Opening Remarks

George Rupp
Jonathan Cole

RUPP: My name is George Rupp. I'm delighted to welcome you to this third and final conference in the series commemorating the 50th anniversary of Vannevar Bush's *Science: The Endless Frontier*. Many of you who attended the first two conferences will know that those sessions focused on the history of Bush's policy design and on the impact of that policy design for American research and development.

Today, the focus shifts to the future, to an examination of what science and technology can or should be in the coming years. As we begin this look ahead, I'd like to take just a few minutes to underscore a point that we probably all agree on but that seems to me ominous nonetheless. The point is that developments in the relationship between government and universities, changes that are underway right now, threaten to undermine what has been a remarkably successful interaction since the close of World War II, in the sense the first two sessions of this conference celebrated.

The changes can be characterized as a transition from a confident partnership intent on seizing opportunities to a stagnant or declining enterprise with a focus on downsizing or cost shifting. That is admittedly a somber way of characterizing the transition, but I think it's not an inaccurate one. This transition is of course not confined to research. Government support for student aid and medical education and health care reflects the same pattern. In each case, costs are shifted from the government to non-government partners.

As we all know, the post-war recognition of the value of the government/university partnership resulted in growing funding for research and a research establishment preeminent in the world. But the years bore as well the seeds of a dilemma, bred by the very success of this partnership. Together with the new ideas and the new applications of those ideas came a rapid increase in the number of trained scientists who now seek support and an expectation that government support would continue to increase indefinitely.

The leveling of research funding in recent years and dire forecasts of significant declines in the coming years are the clearest indicators of the change in the government/university relationship that I've described. But other less obvious forces are also at work here. Pressures to reduce indirect cost recovery and increasingly prevalent instances of cost sharing by universities have the effect of shifting costs from the government to the institutions that perform research.

Whatever our views on the advisability or feasibility of cutting government spending, shifting costs to other institutions is not reducing expenditures. It is instead – to use the rhetoric of the cost cutters against their own approach – a form of unfunded mandate. And it has the effect of undermining partners in the research enterprise.

What can we do? How can we work together to mitigate the impacts of this changing environment in government/university relations and to re-establish an effective public sense of research as a means of seizing and realizing opportunities as it was during the decades following World War II?

First, we must re-double efforts to articulate the importance (BREAK IN TAPE)

RUPP: … in the pursuit of knowledge and in the education of scientists and engineers. Individual universities, consortia of universities, and coalitions of universities, scientists, and industrial leaders are hard at work at this admittedly difficult task.

Second, we can and should resist the insidious progression of the shifting of costs from government to universities and other research institutions. Decision makers must be made to understand that those shifts cannot be expected to occur without substantial negative impact on the research effort itself and on the institutions that perform research. Cost shifting is not cost cutting.

Third, we at universities must re-think the ways we seek to enhance our academic and research programs. For decades, such enhancement has been presumed to be pursued through growth by the addition of faculty and research staff and research programs. That much change. It must change to a strategy of enhancement through consolidation and integration. A strategy of drawing together and building upon already developed strengths instead of simply adding new programs.

At most universities, these strengths are now fragmented and dispersed. This isolation and insulation of individual scientists and departments must change. Happily, the need to link disparate elements of the university coincides with an increasing recognition of the importance of interdisciplinary collaborations in the conduct of research. More and more, important research issues lie at the boundaries of established disciplines or even at the area between them and require the efforts and expertise of groups of scientists from multiple disciplines.

Two brief examples of recent initiatives here at Columbia illustrate this point. The study of neurobiology and behavior now involves research efforts, many of them collaborative, of more than 40 faculty members in ten different departments on both this campus and our health sciences campus. A new university-wide Ph.D. program ties together the various elements of this field. That's one example.

Another example is our emerging initiative to engage global and environmental issues more effectively. To that end, we're in the process of establishing the Columbia Earth Institute. This Earth Institute will connect research efforts in the Lamont Doherty Earth Observatory, the Center for Environmental Research and Conservation, which itself already pulls together multiple research efforts both on this campus and in other New York institutions, the Geology Department, which we're now renaming the Department of Earth and Environmental Sciences, the School of International and Public Affairs, and the Business School.

Now in both of these instances – the brain and behavior and the Earth Institute – we are drawing together widely dispersed strengths to address issues that can be investigated more effectively if we overcome tendencies towards separation and duplication. We cannot but be apprehensive as the magnificent research enterprise built in this country over the last several decades is threatened. We all know that this enterprise faces substantial changes in the coming years. The focus of this meeting is on those changes.

What will they be? What should they be? How do we get there from here? I think you will agree that the organizers have gathered a group of speakers and panelists who by virtue of their experience and expertise can shed light on these and other pressing questions that confront us all. I am therefore delighted to welcome you and I look forward with you to a stimulating and productive conference. Thank you for coming.

COLE: Thank you, George, for launching us. I want to thank the approximately 30 speakers and 300 participants for joining us in this third part of our conference series, "Science The Endless Frontier, 1945-1995." For many of you, I know this is really the occasion for a reunion, and we like that and we hope that the conversations among you will shed light on the issues at hand.

Let me speak very briefly about the context in which we have placed this third session and indeed all of the sessions of this conference.

About three years ago, as the 50th anniversary of the publication of Vannevar Bush's *Science: The Endless Frontier* approached, a number of us interested in science policy at Columbia and other institutions across the country thought that it would be appropriate to hold a series of conferences that attempted to do really three basic things.

First was to consider in analytic terms the origins of the extraordinary work that helped to shape the discourse and the policy that has produced some of the most glorious scientific and technological achievements certainly since the 17th Century in England, probably in all of human history. What were the origins of the Bush report? What were the debates shaping it? What were the causes of the formulation of the elaborated role of the federal government in shaping the future of science and technology in America? What aspects of the report were implemented? What was changed as a result of debate and Congressional action?

We were fortunate to have at that first meeting on December 9, 1994, some individuals, such as Harvey Brooks, I.B. Cohen, and Bill Golden, who participated actually in the birth of the Bush model for national innovation. Some of them are here today.

The second conference held here on June 9, 1995, had the explicit aim to think analytically and collaboratively about the historical achievements and failures of American science and technology as it has operated within the framework of the Bush manifesto over the past half-century. To analyze the strains that have developed in the alliance or partnership between the federal government and research universities and to identify the causes of what have been perceived widely as a crisis over the terms of that partnership.

Third, we come to this very rich and full two-day conference. From the outset, we believed and said that this conference would pose the most difficult set of questions and problems for an extraordinary group of presenters, panelists, and active conference participants.

This third set of sessions is intended to be more prescriptive. We are now looking forward and trying to formulate ideas about how problems in the current national system of innovation can be solved. We are trying to look toward the next half-century and, within the context of a very changed world from that which confronted Bush and his colleagues, develop ideas for the next phase of science policy in the United States.

We have asked a truly distinguished group of individuals to present their ideas for the future of science and technology policy as it relates to specific areas of the institutional landscape. Unfortunately, many of the political leaders who had hoped to participate today had to withdraw even quite recently. The elections must be getting near.

This specific set of issues that we face today and tomorrow relate of course to the most fundamental global question and that is, is there a need for a major or radical shift in the national system of innovation? Is the Bush framework truly exhausted? Are the social, economic, and political conditions of globalization and internationalization without a well-defined military threat so different from the context in which Bush worked that the system needs fundamental restructuring?

In short, is the system really broken in fundamental ways in the sense that continuing along the current path will lead to the loss of America's preeminent position in the development of science and technology? Or, are we really discussing tinkering with the Bush model in light of significantly new conditions? But tinkering that does not require fundamental changes in the values or structures that were designed and built 50 years ago.

Beyond these issues, we ought to be addressing the question of mechanisms. If the system is at substantial risk, or if it simply needs a far less dramatic tune-up with a few new design features, what are the political, social, and scientific mechanisms required for achieving those changes?

Beyond these global matters of science policy, there are other issues that need to be addressed: the public perception of the value of science, the nature of discovery and the linkage between science, technology, and social welfare. The level of scientific illiteracy in the United States has reached alarming proportions. It needs to be addressed or the field will be ripe for the growth of very strong anti-science movements. So we have much to do.

We are under no illusion that the outcome of this conference series will be Bush II, but we do hope that the analysis and discussion over the next two days will set the stage for some concrete proposals for changes that may be needed in the national system of innovation.

I want to thank those of you who have agreed to make presentations and those who will be responding to those presentations. We do very much appreciate you joining us here at Columbia. We realize how busy you are and take your presence here at Columbia today as indicative of your sense of the importance of the subject that we are discussing. We thank you for your concern and your interest.

I also want to thank the nearly 300 members of the audience, who I trust will become active discussants following our panel presentations. Many of the most interesting moments of the first two sessions by the way came during the give-and-take in the periods of discussion.

Finally, I want to give special thanks to my three collaborators in the formulation of the conference series – to Professor Richard Nelson and Vice Provost Michael Crow, who will share the task of introducing our speakers, and to an extraordinary graduate student, Chris Tucker, who played a very active role in generating ideas for this conference and in seeing that they came to pass.

We're delighted that Columbia University, which has benefited greatly from the partnership between the federal government and the research university and has in some significant ways contributed to the achievements of science and technology that are the consequences of that enlightened policy, can host these conferences. Now to our busy program

Following the format established in the first two conferences, we'll keep the introductions of the speakers to a bare minimum since you have materials that give you longer descriptions of their many accomplishments. It is now my great pleasure to turn the podium over to Professor Richard Nelson who will introduce our first set of speakers. Thank you and welcome.

Design Area One: National Innovative Systems and Global Science and Technology

David Mowery
Eugene Skolnikoff
Edward Steinmuller
John Zysman

Moderator
Richard Nelson

NELSON: We have as our presenters David Mowery and Eugene Skolnikoff and as our panelists Ed Steinmuller from MERIT and John Zysman.

This first session is focused on the tension between two striking developments that have occurred in the era since the Vannevar Bush report, developments that have intensified significantly over the last decade. On the one hand, nations have become increasingly self-conscious about their own national innovation systems, whereas on the other hand, the system of science and technology and the web of interactions between the participants have become increasingly global.

David Mowery, our first presenter, is Professor of Business and Public Policy at the Haas School of Business at the University of California.

MOWERY: Thank you, Dick. Very pleased to be here and honored to be asked to open this conference with some remarks on the interaction conflict between international interdependence and domestic science and technology policy.

Obviously, much of what I say this morning will be familiar to many of you in the audience since so much of it reflects insights that I have learned from reading or working with various members of this audience. I think that certainly some historical context is useful to try to lay a foundation for an understanding of the issues we now confront as we move out into the next 50 years after the publication of Vannevar Bush's important report.

