of the post-war period by the sheer absence of credible competitors to American firms across a wide swath of high-tech product markets.

The painful structural adjustments that many American industries have been making in the past 15 or 20 years are part of the process of adjustment to a more competitive world economy after other industrial powers recovered from the devastation of the second World War and largely completed the process of technological catch-up with America.

This leaves us still with some fundamental unanswered questions. The widespread public impression is that we live in a world of unprecedentedly rapid technological change. If the purpose of science and technology policy is to accelerate technological change, it would appear to have been a spectacular success. We talk routinely about information superhighways, the internet, a remarkable assortment of new medical technologies, and Gordon Moore's law, which states that the memory capacity of a chip doubles every 18 months. Computers are everywhere.

At the same time, the rapid technological progress of the last 20 years also coincides closely with a rather abysmal slowing down of American productivity growth. The question that must be posed is: what's going on? In Robert Solow's succinct formulation, we see computers everywhere except in the productivity statistics, and that is really surprising.

If one wanted to be even more paradoxical, one could point out that the U.S. was the leader in productivity growth among industrial countries before the second World War, when she was far from the frontier, in most cases, of scientific leadership; and that she lost the leadership and productivity growth in the post-war years, at precisely the time that she came to a position of undisputed scientific leadership. One might add that America pre-World War II looks, in some rather striking respects, like Japan post-World War II. The similarity is precisely the lack of correspondence in both cases between scientific leadership and leadership in productivity growth.

I'm not going to unravel all of this, but I think I can make a couple of useful suggestions. Deeper insight can be gained by even a crude sectoral breakdown of the economy. Although the rate of growth of GNP per capita has indeed slowed down, not all sectors have been performing equally poorly. Indeed, our earlier investments in agriculture have paid off so handsomely that only about 3 percent of the labor force is now in that sector, and yet it still manages to produce far more food than the American public is prepared to consume. In 1940, federal R&D for agriculture substantially exceeded federal R&D for all sectors of our military establishment. That is worlds away in time.

Manufacturing productivity has also been growing at a very significant rate. There does not seem to be a complete awareness of this. That is precisely the issue at hand when we express concern over downsizing in the manufacturing sector. Downsizing is productivity growth – it is simply the flip side of the coin. The slowdown in the overall rate of growth seems to owe a great deal to the fact that the American economy has been transformed in the post-war years into a service economy.

Currently more than 40 percent of the American labor force is in services, and we may be understating that growth. Although it is certainly true that there are huge difficulties in measuring the productivity of service workers – how do you measure the productivity of doctors, college professors, policemen? – I think there is a deeper problem.

There appear to be enormous difficulties in turning our technological sophistication toward raising productivity in the service sectors. An important part of the problem is that it seems to be inherently difficult to raise productivity in the service sectors without at the same time bringing about unacceptable reductions in quality. Doctors can see far more patients per day – in other countries, they do. Elementary school teachers can teach much larger classes. But most people would not regard these measures as productivity-increasing.

The quality issue raises another subtle but crucial point. Along with our growing technological sophistication, there has been a collective increase in standards and expectations. Much of this increase takes the form of a higher trade-off, for example, between risk and safety – that is, a willingness to incur cost increases in order to reduce certain risks. This seems to be the common denominator underlying an expanding swath of government regulations, including the National Environmental Policy Act, the Occupational Safety and Health Act, food and drug regulations of all kinds, the Toxic Substances Control Act, increasing safety controls over nuclear power, and so on.

The growth in expectations emerged with particular force in health-reform discussions. Achieving agreement on some basic package that would be available to all proved to be impossible because such packages necessarily involved excluding significant segments of the population from access to highly expensive technologies that are now part of the medical armamentarium.

Massive federal investments in medical research have yielded massive improvements in medical technology. But unlike investments in agricultural research earlier in the century, they have proven to be cost-increasing rather than cost-reducing. It would be easy to reduce medical costs if we were satisfied to take what is sometimes called the Sears Roebuck catalog approach.

Suppose we go back to 1960. If everyone today would be satisfied to receive only the services that were available in 1960, we could achieve a considerable reduction in medical-care costs. But I suspect that there are few people who would want to go back to a period where there was no kidney dialysis, no bypass surgery, no angioplasty, no hip replacements, no laparoscopic surgery.

I trust that it is clear that I am not advocating a sweeping away of CAT scanners and magnetic-resonance imaging devices. I'm not advocating 1960, I'm simply observing that a rapid advance in the endless frontier of which Bush spoke 50 years ago has brought with it an escalation of standards and expectations that he probably did not anticipate.

NELSON: We have about 15 minutes for questions and discussion.

HILL: I'm Chris Hill with George Mason University. First, a quick comment on Nate's observation about downsizing. Downsizing is productivity growth without output growth. And one of the problems that has plagued the manufacturing sector is a very slow rate of output growth relative to manufactured goods consumption in the country, such that we're now 42% plus dependent on imported manufactured goods in the economy, running $150 billion, roughly, trade imbalance negative in that area. That's not necessarily bad, but downsizing is more than productivity growth. It need not be associated.

Back to the notion of the institutionalization of the ideas in the Bush report, we talked a lot today about the fact that the National Research Foundation did not come to be, but the National Institutes of Health was strengthened, ONR, ARPA, et cetera, on the military side were established. The lacunae that seems to have emerged that troubled no one at the time, was the failure to do anything institutionally with respect to Bush's third goal, which was prosperity of the nation. Nothing was done to institutionalize the concern for the economy.

Now, I don't know whether that was because the economy was so strong relative to everyone else at the time, that there was no need, or whether it was because Bush and his compatriots were so oriented to the market that they couldn't imagine the need even if there was one, or what other forces were active. And I wonder if the panelists could comment on why it didn't happen in the post-war period, and in fact why it continues, to this very afternoon, to be a problem for us.

BRANSCOMB: If I may, I would like to comment on part of the question but not on the historical question, which I'll refer to those who've studied the history more carefully than I.

To me, the essential feature is that there has not been a focus on what I call a diffusion-oriented way of looking at policy, and that's for several reasons. One of them is that so much of technical knowledge was tacit knowledge back in the ‘40s and ‘50s. So much of it is now explicit knowledge that it's much easier to organize the innovation process and manage it deliberately.

It's easier to simulate in model processes, it's easier to drive to do product development and manufacturing in a single simulated exercise. The integration of the elements of the innovation process is easier. And if in that environment, you separate science from the innovation process – now I'm repeating what Nate said – you're unlikely to get a very good effect.

So I can understand why it is now so very important that we understand those diffusion-oriented strategies. I think that's where our policy discussion has to go in the future, and how do we now preserve the values that Bush identified in that context.

Why he didn't do it at the time is less clear to me. I'll make the obvious statement, which is there was an overriding concern on the part of Jewitt and others, and maybe even Bush himself, that the government should be very, very cautious about the extent to which it touched the independent decision-making of the private sector. And it's pretty hard in that environment to begin to engage the coupling of government and private activities through the diffusion of research. It would not have been hard to invent policies to encourage industry to invest more.

And I close by noting that when I became director of the Bureau of Standards, the first thing I observed was that we had a program structure that made sense to people who did standards work but nobody else on earth. And I spent three years trying to build a new program structure that was output-oriented. In effect, I did to myself what Mikulski just did to NSF. And when I was all done, I was very proud of this and all my people had accepted it, and I was about to take it to the Congress.

Churchill Eisenhart, who is an amateur historian, came in to me and said, I’ve got something you ought to read. I said, what's that? And it was an annual report from Herbert Hoover, who was Secretary of Commerce back in the late teens, in which he had developed exactly the same program structure that I had re-invented many years later. And Herbert Hoover in that report talked at great length about how he jawboned private industry – he'd call up the chief executive officers and he would say, I read your annual report, you're doing a lousy job, and that may satisfy you, but it doesn't satisfy your customers, it doesn't satisfy your workers, it doesn't satisfy your investors, and it sure doesn't satisfy the United States Government.

Now, I don't know if that would do a lot of good today, but I would suggest to you there was Bush's admired mentor, if you like, taking the view that you had to create a demand function in industry in order address this problem.

HART: I just wanted to point out there is a hidden history here which hasn't been exposed very much in the literature. There was a technology policy after the war that, as I mentioned, Wallace ran. It was embodied in the Fulbright bill, which was rejected in 1946, and it would have vested these functions in the Department of Commerce.

But there was a lack of constituency. The left didn't really understand the relationship between technology and economic growth. The right was repelled by fears of socialism and traditional ideological problems. Small business, which might have been attracted, also fell into something of that category, although there was an effort to try to build a sort of small business and labor coalition behind this at one point at the end of the war. But I think the post-war boom took the steam entirely out of it.

