

## ***Science: the Endless Frontier as a Treatise***

Professor Donald E. Stokes

### **Panelist Responses**

Professor I. Bernard Cohen

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### **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 19<sup>th</sup> 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 20<sup>th</sup> 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 20<sup>th</sup> 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 19<sup>th</sup> 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 20<sup>th</sup> 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 21<sup>st</sup> 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 21<sup>st</sup> 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 simple 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.