The extent of the transformation launched by World War II and the Cold War in the science and technology system within the U.S. reflected the vision of Bush if not his specific organizational recommendations. Indeed, perhaps the most enduring and important legacies of Bush for the post-war period were his report less its recommendations than its vision, but also his development of the device of institutional overhead as director of OSRD (Office of Scientific Research and Development) during the war, a major pillar of the university-government partnership that had propelled this system.

The system that developed in the decade following the Second World War was, as most of you know, highly de-centralized and reflected the operation and funding priorities of individual federal agencies rather than the more comprehensive science/scientist-driven priority setting and allocation mechanism that Bush outlined. Nevertheless, this uniquely American system did produce significant domestic economic payoffs, reflecting the operation of a number of factors during the 25 years following 1945: the importance of military technology in industries such as computers, commercial aircraft, and electronics; the wide gap that characterized the international economic, scientific, and technological landscape between the U.S. coming out of the Second World War and other industrial economies. This was reflected in part in a much slower pace of international diffusion and exploitation of the results of scientific research, much of which was performed in the U.S.

In addition, the early post-war period's spawning of important economic benefits from domestic investments, particularly by the federal government in science and technology, reflected policies that inadvertently or not – in the area of anti-trust, intellectual property rights, procurement – were highly supportive of the growth of new industries in the high-technology sector, particularly in the United States.

Finally, I think the political underpinnings of this policy structure within the U.S., dependent as they were in large part on the Cold War, imposed a set of priorities that gave a secondary weight to purely economic objectives and payoffs. I would argue that they also countered pressure on the system for purely redistributed objectives and politics within both the Executive and the Congressional branches' resource allocation decisions in the area of R&D funding.

Beginning in the 1970s, again as most of you know, we see a series of trends developing that impose severe pressure on this U.S. domestic system and begin to induce change that comes about in a rather sporadic, piecemeal fashion and change that by and large I think reflects continuity more than significant breaks between Republican and Democratic Administrations, reflecting perhaps the fundamental nature of many of these trends.

First, obviously, is the very rapid – and I would underscore the term rapid– internationalization of the U.S. economy, which spans the flow of goods in trade, investment, and technology. And if you simply look at the numbers, this economy has undergone a much more rapid transition from a closed structure relative to international flows of goods and technology in the mid-1960s to a structure that by the mid- to late-1980s is substantially more open – a doubling, for example, of the share of GDP accounted for by imports and exports.

Related to this is the significantly increased importance of foreign markets for U.S. producers of high-technology products in areas such as commercial aircraft, semiconductors, and the like and the increased importance of foreign sources of technology and components and similar inputs to the products of these industries. Another change agreed upon by most observers is a decline in the economic significance of the flow of technological applications – technology benefits, if you will – from military to civilian R&D and procurement spending.

Finally, in the wake of the oil shocks of the 1970s and other structural changes, factors not yet fully understood, we have a transition from a period of what by historical standards were relatively high rates of productivity and income growth to a period of much lower growth in productivity and incomes. In response, we see a series of changes in the ground rules and in some of the terms of the debates over domestic science and technology policy that begin to spill into and affect the interactions between U.S. and other industrial economies' S&T policies.

Particularly with the demise of the Cold War and slower growth in the economy, the terms by which the results and the payoffs and the outflows of federal science and technology programs were judged begin to change. Rather than being judged in terms of contributions to some mission of technological competition with another superpower as well as direct contributions to defense, the payoffs, the benefits now are judged much more in terms of their economic dimensions. Indeed, I think this shift in the criteria from more geopolitical, more purely defense driven to economic has been aided and abetted to a great extent by statements from leading political supporters for federal R&D spending as well as statements from leading beneficiaries, people like myself, university administrators, researchers, making the arguments for the near term economic benefits that flow from basic research spending.

So domestically, the nature of the criteria for evaluating these programs has changed, and that has important implications for the ways in which the interaction between the domestic and the international science and technology systems within the U.S. and other industrial economies are now judged and the politics through which the policies that influence these links are formulated.

And very important, I think you find as a result of the internationalization, the economic integration of the U.S. and other industrial economies, a much tighter link being forged between trade policy and technology policy and science policy. What we find particularly as we move into the Uruguay round of multilateral trade negotiations, for example, is that on the agenda of the multilateral and increasingly bilateral negotiations in which the U.S. trade policy community is engaged are more and more instruments of domestic science and technology policy. Subsidies, intellectual property rights, even to a great extent foreign investments, and competition policy are increasingly becoming important issues in trade policy.

So the trade policy agenda now spills much more into the domestic science and technology policy agenda and vice versa. With this as background, I think I would single out at least five broad challenges to U.S. policy makers and policy makers in other industrial economies in trying to manage within this new context and trying to address the changing demands and the changing criteria for evaluating these programs and arguably maintaining political support for many of them.

The first is that we have seen in response, certainly in the U. S., to the changing interaction between military and civilian technology development a shift in a number of defense-related R&D programs, from support that sees as its primary target the strengthening of military technology and applications to a much broader support for both civil and military technology and applications. This reflects a recognition, in many cases well founded, of the changing direction of these technology spillovers – formerly flowing from military to civilian applications, now in many sectors flowing from civilian to military applications – and a change in the importance of competitiveness in civilian technologies and markets for the economic viability of many suppliers of defense-related components.

In response to this, we've seen – beginning under the Reagan administration and moving through the Bush and Clinton administrations – the growth of R&D funded by the Defense Department, whose objectives are to support both sides of the house. This raises one of the first important challenges that policy makers face: preventing the increasing importance of the so-called dual use programs – support for R&D in civilian as well as military basic and in some cases applications research – from opening very large loopholes in existing international agreements on subsidies and procurement by invoking a national security rationale.

This is one part of the challenge to policy created by the growing importance of these dual use programs. A second part is that certainly the historical record, at least through 1990 or so, on the success of using military programs in procurement and in some cases in R&D to support civilian technology development is very mixed at best – and I think negative in the view of many, if we look particularly at the experience in many European economies.

A second major challenge is trying to prevent what has become a growing chorus of political demands that U.S. science and technology programs yield economic benefits that are captured by U.S. taxpayers. Policy makers face a real challenge in trying to prevent these demands from resulting in a profusion of what are in most cases ill advised and in many cases mutually inconsistent and contradictory barriers to foreign access. These restrictions create bad precedents for retaliation by other governments to U.S. multinationals operating offshore. In many cases, the ultimate effect of these restrictions are undercut or in come cases perhaps reversed by the private actions of U.S. and foreign firms.

One thing to keep in mind here is that governments remain sovereign, but in some dimension their sovereignty is modulated or mediated increasingly by the strategic behavior of multinational and transnational firms. So the issue of trying to prevent the balkanization, the erection of barriers to the free flow of scientific and technological information, is an important challenge and is one that I think remains to be addressed in a more systematic way.

A third challenge goes to the issue of the changing criteria by which the programs of federal support for science and technology are judged in the domestic politics of the 1990s – which is to say, trying to create a more realistic and perhaps a better informed view of the sources and the channels through which the economic benefits are realized. In many respects, we as a science community have been somewhat disingenuous in overselling the near-term economic benefits of basic research. In response, U.S. policy remains heavily tilted toward support for the creation of intellectual assets. But regardless of how many restrictions or laws or formal instruments of protection are imposed, these assets remain highly mobile internationally. Therefore, the emphasis on support for their creation in S&T policy does need to be balanced by a recognition that the bulk of the economic benefits from many of the technologies flows from their adoption within the U.S. economy.

And to the extent that U.S. policy can achieve a better sense of balance between the objectives of creation of intellectual assets and support for their adoption, I think we can begin to address some of the underlying issues between international interdependence and domestic support for S&T. And we can also develop domestic policies that do not result in the imposition of political demands for restrictions on international flows of scientific and technological information.

This implies some important responsibilities for universities as well. We have seen a view expressed that the U.S. universities and federal laboratories are somehow treasure chests of industrially relevant technology, commercialization of which is facilitated by the establishment of strong intellectual property rights and the creation of offices of technology licensing. While these programs may contribute to commercialization, this is a grossly over-simplified view of the channels through which university-based research results flow into the domestic economy. In the long run, advocating this view undercuts some of the broader sources of political support for the university research enterprise.

Fourth challenge: managing the system frictions that arise from differences between the structures of the innovation systems of the different industrial and increasingly newly industrializing economies and the continuing demand within the U.S. for economic payoffs. Significant differences in the structure of domestic R&D systems of capitalist economies – Japan, the U.S., Germany, France – have major implications for the creation of tensions in the trade and technology spheres as these economies become more tightly integrated. Yet, the structural differences – for example, the ease with which U.S. firms can acquire the intellectual property of firms in Japan or Germany relative to the ease with which the assets of U. S. firms can be acquired by foreign firms – have significant economic implications.

These structural differences are themselves the result of a complex historical evolution that involves the actions of governments and private institutions and domestic systems of industrial finance and governance. Therefore, they are enduring, and these tensions therefore are going to be with us for some time.

What we’ve seen in response is sort of a two prong set of policies. On one hand, industrial economies in particular have focused on negotiations covering specific instruments of policies, subsidies, intellectual property investment, and perhaps competition among them. On the other hand, we've seen a growth of sector agreements covering specific high-technology industries such as commercial aircraft, and perhaps we’re beginning to see the negotiation of a regime that covers the semiconductor industry.

There are pros and cons for each of these approaches, and the long-term viability of each remains to be seen. There are certainly serious implementation problems associated with reliance on sector agreements. At the same time, the use of functional agreements to try to control specific areas of behavior also encounters significant problems as clever governments, as clever firms rapidly develop responses that may work around these specific agreements or areas of policy.

Final challenge, a more domestically oriented one: development of stronger policy-making structures within both the U.S. Congress and the executive branch for integrating trade and technology policy. We've seen continuing tension and in some cases significant inconsistencies between trade and technology policy, in their formulation and implementation in the U.S. Much of this reflects I think continuing absence of reasonably robust structures for formulating, reviewing, allocating, and otherwise making decisions on the R&D budget across the board within both the executive branch and the U.S. Congress. So until we have a stronger mechanism for policy coordination in the technology policy area, these S&T policy areas will continue to see serious tensions.

In some respects, these problems flow from the failure to implement Vannevar Bush's original vision of a federal science board that was all encompassing – as well perhaps as insufficient allegiance at least by some political actors to his advocacy for the free international flow of scientific and technological information.