WAGNER: I'm Caroline Wagner from Rand Critical Technologies Institute, and I'd like to address my question to Don Hornig and David Hart. In kind of a switch, I think, in policy in President Johnson's address to his cabinet in September of '65, he expressed frustration that 20 years of science funding hadn't yielded the results that had been promised and that his administration would focus on the practical applications for science. And also, in a geographical distribution for funding.

It appears that much of science policy has kind of swung between what Harvey Brooks identified as, on one hand, the autonomy for science and, on the other, an interest in squeezing the inefficiencies out of the system. I wondered if, Don and David, you could comment on which of these ends of the spectrum seem to provide the best environment for successful science. And further, to comment on where other administrations have fallen along this spectrum.

HART: I think one can only observe that this is a perennial and unresolved question. In President Johnson's Administration, he made a speech at the National Institutes of Health, in which he essentially said, we've spent a lot of money here and what have you done for us lately? He retreated after subsequent discussion, but that didn't remove the issue. But it did not end up producing any cuts, for instance, in basic research expenditures. The other half of the question can go to Don.

HORNIG: Well, perhaps Lew can speak to that better than I can. My impression is, though, that my statement that it's an ongoing tension simply stays true in that there hasn't been any clear shift in policy. One other point you mentioned – this has to do with the geographical distribution of science. One of the initiatives that Lee Hayworth and I undertook particularly, and received a lot of flak from the scientific community, was the notion of abandoning the pure merit system for distributing science and looking at the whole educational system.

The basic idea was to find the next tier to the top, look at those places that showed an opportunity to become tier one, and invest some money there. This program actually was carried along. But that represents another tension – it's just unimaginable, you see, that we can increase the number of students at Harvard forever, as a national policy. Nevertheless, the notion that you consciously steer some money – well, it's like steering the magnet lab from M.I.T. to Florida, recently – that this might even represent rationality is very painful to many people.

BRANSCOMB: I would just like to observe that the process whereby an entire nation comes to think differently about how innovation works and to adopt a new point of view has got to be a slow, complicated process. It's an arcane subject, not everybody cares about it.

And I would give you two pieces of data. One is, in 1972, when the SST failed, and Bill Magruder, who had headed the program in transportation, moved over to work with Ehrlichman, they invented something called the "New Technology Opportunities Program." And this was going to be a big initiative in the second-term Nixon budget, in which they were going to invest tens or hundreds of millions of dollars in aggressive megatechnologies for commercial industry. I remember 10-megawatt, superconducting motor generator sets, and that sort of thing.

And I vividly remember that at the very first meeting of this interdepartmental committee – I represented Commerce and David chaired it when he was Science Advisor – the head of ARPA said we could save a lot of time if we would agree on some ground rules. And the first ground rule should be, we should all recognize that nothing the government does ever has any technological effect unless you've spent at least a billion dollars, so we won't discuss here any projects that cost less than a billion dollars.

Well, needless to say, Commerce, Interior, and a few other agencies didn't buy that. That program died, and it led in the final days of the administration, thanks to George Shultz, to doing something I had recommended, which was the ETIP and ERIS Program, the Experimental Technology Incentives Program, specifically aimed at not having the government invest in the technology, but trying to find a way to provide more incentives for private innovation.

My second observation is that Carter had a big effort run by Jordan Baruch, explicitly an incentive environment diffusion program. It didn't have in it a significant component of increased government R&D spending. And it might have made sense. The problem was, it went to the Congress a few months before the election, which the president lost. And that was the end of it.

NELSON: In our second conference, we're going to be getting in more detail on the history of various areas of government policy, including civilian technology policy. Yours will have to be the last question from the floor, this session.

SESSIONS: Vivian Sessions, City University of New York. The year 1995 will also be the 50th anniversary of another important Vannevar Bush publication, his article, "As We May Think" in *The Atlantic Monthly*, which is broadly regarded as the genesis of modern computerized information systems. And I wondered if somebody would want to comment on his influence after it, or maybe you're going to take it up in some future conference.

HART: Interestingly, he's better remembered for this, partly because of the rise of the personal computer. He's embraced, improbably, by the nerdy programmer as the godfather of personal computing and also this field called hypertext. He's credited, probably overly generously, with conceiving of the idea; he called them "associative links and trails."

And Bush imagined in this article – which was a reprise of some writing he had actually done in the 1930s, an article that appeared in the Technology Review – that you could almost download your entire brain, or all your associative trails, they could be etched onto microfilm, and this microfilm would then be a permanent record of all your thoughts. This is not very far from the more extreme visions of the merger between the biological organism and the computer chip that people like Hans Moore talk about. And what drew Bush to this whole topic was actually something that was referenced early this morning about the inability to mine scientific research.

Bush was perhaps the first important public figure to talk about what we would call information overload. And he felt that this held the key to harvesting the future benefits of science. The Committee for Scientific Aids to Learning, which was an important precursor of the NDRC in many ways, was formed because people like Bush felt that there might be technological solutions to the problem that was facing researchers. There was no point in spending more money on research and creating more research, if all that happened was that you drowned in the results. So in many ways, this was integrally connected to his other concerns.

NELSON: We have a second panel that will convene next. Lot of interesting topics and wonderful people on it. I'd like to thank this panel for a fascinating discussion.

Roundtable Discussion (II)

Dr. Donald Fredrickson
Professor Evelyn Fox Keller
Dr. Orville Bentley
Professor Susan Cozzens
Professor Eugene Skolnikoff

Moderator
Vice Provost Michael M. Crow

CROW: We have an eclectic group for this panel. We're trying to touch a number of bases. They are connected, to varying degrees, and so we're going to look at health sciences, defense sciences, agricultural sciences, and social sciences in the post-World War II era. And we're also going to look at the effects of Bush-based science policy on a particular group.

To start, we have Don Fredrickson on the far end of the table, who is trained as a physician, has worked on Capitol Hill in his career, has been President of the Institute of Medicine, Director of the NIH, world traveler – including today [laughter], which is why he's going first. Someone is awaiting him in Europe. And he's going to talk on the impact of Bush policies within the health-sciences sector itself. So, Don?

FREDRICKSON: I have a broad mandate, so I'll do it in compressed bites.

The year 1944 was a very pregnant year, as many of you know. It was the year that Avery and his colleagues reported the transferring principle in the small laboratory they had in the hospital of the Rockefeller Institute – itself a remarkable institution from 1910 onward in America.

It in itself was the dawn of molecular biology, and the twilight of philanthropically supported research because, by that time, the benefactions, which were large, to the medical community for many years from the various foundations were draining off in the Depression.

When the Bowman committee met, they said what they thought about the state of American science. And they said that not one of the scientific disciplines could compete with the European universities, except the medical schools – the fact that they did well when it came to getting private support, which was so essential in that time.

The next era was about to end in 1944 because the Committee on Medical Research, which had funded $20 million's worth of contracts to research for the OSRD, was going out of business. Now, when he wound down the OSRD in 1945, Bush was very much determined to keep medical science within his proposed new foundation. The medical-research division was to have about $5 million out of a proposed budget of some $30 million, and it takes really nothing from Bush's tremendous contributions to conclude – even if it means I'll be stoned – that in the case of medical science, he'd been badly mistaken.

I think Bush's failure to sequester medical research was to prove over the next 50 years to be a blessing, not only for biomedical research but for the nation's universities and non-profit research institutions. The Public Health Service, which was the successor of the Committee on Medical Research, had not been Bush's idea of a proper steward for the research that was to come.

You can see this in reading *Science: The Endless Frontier*, where there's a great deal of text about his idea that any existent federal agency could be trusted to preserve the scientific freedoms that he and his advisors considered absolutely threatened by federal involvement in science. This new mechanism was also opposed by his own Medical Advisory Committee, which had tried to get their own separate organization, and by, really, the great bulk of the many academicians who tried to support the Magnuson version of the proposed new charter on the NSF.

I just wanted to give you a chart of the testimony of the head of Cold Spring Harbor Laboratory, a distinguished molecular geneticist at that time, who said the record showed clearly that placing fundamental research under the control of agencies that anticipate practical applications seriously limits and restrains the freedom of thought essential for basic advances.

Well, how did Public Health Service finally pull off this caper? In 1930, Congress converted an old hygienic laboratory of the Public Health Service to the National Institute of Health. Now, parental hopes were really grandiose, but the rechristening was most inauspicious for the daughter of the great Depression – NIH had no gallery at all. A few leaders of the Public Health Service, notably chief scientist Jimmy Thompson and Surgeon General Perrin, however, were determined to increase the medical research responsibilities and capabilities of the NIH.

