Opening Remarks

Provost Jonathan Cole

COLE: In the 50th anniversary year of the publication of *Science: the Endless Frontier* (1945), the challenge that confronts us is to learn from the past and design for the future: not simply to celebrate what was begun by Vannevar Bush and his colleagues, who formulated a plan for the growth of American science in the aftermath of World War II; not simply to opine about what may seem to some today like the halcyon days of the past when we witnessed enormous rates of growth and scientific resources that produced exponential rates of growth in scientific knowledge. In short, we should not try to socially construct the golden past or a lost Eden, one that surely never existed.

Rather, we should engage collaboratively in the analysis of the intentions and purposes behind the creation of the Bush model that led to the close partnership between science and American research universities. We should think critically and analytically about the historical achievements of American science and technology, as it has operated within the framework of the Bush manifesto over the past half century. We should analyze the strains in the alliance or partnership – the sources and types of breakdowns in the system that we have created. And finally, we should develop new ideas for a reconstructed model of science in the national welfare that will serve the nation as well over the next 50 years as the Bush structural model has over the past half century.

In reviewing the history and context in which *Science: the Endless Frontier* was produced, we are made keenly aware of the multiple purposes and motivations behind the Bush report to President Truman after President Roosevelt's death. A principal objective of Bush and his colleagues was the continued importance of maintaining military superiority for the United States. This, they reasoned, would require heavy investments by the government in defense-related research at universities and national laboratories.

Second, in reflecting on the past, we try to get the history right and to speculate on motivation and intention, as well as on the unanticipated consequences of the creation of the structure derived from the differing organizational perspectives championed by Vannevar Bush and Senator Harley Kilgore, Democratic Senator from West Virginia.

We can now perceive how much the model that led to the National Science Foundation and to the thorough institutionalization of the National Institutes of Health grew out of a national military crisis. We can see how the design for investments in science and technology was pieced together rather rapidly but represented perhaps the most systematic, nationally organized effort ever to structure the support for scientific and technological growth – one that understood how investments in young people and their education at the finest American universities could create American superiority and preeminence in the production of knowledge in these areas. This vision was remarkably prescient. And its implementation has brought truly extraordinary benefits to American society.

Fifty years later, we venture into more difficult terrain. We must analyze the consequences of the partnership between government and university-based research and discern how the areas of knowledge and practical activity outlined by Bush in his report affect human health, economic change, national security, and investments in human and intellectual capital and public welfare. We have expanded that angle of vision to include the consequences on studies of the environment and the social problems beyond those that are directly health-related.

We must also inquire into the many achievements as well as some of the failures of the structure of scientific and technological innovation that we have created over the past half century. We must describe how this system of national innovation led directly to an organized arrangement for scientific growth perhaps unequaled over the past several hundred years. But we must also analyze the unanticipated positive and negative consequences of this system of innovation – the displaced scientific and organizational goals, the missed opportunities as well as the opportunities seized, the organizational and structural problems as well as successes of the Bush paradigm.

As we reflect on the system of support for science and technology that has dominated our society over these past decades, let us not fall prey to hyperbole in extolling virtues and overstating shortcomings. The system has been around a long time, demonstrating extraordinary vitality. We continue to produce extraordinary science and technology. We have not lost our scientific capabilities or imagination. We continue to produce exceptional talent among our younger population. We continue to attract some of the brightest young people in the world, who want nothing more than to study with American scientists and engineers.

Indeed, science and technology, which are fundamentally dependent upon federal resources, have expanded and grown at rate that was never dreamed about by the founders of the system. American science and technology has been the dominant social system of science during the post-war period. Perhaps the central message of the history of the Bush era of scientific development is one that emphasizes rapid growth and unparalleled successes in the advance of knowledge. Much in the system remains strong, but it is under strain, and not only from Washington lawmakers. Even if the system is not totally broken, and I do not believe it is, it is an old system and is clearly suffering from serious fatigue.

The partnership between the federal system of support and the university as the principal site for innovation is under severe strain today. While the system is not coming totally unglued, even if the current scene in Washington might lead us to believe otherwise, let us acknowledge that the national system of innovation needs some serious rethinking, reconceptualization, and perhaps some restructuring.

This should not surprise us. The positive heuristic of any system is apt to become tired if not exhausted over time, if for no other reason than that the context changes. The society that produced it has evolved in important ways that make the older model problematic. And the social context in which we are producing investments in science today is markedly different from that considered by Bush and his colleagues. More importantly, the external threat of Communism no longer dominates the perspective of policy makers in Washington. The Cold War is over.

One significant rationale for the Bush plan has limited saliency today. While the results of fundamental research in the biological and health-related sciences are still only in the early stages of reaching their full potential to understand and prevent disease, the system of health care is in a transitional, if not chaotic state. And the relationship between current health care costs and needs is clouding the issue surrounding investments in basic biological and health-related research.

There would also seem to be a crisis in industrial commitment to basic research, with many of the industrial laboratories downsizing to the point that questions of their future vitality may be legitimately raised. There remains little commitment to research in the social and behavioral sciences with little apparent understanding of the critical importance of focusing on the interrelationships between social structures, social systems, and primary foci of scientific attention. Whether the problem is preventing transmission of AIDS, preventing substance abuse, an epidemic of violence in the streets and in families, suicides among America's youth, or investments in the human capital through education, these social aspects of public health have yet to be woven into the fabric of national innovation and remain constant objects of skepticism.

And finally, the number of those among critical policy decision-makers who fully understand the national payoffs to investments in science and technology seems to be dwindling, a fact for which we in the scientific and academic communities are partially responsible. In one sense, of course, all of this is familiar. Most great organized efforts at scientific advance have had goals similar to those articulated by Vannevar Bush and his colleagues. As we know, for instance, in the 17th Century, the Royal Society was formed and as Robert K. Merton put it in *Science, Technology, and Society in Seventeenth Century England* (1970):

> 'Science was to be fostered and nurtured as leading to the improvement of man's lot on earth by facilitating technologic invention. The Royal Society…does not intend to stop at some particular benefit but goes to the root of all novel investigations. Further, those experiments which do not bring with them immediate gain are not to be condemned, for as the noble Bacon had declared, experiments of Light ultimately conduce to a whole troop of inventions useful to the life and state of man.'

The analytic question is how the systems of innovation have been organized. What caused them to be organized as they have been? And what values, norms, and structural factors have been related to the successes and failures of these social systems of innovation?

Bush envisioned the creation of communities of scientists and engineers at universities as a means towards furthering the advance of individuals. The individual is adumbrated also in the operation of the Royal Society and its relationship to science at Cambridge. In reflecting on the Merton thesis in *Puritanism and the Rise of Modern Science* (1990), I.B. Cohen noted,

> 'Newton's career also illustrates an important observation by Merton on the significance of the formation of a scientific community; almost certainly Newton would not have written his *Principia* had there not been a discussion by the

> London virtuosi of the Royal Society of the possible force responsible for the observed Keplerian motion in the planets. It was as a result of this discussion (by Hooke, Wren, and Halley) that Halley went to Cambridge to see Newton and to explore this topic with him. The subsequent encouragement of Newton by Halley and the approbation of the Royal Society were significant factors in pushing Newton to complete his researches and write them up for publication under the Royal Society's imprint. It is doubtful that without the Royal Society there would ever have been a *Principia*.'

It is our principle mission to begin to understand better the changed landscape and environment for science and technology, and how it is affecting the operation of a system created more than 50 years ago. What our analysis tells us ought to be recast in the current partnership between science and universities. We must focus on the achievements of the Bush paradigm as well as on the points of strain in the current system.

I'd like to suggest a number of themes and questions that, from the perspective of an inveterate sociologist of science, ought to be addressed.

First, in each of the primary areas of research considered, what have been the most significant achievements resulting from the Bush system of innovation? And what structural features of the system can be credited for facilitating these successes? In fact, how have the organizational principles and structure of resource allocation used to identify scientific talent and problems meriting exploration at the National Science Foundation (NSF) and the National Institutes of Health (NIH) determined the outcomes of the research? In short, why has the Bush model been so successful for the better part of a half century?

Second, in what ways has the Bush model's explicit prescription to link basic research at universities with the training of the next generation of scientists and engineers been essential for the productivity of science and technology during this period?

Third, how essential has the peer review system been for the successful development of the basic sciences and engineering? What are the problems with the peer review system today? And in what ways, if any, has it become dysfunctional for the continued and sustained growth of scientific knowledge?

Fourth, the normative ethos of science, which was reinforced within the Bush framework of innovation, emphasizes an open system of knowledge, production and communication. Can a system flourish that limits such an open society? Are we experiencing greater pressure to limit the free distribution of the fruits of science and technological innovation? And if so, what are the consequences for the production of knowledge and its uses?

Fifth, while there is some significant political pressure for scientific isolationism, will a modified national system of innovation have to be structured in such a way as to share responsibility even more with our international communities, both of scholars and nation states?

Sixth, what role should industrial research and support for science and technology play in the tripartite and remapped system of innovation? How should the current roles be altered?

Seventh, how will the shape of scientific communities change in the restructured system of innovation? Will the traditional and visible colleges undergo significant substantive reconstruction with the further development of communications technologies? How will this affect scientific publication and the reception of published work? How will the information revolution affect the role of the traditional scientific journal and methods of peer evaluation at work? And how will all of this influence the existing system of organized skepticism, that critical piece of the evaluation system of science?

Finally, are there real threats to the scientific enterprise coming from those who take a strong relativist or social constructivist position about the development of knowledge, and who believe that the scientific method and its results are little more than a matter of social consensus and power relationships? Or is the anti-science movement one that represents a passing intellectual fashion?

Consider a few more problems with the current system of innovation. The system has become extremely large, competitive, and bureaucratic. Scientists and engineers appear to be experiencing significant displacement of goals. Scientists spend an inordinate amount of their time obtaining resources to conduct science, impeding their efforts to actually remain active researchers. There are now scientific rainmakers whose principle occupation is finding resources for large numbers of workers in their local scientific vineyards. There are continual problems raised about the nature of the process of funding research. Is it fair? Does it approximate a meritocracy? Does the system of resource allocation match the types of problems being attacked? Does it place too much of a premium on the quality of proposals rather than the track record of the scientists and engineers? Are priorities too often set for political rather than substantive scientific reasons? Is this resource allocation system undermining the interests of young, potentially talented scientists and engineers?

Has the system supported too many people, programs and universities without sufficient concentration of resources? What is the optimal level of competition in the national system of innovation? And how would we know when we have reached it?

To what extent is the strain in the Bush model a function of the relationship between the number of scientists and the availability of resources to carry out science? Or does it lie in the structure of the system itself? Is the system producing too many scientists and engineers given the labor market for such highly-trained members of society? How do we begin to calibrate the production of scientists with the larger labor markets in need of their talents?

The current system seems to inhibit the development of criteria of scientific choice among competing claimants to scientific resources. Almost 40 years ago, Alvin Weinberg spoke of the need to articulate such criteria. I'm not aware of any effort that has succeeded in establishing a basis for determining scientific priorities. Should that be a goal of a national system of innovation? These and other questions need to be developed.

Discussion on the Evolution of Research

Evolution of Defense Research
William Spencer
Evolution of Health Research
Donald Fredrickson
Evolution of Industrial Research
Professor Nathan Rosenberg
Evolution of Basic Research
Professor Joshua Lederberg

Moderators
Richard Nelson
Provost Jonathan Cole

Evolution of Defense Research

NELSON: First up this morning is William Spencer, to get us thinking about the last 50 years of military research and development. The issue of national security undoubtedly was the prime motivator for the Vannevar Bush report. And the defense military research and development operation in the United States has played a role over the last 50 years perhaps even much grander and more broad than Bush thought it was going to be.

William Spencer is a physicist by training. He has had a distinguished career, first as an operating scientist and later as a research administrator at Bell Labs, Xerox Park, and Sandia. He is now Chief Executive Officer at Sematech. It has been my privilege over the last three or four years to come to know Bill Spencer quite well and to appreciate the sophistication, the thought, and the wisdom he directs to the subjects he addresses. So, Bill, it's very good to have you launch our discussion.

SPENCER: Thank you, Dick, for those kind remarks. Having spent the last five years at Sematech and being accused by my critics and actually some of my friends of having sustained myself on technopork and being driven by technonationalism, it's nice to hear that I was involved at one time in other things.

This is an important series of presentations that Columbia has organized. I think science and technology are key today to the economic health and security of our nation. Participating in the global economy and maintaining national security are two roles undergoing dramatic changes, and our country faces major new challenges in each of these areas. I think it's a time for leadership, and I'm pleased to see Columbia University taking on that role.

Now, I've got a rather daunting task this morning because, as I look around this audience, I see a lot of people out there who know a great deal more about defense research than I do. And I know from talking with some of you that even those of us who don't know a lot about this area have very strong opinions of it. So I find that daunting.

As I walked in this morning, I couldn't help but think what my associates in Texas might say if they walked into this area. I suspect the first thing they'd do is look up into this rotunda and say, "Man, could you put a lot of hay in here." So much for my sophistication.

I'm also going to steer away from the role that defense research has played in the superiority of our military facilities and our military capability today. Still, I have to admit that I had a real sense of pride when I heard yesterday that some of this electronic wizardry helped us find a young man who'd been living on bugs and grass and rainwater for six days. That sent a real thrill through me.

I believe that military technology and national security are so dependent on industry – which is many years ahead of the military today – that a strong industry is essential to national security. Because it's impossible in the short time I have to cover all the contributions that defense research has made in the United States, let me tell you about a couple of them.

First, I think it would be a major tragedy if the world's leading nation were not to remain a leader in basic science and technology—in particular, applying the technological results of this research to the well-being of our own citizens and ultimately to the entire world.

Second, I believe it's no longer possible to separate basic science from applied science, basic research from applied research. That's not a new idea. Many people have said it before, and not everyone agrees with it.

Finally, I believe the linear chain model of research, applied research, development, and manufacturing is no longer valid. You'll see those biases as I talk about three areas today: computers, semiconducters, and if I have time, nuclear energy.

The history of computers is well chronicled. The information I have came principally from Ken Flamm's book on creating a computer *(Creating the Computer: Government, Industry and High Technology*) and Emerson Pugh's fine history of computing at IBM *(Building IBM: Shaping an Industry and Its Technology*). Let's assume that the computer got started after the Second World War, and then I don't have to face the issue of whether it was invented at Iowa State University or Princeton or MIT or in the UK.

