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.