In 1937, their efforts resulted in the acquisition of the National Cancer Institute, which went with astonishing unanimity and fervor through Congress when it was introduced. I think that some of the volcano heading up for the future of biomedical research was quite visible then, but it was not heeded by Vannevar Bush, who was an engineer, nor even his more conservative Medical Advisory Committee.

Well, the new Institute of Cancer was given the authority to give grants – a very small program, which ended during the war because it had no money. But the NIH learned a lot at that period about how to have peer review, how to do it by dual review with their committees, and they also learned those delicate and important differences between contracts, grants and aid, and grants. But an even more important and timely dividend for the Cancer Institute authorities came in that same year, the summer of 1944, when Perrin and Thompson were assisting assiduously Congressman Alfred Bullwinkle of the House Commerce Committee to reorder the chaotic public-health statutes.

A bill emerged that extended the features of the Cancer Act, including its granting authority to all of NIH. And embedded in Section 301 of that very important Public Law 78410, which you have to memorize as director of NIH [laughter], were the most sweeping authorities for support in conducting research ever extended to any government agency.

Now the next year, NSF legislation was beginning to stall for the reasons we've heard today. The CMR was faced with expiration of all of its short-term research contracts, and at the last moment, NIH director Eugene Dyer was given a kind of cryptic message from Bush, a fraction of the unexpired grants.

The next year, 30 sections were already put in place. And within a year, an estimated half of the medical research in the United States was being funded through NIH. It's very interesting that the NIH decided to go right to project-research grants and fellowships as the major source of funding. And it continues today to be still the primary mechanism.

And it's interesting because the Bowman committee and the Medical Advisory Committee that served Bush both recommended large-scale, block grants to institute universities, which once in, would be kind of perpetual in endurance as long as the university had a committee to allocate the funds locally and kept the books for the auditor.

Now, why did the NIH do this? Well, I think they did it because Vannevar Bush had a penetrating effect on them, and so did the arguments that were loudly voiced in opposition to their being a potential steward. They were determined not to fail as a steward to this new program, as their detractors had all predicted in '45, and they chose proven methods, which certainly were known to be acceptable to Bush. They imitated the old OSRD model: study sections, peer review, an investigator-initiated project as the mechanism.

Now by 1950, the NSF opened with an authorization of $5 million. The NIH appropriation in the same year was $50 million, and Congress had already added four more categorical institutes. Nearing completion on the NIH campus was a clinical center to be the largest hospital in the world dedicated solely to research. And the initial funding for this had been obtained by NIH just a year after the money for the first grants.

You know, it's interesting that there was a backlash against what the NIH did in '50. In 1955, the secretary of HEW decided to put up a committee, for reasons that are not clear, to see if NIH was a proper steward for the money that it had. A committee was guided by the NSF, which had the authority to do so, and it had as its head the Dean of Yale Medical School, C. N. H. Long. The committee didn't take very long to come back with the report that NIH should be severed, the intramural program could go off by itself, and the extramural funds should all be put into a new organization and the bulk of the money distributed to the universities in bulk grant.

Well, this report had an undesirable effect from the standpoint of its opponents because the next HEW chairman, Marion Fulsom, put up his own committee. It was headed by another dean of Yale Medical School, Bing Jones, and they diametrically opposed the first study – recommending that the NIH continue with a doubling for the next 10 years of its budget, that it assist the medical schools, and that it address the education and structure needed for better and more research.

Now this event took place just before the long reign of Senator Lister Hill and Congressman John Foray, who were the most remarkable troika, counting James Shannon, the director, that ever existed in NIH funding. The congressmen would bring citizen activists in to hustle up these hearings and to make the demands upon the Congress, with the real result of the doubling every three or four years of the President's budget between the period 1955 and '65.

Note that Bush insisted that the NSF not have an intramural program. I think it was such a multi-categorical group that it would be impossible to do that. The NIH, however, had an intramural program, and it turns out to be, in our view, extraordinarily important for maintaining the institution, the agency of the central core of the world's biomedical research for a long time.

With it setting generic rules, the ethics of clinical investigation, shepherding public acceptance of recombinant DNA research, for a great many reasons including the Selective Service Act – the intramural program and the clinics there actually trained a very large fraction of the next two generations of teachers in medical schools here and abroad. The clinic brought to a final point the conversion of clinical investigation to a really full-time profession from a previous avocation.

You know it's also important, in thinking about the various acts that occurred at that time, that in '54, Bill Carey of the AAAS wrote an executive order for President Eisenhower that ended Vannevar Bush's ideal of the single agency for the funding of fundamental science. It had two aspects. One was the relief of the NSF and its science board from defense research, but it also was an order that the mission agencies, of which NIH most certainly was one, could conduct basic research.

And after that act, NIH more or less remained outside, as did some of the other agencies, any attempt at coordination or oversight by the NSB or the presidential science advisory apparatus. Perhaps this will change for the President's new NSTC, which has for the first time included the NIH director.

I think that this may be the end of another era, because we have talked now a brief bit about the history that occurred during an almost entirely long period of Democratic Congress. I hope the prediction from other members of the panel that NIH should still prosper, or health research should prosper, is true. We'll have to wait and see.

CROW: Thank you. Why don't we go with one or two questions for Don?

ROBINSON: David Robinson with the Carnegie Commission. Do you have any advice for the physics and chemistry communities of how they can follow in the footsteps of the National Institutes of Health, other than hiring James Shannon to be the head of NSF?

FREDRICKSON: Well, I think it may have to be partly just serendipity, because certainly Mary Lasker wasn't recruited by the NIH in 1944 when she got interested in health research, particularly. But she and her followers really had a most remarkable effect on–

UNIDENTIFIED: Perhaps they might find some housewives who are prepared to testify about how concerned they are about the top quark.

FOX KELLER: That's what I was going to ask, what about the citizen activists for physics and chemistry.

FREDRICKSON: Actually, the NIH, by the way, came about because of a chemist who got after Senator Ransfield of Louisiana and kept after him for four years until his act was finally passed. I've forgotten the name of the chemist, but Ransfield went home to Louisiana and was defeated by Huey Long and was not seen again for a couple of years until he came back to recruit with a tin cup for the penniless NIH.

GELOBTER: Michael Gelobter, Columbia University. I think NIH is an interesting case of the tension between the hard or physical and natural sciences and other types of research. There are extreme estimates of how much research and basic sciences have contributed to improvements in health, but it's pretty clear that public health practices and social practices have probably contributed the most to public health improvement over the last hundred or two hundred years.

I think there's a flaw, perhaps, in the model of continued funding for health research, if for some reason the public should become aware of the fact that primary preventive care, for example, would do much more to lower health care costs and improve longevity, etc., in the long run.

How do you see that having been played out historically and into the future in terms of the tension between public health-type research and more social science-type research, into improving basic health and primary care vs. biological models? And the most extreme example recently, perhaps, being the Violence Initiative, in which a great deal of funding is being targeted towards genetic sources and chemical sources of violence, which may or may not be significant.

FREDRICKSON: Let me say that the NIH had an effect on the Public Health Service that perhaps in the historical sense will not have proved to have been very good. The bench scientists took over Bethesda, and the rest went to Atlanta, the few people who had other missions and other ideas about public health research. And I'm sorry for that separation that existed for the next 20 or 30 years.

It's clear that public pressure, pressure within the community itself and not unrelated to the research community, will have to bring back attention to these pressing problems of the value of interventions, of prevention. But NIH, through the years, had such a concentration on science, such a demand on the quality and the way it did it, that it is not the primary organization to reform the health system. I'm sure of it.

LEDERBERG: Joshua Lederberg, the Rockefeller Institute. The remark about project grant vs. the institutional block grant plus possibly some other alternatives – I think there was great wisdom in the focus on project grants, for the following reason: As soon as you have large aggregates of funding involved, as would be the case if the chunks are to institutions, you can't but get into a lot of political jockeying.

You have interest groups trying to weigh in with the Congress; we've seen this happen over and over again with construction grants. Keeping the individual quantum of distribution at the individual projects was the way of at least minimizing the intrusion of secondary political considerations.

One of the problems of the project grants is that there's such a hypertrophy of demand for specification about just what the discovery is going to be, the anticipation before the event, that's strayed quite a bit from Jim Shannon's original perspective, which is to find the best people and give them, one by one, the freedom to do the best that they can. But I just wanted to delineate what some of the policy principles might be about the structuring of that funding. I would be interested in your comment on it.

FREDRICKSON: I'm certain you're right. Today, the NIH, just as then, puts out about 50% of its resources in research project grants, investigator-initiated. And that's despite the fact that there's been an enormous amount of micro-management of the internal programming of the NIH, so it's still possible to do it, but it's a hard fight.