It is important, I think, to note that people like Bill Norris and Tom Watson – maybe it's surprising that I picked those out – played a major role in the computer revolution since the Second World War. Look at the companies that were important in 1950: the leading producers were Bendix Aviation, IBM, Minneapolis Honeywell, RCA, Sperry Rand, and Burroughs. Today, the leading names, with the exception of IBM, have changed to Sun, Compact, Apple, Hitachi, Siemens, and Fujitsu.

There were many defense support programs. I want to talk a minute about SAGE and STRETCH. I think those were key because they led to IBM's dominance of the computer business by the 1970s and 1980s. The SAGE project, of course, depended on the IBM Whirlwind work as a basis for the technology. If you look at that and the program IBM had to provide guidance computers for the B52s, IBM records show that over half of the R&D for those programs was paid for by the government. As late as the mid-1960s, 35% of IBM's R&D was still coming from the federal government.

This was on top of significant contributions that IBM was making to their own research activity. Watson Junior says this was 12% of profits, growing to 35% in the early '50s.

Now, this was not unique for companies. Xerox in the 1950s was spending over 100% of its profits on the development of the first plain-paper copier. The SAGE computer represented more than half a billion dollars of IBM revenue in the mid-1950s. And at that time, IBM was only about a $500 million company.

At the end of the SAGE program, there was a focus on trying to improve the capability of computing by a factor of a 100 – a giant step. Not a factor of 10 or a factor of five but a factor of 100, which resulted in the STRETCH computer. It played a major role in the technology that was the base for the IBM 360 and 370 computers, which led to IBM's total dominance of the mainframe computer business.

There was an interesting comment made by Watson Junior about this. His account said that until the late 1950s, SAGE accounted for almost half of IBM's total computer sales. "We made very little money on the project in keeping with the policy against war profiteering laid down by dad. But it enabled us to build highly automated factories ahead of anybody else." I think that's a key point. And it trained thousands, thousands of new workers in electronics. I believe without Watson Junior's leadership in the computer area, and without a superb research activity in Yorktown Heights, and earlier in Poughkeepsie, the world would have been much slower in reaping the benefits of computing.

Now, you might argue that this defense focus led IBM and DEC and later Cray down a dead-end street into high-performance mainframe computers. But I believe it was an incorrect reading of technology tea leaves that caused these corporate problems and not the defense funding. If you go back to the 1960s, when there was a major emphasis on mainframe computers, the Defense Department through the Advanced Research Projects Agency (DARPA) was starting a new type of computing. The history of this networked computing and personal workstation activity is well chronicled in a series of papers presented on the 15th anniversary of Xerox's Palo Alto Research Center, entitled, "A History of Personal Work Stations."

While Xerox Park is credited with having developed the networked computer, that work actually grew out of funding that was given to MIT, Carnegie Mellon, SRI, and a number of other places.

Xerox had the resources to do a full system demonstration of that capability by the mid-1970s. However, it was the business genius of Steve Jobs when he saw Small Talk, one of the early object-oriented programming languages, running on an alto computer, that he said this: "If I could build that and sell it not as a networked computer but as a standalone system, that's a big business." The Apple Lease-A project, which was the first attempt at that standalone system, was as big a failure as the Xerox Star. But the second attempt, when the Macintosh was introduced in 1984, was a huge success and vaulted Apple into one of the leading computer manufacturers today.

Gordon Bell, in the introduction of that series of papers, talked about the computing industry having been driven by technology devils from the semiconductor industry. And certainly those two industries are very closely entwined. The semiconductor industry, having gotten started here at Bell Labs in 1947, has grown to about $50 billion in sales. The reason: if you look at this industry in 1965, when it was just getting started, a single transistor in a plastic package cost about five dollars. By 1985, 20 years later, a million transistors or slightly more than that in a one-megabyte, random access memory chip, cost about five dollars.

In fact, one of the sage statements in Silicon Valley is that "every integrated circuit will ultimately cost five dollars except for those that cost less." That productivity, 30% per year, has continued now over three decades and I think will continue for at least the next two or three decades.

A less well-known aspect of the invention of the transistor at Bell Labs was the role that Mervin Kelly played—especially his charge to people like Shockley and Bardeen and Bratton, toward the end of the Second World War, that if we don't have a replacement for the vacuum tube, it'll take every power plant in the country to dry the filaments needed for a communication system to serve the nation.

I believe Kelly played a major lead, not only in attracting these people to Bell Labs but in giving them a mission, a direction that was important not just to the telephone system but ultimately to every aspect of life that we have today. Semiconductor technology has had an impact on business, on learning, on medical care, and certainly on our entertainment.

Again, the first customers for integrated circuits were the military. In the late 1950s and early 1960s, when I came to Bell Labs, we had a series of small contracts to develop manufacturing technology for computers. And it was the reliability coming out of those contracts that led to the Air Force choosing this for the Minute Man II missiles. If you look at the sales of semiconductors in the mid-1960s, about 20% or maybe 25% were going into military systems. Today, that's down below 5%, again declining.

Something surprised me as I worked last year on what was called the Galvin Committee. Henry Kendall and I had a chance to look at some of the outcomes of the nuclear program. Among these findings: there are 3,700 contaminated sites and 77 million gallons of high-level radioactive waste. From the nuclear weapons programs alone, there are 385,000 cubic meters of high-level waste, a quarter-million cubic meters of transuranic waste, and two and a half million cubic meters of low-level waste.

A high percentage of the $23 billion spent to this point has gone to administrative and legal costs. We're applying very low-quality science and technology to the cleanup. Today, that cleanup budget is about $1 billion a year. And the estimates are that the total costs of that cleanup are going to be $300 billion to $1 trillion, maybe more. And that's just the United States alone.

No question in my mind that this nuclear situation is a mess, technologically, bureaucratically, and environmentally. About the only positive thing we can say about the U.S. situation is, it's not as bad as the Soviet Union. And we don't know how bad it is in China, Pakistan, North Korean, and India. But that's little comfort in the issue that we have to face here.

The point I'd like to make is that the issue of nuclear energy and the follow-on fusion energy, and I don't know whether I remember enough physics to know whether that's still nuclear or not, is that this is an international problem. The results affect the entire globe. It's not just a U.S. problem.

It's a big science issue. Just in the last two years, the cost of the next fusion reactor has doubled from about $5 billion to $10 billion, as Charles Curtis said last week at a meeting I was in with him. And I think that clearly falls in the area of international cooperation.

One point of interest in the privatization, such as it is, of the nuclear industry again involves the major role of Mervin Kelley. He was commissioned by President Truman in the late 1940s and proposed what ultimately led to the Atomic Energy Commission and the private development and private control of not only the weapons programs but the nuclear reactors as well.

What kind of conclusions can I draw from these rather disparate businesses? First, the government played no role in the funding of the basic inventions. The transistor was invented at Bell Labs entirely on private funds. In the mid-1960s, we moved from having mostly Army funds for the development of transistors to entirely private funding. That happened on a Friday, and on Monday, I came back to work and did exactly the same thing I was doing on Friday. This was true for several dozen scientists at Bell Labs at that time.

Now, when Bell Labs invented the transistor, those at Purdue were on the verge of doing so within the next few months or years. The computer had its roots in private work as well. And certainly the discovery of fission was not funded by our defense department.

But defense funding in each of these areas did speed up the introduction of that technology. I believe the speed at which we got computing capability and the early growth of the integrated-circuit industry would not have occurred had it not been for federal funding of things like the SAGE computer and early funding of manufacturing. One can draw the conclusions, then, that American industry benefited greatly from these and that the defense investment had little to do with the basic discovery.

One point I owe to some of the discussions that we had with Dick Nelson is this: a major difference between the nuclear energy industry and the computer and semiconductor industries was the intervention by government after the start of the business. Can you imagine the situation we'd be in today if in addition to the nuclear regulatory commission we had a computer or semiconductor regulatory commission? Clearly, we couldn't be up with the Japanese or the Europeans or the Koreans or anybody else. I think it'd be a tremendous burden for us.

Along with the funding of industry, Defense, Energy, NASA, and NSA funded large numbers of individual researchers in universities. David Kearns, whom I had the opportunity to work with at Xerox, chaired a government group that published a report in December of '92, which gave some numbers that are very interesting in terms of defense and energy contributions to basic science and particularly to university research. According to this report, universities perform 60% of the defense basic science work.

In 1991, universities received more than $800 million in defense funds. And the view from energy – which I lump together with the defense spending because the weapons programs were a large part of it – is even more compelling. 1992, DOE supported 3,500 active university grants and contracts for over $500 million. And they claim that, in the past 30 years, 20 Nobel prizes have been awarded to scientists who were funded totally or partially by DOE contracts. In addition, they provide $500 million worth of user facilities.

And many of us who grew up in the 1950s with the GI bill, the national defense education acts, or graduate fellowships – as did our current speaker in the House, interestingly – also remember with very warm feelings the contributions that defense made to our education.

But today those funding mechanisms are badly flawed. Whether it's fatal or not is uncertain. Micromanagement by cabinet organizations in the administration and by Congress, defense-unique technology funding when there is industry or commercial technology that's ahead, inflated estimates on payoffs in R & D investments, and poor predictions of cost and schedules by all of us – scientists and engineers all over the country – are some of the defense funding issues that we face.

What needs to be done? I was supposed to stay away from this. But I'm going to spend my last minute telling you some of the things I think are important. I believe we've got to completely rethink the relationship between science and public interest. We as the science and engineering professions have done an extremely poor job in communicating the benefits of our works to the general public good. If we're going to ask for billions of dollars, and $200 billion a year is a significant amount of money, we owe to the general public some indication of what we can do.

I think we have to understand how we can protect basic scientific freedom of effort in the universities and a few other places, and yet ensure that that research is coupled as quickly as possible with national and international needs.

The dilemma faced by universities today is not a government/university issue but a government/university/industry issue. If you look at today's $20 billion companies – Hewlett Packard, Motorola, Xerox – those companies, before the middle of the next century, are going to be $300 billion to $500 billion companies. They're going to grow at a rate that will mean they're going to be a major economic force. Now, I can't tell you exactly which companies those are going to be. If I could, I'd be in a different business than I'm in. And a lot richer. But some of them are going to make it to that stage.

I think that a cabinet-level position in science would be a major mistake. It has to focus on these three areas: on business, industry, and government.

I believe that we as a community have to develop clear roadmaps and priorities in our profession as to what's important. These can be done by the National Academy of Sciences and National Academy of Engineering. They've been done before in physics, biology, and chemistry. They need to be done on a regular basis, updated in technology areas like semiconductors. There's been a semiconductor roadmap that's been put together for the past six years, updated every two years, projecting out to 15 years in the future what we're going to do.

If we as a community do not begin to set priorities in research by which funding decisions can be made, then somebody else is going to set them for us. And I think we're in a position to do that better than anyone else. We then need to be able to communicate what we've done.

And finally, I believe we have to continue to train the world's best scientists and engineers. The American university has done that for the past 50 years, as Provost Cole pointed out. We've got to provide attractive careers in industry, government, and universities if we're going to continue to attract the best people into these areas.

I remember when I gave my last talk here at Columbia. I was at Xerox in Connecticut. And at that time, the entire operations research class – this must have been in 1986 or '87 – was going out of operations research and into the financial business here in Manhattan. The salaries were better, and the careers at that time were so good. I think that in addition to the future Shockleys, Bardeens, and Brattons, we have to train more Mervin Kelleys and more Tom Watson Juniors.

I hope this brief history with all my personal biases will be helpful as you take on this extremely relevant and very important task of science and technology for the 21st century. Thank you.

NELSON: Thank you very much, Bill. You clearly laid out an aspect of the military research, development, and procurement role that's been played in the United States over the last 50 years that I think is little understood and little appreciated. On the one hand, it's had tremendous impact on the development of civilian technology, and on the other hand, it's influenced and contributed to very important fields of research at universities.

You deliberately did not mention another facet. With the Cold War over, there are now issues with respect to who if anyone is going to take on the burden that Defense has carried with respect to advancing industrial technology and who will support research universities in these areas where Defense has been prominent.

There is also the fundamental question about what the defense R&D operation itself should look like in an era when national security is a very different kind of a problem than it has been in the 45 years since the Bush report.

KELLER: I want to take issue with Bill Spencer on one aspect. I think your comments about the nuclear regulatory agency are right on. And in fact, it has had a very dampening effect on the development of that industry.

But I think it's a mistake to confuse that role of government with the original role that government played in the development of nuclear power. I happened to have worked for Admiral Rickover for a number of years in the government program that gave rise to the shipping port reactor – the first civilian plant – at exactly the same time the private sector was failing with the Dresden reactor in Chicago because they had not taken into account proper manufacturing or the needs of the public.

So I think the lesson may well have been that it's a question of what role the government should play. The regulatory role was considerably dampening, whereas the development role in that particular case, given the scale of the industry, was probably a stimulating one. And that's quite independent of whether one agrees or disagrees with the use of nuclear power.

SPENCER: I agree with that. And I skipped that part because I was running a little late. But I agree with your assessment. It was the same as in the computer and integrated circuit areas. It's the government that developed the reactors that ultimately became useful, and without that, it would have occurred much more slowly or maybe not at all.

SCHWARTZ: Could you comment on the statement sometimes made that military research tends to skew research in the directions of military needs rather than commercial needs? Take Star Wars, for example. That was a lot of research, and maybe that kind of research wouldn't have been done had it been left to the commercial sector.

SPENCER: In any large organization – whether it's the federal government, AT&T, or Xerox – that I've been involved with, the funding of research is very inefficient. However, once in a while, you get a transistor out of it. And therefore it pays off.

I think the big defense programs – Star Wars is an excellent example – are the effect first of the personality of an individual. Second, they're misdirected. I think that's been shown after we've had our more careful look at the research results on which a lot of this was based.

But look at the small programs. Funding of the networked computer activity in the mid-1960s led to the things we were able to do at Xerox Park. Bringing all that together, making a systems demonstration of what was already invented, and having a small $25-million annual budget gave us a great deal of freedom.

ARPA is probably an excellent example of a funding mechanism that provides little influence. In our work at Sematech, we've had a wonderful relationship with ARPA. They provided us with $100 million worth of pork each year. We hope that it's been good pork. That, and very little direction. The direction was set by industry. So there are ways for government and industry and universities to work together to the benefit of our profession, I think.

NELSON: We have a written question here that follows very nicely on the last one and your response to it. And it's a question I certainly am curious about and I think most others are as well. "In defense science, what has been the role of sustained basic science investment, presumably by the Department of Defense? And do you see that role changing? And if so, why?"