But the Bush solution is no longer feasible, if indeed it ever was. Others must be developed. Certainly, we've heard now fairly pessimistic diagnoses of the outlook. It's worth keeping in mind that the problems we face are in many cases the result of what has been a very successful set of policies implemented, many of them dating back to the immediate aftermath of the Second World War, in the international economic, international political, and arguably the domestic S&T systems. We face problems that in many respects flow from success rather than failure of economic reconstruction or political reconciliation during the Cold War period.

Given a choice between the problems that policy makers of 1946 faced and the problems that policy makers of 1996 face, I think I prefer 1996 to 1946. Thank you.

NELSON: Thank you very much, David. Yesterday afternoon, Jonathan Cole and Michael Crow and I looked over the list of people who had agreed to offer their thoughts, and our mutual reaction was, what a feast this is going to be! It's starting out that way.

Our second presenter, Eugene Skolnikoff, is Professor of Political Science at MIT, and Gene has been in the business of studying science and technology and international relations for a long, long time. I'm looking forward to your comments today, Gene.

SKOLNIKOFF: The subject of this article is international cooperation in science and technology. To summarize what I have to say, I would note that in focusing on design of the science and technology enterprise for the future, changes needed with regard to cooperation are long range, based fundamentally on the way both the international system and our government are structured. Neither is going to evolve rapidly from the pattern of today.

International cooperation in science and technology is one of those activities we all assume to be of undoubted worth, always good, with important economic, scientific, and political benefits. What could be more appropriate in this age of growing integration of national economies, global issues, and tighter resource constraints than the idea that international cooperation should be a valuable and welcome phenomenon? However, international cooperation in science and technology turns out to be a rather amorphous concept, and not all the activities under that umbrella are of unqualified benefit. In the current political lexicon, the concept of international cooperation can be a rather big tent. In one formulation it can include cross-border information exchange and contacts among scientists across borders. Under this definition, there is undoubtedly more international cooperation today than ever before for the obvious reason of the expansion of international communications and transportation.

But international cooperation can also include informal research planning, support for research in developing countries, research programs coordinated by governments or international organizations, cross national research and development programs of multinational firms or within firms, major projects carried out among governments by agreement; and that doesn't exhaust the list – there are many others. In fact, the Title V reports from the State Department to Congress, which in principle list all of the international activities of the U.S. government, include some 200 pages worth of programs, some of them legitimately considered to be international cooperation. It's a big tent, with a lot going on and not very well circumscribed.

I am going to focus on those areas of cooperation that involve explicit agreements and incremental funding, rather than on information exchanges and interaction among scientists. This is not because these areas are more important than others; in fact, they are probably less important, but they are the only ones that we really have any focus on and have any data about now.

I will discuss four areas of public sector science and technology: programs that include formal cooperation among scientists in basic research; those that involve cooperation around large, high-cost research equipment such as accelerators; programs that might be aimed at large technological objectives such as cooperation on the space station or fusion energy; and, those that grow out of emerging global scale problems such as ozone and greenhouse warming.

There are many reasons why we might expect that international cooperation in these public sector topics would be a prominent part of the world scene today, and that financial commitments, numbers of scientists and projects would be on the rise. But in fact, contrary to expectations, the extent of cooperation in these four areas is a smaller part of national commitments, at least for the large industrial nations, than the rationale for cooperation would justify. And it appears that the trend line is down and not up.

That conclusion may not seem obvious on quantitative grounds. First of all, some of the decisions and definitions are quite arbitrary and almost impossible to disaggregate credibly. Just a few benchmarks help make the point. European Union countries are probably those most committed to cooperation in science and technology across national boundaries: they have created the European Space Agency, Euratom, Eureka, and many others. But it is worth noting that the European Union's Framework program for research, widely touted and given a lot of attention, actually accounts for less than 4 percent of the total R&D funds in the European Community. In other words, it is only a minor part of the R&D effort and there is little indication that it will increase substantially in the near future.

Germany recently announced that it will reduce its commitments to European science agencies by ten percent; that may mean they will violate their prior commitments. Both France and Germany have drawn back from offers to site an experimental fusion reactor on financial grounds, because the siting nation has to pay a larger proportion of the costs. The United States looks as though it is going to be cutting its budget for fusion research which will almost certainly mean that we are not able to participate, or at least participate fully, in this new experimental reactor.

The Title V report activities, though they look substantial, actually amount to a very small part of the nation's $60-plus billion of federally-funded R&D. The reasons for expecting that international cooperation would be a larger part of the whole are quite commonplace. The most obvious one is cost-sharing. However, the difficult financial situation in many countries that has served to reduce the R&D budgets of most makes even substantial cost savings through cooperation irrelevant. If countries are deciding to eliminate projects completely, it doesn't matter whether money can be saved by doing them jointly.

The emergence of global scale issues, clearly one of the hallmarks of the current era, is another incentive for cooperation. Many areas that can be studied independently will benefit from coordinated or joint research. In the long run, the most important aspect of joint cooperation on global issues is not so much the new knowledge as it is the involvement from countries all over the world, who later may be asked to make commitments of one kind or another based on the results of the research on those global scale issues. It makes a big difference if the nationals of the countries involved have been part of the process of determining what is necessary.

A third incentive is the diffusion of scientific competence around the world. No longer does one nation dominate as the U.S. did after World War II. Cooperation allows nations to tap competence wherever it exists.

A fourth motivation is foreign policy benefit. During the Cold War, we claimed the political benefits of cooperation, particularly east and west, as one of the most important reasons for joint programs. A lot of that has faded, but it still remains important.

A fifth motivation is the domestic political incentive: agencies of government have not been above using international commitments as a way of insulating projects from budget cutting. That is still going on, though in the inward-turning country we have today that's a less valuable device than it was in the past.

The question of building indigenous capacity is another important motivation for cooperation. If there is one thing that is agreed about the relation of technology to economic development, it is that nations have to have their own indigenous capacity to relate technology to development. Cooperation is one way of fostering this result.

With these incentives, why do I believe there is less cooperation than would be expected? The primary reason stems from the fundamental fact of the international political system: it is organized as a collective of nation states and it will remain so for the indefinite future. The consequence is that public sector science and technology are primarily supported by governments to further national goals and that decisions about projects are made in a national policy and budgetary process dominated by domestic pressures.

The observation that science and technology are largely national endeavors greatly complicates the process of developing international cooperation. National objectives are not identical, opportunity costs differ from country to country, criteria of choice among competing projects vary, government structures are not parallel, policy and budgetary processes are not only different in substance but also in timing, and domestic political pressures vary from country to country. Political goals, or goals that are to be served by cooperation, may not be identical.

All of those problems and incentives are as relevant to the United States as they are to other countries, but we have a series of special difficulties which stem primarily from the structure of our government. We have acquired a reputation – I don't think wholly justified – of being an unreliable partner in international cooperation. We change our mind too often. The fundamental structural issue is the nature of our government and the separation of powers, which has several effects. The executive negotiates agreements, but the Congress, not tied to the executive as is the case in Parliamentary systems, has to approve and appropriate the funding.

This is always a dicey proposition. There may be differences of views, politics may be different, or views may change and diverge over time. Annual budgets, which have become a staple of our system, mean that firm commitments cannot be made beyond the initial year. Through we have done it from time to time, we are not happy to appropriate the full cost of a project in its first year. That is difficult to do to begin with and particularly difficult in a tight budget time.

The bicameral legislature and the Congressional committee structure mean that projects are dependent on action by several committees, themselves comprising many different actors, personalities, and politics. Projects are vulnerable to the idiosyncratic views of individuals, views that may change over time as a project goes ahead. Individual members of Congress, because of their separate elective base from the Executive, are typically more dependent on the views of their constituents than are legislators of Parliamentary systems.

Domestic considerations tend to dominate, breeding skeptical attitudes towards international cooperation and, sometimes, direct hostility. Moreover, it is still true in American government that foreign travel has the atmosphere of a boondoggle, so foreign travel costs are typically much more constrained than domestic travel costs.

Separation of powers is not the only cause of America's problematic performance. The relative isolation and self-sufficiency of the past makes it hard for us to recognize our growing dependence on other nations. As in many other matters, it is hard to accept when a project requires sacrifice of unilateral control. In short, perhaps for understandable but no longer viable reasons, we continue to reflect a parochial view toward cooperation. That is going to have to change, but it can only change gradually.

There is one other difficulty worth mentioning, which primarily affects smaller-size projects – our competitive process for approving projects. This is a much larger part of American science policy than it is of most other governments. The competitive peer review process makes it hard to allocate up-front money. Often, to develop an international project, even a small one, you have to have planning and travel money at the start. Secondly, you never can be sure that a project, once developed, will actually be approved in the American system. This makes it more difficult to build the individual collaborations necessary for cooperative research at the small scale.

Finally, there is an issue that is particularly important for larger projects: Who benefits? Is it a level playing field? When knowledge is developed that is presumably open to all participants, will that knowledge be turned into commercial products more readily and rapidly in other countries than in the United States? That question encompasses more than science and technology alone, but it relates to our general attitudes towards protectionism and towards our technology policies.

What can be done, what can be changed, and what is possible? If the judgment is correct that international cooperation in science and technology is well below the optimum, what can we do to change the atmosphere? The basic impediment to cooperation is one that cannot be removed. The nation-state system is alive and well, notwithstanding the rhetoric of the global village and the growing interdependence of nations and economies. The rhetoric is not wrong, but it will not bring about the end of this form of organization of international affairs.

And that organizational structure leads nations to ask about any potential cooperation: "What's in it for us?" The 'us' can be and should be seen as an entity that is larger than the nation itself. It is not normally seen that way, but if there was one long term recommendation one could make, it is that we have to recognize that our national interests are much closer to global interests than we tend to assume in our political process.

Somehow, scientists must find a way to convince the public that our nation's parochial interests need to reflect a much different view of the international scene and where our real goals and objectives in science and technology lie.

As far as more specific policies are concerned, one place to start is for the scientific community and universities to demonstrate to students the significance of international ties and knowledge of other communities, and how their work relates both to the work of others and to the larger community of nations. We have not done a terribly good job of that. Most research universities today talk about expanding the international dimension of their education, but it has yet to happen. Just a few years ago, only about two percent of all U.S. science and engineering Ph.D. recipients planned to work outside the United States, and this, NSF data said, had fallen by half in the previous two decades. Only the senior faculty and administrators of the universities can correct this situation by insisting on adequate attention to the international dimension. Change will be slow, but it must be done.