SIMON: Yes, I'm Bob Simon with the staff on the Senate Energy Committee. We've talked a lot about project grants, institutional block grants. In the biomedical area, there are also track record grants, for lack of a better term, where the funding is given to a person based on an appreciation of what that person's track record has been, not so closely tied perhaps to a specific proposal for the next one or two or three years.

Would you like to comment on that? – because I think there's been more experimentation with that in the biomedical area, than perhaps in the physical sciences area. And a subdivision of that question is: What do you see as being the likely long-term impact of the entry of the Howard Hughes Medical Institute into the funding picture in biomedical research in this country?

FREDRICKSON: Well, one can never predict for long terms, but I think that the project grant system will still remain very strong for many years, unless there's a drastic change in the funding pattern. NIH has experimented back and forth with program project grants and grants of different sizes, centers for the various institutes, so there have been various experiments in different mechanisms.

But back when Shannon left and the funding of the NIH dwindled again, and the president's budget wasn't augmented, we had to go back to the question of counting grants, and we did so, and got a guarantee from the House that they would fund at least 5,000 new grants. I thought that would only last one year, but that was actually written into statute and endured for nearly eight to 10 more years, until the grants got too big and they lasted too long. The tendency was to increase the term and the amount involved. So it's a constant campaign to keep ahead, one step ahead of the system and of the political pressures upon it.

CROW: Our next panelist is Professor Evelyn Fox Keller from the Science, Technology and Society Program at M.I.T. She was trained as a physicist and has since moved on to the philosophy of science and the history of science, and in the last 10 years has authored five significant books in the issues related to science and society and society and gender.

And we have asked Evelyn to think about something that we're thinking about in the long term of our three conferences that we'll be working on: If Vannevar Bush's *Science: The Endless Frontier* was a manifesto, as people have suggested here today, it set about a series of designed conditions on the way science resources are allocated, the way that science is organized, the way that scientists behave. All that had effects, in different ways, on various segments of our society. And Evelyn is going today to talk about some of those effects as they relate to women.

FOX KELLER: I am going to take this opportunity to use aspects of the history of women in science to illustrate some more general points.

There's been a great deal of discussion about how productive the vision of *Science: the Endless Frontier* has been for scientific research in the 50 years since Vannevar Bush, especially in the physical sciences, but in medical sciences as well. But very little, if anything, has been said about the productivity of that vision for other parts of the university: the social sciences, the humanities. In fact, in the post-war period it is not only the natural sciences, but also the university as a whole, that grew tremendously in size and importance.

The balance of natural and social sciences shifted and the balance of the sciences and humanities shifted, but all three parts of the university just mushroomed in the post-war periods. And all three parts benefited, however indirectly, from Bush's manifesto and the sharp distinction he drew between basic and applied science. That "manifesto" – that is precisely what it was, given its political content - was aimed at securing the autonomy of the scientific enterprise; but, in fact, it helped to secure the autonomy of the university as a whole.

As we now know, the dichotomy between pure and applied science was, in part, a fiction. It was a useful fiction, perhaps even a necessary one, certainly for the conduct of natural scientific research. Especially after the Manhattan Project, as we saw from the editorials, the temptation to continue along the model established by the Manhattan Project was strong. So it was an extremely important, useful, and necessary fiction.

Bush's manifesto – or the indirect benefits of Bush's manifesto – bore out Descartes's prediction centuries before about the widespread benefits of the support for the basic sciences. We can learn about the fringe benefits for other parts of the university, or the university as a whole. They are important to understand, and important to understand in relation to the ideology of the time.

Not everyone benefited from this agenda. One group that did not, and is a notable exception, is women in science. At least, they did not benefit in the short run. In the 15 years after *Science: the Endless Frontier* was published, the participation of women in science declined drastically, so much so that we tend to forget about the presence of women scientists in the years just before – for example, Ruth Howes has identified 100 women scientists who were involved in the Manhattan Project. But in the 15 years after the Bush report, many factors contributed to this decline. Certainly women were not pushed out of science by any overt or explicit program, nor was it a result of explicit sexism. Rather, it was a combination of many factors. One factor was the widespread belief that women just didn't have it, and since our aim was to produce the best possible science, we should involve only the people who did.

The degree of disenfranchisement of women scientists can be seen most poignantly in a report of the Hamilton Committee, established in 1956 at M.I.T. to consider whether after 100 years, M.I.T. should admit women. The unanimous verdict of that report was no, it should not, that it was not in the interests of women and it not in the interests of M.I.T. It was, as they put it, a misuse of resources.

Now, as we know, that situation has changed, and changed very dramatically. The turnaround began in the early 1960s, in part in response to political pressures from above, in particular as a response to Sputnik, and the concern about the growing shortage of scientific manpower. So the government, and the National Science Foundation (NSF) in particular, made explicit attempts to recruit women into the natural sciences. The turnaround was a result of pressures from below, more diffuse social pressures, and the rise of the women's movement.

The success story of the increase of women in science in the 1960s, 1970s into the mid-1980s was very dramatic. While the proportion of women doctorates had declined by the late 1950s to roughly half of what it had been in the 1920s, it quadrupled in the decades afterwards. This is a success story. It is a story that illustrates a very productive interaction between the institution of science, the natural sciences, and the political processes. As I said, the pressures were both from above and below.

It also illustrates the problematic nature of the notion of the autonomy of science. The autonomy of science was an extremely useful, and necessary, ideal in the years after World War II. It was necessary to emphasize that ideal, but as an ideal, it was ill-thought out, and it failed to do justice to the equally important ideal of responsibility of science to the society in which it exists.

In some of my work, I have developed the notion of a dynamic autonomy as an appropriate ideal for individual development. Perhaps some notion of dynamic autonomy is also more appropriate to thinking about the contract of science with society.

The question of when, where, and to what extent science should be responsive to political pressures, and to what political pressures, is a fundamentally and irreducibly political question. It cannot be answered on the basis of any scientific, statistical study. Even the story I tell you about women in science is arguable. There are people who would say the increase of women in science has not been good for women or the social fabric because it undermines the family.

I believe it has been extremely productive both for women and for science. Most obviously, it has brought into the practice of science a whole new reservoir of talent. Some people say it has enabled the introduction of different perspectives. I am skeptical about this argument, but I do believe that the increasing number of women in science has contributed very critically to undermining what had been a very costly ideology for the conduct of science, the ideological equation between thinking scientifically and thinking like a man.

Not everyone will agree with me about the productivity of that responsiveness to political pressure. My point here is that if neither the Manhattan Project nor some ideally autonomous insulated institution is an appropriate model for science, just where we are to come down between those two extremes is not something that we can answer independent of context, independent of time.

After World War II, the fiction of an autonomous science was necessary because the dangers to the conduct and autonomy of science were so great. In the late 1940s, Bush sought to secure for the conduct of science freedom from convention, prejudice, and commercial incentive. He saw the university as the best location for the conduct of research, the location that would best guarantee these freedoms. He also helped to guarantee that the university was the proper locus for these freedoms.

By the 1970s and 1980s, the situation of science in the universities had become very different. As an institution, it had become vastly stronger. Some people would say it had become too insulated. Certain limitations became evident that were consequences of too great an insulation of the scientific community. The scientific community had become exclusive. There was no conscious prejudice in keeping women out of science, but certainly there was a great deal of unconscious prejudice that has since been exposed by historians.

Today there are other issues. One question that has arisen is "Where are the scientists of the next generation going to come from?" There is obvious exclusivity still operative in the scientific community. And we need to think carefully about how these exclusivities come about. There are appeals for science to take more responsibility for thinking about the underclasses of the world, and I agree. All those extensions of the mindset of the scientific world need to be practiced.

At the same time, however, there is a way in which I would have to agree with many who would say we have gone too far. In the 1990s, there is a new danger to the autonomy, not only of science, but of the university as a whole, and this danger, I suggest, requires us to rethink and regroup.

The danger I refer to comes not from the left, as some people have argued, but from the right. It is promulgated by a different set of fictions, not the fiction about the autonomy of science or the sharp separation between basic and applied research, but by fictions that are as useful to those who would wish to undermine the autonomy of the university today as Bush's fiction was to him in underwriting that autonomy.

One of these fictions is that science has become a hotbed of fraud and deceit, and therefore requires surveillance by independent committees, perhaps Congressional committees, to oversee the proper conduct of science, and perhaps even direct management from the public sphere. This concern with the inability of the scientific community to manage itself coincides with the decline in funding for basic research, and surely it serves as fodder for the arguments in favor of more direct management of science.