SPENCER: The summary put together early this decade showed that small research contracts out of Defense, Energy, NASA, and other places have been a major way that basic science in universities in particular has been funded. We need to find ways to keep that up. The National Science Foundation might pick that up if a couple of billion dollars were moved out of Defense, Energy, and NASA into NSF.

My reading of Washington and what's going to happen in the 1996 budget says that basic science and defense – and probably health R&D, and we ought to be interested in Don Frederickson's view – are going to survive okay in the budget. It's the cooperative efforts between government and industry and universities that are liable to get hit.

I'm concerned that we don't understand the connections between basic science, applied science, and technology developments as well as we should. I certainly don't understand it. Maybe there are people here who do. But I think we've got to work on those issues and make sure that our congressmen understand it.

I also believe that we've got to take this time of change right now, which I think is going to be substantial, and look at it as an opportunity. There are a few people in Washington, believe it or not, who I think are very bright, very hard working, very dedicated to the role that technology and science can play in the solving the problems and providing opportunities to our country. We need to develop close ties with those individuals and ensure that they have the best information about our area as they possibly can.

So I think there's a great opportunity right now. I don't think we ought to be battening down the hatches, getting ready for a storm. I think we ought to be out selling what we can do. And getting support from Washington, business, anywhere we can.

BROOKS: I want to comment on your remark about the importance of priority setting. I worry about this. While I agree with you in principle, I think there is a great danger that priorities set at the top of a system tend to become rigid plans and that we overestimate our ability to predict the future.

I think one reason for this problem is that there are really two different issues involved. One of them is priority setting from the top down. And the other is the balance between top-down and bottom-up in general. My worry is that in enthusiasm for setting criteria, we don't allow enough room for the ideas that bubble up from the working level and almost are never conceived or imagined in any top-down process. So while I agree with you in principle, I worry about the actual implementation.

SPENCER: Harvey, as you might guess, that's not the first time I've ever heard that complaint. It is a real problem. You probably know better than I do why Phil Handler's work on biology and Bill Brinkman's update have not been repeated and have not had much of an impact. I think you're right. How to fund those areas where there's a possibility of unexpected breakthrough is an interesting question when you talk about setting priorities.

On the other hand, I think there are some large areas where, if we don't set priorities and don't get an agreement to continue funding, we'll waste billions and billions of dollars. And I don't know how to get that balance. If we set aside a certain amount for basic science with very few ties to it, we still have to decide how much is going into biology versus physics versus chemistry versus something else.

I think those are solvable problems. And I think the benefits would be very great.

Evolution of Health Research

NELSON: We have a number of good questions and comments that have been submitted in writing. But in order to stay roughly on this morning's schedule, I think it's time that we shift over to Donald Frederickson and the preliminary discussion of health research.

There were, as you know so well, two areas of science and technology specifically identified in the Bush report. National security related work, of course. But health related research also plays a prominent and explicit role in *Science: The Endless Frontier* and the recommendations emanating therefrom.

Donald Frederickson, as most of you know, is a biologist and medical researcher by training – with many, many years at the National Institutes of Health as researcher, administrator, and for a number of years, director of the National Institutes. I can't imagine anybody in a better position to get us thinking fruitfully on health research in the U.S. in the era since the Bush report.

FREDERICKSON: My blasphemy at the original session has already been publicly recalled. I simply make a statement that I think it was not Bush but his advisers who failed to perceive how ready the United States Congress was to accelerate health research when this plan was put into effect.

There were, however, people in Washington who did realize this. And I've a picture here of them, just three important people to remember: our Surgeon General, Thomas Parran, Jr., with Jimmy Thompson, and Rolla Eugene Dyer, the director of NIH in 1944 when the picture was taken.

Long before the war, Parran and Thompson went to the Congress and presented a plan for a National Cancer Institute. When it was introduced by Senator Homer T. Bone (D-WA), it was endorsed unanimously by the Senate and passed in 1937. In 1944, these two people went back to the Congress and encouraged Congressman Alfred Lee Bulwinkle (D-NC) to rewrite the public health statutes so that they might gain an authorization that was already contained in the cancer bill and roll it over to NIH. That was the broadest authorization ever written for the ability to solicit funds from the government for the conduct and support of research outside. They also got permission to do clinical investigation within the service.

This occurred in 1944, just as Bush was beginning to wind down. It gave them the opportunity in 1945 to pick up all those medical research contracts that were going to die in the office of the committee of medical research of OSRD and take them back to NIH. This slide shows the ascension of great activity. In 1946, Dyer went to the Congress and got $800,000 to start all those contracts and turn them into grants. They also got $40 million to build the largest research hospital in the world, to be put on the campus at NIH. By 1948, four new institutes arrived at NIH from the Congress. And two more before 1950.

This slide is meant to illustrate what they do with the money. First of all, NIH had an appropriation of $50 million by 1950. It was the year that the NSF was first begun and given an authorization of $5 million. From that time, the appropriation of NIH began to ascend very rapidly, particularly after Sputnik in '57. It reached a total of more than a billion by 1970.

As you know, the architects of the extramural plan, in 1946, began at once to use the research grant as a major instrument for dispersing money. And it established what continues to be a relationship between investigators and their institutions that remains essentially the same. Now, the research grants were time limited and renewable. The ideas of the participating scientists judged by peer review set the priorities and the directions of the research, and basically that is what happens in that pool still today.

Intramural research, so-called direct year, was held after a while to 10% and remains about that today, except for one institute. There were also funds set aside at the beginning for training, because there was a great need for trained manpower to use this money that was becoming available for research. The peak of training as a percentage of NIH resources was reached in about 1968 when it was 35%. Congress suddenly called a halt and said, you're beginning to train nothing but clinicians instead of career researchers. And from that time, the amount of training allotted from the NIH appropriation has been limited. It now is about 5%. And I would confess that I think there is still is no template accepted by everybody as to what the amount should be.

Congress in 1970 took a certain group of these grants – which largely were the investigator-initiated, small-group, or single-investigator grants, now called the R01 grants – and separated them for special husbandry. NIH, it must be said, was and is a creature of Congress. During the period 1955 to '68, during its great growth period, Senator Joseph L. Hill (D-AL) – the son of a doctor in Alabama and named for the father of antisepsis – was the head of the appropriating and authorizing committees for health research, the most powerful figure in the United States in determining NIH's rate of growth. Next to him in the slide is Jim Shannon, who was the director of NIH at that time. Shannon was the perfect agent for wisely and imaginatively distributing the reappropriations as they came. He also had to make sure that institutions were strengthened to care for their grantees. Support mechanisms were devised that made a very great contribution to the expansion of the American university system into the great research system that it is today.

As key people changed, the NIH faced a small political crisis. In the slide, you see its appropriation curve, and you can use that topography to plot all kinds of events in the history of NIH.

Now, largely due to the maneuvering between Richard Nixon and Edward Kennedy, legislation was introduced that nearly split cancer from the rest of biology. Fortunately, again, a white knight appeared on the House side, Congressman Byron G. Rogers (D-CO), and the schism was forestalled. And good sense eventually preserved the system.

I think the important thing is that when the research grant program was begun, Congressman Dyer put all of the process towards selecting grants to be funded later by the appropriations of the separate institutes into a central office. And in that way, the issues of what disciplines needed to be supported to address diseases could be more or less controlled.

And I think this is what saved the NIH from a great disaster during this time. All the scientists were sure that the money was now going to flow into cancer research. But in fact, very large sums went into biology, molecular and cellular biology, and immunology.

Now, another issue occurred in this endless game – meant in the best way– between the scientific community and the public and its congressional representatives. In the late '70s, appropriations began to fall again. The NIH made a plan and convinced key members of both the administration and both houses of Congress to accept the challenge of providing funds at least for an annual minimum of 5,000 grants.

So while the appropriations sagged for all NIH, the amount going to RO1 project grants never flagged. And this is an important comparison to show you that a certain amount of novelty and originality must continue at the hot interface of science and politics. This acme of 5,000 grants went on to 6,000 grants. It was written into appropriations language as though it were a fundamental principle on which the whole house was built. But Lowell Weicker (D-CT) came in from Connecticut on the Senate side, and decided why not 6,500? – which they did for one year. It was too much because it began to introduce an inflationary curve, and the House said we have to have another plan.

But the importance of the 5,000-grant issue can be illustrated in two important years. In the last year of the Carter regime, he became depressed and decided to have a recision of NIH funds. More important, when Mr. Reagan came in, he met the Secretary of Health and Human Services and said to him abruptly, "In five days, I'd like a plan to reduce the NIH by 10%. And don't talk to me about those goddamn 5,000 grants." But he didn't win because the solid front held, and there were 6,000 grants that year.

Note here that NIH in 1983 doesn't look very different from NIH in 1993, with most of the money or a big portion of it going into research grants. And about 77% of these continue to be of the RO1 type. Intramural research remains the same. Training is much lower. There are support mechanisms and contract research, largely on cancer, and administration in the NLM (National Library of Medicine).

So this paradigm holds fast, even though between those two decades the Congress has begun to micromanage much more in its demand that NIH pay attention to certain disease areas. A billion dollars worth of AIDS research, great expenditures for Alzheimer's disease, and so forth, are nevertheless managed largely by using research grants to attack the research that is required.

Now, in spite of all this, I wonder if we do need some kind of new way of giving out research. It is interesting that the Cancer Institute has always been a little different. It's put much more of its money into intramural stations that are more or less managed by committees and by the intramural force. It has the largest intramural program, nearly 25% of all the others. And yet this week, they received a report, a tough report from the outside, which prescribed that they ought to go back to the ways of the other institutes.

So there have been some experiments, perhaps unwitting ones, in changing this mechanism. But today, it remains the most significant manner of determining the priorities and the directions of research that is funded by the federal government.

Now I suppose that a brief history like this cannot overlook the arrival of the most significant paradigm shift in biology in the 20th century. It occurred, as you know, at high noon on February 27, 1975, when scores of molecular biologists gathered from the premiere laboratories of the world decided that the moratorium they had put on their own research with these techniques should now end. And a newly established committee from the NIH – called the Recombinant Advisory Committee, hereafter known as the RAC and still active today, but only in another area of gene research – established guidelines, which were promulgated.

When that happened, voices, some of them belonging to scientists, made high warnings of man-made cancers and new strains of bacteria. Such cries aroused the Congress, and before very long, at least 12 statutes had been proposed from the Congress or from the administration to manage the advance of molecular biology. Even environmentalists got into the action and were very eager to require the NIH to produce an environmental impact statement. And believe it or not, as a resource for the Justice Department attorneys who defended that suit, a document was produced that was an Augustinian exercise in calculating unknowable possibilities that would come at an unknowable incident from the use of this work.

Well, 20 years later, you know that none of these dread scenarios have run. And scientists and their institutions deserve credit. They also took compulsory lessons in civics and in procedures that would keep the public informed when we're going along together in pursuit of the unknown. I think these lessons will continue to be very useful as we consider ethical and commercial questions that are going to come out of the rise of this new technology.

Today, there are maybe eight or 10 products that are indeed very profitable, and we have the most romantic of all the possibilities, the possibility of gene transfusion, that has just begun to capture the imagination of many people with many degrees of interest.

But I want to assure you that here we come to a little bit of the crisis, and a natural change, I guess, in the nature of health research. Since 1977, when NIH was such a major supporter as compared to industry, we find that in these last several years, industry has begun to put more into health research than any other group.

This brings us to an extremely important point: the entrepreneurial drive exists in commerce as it does in the universities and in government, and the relationships between these sources of funds for health research are going to have to be constantly watched, because the direction of biological science is in danger of being skewed toward profit by all of these participants.

That brings us to an aspect of the commercial side of health R&D – that is, R&D, for so many years the main thrust, has been joined by a new *fin de siecle* structure, MM, for marketing. Marketing is absolutely as important to these companies. They're undergoing very rapid transformation, mergers, and acquisitions. The big hardware houses are trying to capture all the little software houses, biotechnology companies that are independent or almost don't exist. And that's the normal thing.

But these profits mean that the companies are changing themselves radically. They're also designing themselves into disease management programs. So your company X will stake out diabetes and bring its products to play on that disease, or perhaps on Alzheimer’s, or all the major diseases as a major means of competing in a market that is being seriously constrained by another MM movement, managed care.

So you see these companies in play undergoing many, many changes in their relationship to a field that is completely changing, after the default of the government in trying to capture some mode of health care. The private world has, as you well know, taken that over.

My last slide indicates what this means to NIH's whole research system, which has most of its activities in academic medical centers. If you note these major sources of income of those centers, most of them are in grave danger of change from managed care, already occurring in many hospitals. And if NIH should have a bad day at the budget committee, as it's been warned, then it will play a small role in structuring the sources of most clinical investigation.

So as we see this tremendous future created by biology, the ability to capitalize on it – to bring it to fruition and make it available to the people who long for cures and prevention – is fraught with new problems of change. Thank you very much.

NELSON: Thank you, Don Frederickson. I would like to highlight the remark that you began to make at the end of your fine historical statement. And that is, while the end of the Cold War has provided a major break in the continuity of our defense R&D, the growing concerns about health care expenditures are providing a comparable change for the biomedical research enterprise. It's a different world.

FELLER: Irwin Feller, Penn State. I'd like to ask you a question on the economics of the RO1 grants, which relates to the assumption that the federal government will pay the full cost of academic research. Could you describe in terms of NIH's maintaining the number of RO1 grants, what its policy has been on matching funds and cost sharing by institutions and faculty in the award process?

FREDERICKSON: First of all, cost sharing has not been a major part of overall granting. The indirect costs now rise up to about a third or more of the total cost of the grant. And there is a continuing jockeying between government accounting and the universities as to which share of those grants will be paid. At the present, there's a new cap on administrative costs to 25%, which is a major part of indirect costs, so indirect costs are paid, but neither side is satisfied that all the costs are met. Cost sharing just has not been a major feature of the RO1 grant from the beginning and even today.

NELSON: We have an interesting written question: "In 1994, [Senator] Hatfield (R-OR) proposed a tax or a set-aside on health insurance with the objective of funding medical research through that vehicle, and the question is, is that a good idea? Should it be pursued again in 1995 or 1996?"

FREDRICKSON: Mr. Hatfield has been in the news from our standpoint in a very good way for several reasons. This first proposal was an attempt to blunt the threat that Medicare funds that go to academic institutions would be somehow saved before the onslaught of many knives that have threatened to cut it. It's been a very big activity on the part of the academic medical centers to attempt to save some of that funding, and this was a proposal of how it might be collected, in terms of taxing the health providers of the private sector.