There are a few specific more steps that are possible, though in periods of tight budget, are unlikely. One is the willingness to appropriate funds on a multi-year basis for projects. Second is to recognize the need for up-front money, for small science cooperation at least, perhaps sequestering some funds so that the peer review process doesn't throw out a project after it has been laboriously developed. Lastly, we need more support for the International Council of Scientific Unions which is probably the most cost-effective international organization that we have. And it works on a shoestring.

As far as the administration and the policy process is concerned, I think we need more focus, oversight, and planning at the center to make it clear throughout the government that international cooperation is in fact welcomed rather than something to be avoided. This requires leadership, planning, and oversight – things that neither the Department of State or any individual department can provide. That does not mean detailed oversight or detailed management. It does mean at least knowledgeable oversight. We do not have that capability today in the U.S. government, as agencies operate pretty much independently. That is necessary and overall probably a good thing, but there needs to be some type of oversight mechanism.

And finally, I note with regret that we do not have any intergovernmental organization concerned with or devoted to science and scientific cooperation. There is an S in UNESCO, but we are not members of UNESCO anymore, and it would not have made much difference if we had been – it was never a very successful organization. I think it is unfortunate that science was included in the creation of UNESCO at the last moment. If an international body had been devoted only to science, it might have made a substantial difference in this whole area of cooperation. But I would argue that it is too late.

In sum, international cooperation involving explicit projects and identified funding in public sector science and technology, though not automatically always desirable, appears to be operating at considerably less than optimum scale. The impediments are substantial, but they relate primarily to the dominance of national considerations when cooperation is considered. Those national issues are not inappropriate, but they are normally based on a narrow, short range of criteria that do not reflect the real needs and opportunities of an increasingly global society.

NELSON: Thank you very much, Gene, for a very interesting and thought-provoking presentation.

In each of our sessions, we have two presenters and two panelists. The panelists have had a chance to look at an outline of what the presenters were going to say, and their instructions are to begin their comments there but they are free to wander and to range. The panelists serve as a convenient vehicle for getting into a more general discussion involving all of you.

Our first panelist is Ed Steinmuller. Steinmuller is an economist by training at Stanford and spent a number of years there as Deputy Director of the Center for Economic Policy Research. When I was visiting Stanford a few years ago, he was clearly the guiding spirit and organizer behind a wonderful Stanford institution called STEW, the Science, Technology and Economics Workshop.

Ed Steinmuller, a couple of years ago, joined NERIT in the Netherlands as Professor of Economics. Just this year as part of this long-run program to teach at all the interesting places doing research on science and technology policy in the world, he is moving venue to the Science Policy Research Unit at the University of Sussex.

STEINMULLER: I indeed do have a world tour package, having come in just last night from Amsterdam. My comments are going to be free ranging in the sense that I have prepared to speak largely on the basis of what the written comments were.

One of the things that strikes me about this morning is the degree of contemporary-ness in which we are considering many of these problems, so perhaps part of my purpose is to step back a little bit from this. Science policy has passed through two great thresholds in which the vision of the role of science in society has been transformed.

The first of these began when Bush's vision of demobilization and a civilian science establishment in a peaceful post-war world failed to occur. Instead, the atomic age was born out of the application of science to warfare, an application that was to result in expenditures equivalent to fighting three more world wars in the past half century. Those of us who were children of the atomic age found ourselves in a world of science in which the power of human invention was capable of reducing the cities we lived in to lakes of glass. It may seem paradoxical that so many of those born after the war joined the scientific community, but those years were marked by hope as well as dread.

The power unleashed in Hiroshima and Nagasaki and the industrial might that leveled Rotterdam and Dresden were of biblical proportions, but it was biblical proportions that might also feed the hungry, clothe the naked, and heal the sick. This was a message that Vannevar Bush preached, a hopeful message on the eve of a Cold War. Bush's report was not only an instrumental plan for technology and science policy; it was also about values.

Now, for an economist to recollect visions of hellfire and salvation may seem unusual in this day and age, the fears and dreams of the atomic age have yielded to a different ethos and a different vision of science's role in society. The applications of science and the shortcomings of competing dreams have created a world in which the values of consumption and production, the economist's stock in trade, dominate.

For science, the second transformation – to a post-Cold War world – is a much more difficult challenge than the visions of hellfire and salvation offered by the atomic age. In dollars and cents terms, science must be justified as a public investment whose returns justify the scale of public expenditure devoted to it. That is the message of the commercial ethos of our age. Science may be defined broadly as the curiosity-driven pursuit of knowledge, and thus when I refer to it, I'm also including the engineering disciplines.

Economists have argued since Richard Nelson and Kenneth Arrow's seminal papers that the results of scientific investigation, to the extent that they are disclosed and others have the capability to apply them, are public goods. In this view, scientific results and the conduct of scientific research are indeed footloose across international boundaries, as Professor Mowery has suggested, while the funding for their creation is indeed national in the sense elaborated by Professor Skolnikoff. Creating national allegiance to international public good and creating the governance structures are difficult tasks, as recent experience with not only science policy but with the United Nations suggests.

Yet it is exactly this path that we are led to by economists' justification for investment in science as a public good. And the public good argument does continue to be relevant in explaining why science cannot be privatized. The logic of the public good, however, is subject to the same criteria that we apply to any other public good investment such as highways or the arts. For those who keep the faith with economics, what we need is a proper cost-benefit analysis to justify our public expenditures in science.

As someone who has investigated this issue in a variety of contexts, I must report a skeptical view. The size of the scientific community created by the Cold War as well as many major initiatives falling under the heading of Big Science are unlikely to find the returns to justify the investment they require. Without a restructuring of the rationale for the support of science, downsizing is only just beginning. At the heart of this problem is the tyranny of how we economists account for investment.

The returns from science are long delayed, and we are now reaping the bequest of past generations who cannot revoke their gift to us regardless of what we choose to do. Nor can future generations, who we continue to believe will be far more wealthy than we are – they can't protest our failure to provide a larger stock of scientific knowledge, as they are not yet born. Even if we were to somehow to remedy this problem with our accounting, I am skeptical of the instrumental justification of the science enterprise.

The best hope for constructing an economic justification lies in the role of science in education. In the pursuit of scientific knowledge, we have one of the best instruments for the acquisition of skills and competence in society. And that is what leads us away from the traditional preoccupation of economists with measuring scientific knowledge as an investment good and attempting to trace its returns.

Instead, we are led to consider the impact of scientific research in creating skills and developing networks of knowledge exchange. It leads us to the difficult task of tracing individual careers, and the synergistic affect of social networks in which knowledgeable individuals participate. And it leads us to longer term considerations of labor force evolution in which we have so little capacity to offer young people advice about their future careers.

I suspect that in such investigations are answers to important questions in the field of economics, such as the premium that college-educated labor has been able to earn in the labor force, and in the true cost of labor force frictions in Europe.

Why is a fundamental reconstruction of the rationale for public investment in science necessary? Professor Mowery has suggested that a major issue is getting the balance right between footloose science and less footloose embodiment of skills in the capacity to exploit knowledge. To achieve this re-balancing, we will have to have a much clearer view of what is “public” about science.

Here, I think it is worthwhile to consider one of Vannevar Bush's less profound ideas. He suggested that in the relationship between science and applied pursuits, that practical technical pursuits would tend to displace curiosity-driven investigation. Bush's intuition was based on the logic of expediency. This logic is under contest. European scholars such as Michael Gibbons are contesting the view that science is a public good by arguing that the tools and methods of scientific investigation are spreading throughout society. The widespread diffusion of the methods of science is destroying the privileged position of the university as the predominant source of scientific knowledge. And in Colon's case, the argument is posed even more sharply by his contention that scientific knowledge cannot exist outside of networks of individuals engaged in scientific investigation, wherever that might be.

Colon and others are taking aim at the public-good character of scientific knowledge, and if we accept these claims, we can also explain Professor Skolnikoff's paradox that nations cannot seem to see beyond their immediate political agendas to a more internationalist strategy of cost-reducing investment in scientific cooperation. If science is a private rather than a public good, the process of private sector internationalization is a continuation and an enlargement of the role of multinationals in bypassing national governments in order to create coherent international networks of wealth and knowledge creation.

Traditionally, what has distinguished public from private funding of research is the requirement of public disclosure. Paul David and others have suggested that there is a fundamental contradiction between the requirements of public disclosure accompanying public scientific funding and the growing pursuit of science in the private domain. Clearly if we accept Colon's view, there is no public knowledge. If we follow David's view that what is important about publicly funded science is public disclosure and the use of this knowledge as a public good, then we need to know why private sector researchers publish and what is different about the publicly funded research programs.

What is the prospect for national governments to shift gears from the Cold War world of science as a military, strategic asset to science as a tradable commodity in which collective gains are possible? Recent U.S. experience with export controls on software as well as other examples cited by Professor Mowery are not encouraging. And I can't report that Europe is prepared to lead on this issue either. In Europe, we have just had a strategic view of advanced telecommunication developments that produced a short list of recommendations. The first was the need, as we now have throughout the world, for a coherent information infrastructure or as we would refer to it here, a global data highway. The second of these was the recommendation that the process of liberalization in telecommunications and broadcasting infrastructures and services should be speeded up.

The third was, and I quote, "only unique European standards will protect investments of industry and consumers." Clearly, the meaning of liberalization is confined to the national and, in this case, regional aims and agendas noted by Professor Skolnikoff, and the idea of technology as a strategic good noted by David Mowery is alive and well. I doubt that the current enthusiasm for strategic planning will easily give way to a reconstruction of the rationale of the scientific enterprise to rival that of Vannevar Bush. In this sense, Bush may be a man for all seasons in arguing for the fragility of scientific investigation in the face of practical interests.

This leads me back to my key message that the rationale for public funding of the scientific community largely hinges on the vision of the social purposes of technology and science. If science is to be viewed again as an endless frontier whose exploration should be among the highest aspirations of new generations, we need to understand how this enterprise can do more than contribute to international competitiveness or the generation of wealth for innovators.

Tracing the role of scientific research in education and the consequences of scientific education for economic progress are first priority in these times. Such an investigation may or may not bring good news for some portions of the scientific community such as Big Science, but it must be done if we are to have any hope of defending scientific investigation in these instrumental times.

We must also re-examine the value of public disclosure of scientific investigation and improve our capacity to distinguish what is private and what is public in the generation of scientific knowledge, so we are able to distinguish what the appropriate role of public funding is. This will involve a reassertion of other standards of accountability related to disclosure, rather than those of economics and accounting where we have been attempting to provide the incentives in the form of publication counts and other mechanisms to link academic and financial accountability.