On the other side of the campus, in the humanities and social sciences, another fiction has been promulgated – that is that the university has been taken over by a band of subversives, feminists, multiculturalists, post-modernists, and that the humanities and social sciences need more vigilant control. Many people expect, in parallel with the decline in funding for basic research, that the chances for survival of the National Endowment for the Arts and the National Endowment for the Humanities in the next few years are quite minimal.

Some people have even blamed this band of subversives for the decline of funding for the basic research. But given how difficult the literature of high theory in the humanities and social sciences is, not to mention the literature of the natural sciences, I can't imagine that Congress has been so influenced. Still, both of these are useful fictions. Not that they aren't based in fact, there were frauds and misconducts that were available to identify and inflate public anxiety, just as there are scholars in the university writing irresponsible about the sciences, coming from the humanities, who are not adequately informed. The use of examples of abuse to discredit the whole enterprise – on the one hand, the natural sciences; on the other, scientific study or even the university as a whole – is the problem I want to call your attention to.

As for the solution for these abuses: I believe those in the natural sciences can manage their own affairs. They are as concerned as anyone with the practice of fraud, deceit, and misconduct. And I believe the university as a whole can manage its own affairs, that the time-honored practice of dialogue and mutual respect and good old-fashioned academic freedom is exactly what we need.

If people are writing irresponsibly about the sciences, it is the responsibility of the scientists to educate them. My fear today is not the undue power of the natural sciences that worried many people in the 1970s and 1980s, including myself. Indeed, today the power of the sciences does not seem to be so great. Far more, I fear the power of a political structure to roll back all of the positive gains bequeathed to us from the Bush legacy, not only for high-energy physics but for the university as a whole.

CROW: Are there a few comments or questions before the last three panelists?

L. HORNIG: I'm Lilli Hornig, Wellesley College. I'd like to elaborate a little bit on some of the early part of the history that Professor Keller just described, and that has to do with the immediate post-war era and the, let's say, relative disenfranchising of women in higher education and in the sciences, both, during that period.

Earlier today, Harvey Brooks alluded to the effects of the G.I. Bill, and it's not often recognized that bill had an enormous impact on the education of women, in the sense that it made unavailable many of the places to which they had previously been admitted. The major state universities, for example, the University of Michigan at Ann Arbor, literally arbitrarily reduced their admissions of women by 30% in order to be able to accommodate the entering GIs. And the after-effects of this policy, or by-products, surely unintended, were to create a climate in higher education that envisioned it as a predominantly male enterprise. This had not been the case before the war, and I think that's not widely appreciated.

In 1940, women were approximately 42% of graduating classes. This dropped down to well below 30% within the next five years. And the effect was not due to declining enrollment of women; they were simply pushed into other institutions, predominantly what had been the normal schools, which were now rechristened state colleges. But they did not in the course of that rechristening add large laboratory facilities or a great deal of science faculty, nor did they have engineering schools. In other words, the institutions to which women were more or less freely admitted did not present possibilities for pursuing science in any way we now regard as meaningful.

And I think this is an excellent illustration in the context of the policy discussions during this meeting, of the unintended effects of what are otherwise wonderful policies. We need to, as Professor Keller emphasized, really try to include all of the possibly affected groups when we devise some of these policies. [applause]

FOX KELLER: Thank you for the comment.

ETZKOWITZ: Henry Etzkowitz, State University of New York at Purchase. My question is inspired by my former colleague at Purchase, Evelyn.

In the 1930s, Carl Compton, as President of M.I.T., went to Washington and tried to persuade the federal government to "put science to work." To help end the Depression by putting science to generate new economic growth for the country. At that time, the scientific community rejected Compton's initiative, fearing that if the federal government invested money in science, put money into the universities, that this would inevitably lead to influence on the research agenda, to control over the direction of science.

Now, after the years of the fiction of autonomy being stripped away, does this finally come to roost? And must we now accept the consequences that if large funds are to be distributed, that eventually the holders of the purse will want a say in how those funds are distributed? I recall just a few years ago when there was a debate in Congress over whether funds should be earmarked, whether universities could apply for special centers, as Columbia did for chemistry, in fact, and successfully won one.

The argument was that, no, they should go through the peer review process. And a congressman said, what is this, what is the peer review process? What do you mean? You mean I can't have a say over public monies that are spent? That's unheard of, money to be outside of the regular political process. And so now that the fictions are stripped away, won't we have to make our justifications for funding for science on the basis of new ground of legitimacy? For the past several decades, it was implicitly on the basis of support for the military. Now that's lessened.

The issues that are now before the nation have to do with economic competitiveness, with social problems that we're only beginning to recognize officially. This may mean perhaps involvement with social scientists, perhaps not. The debate may say that government should not be involved in any of these questions.

I would suggest that we're about to enter upon a debate over the future of sciences, their role in society, and we should be willing to enter into it and engage in lobbying and other political activities that are incumbent upon any group in society that wishes access to the public purse.

FOX KELLER: I don't think there's any doubt that we have a very difficult task before us, that we cannot return to the fictions of the '40s. Not all bargains are equally Faustian, but I do agree that the particular illusions that helped to build the strength of the research enterprise can no longer be maintained. That doesn't mean that we immediately forget any notion of the autonomy or the independence of science. Science does require insulation from direct political process, there's just no question about it.

The notion that it can be completely insulated from social and political pressures is a joke. But that doesn't mean that we don't have to think of ways that guarantee some capacity of the university as a whole and the scientific professions themselves to govern themselves.

CROW: One last comment or question.

CLAUSI: Yes, Al Clausi, the Institute of Food Technologists. I would like to compliment Professor Keller for using the expression "to dispel the fiction of the independence, if you will, of basic science." I think it has been a fiction and I think if we draw a new manifesto, if you will, that's based on that fiction, in the new society, we're not going to succeed.

We should draw a line more on the basis of maintaining the basic-science capability, which is important – it's just as important today as it was back in the days when the original manifesto was laid out by Dr. Bush – the creativity that's connected with that, but build it into a better structure, if you will, a more responsive structure that can deal with the 21st Century and our country in the 21st Century and our science in the 21st Century.

We are leaders in many areas and we want to remain leaders in those areas, but we're not going to do it, in my judgement, by perpetuating a fiction. We're going to do it by talking about what the need is and how we can best accomplish that need. Thank you very much.

FOX KELLER: I second that.

CROW: We'll take the next three panelists as a group, because they're each going to be looking at a sector of science and technology or science itself in the post-Bush era, and then we can deal with questions and comments from the floor.

Our first panelist in this section is Orville Bentley, former Assistant Secretary for Science and Education under President Bush and President Reagan in the Agriculture Department, Dean Emeritus at the University of Illinois, and a lifelong agricultural researcher trained in biochemistry. We thought it would be very important to have a leading agricultural scientist and a leading agricultural science administrator as a part of this panel. So, Orville.

BENTLEY: Thank you. I haven't heard much speaking about little science and big science here. I represent sort of little science, as far as the federal budget is concerned, even though it is an event that started over a hundred and some years ago.

I want to just track a little bit of the history of the formation of the so-called agricultural research system. It's not a firm system, it's a cooperative, diversified, extensive system of people working together.

It started really with the grant at the time of the Civil War to start public universities teaching agriculture and the mechanical arts and those sciences that related thereto. The system of research, though, really had many origins. Certainly, it was greatly influenced by the science that came from Europe, and the need to have a kind of scientific development system that was more apropos to the needs of a rapidly expanding nation in the post-Civil War era.

Remember that there was a great western movement, there was a movement from people that had been in the Eastern part of the United States, who left because they said the land was farmed out. And they moved to new lands that were available in the Central United States and to the West. The political pressure that came on to create the experiment stations, like many things we talked about, took a long time.

A Yale University teacher in Latin who came to Central Illinois to teach Indians Latin in the 1830s said this is a hopeless task, what we need to do is help people learn how to farm. And he said, let's have something like medicine or something like engineering technology, and we'll apply it to the needs of what he called the working class – the farmers and their offspring. Well, the experiment station really in a sense is kind of a model. It's one that has been maligned over this 120 to 130 years. It's had successes and it's had failures.

By the Hatch Act, the Congress said it shall be the policy of the United States to promote research in agriculture and all related areas. It's only about four or five sentences long, and it's about as well-crafted a statement as you can find by Mr. Hatch, and he apparently wrote three or four other pieces of fine legislation. It created a system of agriculture experiment stations in every state to think about the diversity and the site specificity of agriculture, and it addressed the need to get some system that would be functional at the local level. Almost all of those experiment stations were attached to the then-developing colleges of agriculture and universities.