Mr. Hatfield very recently did something much more like the old Hill-Fogerty activities, in which he managed as chairman of the Appropriations Committee to slim down a threatened 10% cut in NIH over a period of years. That agreement has been reached. It may be at the cost of several other sciences, and we still have to see what happens when the final budget proposals and the appropriations all go to work after this holiday. Does that answer your question?

FELLER: It seems to me that the question is really meant to ask whether politically it's wise to have an independent tax on health insurance premiums, which provides an independent revenue stream to the National Institutes of Health, or whether that must always be a totally appropriation-driven process.

Those who argue for the independent stream say that in the political process the sufficient monies will never be there. Those who argue against it say that unless it is in the political process, the Congress will inevitably discount the independent stream in making its political decision.

FREDRICKSON: That's a good point, and I agree. And that question is going to continue to reverberate back and forth.

FELLER: And yet for the academic community, perhaps, and for the science community in general, there is a policy decision. Does one get behind Sens. Harkin (D-IA) and Hatfield (R-

OR), again, in ‘95 and ‘96 and/or does one say let it happen at the political process level? That's a real decision. It's not an academic one.

FREDRICKSON: I agree with that. It's a political decision, however.

GREEN: My name is Bill Green. I'm a former member of the House Appropriations Committee, and that may be a vain solution because it would not be the first time that monies in a trust fund were not spent by the Appropriations Committee, in order to stay within the limits that are given under the budget process.

That's happened to the Highway Trust Fund, the Airport Trust Fund, and a number of other trust funds so I would not want anyone to leave here with the illusion that simply creating another trust fund is a surefire way to ensure that you get the money.

NELSON: Thank you. We have a question here on paper that I take it as related to your earlier discussion of the, up to now, somewhat wondrous ability of the National Institutes of Health to have itself defined politically in terms of disease categories and yet to allocate its funds, in terms of relevant areas of science. The question is, “Do you think that the Balkanization of NIH into separate disease-based entities will ultimately help or harm the NIH budget in the long run?”

FREDRICKSON: Well, a long-lasting question. I'm certain, still today, that the fact that all these categorical institutes were created has meant more funds and more Congressional interest and more political interest or public interest in the National Institutes of Health than were it to be a great institute of general medical sciences.

As someone said wisely in the Appropriations Committee, “Nobody ever dies of immunization. Nobody ever dies of urology. They die of infectious diseases.” And that's why the institute actually was changed from its original name. I think that it's not a deceit, because the institutes are really held to the mat by the Congress in long appropriations that certainly exceed in time any other kind of appropriation in the government, in the Congress.

They’re held to the mat to make sure what they’re doing about diseases. I will always say that one of the remarkable things I learned in my first appropriation hearing was that Congressmen really understand basic research. They don't know what it is, but they know how important it is if you're ever going to get to answer practical questions. That has never been a problem in the political relationship between NIH and the Congress.

We had one bad period in ‘62 when Rep. Lawrence H. Fountain (D-NC) had a committee of government operations, whose staff demanded that Dr. Shannon send investigators into the field to be sure that researchers were adhering to precisely what they said they would do when they got their research grant.

But that's really the only time when that kind of issue has raised a great shock and anxiety. Shannon, buttressed by those two very powerful Congressmen on each side of him, tried to explain that freedom of science is the first principle upon which these grants and all these awards continue to be given.

NELSON: We have a question from Lorraine Lasker, which I think gets very nicely at this conflict between biomedical research funding and cost containment that I think is going to plague this system in the future. "The president of a particular hospital administrator society stated publicly that NIH research funding should be decreased until health care costs are decreased based on the theory that new technologies drive demand and increase expenditures. Medical scientists, in contrast, insist that biomedical research drives down health care costs by disease prevention and control through vaccines, antibiotics, etc. Question: In our cost-conscious environment, what is the best approach to this issue? In thirty second or less." (laughter)

FREDRICKSON: Very fine question. It's an excellent question. I believe the NIH system must be held about where it is, and not let it fall too far, because the priorities for where we're going with biology are largely set by the scientific community. They are not set by the manufacturing community, which has enormous investments and must make profit and cannot be expected to tend profitless projects as a balanced life. There will probably not be any gene therapy, for example, by any government operation, by any hospital without industry participation. So they're already there. But we don't expect them to fund basic science undifferentiated in the academic world.

NICHOLS: Rodney Nichols, New York Academy of Sciences. Don, I'm not sure you quite got at what I thought the questioner had in mind. What are the incentives for hospitals and medical centers when they're competing on basis of price and holding down cost to pay any attention to building a bridge to the research community and the training community? It appears that all those incentives now are being drenched by efforts on cost control.

FREDERICKSON: Well, that is true, Rod, but it would be kind of foolish, wouldn't it, for any of us to argue that we're where we want to be in health technology. We're maybe halfway there or a little beyond Lou Thomas' phrase. There's still so much to go, so much possible cost-saving advantage that it would be ridiculous to allow that to happen. And that would have to be a public response and a political response to that question.

Evolution of Industrial Research

NELSON: Thank you very much, Don Frederickson. Thank you very much, Bill Spencer. The discussion has just begun. Next, Nathan Rosenberg will be commenting on what's been happening in the field of industrial research. Well, national security and health are mentioned prominently and explicitly in *Science: The Endless Frontier.* From the vantage point of the present, there's a curious omission in that document, and that is any expressed discussion of the role of government research funding or government R&D support in the development of commercial technologies, industrial R&D.

As we've begun to discuss this morning, in fact, R&D has over the years seen an enormous amount of influence on civilian technology and industrial R&D through defense procurement and

R&D support and through the support of the NIH and the biomedical sciences. But nothing in the Bush statement on the whole set of issues that gradually came into view over the last 20 years regarding an expressed, explicit civilian technology policy.

Nathan Rosenberg will talk about what is happening to industrial R&D over this period of time. Nathan has more than just about anybody else I know contributed to the growing understanding by economists, social scientists, historians, about the way that technical advance occurs in industry. Speaking personally, I have learned so much from this person, and I think you will, too. Nate?

ROSENBERG: The lack of explicit references to research in the private sector in *Science: The Endless Frontier* is curious. In fact, the most important institutional innovation in the American economy in the 20th Century is the emergence of the industrial research lab and the extent to which scientific research has been carried out in industry.

This has not been, of course, a uniquely American phenomenon. Indeed, America did not invent the industrial lab, the Germans did, just as they also invented the Ph.D. degree; but the institution has been more widely diffused in the United States than in any other country, especially in the post-World War II years. There are around 12,000 industrial research labs in the United States today.

In industry, the purpose of research is not consummated unless it eventually leads to, or at least provides some additional guidance to, new product or new process development. In fact, private industrial research of a kind that academics would recognize as science constitutes only a very small fraction of industrial R&D, so that from this point of view, to discuss "R" without discussing industrial "D" would be very much comparable to discussing *Moby Dick* without the whale. The "D" is quite a whale.

A great deal of scientific research is performed in industrial research labs, including much scientific research of a fundamental nature, the kind that sometimes wins Nobel prizes. Such research is never an end in itself. It is, rather, an instrumental activity that must eventually be justified, if it is to be justified at all, by its contribution to a financial end – the famous bottom line. Decision makers in private industry look upon spending on research as an investment to be approved or rejected by roughly the same criteria, and by the same calculations, as an investment in tangible, physical capital. It is very important to be clear about this.

Business executives, unlike academics, do not look on scientific research as an intrinsically good thing, nor should they. Rather, profit-making firms (or firms that would like to be profit-making) perform scientific research when they can acquire necessary, useful knowledge in no other, less expensive manner. Even when senior executives at Bell Labs, before its divestiture, discussed the basic research that was being conducted there, they were rather careful to call it mission-oriented basic research. And while many academics might regard this term as a kind of an oxymoron, I disagree.

The most efficient way I can think of to approach the evolution of industrial research is to offer a perspective that is substantially different from the linear model. According to that model, which was invented in Vannevar Bush's landmark report, the causal sequence and innovation process begins with undirected upstream research in which scientists pose fundamental questions about the nature of the physical universe. New scientific insights are exploited by applied scientists whose further research extracts potentially useful concepts. These are passed along to product designers, who fashion new or improved products or processes. These products are then placed in the hands of production engineers and then go to marketing specialists. The process is rather like that of a relay race in which the baton is passed along to a succession of hands.

This model, however, in many instances fails to account for the history of the interplay of science and technology. The existence of a transistor, once it had been invented, gave rise to an enormous amount of subsequent fundamental research in fields such as classical crystal physics, dislocations in semiconductors, and surface chemistry. Notice that the linear model has no explicit place for a causal chain in which technological change generates fundamental science.

The basic limitation of the linear model is that it represents a sequence that, while it certainly occurs, should not be regarded as representative or typical. And there were historical reasons why the linear model was a more accurate description of the American situation in the first quarter century after the publication of Bush's report than it has been subsequently.

The nature of the innovation process can be much better represented, and a deeper appreciation achieved for both the accomplishments and the problems of industrial research, by reference to a so-called chain-linked model. This is discussed in a paper I wrote some years ago with Steve Klein, professor of mechanical engineering at Stanford.

Let me point out several distinct features of this model. First of all, the central chain of innovation, running from left to right, begins and ends with markets. The first situation on the left represents an awareness of a potential market and the second situation on the right represents the attainment of a commercial position in that market.

Second, instead of the single well-defined sequence of the linear model, there are a number of possible sequences. The idea for a new or improved product may be initiated in a wide variety of places. Third, and very important, scientific research capability does not simply sit at the top. It does not sit at the apex of a hierarchical process as the unique initiator of invention, as in the linear model. Rather it exists alongside a number of sources of technological improvement, including the existing stock of knowledge. There is a lot of interplay, and it is a very interactive world.

Scientific knowledge required for new product development may already exist. It does not necessarily need to be created by new scientific research. The research capability is sometimes invoked, but only when other sources are unable to supply the necessary knowledge. And it bears repeating that industry is not interested in science for its own sake. It is very much interested in innovation, but likewise not for its own sake; rather, only to the extent that innovation provides competitive advantages and therefore contributes to profitability.

Finally, feedback of all kinds constitutes a dominating feature of the model. The acquisition of economically useful knowledge is, by its very nature, the outcome of an interactive process, in which downstream activities shape upstream ones as well as vice versa. There are feedback lines from the market going back, including going all the way back to the consideration of potential new markets.

The influence of downstream upon upstream should not be surprising because of the inherently commercial criteria involved in industrial decision making. The choice among technological alternatives and the decision as to how much performance improvement it is worth acquiring involve commercial and economic judgements and not only technological criteria.

There are typically many ways to strengthen a bridge or reduce the weight of a new commercial aircraft design or improve the conductivity of an electric transmission system. The exchange of information among specialists in both upstream and downstream directions is therefore a central part of the firm's decision making process.

At the same time, a competitive marketplace involves frequent changes in response to the behavior of competitors, in addition to feedback concerning product performance and the consumer's reaction to the characteristics embedded in any particular product design. The essential nature of innovation and research in a complex and disorderly commercial world is at least in part complex and disorderly.

Against that backdrop, I want to turn to some of the events of the post-war years. In those years, industrial research in the realm of science played a major role in establishing and consolidating decisive American technological leadership in many product lines in international markets: aircraft, the jet engine in particular; the whole realm of electronics, especially computers; chemicals, especially petrochemicals and perhaps most especially pharmaceuticals; and nuclear power.

In the second half of the post-war period continuing into the present day, we see industrial research becoming increasingly myopic. One obvious index of this myopia is the gradual closing down of the central research labs in firms that had them, and a reallocation of research budgets to decentralized labs that are much closer to specific product lines. In effect, this shifted the focus of research to projects closer to the marketplace, those projects which offer the attractive prospect of shorter term payoffs.

The experience of Dupont and General Electric are obvious examples. Bell Labs has been similarly transformed in the wake of the 1984 divestiture of the operating companies. While this reallocation has been widely deplored outside of industry, it is not obvious from a commercial perspective that closing down central research labs has been a mistake.

Consider Dupont. Dupont's post-war strategy of focusing on its central lab in order to repeat its spectacular pre-war success in nylon, rather than acquire technologies from outside the firm as it had done very successfully in the pre-war period, was dictated in large measure by antitrust concerns. But this centralized search for new nylons, as the people at Dupont like to describe it, can hardly be judged to have been a success commercially. As it turned out, the centralization suffered from some rather grievous faults because it separated fundamental research activities from other essential functions of the corporation that are central to commercial success.

The central research lab was cut off from close connections with the marketplace, which can be fatal in industry. It was also cut off from the capabilities of the parent firm, so that in a number of cases, Dupont executives simply failed to recognize the considerable potential value of technologies developed by their own lab. Dupont was not necessarily unique in that respect.

An additional reason for the failure of the central lab requires that we look outside of the large firms and add a feature of the post-war American economy that contrasts sharply with the pre-war experience and with the dominant arrangements in other industrial economies: that is, new start up firms have played a prominent role in the successful commercialization of new technologies in the post-war period, especially when these new technologies have established entirely new industries.

This was especially true in the new electronics technologies such as semiconductors and computers. Currently it is going on in biotechnology. New entrants played a role of growing importance in the semiconductor industry, and especially in the new devices that were built upon the semiconductor.

Whereas large firms such as IBM played a dominant role, in IBM's case, in mainframe computers, new firms such as CDC, DEC and Cray achieved dominant positions in mini computers or super computers, as did Apple and Compaq along with IBM in micro computers.

That is an essential feature of the post-war industrial landscape, and it is central to the question of who captures the benefits of the new research, including the new research being conducted by the large commercial firms. The reasons for the more limited success of large firms include the high degree of labor mobility among firms, especially in industries that exhibit extreme localization as in Silicon Valley and along Route 128 around Boston; an active venture capital industry; and anti-trust actions against such giants as IBM and AT&T which led to consent decrees (both in 1956) mandating liberal patent licensing terms and so encouraging new entrants into micro electronics.

Perhaps above all, among the reasons for limited success of large firms is the multiplicity of large basic research establishments in universities, government, and a number of private firms. These establishments tended to serve as important incubators for the development of innovations that, frequently, simply walked out the door with former employees who established new firms in order to commercialize them. That is almost a way of life in Silicon Valley.

In addition to these factors, the inability of firms with large central research laboratories to capture the benefits of their own discoveries was compounded in the past 25 years by strategies of conglomerate diversification that may have clouded the commercial judgement of senior management.