Finally, and this is particularly the view of an American living in Europe, we must examine more deeply the values that we hold for the future of society and individual aspiration. Such a re-examination might just lead to a re-discovery of Bush's enthusiasm for choosing the life of the pioneer at the edge of the frontier. Thank you.

NELSON: Thank you very much, Ed, for an interesting and provocative set of remarks. Our second panelist is John Zysman. John is a Professor of Political Science at the University of California at Berkeley, and for many years has been interested in the connections among politics, economics, science, and technology. It's a pleasure to have you contributing to this discussion, John.

ZYSMAN: Thank you very much. In part, as a transition to audience discussion, partly to keep my own remarks limited since we are running a bit behind, and partly because in response to the very interesting comments of those who preceded me, I've scribbled a range of notes that I won't be able to read if I stand up, I'm going to speak sitting down.

I think there really is a double set of questions put on the table. First, given our belief that science and technology are essential to the long-run well-being of our communities, to the stability and power of our countries, and to the long-run growth of our economies, how do we maintain that commitment? That's the first question, and I think it's a very basic one.

The second is, assuming we conclude that we do want to maintain that commitment, how do we organize it? How do the new challenges and problems differ from those around which we organized science and technology in the past?

In a funny way, the sense of crisis of the last few years – that science and technology would be de-funded – is an opportunity to confront the fact that the demands on science and technology have radically changed and that consequently we need re-thinking and restructuring as to the appropriate ways of proceeding.

The crucial question is, what in fact do we want science and technology to do? The purposes of science and technology and the role they've played have certainly evolved. But there's a double change. The tightening of the links between science and industry, between science and practical application, has captured our mind, but also the issues that we faced have shifted the way in which we think about science and technology and the kinds of demands we make on it.

In my mind, there really have been four different stages. The first, of course, was the original era in which there was a sense of linear progress from basic research to application – the end of World War II and the years after it, of large projects in which large scientific breakthroughs could support broad national purposes. The second phase, sort of the post-war reality, was one in which investment in science and technology was often driven by military concerns, which lifted the technological plateaus. The commercial market demand drove the application of that science to technology as it moved from plateau to plateau. Here, in a funny way, it was the insurance industry that drove the emergence of the IBM mainframe and of broad-scale computing in the end, not the military.

And then, of course, there was the third stage, forced on us in a way by Japanese competition, which had a reality of two parts. One was that productivity rested not just on broad scientific application but also on the simpler reality of work organization and changes in how we structured the behavior of companies and the shop floor.

Second, the leading edge applications were increasingly, as David and others have suggested, driven from the commercial sector – not just from the commercial sector as in the case of the IBM mainframe but driven from the consumer durable sector. The mundane things of everyday life that we produced in large volumes – when we opened them up and looked inside, suddenly we saw components and subsystems that were in fact cutting edge, where scientific knowledge was often being applied. And precisely because it was being applied in volume, the consumer durables industry was able to support research or draw on the research in a new kind of way. The logic and funding of technology, not necessarily of science, was shifted. One only has to look inside a Sony Camcorder or at the fluid dynamics in an inkjet printer to reach this kind of conclusion.

Of course, some of this took on the tone of national rivalries, as we became fearful that other people's great success at organizing production and using consumer durable sectors would give them enduring advantage not only in the technologies but in the supply base. That is, the machines, the know-how, that were required to produce a good today and support its production and the technological innovation and scientific investigation required to maintain it tomorrow.

Now, we move to a fourth era, which is really a derivative of the third, characterized in the 1990s by two things. One, which I loosely call Intelism, is the formal realization that the competition is no longer just about the assembly of products as it was in the era of Henry Ford. It's about what goes into those boxes, that the real value added in this laptop computer is in the microprocessor and the screen and the operating system, and Microsoft and Intel and the Toshiba/IBM venture are likely to be the only people making money out of the sale of this particular product. Therefore, Intel Inside in a sense represents that. Many of the components have the possibility of application of scientific and technical knowledge in a wide range of countries and in a wide range of sectors in this era of cross-national production networks.

The other aspect of this is the emergence of software and digital innovation because it, in a funny way, breaks some of the links between scientific technological growth and productivity. What in fact is the scientific investment required to develop Netscape? It's not so obvious, apart from the development of broad-scale mathematical training, what one really needs to do. And if one looks at the innovators in many of the digital areas, it's often a generational set of jumps in which those who are over 30 are often not the drivers – in which the relationship between innovation and scientific and technical training is oftentimes reversed, with investment driven by innovation.

I don't have many answers or policy proposals, I'm afraid, but I do have five questions. The first is, who invests in science? In an era of corporate downsizing – in which we can't depend on the IBM labs or Bell Labs in the role they played before, sort of an oligopoly or a regulated industry playing a para-public kind of role – the choices that national governments or state governments make about spending and science become crucial. But the spending is being cut at the same time. Therefore, it becomes critical who pays for science and what kind of science we want. I think that begins to be the kind of questions that Ed Steinmuller was suggesting. How do we organize the scientific enterprise at its core? I don't think, without opening up another conversation, that the answers of the past are terribly persuasive to me as answers to the future.

The second question is, do we really invest in this scientific base or do we invest in applications, be they the fusion of science and technology or the kind of mission-oriented work that the United States has been good at over the years? And if we emphasize mission-oriented work, is it our military missions or the broader social policy kinds of missions?

Third, if we emphasize these applications, it seems to me that we can characterize the choices we have as two different kinds of games that we have to play: one is chess and one is poker.

In the established oligopolies, there's a complicated chess game about investments in which people investing in technology and science try to position themselves not to lose in a market. On the other hand, in a whole range of new technologies, you're simply playing poker. In fact, the organizations that are set up to do one often don't do the other very well. And that's particularly a problem in sectors such as the electronics industry, where today half comes from businesses that existed ten years ago and half from businesses or products that didn't exist ten years ago. How do we play both games?

Fourth, do we invest in science or training? Fifth, are the investment issues the critical ones at all? In the whole telecommunications and digital area, the American decision to de-regulate the telecommunications industry – which I was very skeptical of, at the time – is ultimately what has given us real leadership in network computing and what I would call the new consumer electronics.

These choices that we make at home are part of this more complex world of rival national innovation systems. Now, I would say that these are differently structured national economies with quite distinct national market dynamics, but we're talking about the same thing.

The world is supposedly more global. I don't want to start the conversation about whether the world is really more global, but the numbers in 1914 and 1996 don't look so different. FDI, Foreign Direct Investment, looks different. It may be the entrance of Japan and China into the world market that gives this flavor of globalism. The world is very different, but exactly what this globalism consists of isn't so clear. We often use a code word to explain what we don't understand, to point at things rather than to really analyze them.

In any case, the co-existence of these different kinds of national systems raises two possible relationships. One is that in this world of Intel-ism – with Thailand, Taiwan, and ultimately parts of eastern Europe becoming players and investors in different components and subsystems and the emergence of regionally based cross-national production systems – we're going to see a much finer division of labor.

It's sort of Adam Smith applied to science and technology, in which case one sees the success of the Danish model, where I have been spending the fall – the clear ability to monitor and apply technology, the clear success of the IBM/Toshiba deals to produce the active matrix screen, the cross-national production networks themselves, which exist in Asia and have been a large part frankly of the American comeback in electronics.

Now, that is a world in which we all co-exist, in which the effort to build national systems of science and technology is a race everyone can win. It's sort of the decathlon. Everybody gets points, and the winner is the one who gets the most points, but you know it's a game everyone can play and everyone can win. An alternative is chess or perhaps lacrosse in which you play moves and you take your opponent off the playing field. And the metaphor we choose I think is quite significant because the other side of the story of these confrontations is one we've all told. I've spent time telling it about Japan, one could tell it about Korea, the French Air Bus story is certainly one that one would highlight.

Now, the reality is that these two stories are going to co-exist in very intimate ways over the next years, and it's going to be very difficult to keep the game open while not allowing others who are playing chess at a national level to gain advantage. To say we're going to eliminate these efforts to create national advantage isn't the question. The question is how to moderate them and how to keep them in bounds so that the underlying finer division of labor can in fact proceed.

The optimistic part is that many of these rivalries will start to become cross-national coalitions. The efforts to create digital copyrights, for example, create the same splits in Europe that they do in the United States. Interestingly, the lobbying over precisely those issues is now consisting of European/American coalitions that lobby on both sides of these issues in Europe and the United States. So I think the critical question here and in the international policy arena is to keep our balance. That is, to recognize there are great gains from this finer division of labor but there is no set of world trade organization rules or any set of intergovernmental agreements that's going to ultimately eliminate these temptations toward offensive technology policy.

And finally, I would say that in this supposedly global world, what the evidence from the economists has suggested is that we see an increasing specialization in the kinds of product, in the kinds of technology. That put us back to the choices that we have to make at the national level in the first place – how do we do it right so that our particular specialization assures that we capture our share of the high value added, high wage kinds of industries that everyone wants to be a participant in? Thank you.

NELSON: Thank you very much, John.

ADAMS: I am Dorothy Adams from Columbia University. Since we have been speaking about integration, about various global considerations in a geo-political theater, which has become a diffuse conflict, what I would like to know, looking back at 1945, is where is the George C. Marshall, Marshall Plan creator of our era for science?

ZYSMAN: I think we're in a fundamentally different period. Vannevar Bush had the currency and the visibility that stemmed primarily from World War II. There would have been no such response to his interests and his recommendations if it hadn't been for the role he played as a centerpiece of science and technology during World War II. Since then, the communities have grown, and the only time traditionally that you get a focus on one person or one place is in a time of crisis. After the Russian Sputnik, we once again for a period of time had a crisis and a response with a focus of government attention to the field that made a huge difference.

If you're looking for a new General Marshall, I guess we need another crisis first, which I would just as soon not have as a way of stimulating that. I think the community is too large, the interactions between technology and science and society and the economy and the international scene are too great to imagine that any one individual or one person is going to create that kind of following and that kind of vision.

HAUBEN: I'm Ronda Hauben, I'm a student at Columbia, and I've done an online book on the history and development of the global computer network. Do you see any broader perspective? I've been listening today, and I've only heard a little bit. We have tremendous developments that the scientific and technological communities in the U.S. have pioneered for the rest of the world to make people's lives better. And isn't there some way of one studying and building on all of this, not in terms of chasing short-term kinds of things, which I think is going to be disastrous for the scientific community, but instead somehow really looking into the significance of these broader developments. So is there any perspective in this direction?

NELSON: Yours is an important and profound question. It's a question, however, that relates to the broad subject matter of this overall conference rather than to this particular session, so why don't we file that as an important question and ask it again a little bit later in the conference. I think that we're observing self-organization in terms of people who want to comment.