It made the magnificent sum available, on a matching basis, to each state, provided they adopted the proper legislative authorities, of $15,000. That amount of money didn't go up very fast, and it took a while to establish the idea that book-learning had anything to do with agriculture. At one time, it was said that anyone associated with this kind of activity was not allowed to come on farms, that they were dangerous and dogs would attack them if they did.

I want to go now to the concept that in the agricultural sciences – whether done in universities or in cooperation with the U.S. Department of Agriculture, which had been established also during the time of the Civil War – that teams of people working with the private sector could begin to solve important problems.

Someone graciously mentioned the hybrid corn, and I'm going to use that example a little bit myself. Hybrid corn was based upon the application of the Mendelian laws of genetics, and the inbreeding and the fixing of the corn was done in several locations. That created industries and made Pioneer Seed Company a tremendous organization. And it said to farmers and the country as a whole, that here we could take something of a biological nature, and it could be manipulated in a way that increased productivity.

Yields of corn or maize corn in many of the states doubled to tripled, even in the last 40 or 50 years. This year, we have a 10-billion-bushel corn crop – it's going to be hard to get it sold whether in export or at home. There was an equal headline in the late 1930s that the United States, for the first time in its history, had produced four billion bushels of corn. And the number of acres under cultivation is not a great deal different than it was at that time.

When we had, for example, wheat rust that had devastated the wheat crop in the mid-United States, on the Great Plains especially, we found out you could control that by understanding rust and how it was generated and how it was distributed. The cotton boll-weevil devastated the cotton industry, the grasshopper hoards were legendary in the West – all were examples of how basic and applied sciences could be used to solve problems that affected literally a major proportion of the agricultural area, in many geographic parts of the United States, and for that matter, around the world.

Well, those kinds of things began to greatly legitimize the idea that the federal government should, through these grants to the states, work with these kinds of problems, whether it's with soil, water, rural living, crop production, livestock health, and all of these types of things.

Now, it’s not just an agricultural question, it's a total science question, as far as the Congress is concerned. There've been argument about whether there should be more peer review grants or block grants, as would be the case of a grant to a state. The real success has been that states have appropriated a great deal more than the Congress has ever put into the agricultural research section and that the system was flexible enough to accommodate new developments.

Now, I don't know how we're going to go in the future, but it seems to me that there are some fundamental things that made the system work as well as it did – what made it possible to say today that the entire agricultural productivity of the United States is science-based or drawn upon technology, whether it's in mechanization, biological sciences, the application of molecular biology, physiology, biochemistry.

The multidisciplinary approach that has been used in agriculture is absolutely essential to looking at almost any problem, especially those dealing with water quality, air quality, the environmental impact of agriculture on soil erosion, on pollution, whatever it might be – they require a well-developed, multidisciplinary approach.

And the best way, it seems to me, that this can be done is in the university setting. The provision of both the competitive grants program that's well established and that's functioning very well through the Department of Agriculture or from the existing grants and those funds provided by the state will have to be structured to maintain the strong disciplinary orientation to solving problems.

It's also important to understand the political base. There was always an overarching question: Are we sure that we can maintain a stable, affordable, nutritious food supply or the supply of forest products for the future?

When we consider the population issue and the numbers that people are predicting for our world population and some of the problems of food production and its distribution – this is as highly relevant today as it was when they were talking about the creation of the Hatch Act or the experiment stations in the agricultural research in the 1880s.

The stability and continuity of the system and the ability to work with industry are all important – after all, much of the application of agricultural research and technology has been the initiative and the willingness of the private sector to attack these problems. This is the case, certainly, in the chemical industry, where huge organizations devote a great deal of their research, both basic and applied, in the development of chemicals that can be used to further agricultural production.

Then there's the question: How can the research system deal with the impact of the application of technology to the farm group? Mention was made that only 3% of the work force is involved in agricultural production, whereas a few decades ago that would be up to

50%. It's changed our rural communities. Sixteen percent of our population is in jobs that have to do with food and its preparation, delivery to consumers. We have to be more sensitive to consumer preference, dietary changes, changes in lifestyle, and so on.

Well, I think the system has the capacity if it is funded, even as modestly as it is today, to be flexible, to be able to identify and address some of the problems and to share the scientific and technical capacity to improve the well-being of our people.

Thank you.

CROW: We have asked Susan Cozzens, who is a Professor of Science and Technology Studies at RPI up in Rensselaer, New York, to take on a task that we've bounced around a little bit today: What did the Bush manifesto mean for the social sciences and what has been the impact in the post-Bush era for the social sciences? And so, Susan, that is a heavy load, but I know you are ready to take that on in the ten minutes that we have given you.

COZZENS: My task is to provide some historical perspective on Vannevar Bush and the social sciences. Bush was actually quite hostile to the social sciences in many ways. That was a form of jealousy, because the social sciences were so well established at the time that Senator Harley Kilgore's legislation to establish the National Science Foundation (NSF) began to be formulated.

The social sciences were, in fact, highly influential in government in the 1930's, and they had gotten to that point by quite a different route than the other sciences. The route the social sciences had used was their connection to the Progressive era and the vision of Americans using knowledge to work together to create a better life for themselves. There are a number of examples of this in the Progressive era. I will present two.

Henry Wallace, Secretary of Agriculture under President Franklin Roosevelt, was convinced that the social sciences and the other sciences should share equal roles in the New Deal agriculture programs. He was a bit suspicious of other scientists, afraid that they were "turning loose upon the world new productive power without regard to the social implications, (Dupree 1957).

Another example comes from the National Planning Board, which was renamed the National Resources Board in the early 1930's. It started with three central, very influential members. One was Frederick A. Delano, the President's uncle, who had a background in city planning. In addition, there were two distinguished social scientists on the panel, Charles Merriam and Wesley Mitchell.

These people, as social scientists, were already in power, and there was no question about their position in government. They passed on the work of the National Resources Board to the National Academy of Sciences, which was trying to find a role for other sciences in government. They also asked the group working on this task to prepare a report on how the other sciences might be able to help with the effort. The National Resources Board ended up operating with several forms of knowledge contributing rather equal roles for the natural sciences, social sciences, and education.

That's the background to the controversy over the inclusion of the social sciences in the NSF. This controversy is usually brought up in the discussions about the struggle between Vannevar Bush and Senator Kilgore. It is usually portrayed that Bush's original plan for the Foundation left out the social sciences and Kilgore wanted them in.

That is a bit of an oversimplification. It leaves out the fact that President Harry Truman and his Bureau of the Budget were also very much in favor of having the social sciences in the Foundation, presumably as an extension of the role social sciences had played earlier. It also leaves out the fact that what Kilgore was talking about in his bill was not really a full, equal role for the social sciences in the Foundation, but rather, a reference to the other sciences and related economic and industrial studies – not necessarily the social sciences as a whole.

When the social scientists testified on Senator Kilgore's Bill, they promoted this kind of adjunct role for the social sciences at the Foundation. For instance, Edwin Norris of the Brookings Institution argued that an adequate national defense hinged on the strength of the industrial system and that one needed to understand economic principles and practices in order to have a strong industrial system.

William F. Ogburn, a Chicago sociologist and a student of technological innovation, testified that all important inventions precipitate social change of various sorts, so a government that supports discovery also has a responsibility to support social science research to solve the resulting problems.

Herbert Americk, presenting a public administration perspective, argued that too much emphasis on physical science could lead to creation of "instruments" – this was probably a veiled reference to the bomb – without the counterbalancing knowledge and skill and their proper control and utilization for “the benefit of mankind.”

At that stage, there was a very clear association between the issue of social sciences at the NSF and problem-solving. Social sciences were seen not quite as the social conscience of the other sciences, but more like a kind of intellectual maid service that was going to come along and clean up the messes that were left behind.

The resolution of those difficult issues was a compromise position: the NSF legislation permitted, but did not require, the inclusion of the social sciences. It was left to later entrepreneurs to put the social sciences into place at the Foundation. The entrepreneur who did so, who might be known as the Vannevar Bush of the social sciences, was Harry Alpert, who entered NSF as part of its Program Analysis Office.

Alpert chose not to take up the argument for social-science programs at NSF on the basis of the adjunct subsidiary role that had been argued in the earlier hearings. Instead, he adopted a rationale under which social sciences would be fully parallel to the rest of the sciences NSF was supporting. Alpert stressed basic research in the social sciences, particularly in what he called the hard-science core of the social sciences. He also stressed that social-science knowledge, like the knowledge produced by other sciences, would have long-term impacts on government action, rather than be applied for short-term use. In other words, what he said to the sciences that were already being supported by NSF was, “we're just like you.”