RCA is an outstanding example of this syndrome. Its own basic research campus near Princeton, and it is interesting that they refer to it as a campus, made important research contributions to military and consumer electronics technologies. The firm encountered growing difficulties, however, in reaping commercial returns from its very considerable research capabilities, often because of poor strategic judgements made by a management that no longer had special expertise in the firm's traditional core business. RCA's decision to pursue development of the expensive video disc home entertainment technology was not only a very expensive failure, but at the same time led to a neglect of the firm's dominant position in color television receivers. As a result, while the Princeton research center, the Sarnoff labs, survives - albeit under new management - RCA has simply disappeared.

More generally, the sharp separation of R&D from other parts of the corporation, which was part of the strategy adopted by many firms during the 1960s, began to cause serious problems in the late 1970s and 1980s. The Palo Alto research center of Xerox and the Watson research labs of IBM in Yorktown were extremely productive scientific research laboratories. But other firms very often beat Xerox and IBM to the market with products based upon the research that was carried out by them.

The separation of product design and development from production and marketing that characterized many American corporations also became a very significant liability when Americans were forced to compete with Japanese firms that maintained a much closer integration of design and development with production and marketing, something the Japanese have been extremely good at.

I turn now finally from considerations of internal structure of the firm to the outside economic environment. There is very little in Bush's report on economic policy, but there is a strong focus on the importance of spending money for national defense and health, and also on the importance of federal financial support for the research universities.

The post-war decades saw government policies consistent with Bush's focus. These policies provided a framework within which industry not only could but did excel. But in the first half of the post-war period – roughly 1945 to 1970 – American industry was willing and able to commit sizable resources to scientific research because the new technologies that were supported by such research could be profitably sold not only within the United States but in international markets.

This profitability had a great deal to do with the fact that the major industrial countries of western Europe and Japan had been prostrated by World War II, and it's fair to say that the period up until about 1970 was indeed a period of American economic hegemony. Because of this hegemony, American industrialists were confident of reaping large research rewards by pouring large sums of money into scientific research, a central assumption of the linear model. But that assumption was far better justified before about 1970 than it has been since then, because eventually the other industrial powers caught up and newly industrializing countries started to appear as well, especially on the Pacific Rim.

With the ending of the Cold War, a major rationale that had served to support the immense federal commitment to research, particularly of importance to the military, was severely eroded. The fundamental policy question that we must now confront is how to reorient the infrastructure of federal support for research that is so essential for the care and feeding of the innovation process in the private sector. But I think it is important to face up to the fact that private industry is, under any set of realistic circumstances, unlikely to revert to the positive attitude toward research that it held so firmly in the first quarter century of the post-war years, when America did indeed bestride the world economy like a colossus. And we should be grateful that world is unlikely to return, because it was the product of the most devastating war in world history.

It was a far-sighted policy of U.S. government in the post-war years to rehabilitate the devastated economies of our wartime enemies as well as our wartime allies with policies such as the Marshall Plan in Europe and what I would judge to be a enlightened occupation policy in Japan. These policies were both remarkably successful, and I think they entitle us to at least a small indulgence in self congratulation.

At the same time, many of the technologies of the post-war era, especially improvements in transportation and communication as well as the increasing prominence of multinational firms, have assured that America's advantages as an innovator are likely to confer only short-lived commercial advantages. This awareness feeds back in a powerful way to dampen the enthusiasm of business executives for investing in scientific research.

I think it is essential that concern with economic policies in the years ahead should focus far less than is currently the case on large deficits in our international balance of payments, as if that were some kind of index of economic performance. Those deficits are the product of macro economic phenomena. Essentially, they are the product very low rates of savings on the part of American households and huge budgetary deficits on the part of the federal government. One hears much more about the latter than the former.

In touching on the low rates of aggregate saving in the American economy in the last couple of decades, I call your attention to another connection that is seldom made: that low savings translate into high interest rates. High interest rates mean that a borrower must heavily discount prospective cash flows that may lie off in the distant future.

To put it more directly, a low rate of savings is a direct and powerful source of the business myopia that is so widely deplored. Those who deplore business myopia, as I do, should also be strong advocates of pro-savings policies at the national level.

Instead of the obsessive hand-wringing over an apparent loss of international competitiveness, our real concern ought to be over the very low rates of productivity growth that we have experienced during the past 25 years. The declining commitment to industrial research may well have something to do with this productivity slowdown, but I think it is important to remain open-minded about its causes for two reasons. First, the years around 1970 represented a turning point in productivity growth, not just for the United States, but for all OECD member countries. In fact, the percentage slowdown in productivity growth in most OECD countries, including Japan, was in fact greater than in the United States. That is another story, but it strongly suggests that there has been a systemic cause at work in industrial countries. What is it or what are they?

The pertinence of this question is reinforced by my second reason, which is that the productivity slowdown occurred during a period of apparently very rapid technological change. Indeed the slowdown happens to coincide precisely with the introduction in rapid diffusion of one of the most spectacular innovations of the 20^{th} century, the microprocessor, which was actually introduced in 1971.

The microprocessor has led to the formulation of Moore's Law, postulated by Gordon Moore, which states that the computing capability that can be placed on a single silicon chip doubles about every 18 months.

The more serious, larger question is, how it is possible that an apparently rapid rate of technological change as epitomized by the microprocessor has coincided with the drastic slowdown in the rate of productivity growth? How can we account for Robert Solow's pithy observation that in recent years we have come to see computers everywhere, everywhere except in the productivity statistics? That really is worth wondering about.

In closing, I want to raise a question: when considering the innovative output of the industrial research community, rather than the decline in the input of resources to industrial research, is it obvious that the productivity slowdown reflects a slowdown in industrial innovation, or is there some other culprit or perhaps an assortment of other culprits at work? Are we perhaps overly obsessed with a high-tech fetishism? Is it possible that science and technology, and therefore research generally, have been less important than we have assumed, not only to the slowdown of the last quarter century but also to the rapid productivity growth of the quarter century before that?

I am not ready to offer a categorical answer to that question, but the achievements of industrial research by themselves are terribly impressive. Americans are obsessed with scientific research in the very specific sense that we expect it to provide quick, short term benefits with respect to problems requiring long term research commitments. But it is unrealistic to expect private industry to bear the main burden of society's commitment to scientific research.

NELSON: Thank you very much, Nathan. We only have a very short amount of time for questions. But let me take a question from the floor. Harvey?

BROOKS: I think it was your colleague, Paul David at Stanford, who pointed out a quite similar phenomenon to the one in information technology, with respect to the introduction of fractional horsepower electric motors at the end of the l9th and in the first quarter of the 20th century – that the productivity surge that finally occurred followed, by about 20 to 25 years, the large-scale introduction. His explanation as I recall it was essentially that it takes that long a time for the whole socio-technical system to really realize and absorb the advantages of a radical new technology. Perhaps you could comment on that.

ROSENBERG: Sure, actually longer than 25 years, Harvey, if you date the beginning of the electrical age with the setting up of the first central power generating plant, Pearl Street Station on the bottom of this island, in 1882. And if you look at the productivity statistics, it seems to take at least 40 years. It really isn't until the 1920s that what is going on in factories can be attributed to that specific innovation. And I think there's a great deal to be gained from that comparison.

Fundamental innovations really require fundamental changes in the social and economic system. In the case of electricity, it was relatively simple. It was a matter of redesigning the layout of the factories. You could now bring power anywhere you wanted. It didn't have to be concentrated on the steam power source or distributed by belts and pulleys. And you might argue that the introduction of information technologies involved a much, much more complicated interface between man and machines than electricity did.

And by the way, if that is an apt comparison, then in a sense I would regard this as good news because the productivity benefits of the computer are still down the road and ought to be coming online pretty soon. I think there's a great deal to that comparison.

NELSON: This question comes from Eleanor Barber: "If the isolation of research from production within company R&D can be so damaging to the effectiveness of that R&D, what does that mean for the relationship of university research to industry and for the industry bottom line?"

ROSENBERG: That is a big question. What we've experienced in the course of the 20th century is the development of a whole set of institutions that stood ready and positioned themselves to feed upon the research coming out of the universities.

I would argue that the emergence of the industrial research labs was heavily dependent upon the fact that universities were producing knowledge that industry could exploit. I would point out, which I haven't in my talk, that much of what industrial research labs were doing, and much of what they were intended to do, was precisely to monitor the research that was going on in the universities.

But it turned out that merely to monitor itself took a considerable intellectual sophistication, and certainly in the early decades of the century, I would argue that most of what was going on in industrial research labs – and to a considerable extent, it still is the case today – is that monitoring.

But it's an interface that I think on the whole the United States has been more successful in surmounting than any other country. On the whole, we've done a darn good job at precisely that. And I guess you won't be surprised to hear an economist say that in order to perpetuate that situation, industry must believe in the future profitability of the kind of research that it is very expensive simply to borrow and then convert into more directly utilitarian purposes.

But I think we ought to keep our perspective about that. The United States compared internationally has done an extraordinarily good job. Whether we will in the future is quite another matter.

NELSON: Thank you very much.

Evolution of Basic Research

COLE: This afternoon, we're fortunate to have with us Joshua Lederberg, who will be speaking about the evolution of basic research within the context of the framework produced by Vannevar Bush and colleagues. It happens to be an enormous pleasure for me to introduce Joshua Lederberg to you. We have few polymaths left in the world, and Joshua Lederberg remains one of those few, I should say. He always has interesting things to say, and he says them with real knowledge and authority and can speak on such a wide range of subjects that it never ceases to amaze me. He never yields to received wisdom without strict scrutiny. And he's an ideal person, I believe, to comment on aspects of basic research in the context of the structure of scientific research over the past 50 years.

I would like to point out, aside from him being a good friend and a person who always is interested in listening to opinions and ideas, that he has his bachelor’s degree from Columbia College and is one of the many Nobel Prize winners who have come from this wonderful undergraduate college that we have here. Of course, he also is president of Rockefeller University and has received innumerable extraordinary awards. It’s a great pleasure for me to introduce Joshua Lederberg.

LEDERBERG: Oh, thank you very much, Jon. It is always a pleasure for me to come back to what is literally my alma mater. I can't help but reflect that most of my teachers 50 years ago didn’t think I’d be addressing you here on this occasion. (laughter)

I could use at least ten of my 20 minutes in reviewing all of the accomplishments of basic research since World War II. Of course, any such recitation would heavily overlap the resumes that were presented by my colleagues this morning, especially from health research. In spite of, indeed because of, the applied mission that it has in promoting health, NIH has always given very strong emphasis to the support of basic investigation.

It proved to be rather difficult to devise far-reaching answers, for example, to cancer when we didn't have the foggiest notion what cancer was and, before that, about the dynamics of normal cellular growth. The most important pathfinder for cancer did not come initially from studies within the field but from work on pneumonia, with the discovery by Avery, MacCleod, and McCarty just over 50 years ago that DNA was the core of the genetic material. We then understood that cancer is a lesion of the cell’s DNA.

There're several lessons that one might reach from some reflection on that very hasty, I won't even call it an overview, the view that I just presented. Physics and chemistry had their revolutionary upheavals, their major discontinuities even before World War II, symbolized in a way by the crowning event of that war; the atom bomb.

In physics, we know the deep philosophical cleavages – one example being Tom Kuhn's original definition of a scientific revolution. That is, the change of paradigm that accompanied the introduction of the relativity theory and quantum theory. An era when the older and newer generations were just talking right past one another, and the issues of theoretical predominance had more to do with generational succession than persuasion by specific experimental data. That was Kuhn's original definition of a scientific revolution.

If chemistry had such discontinuities, I would place them in the latter part of the l9th century with the emergence of concrete theories of molecular structure. I suppose Kekulé might be a reasonably symbolic figure for that point. I don't see sharp discontinuities in the history of chemistry since that time but an enormous enrichment, accumulation, vitalization, and introduction of new technology. We now know the chemical composition of almost everything there is around. We're not mystified by structures of polymers and so on and so forth. But I don't see conceptual discontinuities having arisen since that time.

You don't always need Kuhnian revolutions to have extraordinary progress in science and technology. Perhaps some of you want to argue that point with me a little further on. But the organic chemist of 1901 could talk to the organic chemist of 1995 reasonably intelligibly. There is of course a considerable overlay of physical interpretation, the theory of balance, the physical foundations of the structure of molecules, and so forth, that might differentiate them. But that's a gulf nothing like that between contemporary and classical physics, for example.

Biology is somewhere in between, but 1944 really was a revolutionary shift, and whereas there was no DNA in biological discourse up to that time, there is no biological discourse without DNA at the present time. Some people might say, perhaps to a fault, we are seeing in some areas reintegration of what might at one time have been called the classical biological disciplines with the insights that can be obtained by the very relentless reductionism of an interpretation through DNA.

A further observation is that the most important discoveries, and the ones I know best in biology, came unbidden and were unexpected by-products of other inquiries. Avery and his colleagues were not looking for the nature of the genetic material. They were certainly not looking for DNA. They were absolutely startled when this came out as the result of their inquiry. They were not looking to solve the problems of cancer. They were no dummies – they did have more inkling than they indeed led on in their first publications about the broader biological significance of what they were into – but they didn't even come from the disciplines that had been the main carriers of the tradition of that kind of research.

They did not regard themselves as geneticists. And geneticists didn't regard them as geneticists either. People remarked about the prematurity of that discovery, but it was no more than ten years before it became part of the solid rock of biological discourse.

But over and over again, one could find that real discoveries – now here, I'll have to speak primarily from the areas I know that are in biology – came as part of a process of inquiry and very often by no means the things that were defined in the project.

God forbid, you might find something that was not in the research project that you had outlined when you went after your grant from NIH or the NSF, that you got something unbidden. And you even dare hint that it might be what you're really after when you write up a project. God forbid, a reviewer will say the author has not demonstrated the feasibility of the experiments that he intends to make. It's a line probably repeated more often in pink sheets than any other single one that I can think of.

Another historical lesson: NIH, the National Science Foundation, and all the other wonderful descendants of Vannevar Bush's vision did show that research could be fertilized with federal funds and its pace greatly accelerated.

We discovered the institutionalization of research as a major governmental responsibility and function, and the universities and other institutions are more than eager to come in as the instruments for those kinds of development – but always with an uneasy alliance of academic, commercial, and governmental interest. Sometimes in concert, sometimes in conflict, sometimes sub-texted with the divergent interests of the different parties to those agreements.

Most of this research was justified in the halls of Congress by its instrumental applications, that research would give us better tools for dealing with health, would provide technologies that would be in support of industry, would provide one form or another of economic payoff – and that promise has been, I think, quite reasonably fulfilled. No one has argued that historical investments in these programs did not pay off in the national and global economy many times over.