LAIRD: My name is Burgess Laird, and I'm from Los Alamos National Laboratory. I have a brief question for David Mowery. You argued that one of the central challenges facing the science and technology policy-making establishment is trying to prevent growing demands resulting in a profusion of ill-advised barriers to foreign participation, I think you said in our economy or at least in the S&T infrastructure. I wanted to ask if you've seen any evidence of this, because I've seen quite a bit of evidence to the contrary, including that in the past few years, the rate of growth of foreign-based R&D facilities in the United States has actually increased. Just taking that as one indicator, those foreign R&D facilities don't seem to much care about pseudo-techno-nationalist foreign policy programs. Any comments?

MOWERY: What I meant specifically was restrictions on foreign participation in federally funded and perhaps in some cases state funded S&T programs. I think that there are a number of examples of this. There has been some relaxation over time, but there is continuing pressure – some of it from Congress, some of it from within the Executive branch – for restrictions on foreign participation in Sematech as originally designed, restrictions on foreign participation in the high-temperature superconductivity conference back under the Reagan Administration, for a range of restrictions applying to various elements of the advanced technical program, and depending on the specific agency, the Cooperative Research and Development Agreement programs administered by different federal agencies.

Now, there's I believe a report from the Office of Technology Assessment, the late Office of Technology Assessment, issued probably in '94, that documents some of the array of these restrictions. It makes the fairly compelling point that there is really not much consistency among them – as one might expect, since these originated in different agencies, in some cases at the behest of different individuals – and indeed in some cases even the underlying economic rationale is particularly difficult to discern.

So we have quite a contradictory portfolio of these restrictions; in some cases, they have been relaxed, in other cases, new ones are being proposed. I think in many cases they are difficult to justify. Certainly, their internal inconsistency is even more difficult to justify when we look at the role of U.S. firms as beneficiaries of participants in programs in the U.S. and elsewhere, perhaps the underlying rationale is equally open to question.

WESSNER: My name is Charles Wessner, and I'm with the National Research Council. We're pleased to be able to tell you that we have a report coming out on the 17th of October that addresses many of the issues that have been so articulately raised here today. I'm sorry to tell you we don't solve them all, but I think you may find some of the points we raise interesting.

I wanted to raise two questions – actually several questions. The first is, I used to work for the federal government as a Director of International Technology Policy, and I'd like to direct a question to both David and John.

The question to David is, there seems to be much ado about nothing with respect to the restrictions on American programs. There is a simple case that restrictions occur around the world on all national programs. There are some that are open to U.S. participation, particularly in areas where we have substantial technological advantage.

We once asked a coalition of foreign companies whether they really felt that these programs with our restrictions were an obstacle and if they had the chance of opening the programs completely or getting rid of them, which they would choose. The answers were twofold: first, they're not really an obstacle. They can get in if they want to. Second, they would prefer that we got rid of them. I think that's instructive, but I would ask David if he might wish to address that.

On the question of globalization, in the preparatory material, we note that differences in national technology policies have become increasingly costly, in recognition of the global nature of science and technology. Much of the work we found in the government is that these were not costly – the companies adapt, as you would expect, quite successfully – and that the differences in national technology programs, which are increasing very rapidly, notably in Japan, and on an exclusionary basis, suggest that the chess model that John Zysman was talking about is becoming more predominant. I would like to ask John if he thinks in fact we're moving towards a more chess-oriented nature of competition.

NELSON: In order to keep the discussion coming from the floor, let me bypass you this time, David, and ask for a quick response from John.

ZYSMAN: As I suggested, I think both are going on. A generation ago, we were all concerned about Japan, and the issues are not just about participation in science and technology programs. A crucial issue in the Korean case is whether the markets are open for foreign direct investment into Korea. Many of the issues that concern us about participation and about the Korean strategies would evaporate with the proper liberalization of those markets. That's why I personally believe that we should pursue rules in broad ways of trying to address these questions.

DAVID: I'm Paul David from Stanford University and from All Souls College in Oxford. I share the view that the prospects for increasing national commitments to programs of international collaboration of a formal character, especially involving incremental appropriations, are quite gloomy.

There is a set of possible avenues for less formal cooperation involving either cross-subsidies, which run across national fines, and resource transfers, which you explicitly said you were weren't going to focus on. But I wondered whether you could be drawn out, Gene, to talk briefly about the range of issues involving international access to existing large facilities, an issue which is currently before OECD Megascience Forum and more generally about the efforts of organizations that have been cobbled together, like Megascience Forum, to increase, or at least protect, existing levels of cooperation of an informal character, allowing teams from various countries to visit and share facilities which have been financed by other governments.

Another area – and this might touch on a question that was asked earlier – is the longer term prospect for expansion of the facilities in high-speed data networks, which would allow remote access of scientists in differing countries to facilities which have been substantially financed abroad out of national resources.

These kinds of possibilities can create problems, in that they will require tolerant or supportive national policies in allowing such access to develop. They will be replicated, likely, within the national level in the policy decisions that will face many institutions, like universities, whose faculties and researchers will be in active collaboration with other institutions, and will raise questions about who's paying for this. So would you perhaps address this range of informal areas of cooperation? Thank you.

STEINMULLER: Thank you very much, Paul. I had considered originally when thinking about the remarks here whether to focus entirely on the informal dimension that you raised rather than the formal nature of cooperation. In fact, I believe that not only is there more informal collaboration going on today than ever before, but that that's where the large future is.

And it's not simply in cooperation in science and technology. It's general, if you look at international agreements in environment, for example, and their implementation, that much of what happens, happens through what is sometimes called soft law – that is, not formal agreements but informal agreements that nations are quite willing to carry out without the same kind of onerous ratification and formal process of agreement.

But in any case, in cooperation in science and technology, I think as far as access of facilities is concerned, Paul, you said everything there was to be said. It does raise important questions about not only who pays but who benefits. If there is going to be fallout of knowledge that has economic significance, commercial significance, then questions will be raised about why are we letting others participate in a project that has been paid for by one country.

I would recall half a dozen years ago, when the United States saw our competition with Japan as being a high-technology competition, that it was not only serious but that the United States was losing drastically. And during that period, the universities, particularly the one I'm at, were attacked vehemently from the Congress and elsewhere, that we were letting too many Japanese come to our university. Never mind supporting research and cooperative work, but also just visiting, that there was a huge leakage of knowledge to Japan, and that the cooperation was the source of that. And we took a very protectionist attitude.

Right now, that's not a serious issue. If it’s a serious issue, it's not high on anybody's agenda. If we find ourselves once again under stress because of competition, I think that issue will return. And similarly, the question of access to facilities will become a more serious question on those grounds.

Last, let me just mention that there's a very interesting American technology, in the sense that we've exploited or developed it first, which is almost out of our control. And that's GPS, the global positioning system. Military-related development, which has now been thrown open, in effect, to commercial exploitation in the market. And the United States government has been forced – because of the enormous growth of the civilian applications of this technology all over the world, all informal – to make a commitment that we will make that technology available to the rest of the world, presumably forever, free of charge.

What may change some day is that there will be competing systems. But for the moment and for the foreseeable future, here’s a new technology, first developed and exploited by the United States, which is informal in terms of its – and do you call it cooperation? I think it is. And it's a fascinating example of what would at first blush seem to be something in one nation's control, but is, in fact, out of control in terms of any ability to cut it off at some future time. We have to continue it now.

NELSON: We have time for one more question.

LICHTENBERG: I'm Frank Lichtenberg, Columbia University and National Bureau of Economic Research. I thought that John raised the important question of who will fund science, and I thought he suggested that there may be a crisis in both the private and the public sectors, citing the examples of IBM and Bell Labs.

But I think when we look at the data, it's a little bit hard to see that there is, in fact, a crisis or even a serious problem in the private sector. If we look, for example, at R&D intensity or R&D per dollar of sales of manufacturing companies, that has risen steadily over the last 35 years. It's about twice as high now as it was in 1958, when the NSF started collecting the data.

Now manufacturing is a shrinking portion of the economy, so that if we look at private R&D intensity in the economy as a whole, that's been relatively flat. But there has not been a decline in private R&D spending in relative terms, even during this period of apparent downsizing. Moreover, the econometric evidence that I'm familiar with suggests that, if anything, the productivity impact or the kind of social rate of return of private R&D is higher now than it was in the 1960s and 1970s. So I would ask him perhaps to respond to this issue: Is there a problem with privately funded R&D?

NELSON: I think it was a very useful factual statement, Frank, and it's good to have that in our thoughts. I don't think that it requires a particular response, in view of the tight time.

MALE VOICE: I don't agree with that.

MALE VOICE: There’s one thing that I would add, which is that actually the R&D spending in the non-manufacturing sector also has been rising rapidly in the last five plus, ten years, I guess, some of which may be the result of a change in the sample selection used by the National Science Foundation. But there is R&D spending going up rapidly outside of manufacturing within industry as well.

NELSON: John does think it warrants a response. (laughter)

ZYSMAN: I do. I think it warrants a response in two ways. Not a challenge to the facts, but an interpretation of what I was focused on. Which is that the character of what industry is spending money on is changing, and therefore, Bell Labs and Xerox Park and places like that as sources of basic science are changing functions.

We're increasingly spending money on more downstream work. And therefore, the question becomes, if that role is not being played– and it was being played by Bell Labs in part because it was a public monopoly – who substitutes for those kinds of roles?

So the issue isn't just the absolute amount of money. It's the way in which it's organized, on the one hand. And on the other hand, the increasing tendency of many companies to involve themselves in university research, which raises the question that Steinmuller pointed out, which is, what constitutes public and private research. Where are the fines, and what are the appropriate rules? So I actually appreciate it, because it clarifies the point I was trying to make, which is not about the absolute levels of spending as such.

NELSON: This topic won't go away. It’s going to come back when we talk about the future of university research, and it's going to come back when we have a session concerned with civilian technology policy. Let’s take a break.

Design Area Two:
Resource Allocation Among Scientists

Ralph Gomory
David Z. Robinson
Dorothy Zinberg
Rita Colwell

Moderator
Jonathan Cole

COLE: I think we're off to a good start. I can't say we have stuck to our timing, but we'll see whether we can make that up. And one of the ways in which I would propose that we do so is that I limit to almost non-existent time the introductions of our speakers, because they are so distinguished that they really need no introduction (laughter), and the biographies of them appear in your materials.