The strategy Alpert advocated had real consequences for the kinds of science supported by the Foundation. However, he had to make that argument to the National Science Board, and they did not buy it completely.

When Alpert was able to put some programs into place, he supported one that was a straight social science program, but several that represented what they called convergent strategies, areas of social science research that had some affinity with areas already being supported by the Foundation. This led to the rather odd development that one of these early programs was sociophysical sciences in the engineering directorate, supporting subjects like mathematical social science, economic engineering, and statistical design. In addition, because of the personal interest of a division director, Raymond Saeger, there are history, philosophy and sociology of science.

The whole question of the role of the social sciences in NSF has continued to be controversial. It was a hot topic throughout the 1950’s, and as late as 1958, the question of independent social science programs was still up for debate. There was a real concern that by letting these areas of inquiry into the Foundation, trouble of some sort would occur.

The National Science Board set up a four person task force to deal with the question of how independent those programs should be from the rest of the Foundation’s mission. The task force came back evenly split. The negative side worried that social sciences would be "a source of trouble beyond anything released by Pandora," (England 1982).

The organizational ambivalence that can be traced throughout NSF's history in relation to social sciences began with the Bush era. Eventually, of course, the social sciences did get a program at the Foundation, then a division, and now a Directorate of Social, Behavioral and Economic Sciences.

If you know some of the history, it appears that the directorate bears a great resemblance to the early mixes of programs – the Science Resources Studies Division study is there, which purely tracks statistics about science as a whole. And just because there was no place else to put it, the International Programs Division was put into that directorate.

The research programs still stress what Alpert called the "hard-science core" of the social sciences; they still follow the "we're just like you" strategy. Because of that, they do not represent the full range of inquiry that social sciences represent in the university – they are just a particular slice out of that range. In that sense, it is my view that they have contributed to the fragmentation of the social sciences by creating a gap in resources between people who follow differing modes of inquiry.

What is the message in this story? The ambiguous role of the social sciences at NSF has little to do with the character of social sciences themselves, with what social scientists actually do. It has everything to do, however, with the ambivalence of the other sciences toward the social context of their own activities.

We can interpret the marginalization of the social sciences as an unconscious method of pushing aside the broader vision of using a variety of scientific knowledge to create a better life. If we talked about creating a better life, then we would need to have a concrete way of bringing in the people who are actually going to live with the world that's transformed by science in the ways that Bush talked about.

Instead of reflecting something about social science itself, this marginalization of social science reflects a desire for a different vision – a vision of a protected technical world in which bright people can make discoveries in isolation, without regard for the full human context of those discoveries.

Fifty years have passed since *Science: The Endless Frontier*. Those 50 years have certainly demonstrated that that narrow technical vision is not viable for the 21st Century. The benefits that Bush promised can only be produced effectively by considering science in a fuller context. The question that the 21st Century really raises is how to create a fuller partnership than we have seen in the past between a socially responsible science on the one hand, and a full, rich, and independent set of social sciences on the other.

CROW: The last and perhaps historically most complex and gargantuan task is attempting to understand military science and technology before Bush and military science and technology after Bush. In searching the land far and wide, we find a man trained as an engineer and as a social scientist: Gene Skolnikoff is Professor of Political Science at Massachusetts Institute of Technology and has, as you will note in the bios, been involved in a number of aspects related to defense, science, and technology. For the final ticks of our clock, I will turn it over to Gene.

SKOLNIKOFF: Thank you very much. I can't think of a more inappropriate introduction to what I'm supposed to cover. [laughter] I also can't imagine a worse position on the program: I'm the only thing that stands between you and the drinks. Just as Harvey Sapolsky from M.I.T. – there may be a pattern here – stood between you and lunch. Moreover, I'm covering a subject that has been mentioned repeatedly during the day. So I'm going to try to be brief if I can.

Let me make one quick comment on the last points about social scientists. As I recall, in the early days of the President's Science Advisory Committee – their repeated discussions about how to include the social sciences in the work of the White House Science Office and whether a social scientist should be on the President's Science Advisory Committee – there were certainly none during the period that I worked in that office. And if I'm not mistaken, the first one who may have been appointed a member of the committee was Herbert Simon.

And in the report of the meeting of the first successor to PSAT, the PCAST, the President's Council of Advisors in Science and Technology – at the very first meeting, there's a little phrase, apparently in one of the reports when discussing global warming, that the social sciences were apparently unable to make any contribution to the study of that subject, which Jack Gibbons agreed to. So I think we still have a problem of how adequately to represent the social sciences in policymaking.

Let me just mention one personal note. When I started teaching at M.I.T. in the mid-60's, Vannevar Bush was there, and I had him a couple of times meet with my class on Science and Public Policy, to talk about some of the key developments of the report that we're here to talk about and what happened afterwards.

He resolutely refused to do that at all. All he wanted to talk about was, with great relish, how he dealt with the politicians in the Congress and how he managed, treating them with considerable, in those discussions, disdain, how he always managed to come out on top whatever the Congressional attitudes were, and how he manipulated them and how he had them dealing out of his hand.

Though this was a certain amount of showmanship – all undergraduates just loved this – at the same time, I think this did in fact reflect some of his attitudes towards what the politicians were like and reflected his continuing view that we've heard about repeatedly today, that somehow science had to be protected from interference as much as possible, had to be buffered from interference with the partisan political process.

I should also mention, by the way, that landmark is about to disappear at M.I.T., the building that housed the radiation laboratory, what's left of it, will actually go next year. That's 50 years after the end of the war, we finally are getting rid of that old, wonderful, in fact, wooden building. Many of you, I'm sure, have seen it. But it's going to be torn down, not because it still isn't good but because it's got asbestos.

As I said before, we've been talking about the military role repeatedly, and every point that I might have wanted to make, one by one, has been carefully made by somebody else. Let me just make a few comments about the general situation – pre-war, during the war, and post-war – that are relevant.

In the pre-war situation, all R&D essentially was assumed to be carried out by industry in the course of procurement. Some exceptions to that: there were some in-house R&D laboratories, the Naval Research Laboratory was started after the first World War, I believe. But by and large, what R&D was carried out in the military area was assumed to be done in industry and to be paid for through the prices of the products. There were no separate R&D contracts with the private sector.

And as Nathan Rosenberg said, the agricultural research just prior to the war was larger than the military research in absolute amounts. During the war, of course, as we know, Bush turned to the private sector. I think Lew Branscomb called it "privatizing science" in a way, but he certainly had no qualms about moving rapidly in OSRD and DRC to turn to the private sector, especially the universities, for the conduct of R&D related to military purposes.

He established several important ideas at that time, one of them being the full-cost contracts, which pay the full indirect costs. Of course, we see that issue coming back, a serious problem for the universities if the “Contract With America” is carried out as is forecast. The continuing increase in overhead rates, I think, will not continue to be politically viable. Bush also established the principle of parallel paths to development of weapons systems rather than simply one contract to achieve one purpose.

As we know, he worked very closely with the military, with a good partnership, but I think that partnership very much reflected his own diplomatic and political abilities, rather than any real conviction that the military could be trusted with research. I think he had basically no confidence in their ability to understand science or to see new weapons possibilities. And that, I think, was one of motivations for his call after the war to have a division of NSF that would be devoted to defense science to supplement what the military services would actually do.

By the way, Sapolsky and Zachary and David Hart all pointed out the very political context in which the Bush report was made and received, and that, in fact, there was a political necessity for him to have a report. The implication is we shouldn't necessarily take too seriously some of the details. That it was a political context and a political move that had to be made – I think the judgment of that is correct, but I don't think the conclusion is correct. That is, the fact that you had to have a political demarche of some kind doesn't negate the importance of what was actually said, what influence it may have had, and what developed subsequently.

After the war, there was continuation of what had been started during the war, that is, direct military support for R&D in industry and universities and in-house. The whole development was certainly much larger than Bush could ever have anticipated. They talked about a steady state of the Research Foundation being $100 million, and of course, we know what the total support grew to be.

Many contracts were developed with the private sector, both industry and universities. Concepts like spin-off were assumed – I don't know if the word was used at that time. The assumption was clear that military R&D would eventually pay off in economic terms. It wasn't necessary to spell it all out, except in the very general philosophical way that Bush did. At times, there was a lot of direct support from the federal government for development of particular technologies, for example, in computers. At times, there were guarantees, contract purchases if technology was developed to meet military requirements.

I happened to be in a position to receive the first videotape recorder. In fact, it was the second, because AMPEX violated their contract and sold the first to CBS to make a big publicity splash. But in any case, I was a specialist third class in the army, and because I had once been at M.I.T., I received the VCR – it wasn't called that then – and did the initial tests on it. There was no question that everybody felt at the time that this machine would have enormous commercial potential if you could only reduce it in size and volume. AMPEX, the one that produced it, tried very hard, failed, and the rest is history about the Japanese.