As the scale of those investments increases, as the world gets ever more competitive, and as people question some of the tacit values that were being furthered, more questions are being raised. And we have a pretty commonly shared sense of gloom about the slash and burn incentives that seem to be going around in Washington at the present time.

But there's a background of a relationship where social goods were generated by parties who may have had a widely divergent set of motives for pursuing them. Universities, researchers accepted funds in very large measure because they were the most effective way to garner support for more basic lines of research, and if they didn't say it, others before them correctly articulated that one way or another, there would be enormous positive fruits.

But one has to have concurrence that the technologies that develop are ones that everybody really wants. There we come into elements of social valuation and many other considerations that can lead to some controversy. Most obviously in a field of national security and what many political factions, what the majority of folks in this country regard as absolutely necessary for development in self defense – our enormous deterrent capacity, our secure base in military technology – others viewed as highly pernicious, dangerous to the future, of ourselves and the planet and so forth.

Most of us here think that prolongation of life and alleviation of disease is a positive good, but there are always nuances and wrinkles that excite enormous controversy when we come to detail. The technology of prenatal diagnosis and its implied warranty for preemptive abortion would be an outstanding example.

Less problematic is life extension, but this has presented many, many subsidiary problems in its wake. The aging of the population and its economic consequences. The costs entailed in later life in order to have the application of those technologies. The sheer fact that many lives today will only terminate at somebody's will. That if the life support systems are maintained, there will be an object that was heir to what had been a human being and often great controversy as to what are the legitimate criteria by which to suspend that support. Well, that's an ethical burden that many people find unbearable.

It may not always be voiced in those terms, but it's a problem that has touched innumerable people and will touch most of us as individuals at some stage. And there's a great deal of ambivalence, quite fundamentally, about the benefit of having that power, of not having it out of our hands to make those kinds of determinations.

So, scholarship today is in some areas somewhat on the defensive. The culture of science has managed to survive but is under renewed assault – by the negative pressures and the temptations for operating in a direction that's more keyed to immediate technological benefit rather than the follow-the-path-as-it-takes-you kind of atmosphere that I recognize as more typical of basic scientific scholarship.

The university should be the seat of critical examination of all of these issues, starting with the engines of discovery. It's increasingly difficult to do, when support for science moves from the elicitation of creativity to contract for performance and when the tether that the institutions have is ever shortened. The subtext of economies in indirect cost recovery is in fact greatly reducing the autonomy, the freedom to maneuver, and the opportunity for internal choice on the part of our institutions. If all of the resources of the institution have to be put out there as the equivalent of the matching funds, although expressed as unrecovered indirect cost, there’s just nothing left over for things to be done from the institution’s own initiative.

There was a culture of scientific scholarship that I've described as one of the ideals – I wouldn't even want to call it the by-product – that I view as the central product of scientific scholarship. I didn't say the fruits, I didn't say the technology, I said the culture.

The conduct of science elicits and nourishes systematic inquiry of appeal to nature, of appeal to experiment in the settlement of dispute, of disclosure and exposure to a community, and I will borrow your colleague’s and mine Robert Merton's phrase of organized skepticism as a way to winnow out the truth. There's an implication that there is such a thing as truth, which others may have some skepticism about but which the scientific culture really does put very high on the standards that we try to fly. And we do try to maintain a system of publication of scholarly skepticism and of discourse as I think probably the greatest value of sustaining science at an academic base.

There are other ways of settling problems, of reaching conclusions. We see them in the political world, we see them in the courtroom, and I think it behooves us to ask to what extent our very civilization depends on the maintenance of the scientific culture as at least one of those elements.

I'll just close with a very brief summary of what some of the problems are, or their genesis that I wrote about 30 years ago. You can tell the vintage of this writing with such medieval phraseology as "his" as a pronoun and the word "man" and so forth. I was moved to bring this out by a piece I just saw by Freeman Dyson in the *New York Book Review* a couple of weeks ago, "The Scientist as a Rebel." I had found it very provocative, and it resonated with some of the things that I have here. He attacks reliance on the transcendence of science in one breath, and then talks about the obligatory role of science in knocking down the icons in another, which I do regard as a transcendent function.

But this was embedded in the paper, which tried to provide some sort of systematic overview of what grievances might be lodged against scientific and technical development on the part of some larger public. There is a fundamentalist strand of reaction to technocracy. It's being discussed this week in a symposium at the New York Academy of Sciences that Paul Gross has organized. It was headlined in The New York Times, "The Flight From Reason."

And I admit it runs fairly deep. A lot of people are frightened about the pace and the implications of scientific inquiry, that scientists are rebels, that they undermine many accepted icons in institutions at many, many different levels, and this was just an attempt to try to summarize how many there are.

We don't hear very much today about the conflict of science and theology. I guess that battle has long since been completed, except the underground is coming back again in some other versions. But many religious organizations – having gone beyond what they needed to do and staking their credibility on historical interpretation, the literal definitions of the history that's described in the Bible – from Galileo's time and right through the present have found scientific argument as prejudicial to their credibility.

I've alluded to the biological system and the threats to our equanimity or understanding or sense of how things had been, the novelty that's entailed in life and death being so much more in our hands today – if they were perfectly so, there might be less grievance, but there are always problems at the margin. There's also the issue of how the depredations of the environment are a possible consequence of large-scale technological development.

There's a lot of argument in what used to be Marxist circles about whether new technology favored the capitalist exploitation of labor or was undermined by it, and we can be attacked on both sides for those kinds of development.

And of course, science as a transnational enterprise cuts across our tribal allegiances. If you don't think so, remember McCarthyism and the way that Oppenheimer was dragged against the coals – and the notion that science was important to technology and technology was important to national defense, so there was a subversive aspect to the cosmopolitanism of science. And it was attacked in exactly those terms by most of the totalitarian regimes. That Hitler thought there had to be a unique science, and Stalin thought there ought to be a unique Marxist science. Cosmopolitanism was a very nasty word and was enough to send people to Siberia or worse during such intervals.

Well, this is a very bare outline, but I was just trying to remind you how much is shaken by scientific progress in the very name of solving problems, intellectual ones, those of understanding, and technological ones. The old order is changed, and a lot of people don't like that. I think that we need to have very careful tempering of our own views about the beneficence of scientific advance – and when we push technologies all around the world, try to think who does get hurt as well as who has benefited. And I think the universities ought to be the places that are free for that kind of discourse. Thank you very much.

COLE: We'll now entertain some questions from the floor.

LEDERBERG: I'd be particularly interested if anyone wants to quarrel with my contrast between physics and chemistry in discontinuities.

HOLMFELD: I'm John Holmfeld from the Dana Foundation. I don't have a comment on that. I have a comment on the more general themes first raised in the December meeting and that Dr. Cole raised with us again this morning, namely, that the purpose of this three-part series of meetings is to rewrite the Bush rationale. And I think a useful way to think about that is to think about the last paradigm shift or the last shift in rationale, which occurred as a result of World War II.

I think it's fair to say that prior to World War II, science, with some exceptions like agriculture, was considered a highly intellectual but rarely applicable discipline – the classic case, Albert Einstein. If you were a high school student in the '30s or early '40s, you were told about Einstein and you were told about relativity and all these things, but you were also told that those things were done on the top floor of the ivory tower, and it was highly unlikely that that kind of physics would ever find an application.

And then came World War II and the proximity fuse and the mass production of penicillin and the biggest one of all, the atomic bomb. And in the public mind, the atomic bomb was a result of physics, really of Albert Einstein's work. You can quarrel with the details of that, but I think it's fair to say that since then, the rationale for federal government support for science has been that basic research will produce technological or practical payoff.

Now, the Bush rationale came with two important qualifiers. One was that you could not predict ahead of time which basic research project would in fact yield a payoff in technology or new drugs or whatever. And the second qualifier was that for those that did produce a payoff, you could not predict how soon it would come. It might come next year or it might come 30 years from now.

Now, what I want to suggest is that that public understanding, especially in Congress, has not changed. There is still a large degree of faith, even among conservative Republicans such as Chairman Robert S. Walker (R-PA) of the science committee, that basic research is worth doing and is worth spending taxpayers' money for that reason.

So, to push that argument a little further, I would raise a question about whether the concern about coming up with a new rationale is something that comes not from the public, not from the Congress, but from within our own community – and that it has arisen largely because the rapid growth in funding that we saw in the '60s, '70s, and '80s, when things like NSF went up 15% to 17% a year, has ceased.

The NIH director told his advisory committee that, after a year and a half in Washington as director of NIH, he had changed his mind about what he could do for NIH and for biomedical research, and he had come to realize that resumption of growth was highly unlikely in the foreseeable future. Well, if that is the case, then it seems to me that we in science need to look at this business of finding a new rationale in terms of a steady-state kind of science. Varmus (NIH Director 1993-present) said to his advisory committee, "We have to get used to the fact that there no longer will be growth of scientists in our laboratories and there will no longer be growth in funding available for extramural research grants."

Now, to what extent can that concern about a steady-state funding level be married to the kind of perception the public has. I don't think there's anybody among the public or in the Congress who says, "Let's wipe out support for basic research."

Now, you suggested that basic research would be justified in terms of preserving the culture of basic research. But funding along those lines could only probably be justified in terms of 10% of the basic research budget. All of the rest it is still going to be justified in terms of the technological or health benefits that come out of it. End of comment.

LEDERBERG: Well, I don't think we said different things. I agree with you. I think the only way to sell federal investment in research is to point to tangible material payoffs in the same currency as what we're asking for, wheel money in and money out. What I'm hoping is that the other values that I think many of us in this room would also share are just not thrown out the window in the process and that we try to pay some attention to the elements of process and of aspiration that can contain them.

If you give in too readily to the technological-fruits notion, then you also have to ask, "Well, who is going to decide what term of investment is an issue? Who is the wisest person to know how to get the best and the most technology out?" and, "Are you sure that these are technologies that everybody really does want?"

But the basic answer to your statement is, yes, I think the only rationale that Congress is likely to accept at a level of funding larger than it's willing to offer the Endowment for the Humanities is going to be for the instrumental fruits. It still leaves us as the guardian of the other truths that have to be protected.

SILVERSTEIN: Josh, the one area in this whole dialogue that is to a large measure under the control of the universities is the training of the next generation of scientists. We can choose to admit or not to admit, we can choose to train or not to train. I would say there is a raging debate, certainly in the life sciences community today, manifest at the National Institute of General Medical Sciences recently, and on the training grant level just last week, where some of our colleagues are saying, "We're training too many scientists, and we ought to reduce that number."

And there are others, like myself, who would say, "Training is an opportunity, why should we deny the next generation the opportunity that we enjoyed and we made good use of?" I wonder from your perspective, are we training too many scientists, especially in the biomedical sphere? Do we have a moral responsibility to encourage those who would choose this area of investigation, to discourage them, or to remain value neutral?

LEDERBERG: I'm not sure I've reached the final thought I'm going to have on that question, but let me try a few notes on that score. My own belief is that the economic payoff from investment in training and in research is undiminished and that there are grave social losses that will ensue from a failure to recognize that point, but that's my own opinion on the matter.

I don't know the extent to which we can convey that to Congress at a time when so many of its current constituency feel that they're anarchists, that government should be abolished. So, despite our own views as to what would be the best for society, how do we cope with the circumstances now presented?

I have to make the following observation, which cuts across a number of levels of education. One thing for sure – and it's a surety that has led to some of our current economic predicament – is we live in a global economy. There is no way we can separate issues of our productivity from those of competing economies. And that says that if we are to have a competitive advantage in the price of labor, then that labor has to be more productive than it is in other countries or we'll not be able to sustain the economic structure to do that.

There will be a migration of jobs for the cheapest bidder, so that the quality of our labor force becomes an absolutely necessary pre-condition, in my view, for the maintenance of our standard of living. And I'm not sure if graduate education is a key issue in that spectrum, but I don't think it should be excluded.

On the question of what kinds of jobs will be available, they won't all be the high levels of academic research. But there will be technically skilled jobs requiring ever deeper capacities if we are to be able to be competitive in our labor force with that of the rest of the world. I think most of us would say, the first thing that needs to be cleaned up is grade school and high school education, but it's not the only thing. So, those are considerations.

What might we have downstream? Well, maybe something not too far from what it was when I was growing up. Many are called, and few are chosen. Perhaps not everyone can expect to get the kind of job that they would like to. Maybe we need to find ways of at least selecting out the most motivated and the most able for those kinds of situations.

I think the market will take care of itself. If there are not jobs enough available for graduate students, you're going to get fewer applications to graduate school. Exactly where else they're going to go, I'm not absolutely certain, because things are drying up all over in a certain sense. I think we need training grants to be sure that we have a way in which the best talent still has an opportunity to rise to the top, and the rest of our system should support that as well.

Universities at large – I never mean this one, but every other one – are over-extended, have overgrown. We know that every teacher's college has become a university and wants graduate programs. We know there has been serious over-inflation in those dimensions, and perhaps some way needs to be found to reduce that. Eventually the market will do it, but that's got lots of lags in it, lots of externalities, and it may not be the most intelligent approach.

COLE: This question says, "It strikes me that an important element in the public disaffection with scientists or at least with academic scientists is that the other key role that scientists play, namely that of teacher or educator, is seen by the public as suffering as the result of the so-called overemphasis on research. The public in short is not so much against science as it is against poor education, for which science is blamed. Would you please comment on this and beyond that, would you care to comment on whether academic scientists have in fact failed to include in their students the 'culture of science'?"

LEDERBERG: Well, don't expect consistency in public reaction. We're going to be damned for making too much technology and damned for making not enough of it at the same time, and sometimes by the same people.

If our younger faculty could have more autonomy in making their own decisions about how to spend their time – if they're working in a highly competitive project environment, though, they have no alternative but to put every imaginable energy into getting their grants in, getting their projects approved, and I think education at the margin does suffer from that monomania.

I think highly skilled researchers, most of the ones that I know, are eager to teach but not too much, providing the right balance between their allowance for doing research and being accessible to students so that a university can come through on what it promises its undergraduates: an access to these skilled minds.

COLE: I think that will end Professor Lederberg's comments. Thank you, Josh.

Discussion Roundup Rapporteur Summaries

Professor Samuel Silverstein
Professor Richard Nelson
Dr. Joel Yudken
Professor Harvey Brooks
Dr. Lewis Gilbert
Dr. Susan Cozzens

Science, Technology and Human Health

COLE: What about some of the conclusions, observations, comments, witty remarks, and other insightful ideas that emerged out of the breakout group sessions? We'll go through the agenda in the order that they're listed. We'll first hear from Professor Sam Silverstein, who was the rapporteur for the group on science, technology, and human health.