It's a great pleasure to introduce these speakers, who will be talking about resource allocation among the sciences. There are many different mechanisms for allocating resources among the sciences. Three of these are notable. And the designers here will consider how the United States should utilize various historically useful mechanisms. They will engage issues that surround their use, and will consider possibilities of new allocation mechanisms. And our first speaker will be Ralph Gomory, President of the Alfred P. Sloan Foundation whom you all know, I believe. Ralph?

GOMORY: Ladies and gentlemen, it is certainly a pleasure for me to have the privilege of having your attention for an estimated 20 minutes. Let me start by saying that probably the reason we are all here, and why people are looking for some rethinking of the scientific and engineering enterprise, is a budgetary matter. Were budgets larger, we would be going on without much thought.

I must admit that I am actually an optimist about future budgets for science and technology. So I don't share this concern quite as much as many of you. Nevertheless, I do believe that there are some new elements that it is probably useful to inject into our thinking about science and technology funding.

The present funding system, which for many years has worked well, reflects, of course, the mandates of the various agencies, the needs of DOD, of NASA, DOE, NIH, and the broader mandate of the NSF. The emphasis given to these agencies and their programs can, of course, vary greatly over time, and we have seen that. So, in my opinion, it is useful to augment this approach – and I think it's realistic to talk about augment rather than displace – by bringing in other considerations, such as economic prosperity, which are not well represented, in fact, by any one agency.

To consider, in addition to military security and health, economic prosperity and to attempt a simple rationale that encompasses both agency and non-agency goals, I am going to talk about technology as well as science, because I'm quite convinced that if we don't do well in technology, we will not get the payoff from science.

Much of what I'm going to say today is consistent with the 1993 COSEPUP report, "Science, Technology and the Federal Government: National Goals for a New Era," of which I was one of the authors. In spite of these respectable connections, I do not expect everything I say to be non-controversial.

Let me start by asking, what degree of agreement do we have today on the support of science and technology? I think we have a degree of unanimity in our own community that science and technology clearly deserve funding, and preferably on a larger scale than now. There is agreement that the budgets of NIH and NSF should increase. But that level of agreement does not distinguish scientists from any other special interest group.

What is lacking and what is needed is an accepted rationale showing that support of this entire enterprise, and not just the parts of it that are obviously useful or fit an agency's needs, is in the national interest. And I said national. International is much easier. National is the hard part, as some of our previous speakers have pointed out. And more than that, we must also answer the question of what level of support is right for this enterprise.

How much university science and engineering is enough? An accepted rationale for a level of support will certainly not solve all the problems of the support of science and engineering. In real life, there is always the effect of the attitudes of committee chairmen and of real personalities and real politics that are discussed day in and day out in Washington.

But a rationale would help. And I suggest that we adopt in a serious way – and I'll explain what I mean by serious – the goal of being world leaders in science and engineering research. Taking this seriously means drawing the consequences of this world leadership goal, not leaving it as a largely rhetorical or political device.

But if we are to adopt this as a goal, we must also answer this question. Why should we as a nation – not just the people in this room – care if we are leaders? The answer is empirical, in my opinion, rather than theoretical. (laughter)

This leadership that we have had has worked for us in the past and is working for us now as a nation. Scientific leadership has given us, in addition to the obvious contributions to military security and health, it has given us much.

Scientific leadership in solid state physics gave this nation – not some other nation – first the semiconductor itself, then the semiconductor industry, and then the rapid development of the computer industry. Fundamental knowledge in molecular biology – and the people who have that knowledge, a point I will return to later – gave us a significant edge in biotechnology. And also on the engineering side, university-based work has given us many of the concepts and the people who have given us a leading position in software and in networking.

These industries alone have benefited the country far more than the investment in university-based work has cost. Certainly other nations – and Japan is the usual example – have succeeded without this type of leadership. But different nations are different. They have different systems, they have different strengths.

The Japanese strength has been downstream in the manufacturing and development processes, and not upstream. We should learn, and indeed we are learning, to do well what the Japanese or any other nation does well. But we should not give up what has always been or at least in recent years has been our advantage.

Another question that should be asked and hopefully answered is this: why should we bother to be leaders in everything? Why not be leaders only in the fields that have demonstrated their usefulness, the ability to contribute to national or agency goals, or more generally, to the world outside of science and technology? I know that's an unpleasant thing to bring up, but people outside our community do bring it up, and we must have an answer.

And to discuss it, I need to say a few words about the research process, something I was exposed to for a long time. Although the actual work of research is full of ups and downs and is unpredictable in detail, we do make progress.

We do steadily understand more and more. It is not surprising that when you start to understand things in a fundamental way – whether it's how man-made materials hang together, or how living beings function at the molecular level – that at some point, this understanding will let you do useful things you couldn't do before.

I say that is not surprising. On the other hand, there’s something we don't know. In those areas where the practical linkage of this understanding has already occurred, steady applied progress will usually accompany steady research progress. But in areas where no practical connection has yet been established, even the most expert researcher does not – and I assert, cannot – know in advance how this practical impact will occur, or if it will occur.

Nothing indicates this difficulty – or perhaps I should say impossibility – better than the history of quantum mechanics. In the 1920s, there was no subject more pure and more esoteric than the then brand new quantum mechanics. There was the uncertainty principle, the baffling puzzle of electrons that behaved like waves one moment and particles the next.

It was a subject of exciting scientific and even philosophical impact, but nothing could have been further from application. In the 1930s, quantum mechanics began to have an effect in what was then called solid-state physics. And after the war, this improved understanding of the fundamentals of crystalline solids led to a better grasp of the role of trace impurities and their effect on the flow of electrons.

The transistor was not far behind, with all its tremendous impact on computers and on electronic devices of every sort, and through these, on the everyday life of all of us. Not much more than 30 years separated the esoteric and apparently useless from its enormous everyday impact.

In view of this, I think we should adopt the following unpredictability principle. Now brace yourselves, folks. I'm gonna' say this twice, right? (laughter) We can see when some area of science or engineering is useful. We can't see that some area of science or engineering won't be useful.

All right? We can see when something is useful, but we cannot tell that something won't be useful. Therefore, if the United States wants to have the advantage of being competent and able to respond when a field starts to have a practical impact, since this practical impact is unpredictable, we should have a strong position across the board.

This is a rationale for the support of fundamental science in fields that have not yet shown their usefulness. It does not matter whether this practical impact occurs as a result of an event here in the United States or abroad. The point is to be able to benefit from it. And you cannot benefit from it if you are not a player in that field.

So I think that we immediately conclude from this unpredictability notion that we should support basic science across the board at a level that enables us respond. That is to say, at a world-among-the-leaders level. However, we may sometimes decide to do more than that.

If something really starts to happen in a practical way – practical meaning impact outside of the field – we might make a decision that we want to do more than to be just among the leaders in the world. We might feel that there is some benefit to our society, whether that is through industrial leadership, through a contribution to health in this country or around the world – but we might decide that it's not enough to be among the leaders. We might decide that we want to be clear leaders, ahead of the rest. But I've said something there that I want to bring to your attention, though you may not like it, and that is that the selection of areas of really clear leadership should often be measured by societal contribution and not by purely scientific terms.

If this seems at all abstract to you, let me show by an example that if we take these notions seriously, they have practical consequences. And so in order to demonstrate that, I'm going to go back to the SSC, the superconducting supercollider decision.

When the SSC was being considered, we could have asked, is particle physics a field where, because of its clear contribution to society, we must be out ahead of the world as clear leaders, or are we content to be among the leaders? I think the answer to this is the latter.

The record of particle physics, to the extent that I know it, simply does not support the notion of a large societal payoff so far. I would conclude this is not yet a field for clear leadership, though it is certainly, as all fields are, a field in which we want to be an important part of the world. Therefore, I would conclude that the SSC should never have been built or started. And that we ought to work out something with other countries instead, that would allow all of us to move forward together in particle physics.

I don't have time to condemn other fields. But if I did, space would be at the top of the list. But while this group may presume I mean the manned space program, I also would include the scientific program as being funded at a leadership level. If we now turn to molecular biology, with its clear relation to an emerging industry, as well as its application to health, we would, I think, come to the opposite conclusion. This country might reasonably decide that, in the interests of national health and the interests of the emerging biotechnology industry, that this is a subject on which we might wish to be well ahead of the world.

The goal of being either a leader, a clear leader, or among the leaders in a given field, is a measurable goal. It involves a comparison of the level of science and engineering in the U.S. in a particular field with a level of that same field in other countries. We are among the leaders if we're roughly on a par with the work done abroad. But the real point, of course, is to be in a position to participate if something happens. Note that what we have here is a comparison with other countries but within a field.

Testing whether you're among the leaders in a given field of physics – for example, condensed matter physics – does not call for a comparison of condensed matter with particle physics. Or with some field within chemistry. It does not call for the usual endless arguments about whether one field is more exciting than another.

It does not call for arguments about big science versus individual investigator science. It says we should simply measure ourselves against the world standard in each of these fields.

This approach does provide an answer of how much science is enough. It does this not in terms of increases or decreases from whatever today's budget happens to be, but in terms of supporting universities in science and engineering at a level that provides the desired level of leadership. If we were to adopt such an approach, I strongly believe we would find it to be both an affordable and a stable basis for the funding of basic research.

And, by the way, I think that we are pretty much there in many fields, simply as a result of the present system. So we're talking about an add-on, not a complete re-do. However, we also have to talk about benefiting from this basic research in leadership.

And we have to face it, talk about what we have to do as a country to benefit from a leading position in basic research. And this is necessary if we're going to justify the expense of leadership to the people who pay for it in this country, a point that several speakers have already made.

The thing I think we have to concentrate on is the flow of people, not the flow of papers. I think that the emphasis that we hear over and over again about results flowing is a form of academic myopia. If you talk to people in industry, what they usually want is people, the flow of people trained, well trained in what is happening, from universities into industry. And this is a point which one of our previous commenters, Mr. Steinmuller, made in his own way.

Certainly, one of the most important mechanisms for benefiting from research leadership is through the flow of people. One person carries a lot of papers in their head, a lot of knowledge in their head, much of it that has never been put into a paper.

University people played a major role in the early days of Silicon Valley and of Route 128. And today, as I'm sure you know, the biotech companies are full of university people who play decisive roles. This seems to happen naturally enough when a science-based industry is emerging, but less naturally at other times.

It is important, therefore, especially in the areas that have shown their practicality, that we keep a steady flow of trained people into U.S. industry. This country clearly benefits from this flow from universities. And it is this flow that had much to do with placing Silicon Valley in the U.S., rather than somewhere in the British, German, or Japanese countrysides.

There is, of course, since we're talking about government support, the possibility of government support of other activities, those such as advanced development in industry, or even support of the development process itself.