During this whole after-war period came development of the national laboratories. The in-house laboratories in several agencies, not just defense, we often forget, now have R&D funding that's roughly twice what goes to the universities, something like $22 billion in the current budget.

Today, many of these issues are still with us, still dominated by politics, though we insist on trying to talk about them in purely rational terms. Some of the issues are different – the concern about commercialization, it's obviously very different.

The goal of insulating science from politics is a constant issue, that's not a new issue. But what none of the people involved understood, including Bush, was that the scale of support for R&D was going to grow to such an extent that you could no longer insulate it in the same way from politics, and more to the point, you don't want to.

Somebody made the point earlier that when you start having substantial budgets, you've got to have a champion. I think that's true of R&D and universities in general, that if you insulate it too much, you won't have anybody to defend it in the political process, and you really do need and want to have a champion in the process.

I remember Alan Waterman in the NSF essentially never saw the president. He had almost nothing to do with the political process, either in the administration or the Congress, and I think that had a lot to do with the scale of the budgets that NSF had until after Sputnik and after the White House Science Office was created.

It seems to me interesting that after all the war-time experience, there is really not very much attention to defense science in the Bush report. There's one piece of it, one committee and the talk about the NSF division, but I don't think there's really very much a discussion about it, which is a bit surprising.

I think we've paid a price for the war-time focus on setting up large projects with big objectives – Manhattan Project, the radiation laboratory, and then later the Apollo Program – with a general view in the society that all you have to do is put people together, throw resources at them, and you can solve any problem.

I would argue that the SDI Program, which is coming back today, still represents a real ignorance in sort of rational terms, ignorance about the limits of science and technology – but perhaps more important, it represents the political overlay that will continue to govern what actually happens.

Obviously, since the mid-1980s, there's been increased focus on competitiveness and the end of the Cold War, with defense developing new roles. ARPA has now a formal economic responsibility, not just defense or even dual-use, but it is supposed to contribute to the economy. And national laboratories are seeking industrial partners at the center of whole new programs.

Last month, we had this political earthquake, but we have to remember that the concern about this started much earlier. It's interesting that no one, I don't think, has mentioned the word "social contract" today, about the demise of the social contract between the government and the scientific community, but it's an issue that we all recognize predates the election, but which is going to get more serious because of that.

One issue I'm concerned about is this concern about where the money goes and how much there is, the danger of competition between the national laboratories and universities for what look like the same dollars. I don't think that's very prevalent today, I'm concerned it could be tomorrow.

I am also concerned about whether the openness question of research in the context of international competitiveness will become a problem. It started to be a few years ago, and I think at the moment, it's quiescent – but if the trade balances continue to be negative or to deteriorate, I think there are going to be very serious questions raised once again about whether we can afford to have an open research process to which everybody can get access. And that's something that should be of great concern, I think, to all of us.

Finally, it doesn't seem to me that the basis of our support for basic research has been a fiction. I think that sometimes we've overblown the hype about the role of support of basic research. I don't think it's fair to characterize it as a fiction. I do in fact believe we are capable of destroying this national resource that we have in the universities and the national laboratories and in basic research.

I'm obviously in favor of extensive discussion, and I think the symposium is presumably one aspect of that. But I would question that such a discussion should start by throwing into doubt the sources of strength that we really do have and that are the envy of the world. I think there are important questions we have to face about the changing political situation, but I think it would be a bad mistake to throw into doubt what we do have and what has been so very productive for us.

CROW: Thank you. Are there any comments or questions?

BECKLER: I'm David Beckler, I'm a consultant to the Carnegie Commission. I'd like to fill in a few points of the early history. Gene Skolnikoff mentioned the social scientists on the President's Science Advisory Committee, on which I served as Executive Officer from 1953 to 1973. The question on social scientists really was considered in the context of the kind of people who were on the President's Science Advisory Committee. This was an important legacy from the wartime involvement of top scientists of the country, who, after the war, served on the early science advisory committee.

And if you read Dr. Robbie's contribution to Bill Golden's edited book on science advice to the president, you'll find his expression of, shall we say, opposition to involvement of social scientists on the Science Advisory Committee. I think it was due to the concern that one physicist can be reasonably representative of physics, but it's not clear how many social scientists would be required to be representative of social scientists.

I can take some responsibility for urging early on that Herb Simon be the first member from social sciences on the committee, because he had credentials. He was a member of the National Academy of Sciences, and he had credentials in both the social science community and the natural science community. He was followed by James Coleman from Chicago, and then Pat Moynihan [Sen. Daniel Patrick Moynihan] also served on the President's Science Advisory Committee.

I can mention several outcomes. One was a report on educational innovation that led to the establishment of the National Institute for Education and the K-12 model school system proposal that found its way into New York City and the District of Columbia. The report on early childhood education – the first three years of life and the importance of giving attention to that in the context of a more than custodial child care system – that was part of the Johnson initiative.

There was the response to President Nixon's request for an assessment of Alvin Toffler's *Future Shock*– he wanted to know whether this was real and what he should do about it, if anything. There was an interesting report that we did for the Secret Service after the Kennedy assassination, which was to see whether there was a way in which we could advise the Secret Service on identifying potential presidential assassins. There are just some of the possibilities.

And finally, I would just mention that there was this legacy from the Bush era to the next generation of science policy because of the people who were leaders in that era, transferring through the Science Advisory Committee to the Office of Defense Mobilization, and then through Sputnik to the Presidential Science Advisory Committee, which has led to the present committee. So we in effect have our roots in the Bush era transformed into subsequent administrations. Thank you.

WYLEY: I'm Bill Wyley from Battelle. And I am frightened by the fact that we are talking about doing science for the need of the country without involving the social scientists. I know we live by the dictum that nature was created along the lines of the disciplinary departments within the university. But if we only look at the failure of nuclear energy, the failure to get those kinds of technologies into place, we can see very clearly that it is going to take more of an interdisciplinary effort rather than a mono-disciplinary effort that we have exposed in the past.

My question to the entire panel is: What are we going to do in the university to prepare for serving the nation's needs, along the lines that I've just discussed?

SKOLNIKOFF: We're speechless.

COZZENS: Do you want us to answer or do you want to take more questions?

CROW: We'll take one more question and then people can respond.

ZACHARY: Greg Zachary. Got a specific question for Susan. One of the things that Bush feared and predicted – his animus towards the social sciences was transparent – he and his camp said that the private foundations would end up bailing out the social sciences. And as the years went by and more and more of the foundation money did go towards them, he was somewhat disturbed by that.

As a board member of the Carnegie, he had particularly vociferous debates with John Gardner over this. It was his perception that private foundations had bailed out the social sciences. Did they really or were they not compensated for his animus and really kind of stunted?

COZZENS: Shall I go with that one, since that's fairly specific? Well, actually Alpert also had to make a very careful argument when he started social science programs at NSF, that NSF would have a different role than the foundations. And part of his argument was that the foundations were supporting more action-oriented research, and that was one of the reasons to do the hard science core at NSF.

Of course, history, politics, and dollars have overtaken both Alpert and Bush in this regard, and the federal effort is now so much larger than anything that foundations do, that it's now a joke to talk about the foundations bailing out any area of science, even a number as modestly supported as the social sciences.

ETZKOWITZ: My point before was that we no longer need fictions in order to justify basic research. Surely Ed Mansfield's research has shown that there is direct benefit to industry and productivity from basic research.

The Association of University Technology Managers report shows $7 billion worth of products coming out of basic research, and this is without a consideration of movement to a more strategic form of research. And Vannevar Bush's own career exemplifies how working on industrial problems could lead to basic research, which is what he did with his own students in the university.

My specific question, though, is to Susan. You commented upon how social science followed a disciplinary model in NSF, really following the model of the physical sciences, and everything's always been justified on contribution to theory and method without making a focus on substance.

Now, as you know from the recent European Science Policy Meeting, the European community, the European union, will have a different focus in their social science programs, with specific direct attention to major problems, including 50 million ECUs to science and technology policy.

Do you feel that NSF in our country should take a similar stance and move more toward interdisciplinary programs, even as modest moves in this direction are made in science and engineering?

COZZENS: I can't speak against the proposal that you're making, but I was actually arguing that we need a great deal of fundamental social science work to understand the dynamics of the systems that science and technology interact with, and many other factors interact in.

So while I would certainly like the interdisciplinary work of the sort you're talking about, there's also a lot a room for it to support a much broader range of fundamental work in social sciences.

CROW: Thank you.