SILVERSTEIN: Thank you. Let me start out by an observation. I don't mean to be unfriendly because I have great respect for all the people in this room, but this conference is basically older, male, and white. And the people we're going to be talking about and who are basically concerned about what we're talking about are predominantly going to be younger, probably not exclusively male, and of course, of the broad demographic and racial makeup that's going to be this country in the next century. Having said that – and that we desperately need to engage them in the conversation – let me tell you what we talked about.

We decided to talk about the last three questions and basically ended up talking about structures a great deal. But no matter what structure we talked about, it basically came down to one discussion and that discussion was money. So, the structures got dissipated into "How do we fund this, and how do we fund that?"

As we focused on the National Institutes of Health and incorporated the CDC, we included the ideas that outcomes research was important, that population based research was important, that behavioral and psychosocial research was important, in addition to test tube-based bench biomedical research. Given all of that, there was still very substantial disagreement about the way money at the National Institutes of Health is divided among study sections by institutes – is there appropriate attention given to each area of research?

Certainly, there was a lot of head nodding up and down in agreement that there was inadequate attention to clinical research. And there are great tensions – that won't surprise you – great tensions between clinical and basic research scientists about what is inappropriate balance. How do you judge quality? Who should be in the room?

You have to have clinical and bench-based scientists sitting around the same table deciding what's good science, what ought to be funded, what are good questions. I think we haven't worked that out within our own communities effectively at all. There's disagreement about whether the councils at the NIH are effective mechanisms for setting priorities. I can tell you, I actually voiced that concern when Bernadine Healy (NIH Director, 1991-1993) had her meetings on the National Institutes of Health about how to go forward during her term. I asked, What's wrong with the councils as decision-making bodies? I think we need to look at that very carefully, and I think Harold Varmus is giving that some attention.

The group looked at past biases, specifically inattention to women's health issues in clinical trials. How is it that they got ignored and are there ways in which we can avoid such blind spots in the future? I don't think any solution emerged from the group, and I asked members of our group to make a comment when I finish, if they have a solution to suggest.

We moved on from this discussion of money and structures to a somewhat different discussion and that was the political approach. And I was quite surprised at least to see that there was even substantial disagreement there. Some believed we ought to continue aggressive advocacy for above-inflationary funding increases. And others say we'll be fortunate to be flat funded, and there was considerable waffling as to whether flat funded means an annual increase for inflation or just flat, no growth at all, with the budget losing buying power every year. There was disagreement about whether we'd be taken seriously by the politicians if we asked for more.

I didn't hear a lot of fervor in the room that said, "This is the right thing to do. We ought to be asking people to set priorities." It seemed to me that scientists in their advocacy position in this area, at least what I heard around the table, were essentially saying, "Well, we're not very comfortable with the data either." And I don't know how it goes in the bargaining around your kitchen table, but around my kitchen table, that's a sure loser.

There was no consideration given in our discussion to how much or whether we ought to have others help make our case, the pharmaceutical and bio-tech executives, et cetera. And I think there was total disagreement about whether a rising tide will lift all disciplines. There are those who say that even if we have more money, their area won't receive the attention it deserves, that I need to advocate specifically for my discipline, and we need to have structural changes in order to get effective change.

In the area of training, there was, I think, good agreement that clinical researchers are more of an endangered species than ever. There was considerable disagreement about whether physician scientists in training should be paid on an M.D. pay scale or on a Ph.D. pay scale.

It was unthinkable that we should charge graduate students in the health sciences tuition and that we shouldn't pay them a stipend. No other country does this. We'll send our students elsewhere, foreign students will stop coming here. It was also pointed out, of course, that these students, while they're our students, during the latter years of their thesis work, are actually contributing to the research and therefore payment to them is appropriate since they are actually performing work and research.

There was disagreement about whether we ought to limit the number of trainees we accept and discomfort about letting market forces work as to what happens to those trainees. But there was an unwillingness, I think, to confront alternatives to market forces. What other ways do we have of doing it? To illustrate the difficulty we have with our view of the pipeline, if I can call it that, I put up this diagram, which shows an B.A. coming in one end and a doctoral researcher coming out the other end.

This is the diagram Bruce Alberts has been using at the National Academy of Sciences. It shows the alternative careers, industry, teaching, et cetera. And what I've pointed out to the group is that in the eyes of the most articulate and thoughtful spokesman for alternative careers – who is, I think, Bruce Alberts – the alternatives don't appear sprinkling out the end of the pipeline, splaying out equally. They appear as alternatives that show you falling out of the pipeline.

We're in real trouble in our community – I believe, and I don't have group agreement here – in valuing other careers, other avenues. There certainly was agreement in our group that there ought to be better opportunities to pursue a wider spectrum of training during doctoral training in the biomedical and behavioral sciences.

The journalists among us pointed out that there aren't any heroes who are researchers, investigators in the biomedical sciences. The one chosen was Jonas Salk. That's a long time ago. Are there some new heroes that we could point to? And the journalists said, "If not, why not?" And we discussed not only the fact that there aren't popular heroes as seen by the press, but we as scientists had a hard time identifying them.

We also discussed the fact that some universities do a good job of creating heroes, that they try to expose their faculty to the press and to the knowledgeable members of the lay public as often as possible, whereas other universities, do that very infrequently. That failure to use the faculty and to make the faculty, if you like, visible figures, diminishes us in substantial ways.

I pointed out that a Nobel laureate at a major western university didn't even know the governmental affairs people at that major university. We talked a little bit, right towards the end, about cooperation and collaboration within the university and the way the tenure system discourages that until an individual has tenure. And so, rather than working as they do in the very best industries, where people are encouraged to cooperate and collaborate in many instances within the university, until you have tenure, the incentives for collaboration and cooperation are small.

While there were very good feeling and willingness to work together, it indicated to me that many of the divisions that I have seen in our science community are deep and strong and that we have a great difficulty in pulling ourselves together to advocate for very simple things – such as the NIH in total as opposed to the subdisciplines.

I hope I've not been negative in representing what the group says. What I focused on are the tensions and disagreements. And I've done that deliberately because, in fact, those tensions and disagreements, I believe, are as strong as ever. They are nevertheless important to recognize because if we don't recognize them and begin to work on them, I'm not sure that we can represent ourselves to the larger public as having a defined agenda. Let me stop. I'll take questions or comments. Those of you who were a part of this discussion may want to say that I've seriously misrepresented you, if not, I'll be surprised.

BROOKS: It wasn't clear to me from what you said whether you really addressed in your discussion the issue that came up somewhat briefly this morning – that is, how the whole change of emphasis in the priorities of the health care system is affecting the research agenda, and particularly whether the emphasis on high-tech medicine has been an important factor in the escalation of medical costs and to what degree the researcher is responsible for that phenomena.

SILVERSTEIN: We didn't address that topic. What we did address is that with managed care and decreases in Medicaid and Medicare funding, there would be substantially fewer resources for clinical studies and for research and education from the contributions of patient care dollars at medical schools. And that those dollars represent a very substantial input into the research and training activity. Conservative estimates are between a billion and $2 billion. And Don Frederickson's numbers, I think, are higher than that. We did not discuss at all whether technology is driving medical costs or is a solution to medical costs.

Let me just add some data that I have collected in that area. We now have documented $70 billion annually in medical care cost savings from advances in biomedical research supported by the National Institutes of Health. And in addition to that, there's another $90 billion annually in revenues generated in non-medical areas by advances supported by the National Institutes of Health. So, if one looks at the NIH as a contributor in some way to the U.S. economy, I think one ends up with a very positive number.

Science, Technology and Economic Change
and
Science Technology and National Security

COLE: Thanks very much, Sam, and also to members of your group. Next reporter we will hear from is Dick Nelson, who is going to talk about the discussions which took place on science, technology and economic change. Dick?

NELSON: Our group was joined by the group concerned with what was going on in the military R&D area. And I think that turned out mostly to be a fruitful combination of people and interests because it quickly became apparent that a lot of the trends in both areas are intertwined. As Nathan Rosenberg observed this morning, the history of accomplishments and then of troubles and difficulties is interesting and important to understand if you are to understand the nature of the current discussion.

While there is scarcely any mention of a specific civilian technology policy by Vannevar Bush, I think for understandable reasons, nonetheless, the performance of American industry in the early postwar years was really quite spectacular, economically and in terms of American industry forging to the forefront of technology across a very, very broad arena of activity.

As Bill Spencer observed, at a number of the areas where American industry became extremely strong, in civilian competition and products, the industry was drawing significantly on military R&D and procurement, which was clearing the way, as it were, for advances in very major civil applications. Also, the rise to preeminence of American industry in the health arena, particularly in pharmaceuticals, drew extensively and draws extensively on the National Institutes of Health programs.

By the middle 1970s, a number of different things are happening, and the context begins to change. One important thing that began to change is that American industry began to lose its dominance across a wide range of economic activities, and companies based in other countries began to achieve technological parody and, in some areas, even technological leadership over the U.S. At the same time, there was a significant slowdown in income and productivity growth in the United States – a slowdown close to virtual stagnation that's been with us for 20 years or more.

At the same time, by the middle 1970s, the military no longer was the dominant demander of high-tech products in a number of the important fields, nor was it the dominant R&D funder in the areas of electronics or civil aviation. There have been a number of studies showing that spillover to the civil economy from military procurement and systems R&D began to diminish significantly at about that time.

One can observe by the middle 1970s a number of interesting developments that shape the current context. And here we had quite a bit of discussion as to what was happening and why. For the first time in the postwar era, you began to get clamorings for an expressed civilian technology policy arising in various portions of American industry and in the polity – clamorings that in many cases have met quite hostile responses ideologically on the part of some folks and quite enthusiastic responses from other folk.

Closely related to that, there is the beginning of dual use. The military becomes increasingly aware that the preservation of strong military procurement and R&D capability is becoming dependent upon having American industry earning a considerable portion of its profits by selling commercial products, not simply military ones. And therefore it's in the military's interest to spur directly American industry. At the same time, American industry or portions of it are clamoring for some sort of an expressed industrial technology policy.

The group as a whole had a considerable amount of difficulty staying on one particular topic, I think in large part because people had different things on their minds and wanted to articulate them. But as I reflected on what was happening in the conversation, my impressions were forged that there is a real tension and perhaps a real contradiction between, on the one hand, the attempt to develop a coherent civilian technology policy that would involve funding of civil R&D aimed to help industry and, on the other hand, the political perceptions about what the real American economic problems are that color that particular discussion.

The problems that kept on bubbling up in the discussions of our group were general economic problems. They were associated with that very slow rate of growth of real income that has occurred over the last 20 years or so. They were associated with the perception of significant diminishing of the number of good, high income jobs that are available to the American work force, with strong perceptions that a lot of the problems were associated with sharp competition from companies abroad.

On the one hand, we're taking away American jobs and, on the other hand, there is the increasing internationalization of American business – with American business in any case setting up plants abroad rather than in the United States. Jobs were moving away, and when the discussion was focusing on those economic problems, there was a scrambling around initially for a set of research or technology policy ventures or steps that might deal with them.

And there was evident frustration that the solutions perhaps weren't to be found in the science and technology policy area. On the other hand, from time to time, the discussion went over to topics that were much more easily associated with issues of civil science and technology policy.

We had a quite interesting conversation – fragmented, but I think the fragments stayed together in a relatively coherent way, focused on the proposition that while government R&D was plateauing out, civil R&D in the more important industries is continuing to grow. And that the United States soon would find itself in a position similar to the other major industrial countries like Germany and Japan, with a very large share of the total R&D effort being privately financed.

And then the discussion was, what's going on in the private sector such that industry would come together and, cooperatively with government, reestablish long-run industrial research? Or would the situation continue to be a short-term rush?

So, there was a considerable amount of discussion of policies, potential developments that might occur there – and then that string would be broken by commentary to the effect that those kinds of policies really didn't seem to be all that interesting and that the real problem in the United States had to do with slow productivity growth and lack of growth of good jobs. The question then became, what can you do in the United States to essentially preserve jobs or expand good jobs? And again, there was this frustration that the policies that one grabbed for really didn't seem to be in the science and technology policy arena.

I came out of the discussion with a strong impression of non closure and indeed disjunction. It seems quite likely that we will have two discussions in this arena, and they won't be joined. One concerns civilian technology policy and a wide range of interesting and important issues that are connected. But that's not a discussion that will attract much in the way of political or popular enthusiasm. Rather, the discussion will turn to the question, what can we do about jobs? What can we do about productivity growth.

You're not going to get a coherent view for new civilian technology policy, because the more you look at what you can do with those policy instruments and the more you look at the kinds of economic problems that seem to be, maybe appropriately, grabbing high-level attention, the more the two topics don't seem to have that much to do with each other.

COLE: Thank you, Dick. Since we merged the two groups, I'm wondering whether Joel Yudken would like to add anything at this point.

YUDKIN: Dick has made my job very easy, so I just have a couple of things.

There was very little interest in the military group, myself included. I was really glad to be part of the economics discussion. I think it's interesting because despite their downsizing, the military is the largest single federal source of research and development funding. And it's going to continue to play a major role, including industrial R&D.

We did not talk about institutional issues relating to the Department of Defense. We did mention briefly that the military's emphasis in R&D is going to be increasingly away from strategic weaponry. It's going to be more on a whole different type of military systems, increasingly reliant on very advanced high-technology components which they want to buy at affordable prices from the commercial marketplace.

That's part of the civilian-military integration effort in the current administration, something the Pentagon really wants to promote as part of procurement reform. The perception that the military is falling behind rather than being the leader is another thing that's driving the military to push military/civilian integration.

Dual use will continue, although at a smaller scale, to be an important component of the military's R&D program because of its perceived utility to the defense system.

An important topic that we did not talk about was the federal laboratories: their fate, their relationship to industrial R&D, their role in cooperative research and development, and so on. One person did raise issues that had to do with the national security aspects of technology, but we really could not get into that. And so, that basically sums most of what it was that we talked about.

COLE: Thanks very much, Joel. Our next reporter is Dr. Lewis Gilbert, who is the coordinator for the Global Systems Initiative at Lamont-Doherty Earth Observatory. He was the rapporteur for science, technology, and the environment.

Science, Technology and the Environment

GILBERT: The environment group was a small one. To keep things moving along, I'm going to stick to what I have distilled as the two first-order points and some sort of positive considerations for how those points might be addressed.

The group was dominated by physical scientists, and in that respect we tended to have an implicit interpretation of the environment that had more to do with climate and larger regional issues than what considerations people might have regarding end-of-pipe pollution controls and that sort of thing.

It was noted early on that environmental issues span a very wide front – that to address the environment, we need to have strength in a wide variety of disciplines including physics, chemistry, biology, geology, and economics. In addition to a wide disciplinary front, environmental problems have a time scale that is long compared to political and economic structures in our society. These two considerations – that environmental issues are inherently interdisciplinary and they have time scales that are long compared to what we're used to – make environment problems different from a lot of other problems.

As a result, we came to the second first-order conclusion which was, the structures that exist currently are very badly broken.

Currently, environmental research is funded by a wide range of federal organizations including NSF, NOAA, EPA, NASA, and the USDA. Because this environmental research has grown up in a wide variety of institutions across a wide range of agency missions, there's very little coordination of environmental studies, and a large number of environment efforts are actually in conflict with each other. So, in addition to being highly fragmented, we often have conflict between existing missions and goals.

Another problem with the current wave of doing business has to do with litigation. Environmental problems are often synonymous with litigation.

In addition to this, the reward structure within the university is inappropriate to pursuing environmental work across the range of efforts that is necessary. University and industry cooperation is hampered by penalties to industry in particular. It was pointed out that it's often in an industry's disinterest to learn about environmental issues related to that industry. If they don't know something, it's better for them than if they do know it. It can cost them money if they know things.

In addition to that, the reward structure within academia makes it difficult to pursue interdisciplinary and long-term research projects, the two sorts of things that are necessary if we're going to address environmental research, science, and technology as it needs to be addressed.

Finally, in relation to things being fragmented and separated, there are cultural differences between science and engineering and between science and the policy community. This makes it difficult for what advances we make in science to be communicated or to be solved from the engineering point of view.

So, those are the two fundamental things about environment: that it takes a wide range and a broad view in order to address adequately, and we need to put in place structures that can do that sort of thing.

So, recommendations. We need to find mechanisms that can coordinate research across a wide range of disciplines and agencies. We need to be able to support efforts that range from single investigator-driven classic science to large-scale efforts such as the general circulation models which have been so successful at government labs. We need to be able support interdisciplinary work.

We need mechanisms that isolate basic science and scientific advances from politics. We need to find some way of addressing the penalty-to-know factor. And finally, we need to put in place education and training programs that can produce people who can work in interdisciplinary and multidisciplinary environments so that communication across disciplines and across sectors of our culture can be facilitated. Thank you.

Institutional Arrangements in the Organization of Science

COLE: Thank you very much. Our next reporter is of course Harvey Brooks, who's going to talk about matters that were addressed on the institutional arrangements in the organizations of science. Harvey?

BROOKS: Well, we had a very wide-ranging discussion, which included a rather unproductive discussion, in my opinion, on how you allocate resources among disciplines, so I won't say very much about that, although it occupied a disproportionate fraction of our time.

First of all, we began by making a list of the important institutional and organizational innovations post-World War II. And I would just list these quickly. First, the invention of the broad-based research contract based on level of effort, which has become the backbone for the support of university research and which is more or less really unique to the U.S. system. Incidentally, I might say that each of these innovations and advantages has a downside, which I'll come to later.

Second, the idea of indirect cost recovery, also a unique American invention which some of our members thought was an invention of the devil.

Third, the development of large multidisciplinary laboratories, particularly those that were started under the Atomic Energy Commission but have become very much of a feature of the U.S. scene.

Fourth, the evolution of the system that was very different from what was envisioned by Bush, of a multiplicity of agencies and sources of support. And we believe that this pluralism, while it has some appearance of wastefulness, has been a major factor in the health and vigor of U.S. science.

Just to give one example of why I think this is so, we have actually had both very good intramural government civil service laboratories – such as the intramural laboratories of NIH and the National Bureau of Standards – but also contract laboratories. I believe that the coexistence of contract laboratories and civil service laboratories has made both types of laboratories more healthy and more productive. Particularly, it has helped to keep the civil service laboratories from becoming too bogged down in bureaucracy and micromanagement. Unfortunately, it has also resulted in some growth of micromanagement of the major contract laboratories.

Fifth, again fairly uniquely American is peer review of research grants. Incidentally, that was something not at all envisioned by Bush in his famous report. It essentially stemmed from the biomedical research community but soon became applied to essentially all basic research in universities.

There was also some feeling, however, that the peer review system had gotten overly expensive and overly bureaucratic, with too much emphasis on promised results rather than track record. In fact, I quoted an estimate I had seen that if you allocated all the costs realistically, it cost $1.00 to spend $1.00 of a typical NSF grant – if you really compensated for the time spent by people in refereeing grants and in preparing grants.

Another invention which has gotten very little attention and yet has been a very important feature of the 1980s – studied by the group at Carnegie Mellon – is the university industry research centers. The study identified more than 1,000, with an aggregate budget of $4.2 billion, which receive about 31% of their support from industry as opposed to 7% industry support for academic research as a whole. This is a invention that has really gotten surprisingly little attention in the discussion of science and policy.

Somewhat of a downside is the increasing role of patents and proprietary research in university research, largely as a consequence of the change in policies that occurred, I think, with NIH in the late 1970s. This has had great benefits in better coupling university research to industrial research, but it has also, I think, produced undesirable restrictions on the dissemination of university research, which needs careful monitoring.

Also mentioned was the large number of consortia, networking arrangements particularly among universities. There was mention of a few other things that have become important to the system such as Internet and the bottom-up coordination that really results as an important by-product of the peer review system. Finally, there is the growth in public acceptance of science museums as a mechanism of informal science education.

Coming to the problems, I've already hinted at some of them. One that received a great deal of discussion was the future of the national laboratory system, particularly that part of it whose mission has declined as a result of a phasedown of the Cold War.

There was a great deal of disagreement as to the future of the system. Although everybody agreed it was a great national resource, just how it could be used and coupled to the rest of the system in a permanently sustainable and effective way was the subject of considerable debate.

There was considerable feeling in favor of a certain amount of privatization, probably in separate pieces, of the laboratory system, with much more of a downstream operational mission, particularly in those public laboratories that already have considerable downstream activity.

I've mentioned the micromanagement problems that beset both university research and the research of the national laboratories. Let me give my own impression of the state of the present system. The system of research in the U.S. grew up during the Cold War period, and the defense and the atomic energy parts of that research system were about five to seven times as research intensive as the average of economic activity in the U.S.

After phasing down of the Cold War, how do you divide the reduced research and couple it to the expansion of the downstream activity? That I think is the essence of the problem. Now, you could argue that the U.S. expenditure on industrially oriented research is only about half that of our competitors as a fraction of GNP. On the other hand, it exceeds that of our five largest competitors in absolute magnitude. And one of the questions one has to ask is, which is more important, the absolute magnitude or the percentage of the GNP?

Using the percentage of GNP as an index suggests there are no economies of scale in this activity, which seems to be absurd. At the same time, it's a little hard to know exactly how to look at this. Well, I think that's enough.

Science, Technology and Society

COLE: Thanks very much, Harvey. Our final rapporteur, reporting on the group that discussed science, technology, and society, is Susan Cozzens. Susan?

COZZENS: What a job, last speaker of the day.

Although the discussion questions for the groups were written in terms of field of science, we realized that that it wouldn't really do for our group to talk about science, technology, and society as a scholarly field and its accomplishments and problems. So, we altered the questions to some extent and to talk about the relationships among science, technology, and society to characterize the post-World War II period.

We ended up trying to focus our discussion in three areas. One, a sort of broadbrush evaluation of those relationships – what were the accomplishments? What were the problems that emerged? Second, through what organizational structures and processes had those relationships come to be mediated over the post-World War II period, over the last 50 years? And finally, how has the research community responded? How should it respond?

As to the questions that we were posed, is the system as it exists for mediating those relationships broken? If so, how should we fix it?

Like the last group, we ended up pointing out that much of what we're looking at in terms of the relationships among science, technology, and society over the last 50 years can't be attributed directly to Vannevar Bush or the framework that he laid out in *Science: The Endless Frontier*. Harvey mentioned internal aspects of the system that were never anticipated by Bush, like pluralism and the emergence of peer review.

We brought up some external forces that had also impinged and made things work out in ways that the original Dr. Bush would never have anticipated. The transformation following Sputnik, for instance, and the kind of scientific developments that Joshua Lederberg described for us, the sheer growth of the system itself, which surely was not envisioned by the founding father, and then the various ways that the world has changed around the research system, the current configuration of social problems, for instance, where they're located and how they fit into the rest of society, the economic competition, that sort of thing.

It means we can't lay either credit or blame really at the feet of Vannevar Bush – or not too much of it – in the picture that we drew. In terms of evaluation of this relationship, having talked about this for probably an hour and a quarter of our two-hour time, we really had to come to the conclusion that there was no simple way to evaluate this very complex set of relationships – that there were accomplishments in some places, problems in others. In some places, the accomplishments and problems are together, virtually inseparable.

One of the reasons this is difficult to do is because so much of what science has done over the last 50 years is now embodied in technology. So if we look at the public view of science, there's much about science that has changed everyday life that's not recognized and attributed back to science by the public.

The example that was used is that automobile manufacturing at this point has improved a great deal in terms of efficiency and the safety of vehicles by the use of supercomputers in the a process. Yet, there are very few of us who would attribute features of our automobiles specifically to the high-performance computing and communication initiative that the federal government has taken.

Again, the complexity of the relationship comes out as a kind of ambivalence of the public toward the whole science-technology complex. The public may be positive on things like improvements in their quality of life and at the same time fearful of the changes in values, with a sense perhaps of social disintegration that may be vaguely tied to what's going on here. And that ambivalence also then gets reflected in media images and popular images of science.

On the one hand, we have the mad scientist who stays up all night, neglecting his or her family to invent ways of blowing up the world. And on the other hand, we have the medical scientists rushing in and saving lives in various ways. And you can see both of those things out there.

One of the accomplishments that we did attribute directly to the Bush framework was really its long lastingness – it has stood the test of time and laid the groundwork for a kind of long-range view of investment in scientific knowledge and the scientific mode of inquiry for the country.

It's given the kind of innovations that we just described and has been flexible enough to allow a whole range of fields to emerge and rise. One example that we gave was actually the field of science, technology, and society itself and its manifestation in schools as well as in university programs. The framework has been flexible to allow a lot of improvement in media coverage, for instance, of science during the last 50 years, the media obviously being an important mediating institution between what scientists themselves do and how the public sees them.

Finally, it certainly laid a good basis for educating new scientists and engineers – although there are the “haves” of the system and the “have nots.” There are people who have felt that they are incapable of competing, who have been left out and therefore don't get the full fruits of the society that we've created with science and technology.

It was pointed out that maybe this system has taken a long time to produce any benefits for the social sciences, in contrast with the situation for the physical and life sciences. Some members of our group pointed out that the original Bush framework tried to protect the autonomy of science, which is a good thing but can also lead to a lack of attention to what happens downstream from scientific activities.

And in some senses, things may have worked almost too well as scientists worked their way into the political system. They organized as lobbies, which contributed to the growth of science and government support for science, but the flip side is that it can also lead to the perception of science as just another special interest group within the political system.

What are the structure and processes that have led to these results? We've talked about funding structures, pointing out that there may be some inflexibility built into those structures in terms of disciplinary boxes, but that the system has responded in some ways as well. The media is obviously this kind of intermediate structure of the educational system.

In particular, we ended up coming back again and again to the fact that politics has come to permeate this relationship in ways that Vannevar Bush would not have anticipated. And there are a whole variety of forms in which politics appear, including the very strong influence that individuals can have in particular developments. We talked about George Bush and his administration's stance in relation to AIDS education.

And politics of course has highlighted the problem of the disparity between the very long time scales in which we're talking about research playing out into society and the very short time scales in which most politicians operate. We pointed out that product cycles are very short as well. So, what's happened over this 50-year period is that a number of tensions have been set up in the long-term thinking around science.

The question was, "Is the system broken?" I think, by and large, our group felt that, no, it isn't broken. The fact that we're sitting here having this discussion is probably an indication that it isn't broken. We played with the "broken" analogy a little bit. We said, "Well, maybe it's bent. Maybe it needs a little bit of re-alignment in certain ways. Maybe we need to better convince Congress of the worth of science. Maybe there's a little too much bureaucratization in it, that we could clean out the engine so to speak. Maybe there's just a ping in the engine."

And of course that was the amount of the university overhead that goes to administration. That would of course be the ping.

To switch analogies rapidly here, we did end up talking about the fact that the contemporary system may be a bit like a gem that has a hidden flaw – if enough pressure is applied to the gem, it could actually split apart and then really be broken.

Of course, the pressure that would be applied is cutbacks in resources, and the hidden flaw is that, under those circumstances, this wonderful lobbying expertise that has been built up in the scientific community could be applied to setting disciplines against each other; to essentially in-fighting within the community over a shrinking pie.

We ended up optimistic on that. I think, by and large, this group said, "Well, there is another possibility out there, and we think that's at least as likely as that outcome." And that is, enough flexibility in this system that the various groups involved can recognize the inherent interdisciplinarity of the challenges that scientists are facing and hang together in the end. And we ended on that upbeat note. (applause)

COLE: Thank you, Susan. Well, in closing this second of the three-part conference series, I just want to thank each of you for joining us here for this day's session and some of you for joining us really for two sessions, the one in December and now. And we hope you all will return in the fall.

I certainly want to thank as well, the speakers who have presentations here today, and the discussion rapporteurs for not only shepherding the discussions but summarizing them very well this afternoon.

Where do we go from here? Well, I think that we have reasonably well set the stage for the critical third effort, which will be a two-day session. We will be talking about the future of the national system of innovation, possibilities of choices and structural modifications and weaknesses in the system, perhaps preparing ourselves for a report but also perhaps preparing the nation for a serious look at some of these issues sponsored by some national group.

One question that arose early this morning: “Suppose we were re-living the Vannevar Bush report and it's 50 years later. Who would write the letter, and who would it be written to?”

We weren't altogether clear what the response was, but I think that what we will want to consider is the ways in which there may indeed need to be modifications. What are the choices that we have? What are some of the political and social contexts for those choices? We would like very much to have your ideas about what might be included in the program in the fall.

Columbia has been very pleased to be able to provide the financial support necessary to bring this conference off and to develop it. We think it's an important subject, something that we think is of great value to not only the university but to the scientific community and the broader community concerned with science and technology and the nation's future. I thank you very, very much for joining us, and we look forward to seeing you again in the fall. Thank you for coming. (applause)