Most of the commonly used rationales for other forms of government support are very broad brush ones. Now I'll give you an example. Here is one of the most popular broad brush arguments. It is the familiar argument that the U.S. underspends on R&D. It is a discussion of R&D as a percent of GDP, of total U.S. output. This argument has been used indiscriminately by all administrations.

People point out that Germany spends 2.5% of its GDP on R&D, that Japan spends 3%, and the U.S. only spends 1.9% – a thought which is supposed to send shivers up and down your spine. I recommend, stop shivering, because we need to look more closely at this. First of all, almost all reported R&D is R&D done in the manufacturing sector. Banks don't report R&D. The manufacturing sectors of Germany and Japan are bigger than that of the U.S. as a proportion of their GDPs.

The numbers are, in fact, Germany, 30.6% of GDP, Japan 30.8%, the U.S. only 19%. If all manufacturing firms in all three countries were equally R&D intensive, R&D as a percent of GDP would simply reflect the size of the manufacturing sectors.

And this is exactly what these oft quoted numbers do reflect, with remarkable accuracy. These numbers do not mean that the foreign firms are more R&D intensive, just that their manufacturing sectors are larger and have more firms in them.

And more detailed analysis does show the firms in the various countries are, on the average, about equally R&D intensive. I could give you other sweeping favorite arguments, such as the externalities argument – that is, that companies do not capture all the benefits of their innovations. But – and I regret that time does not permit me to fire arrows one at a time into all of them – I believe that none of these sweeping arguments holds up under scrutiny.

To make sense of this area, we need detailed knowledge of what works and what doesn't work in bringing advanced work to fruition. However, that detailed knowledge is not easy to come by, and for a fundamental reason. We are used to the idea that there are things that are too small to see. However, we are less used to the idea that there are things that are too big to see.

But there are. A national economy is one. And the R&D system is another. We cannot see the functioning of the R&D system. That functioning is spread out in thousands of locations and depends on the efficient or inefficient actions of hundreds of thousands of people scattered through thousands of plants and labs and offices.

It would be wonderful if we had a macroscope to see in real time and in glorious detail the R&D system function. We could see what functions right and what functions wrong. But we don't have a macroscope. Statistics is, In fact, our attempt at a macroscope. And it only functions erratically. It functions erratically because if we have the right overall picture, then the statistics can size it right for us and tell us more about it.

But if we don't have it right, the statistics won't tell us that we don't have it right. The example I gave about R&D as a percent of GDP is of this type. If you have, either consciously or unconsciously, a picture of R&D as being done right across the economy, your statistics tell you that U.S. firms are underspending and give you, in fact, quantitatively the average underspending.

But if you have a picture that says the reported R&D is all in manufacturing, you get an entirely different answer from the same statistics. Realistically, today, we know very little about what works and what doesn't, what is needed and what isn't, where there's a realistic as opposed to an ideologically determined role or a non-role for the government.

We don't know where, in practice, the market works and where it doesn't. And we're also unclear of what to do when it doesn't. Basic research is one area where there's a considerable agreement that the free market doesn't work. And in this area, the government has learned to play a constructive role.

But outside of this, we know very little. We do not have in government a large cadre of experienced people who know how to work effectively with industry in the national interest. Nor do we have realistic criteria for when government support is needed or makes sense. I think that we need to learn.

And to learn, we need to experiment. Learning means, for example, that we should see where there is and where there isn't today a flow of people and ideas from our universities into U.S. industry. Is it happening? Is it happening in what areas? In what industries?

Do foreign graduate students still stay in the U.S., or do they head home to areas where there are new opportunities? Are there good ties between academic events and research areas in U.S. companies? Is the flow of new ideas going into our hospitals, or is it going into our school systems? And if we look, we will find places where this is happening, and we'll find plenty of places also where it isn't. When we see areas where we could be benefiting from our leadership but we aren't, then we should experiment.

There could be industries, for example, that are too scattered to do effective R&D either on their own or in connection with universities. A beautiful example is an industry like the powdered metallurgy industry. They're too small firm by firm to make the leap from university-level knowledge to hard, industrial product.

There could be, as we all know, important health items for which the market is too small for the development work. And development work on things like high-temperature superconductors for which the materials work needed for products is too long-range for the potential user companies to do. These are areas we should experiment in and see what works and what doesn't.

If we do follow this path, we will develop some real knowledge and experience about what we get for certain government actions. And we need that knowledge, if the country is to benefit from government actions in this area. If we do experiment and get some knowledge, we can then have a debate, not about process, not about ideology, but whether the outcome of a given set of actions is worth the money and effort they cost.

Different people and different political parties having different social views will weigh these outcomes differently. But nothing could be more appropriate for political debate than a diff6rence of views about the relative importance of different outcomes. This is a much better role for the political process, than a high-level debate about processes that need, in fact, to be understood in detail.

So, ladies and gentlemen, I do think that we can put together a rationale for across-the-board leadership, for clear leadership in some industries, but that we need to do work to establish the technology connection that translates that into a national benefit. And I say "national" not because I'm indifferent to the real needs of the rest of the world, but because I think that is a practical necessity. Thank you very much. (applause)

COLE: Thank you, Ralph. Let's move directly to David Robinson. You have his biography in his materials. David?

ROBINSON: The problem of budgeting for R&D has been with us a long time. Simply stated, it is this: where is the money going to come from? Today, we still do not know where the money is going to come from. My major thesis is that broad considerations of resource allocation among sciences make little sense. For most activities of government, science and technology are not goals in themselves, but are linked to major societal goals.

There is a long list of major societal goals to which science and technology contribute, including:
- improving quality of life, health, and human development
- increasing knowledge;
- expanding education and the diffusion of knowledge;
- improving personal and public health and safety;
- contributing to a high standard of living;
- creating and maintaining a civic culture;
- fostering community harmony;
- stabilizing population growth;
- nurturing a resilient, sustainable, and competitive economy;
- promoting economic growth, including increased employment and work force training;

- improving international competitiveness;
- modernizing communications and transportation;
- increasing environmental quality and sustainable use of natural resources;
- fostering worldwide sustainable development;
- enhancing resource exploration, extraction, conservation and recycling;
- securing personal, national, and international security; and
- improving social justice, individual freedoms, and worldwide human rights.

Science and technology contribute to all of these societal goals, yet most discussions of fund allocations ignore them and focus only on the economic and competitive aspects. One of the important national goals we have agreed upon is the advancement of science itself. In this area, we can talk about resource allocation. But if 90 to 95 percent of the federal expenditures on science and technology are discussed in the context of other goals, then it is the priority and balance among those goals that should be the major factor in the choice. In short, budgeting for science and technology is a major part of the political process. Instead of looking at fields of science as competing against each other, we should look at what our national goals are and how we make decisions regarding the allocation of funding for them including their science components.

Expenditures on science and technology are going to uncover new knowledge. They're aimed at improving things in the future, often the very far future. When preparing budgets, mission agencies have to balance funds they need to address today's problems vis a vis funds that will (or may)make their job better in the future.

Today, how much is the nation spending on cancer treatment? How much on prevention and education? How much on cure? If we develop a cure for heart disease and cancer, can we let kids starting smoking cigarettes again? Technology fixes are always something we're interested in.

To summarize, my thesis, is that there is not a single science and technology budget. There are science budgets linked to various societal goals (as defined through the political process), and the budgets should be determined by how they fit those goals. The priorities should be attached to the programs, and should bring along the appropriate science and technology budgets with them. It should be left up to the agency or the research lab to make the case that the funds spent on science and technology are worthwhile and are going to make measurable progress towards these goals.

In the 1960's, I saw how this case was made at the National Institutes of Health. James Shannon – the brilliant leader of NIH when I worked in the White House – had a long-term, three-step plan for supporting scientific research. Year after year, he inveigled more money from Congress than the administration had proposed.

Shannon's first step was to promote both non-governmental and Congressional support. Enlist non-scientists like Mary Lasker and private, disease-oriented organizations. Cultivate Congressional committees. Shannon was wonderful with Congressman Tom Foley and Senator Lister Hill.

Second, demonstrate that immediate breakthroughs are possible. Be disease-oriented rather than health-oriented. It is much easier to list the diseases that you hope to cure rather than to explain the connection between current appropriations and the long-term health of the nation.

Third, invent special institutes. Every time you focus on a new disease, set up a new institute. Describe the budget by working from the specific to the general. Shannon would always talk about how much he was spending on heart catheters, for example, and then expand from the arteries of the heart to the heart as a whole, to the body as a whole, to other diseases as a whole. In this way, he could justify his budget.

Shannon also invented a research project category which, as a physical scientist, I had never heard of when I came into the White House. It was called "approved, but not funded." Scientists would apply for grant funding and would have their applications approved. Shannon would then go to the Congress and say, for example, "We approved research grants worth $500 million. But we only had $400 million to spend. So we have $100 million of grants that were approved, but not funded." On hearing that, Congress replied, "We better give you the extra $100 million to enable you to fund everything that you've approved." The next year, Shannon would come in with an additional $100 million of projects that were approved but not funded. So it went.

The other strategy Shannon perfected was funding multi-year programs "subject to availability of funds." He would approve a five-year grant, but only fund the first year. Since the federal budget is for one year at a time, the next four years would be "subject to the availability of funds." Shannon would go to Congress the next year and say, "We have $400 million in grants we've already promised subject to availability of funds." Congress would start from that spot and vote additional money.

The other major point Shannon looked at was expanding the infrastructure. He started development programs and research in undeveloped areas of the country. He started a whole computer program in the 1960's, before anybody thought that computers would be important in biomedical or biological research. He supported proven investigators long-term and junior investigators short-term. He invented "Training Grants." He was allowed to fund research only, but he supported graduate students by calling it "research training." In sum, by having a program that he could justify to the American people over a long period, starting in the 1950's, Shannon built an NIH which spends significantly more than the National Science Foundation (NSF) spends on research.

One could give similar examples in other agencies. The Department of Agriculture started out as a research-oriented agency in the 19th century. The DOA used its support of science and technology through field stations and agents to develop general public support of science and technology.

I started out by saying, look at the institutional goals, look at the science and technology needed to meet those goals, and try to develop programs to justify that science and technology. What's wrong with this picture? Why can't we just look carefully and frugally at all of the government missions and opportunities, put together the required science and technology budgets, and go home?

For a first approximation, that's fine. But in the second approximation, these mission activities often overlap. I was involved in a situation once where three agencies, the Air Force, the Weather Bureau, and the Geological Survey, were all interested in research on hail. All three agencies were sending airplanes to the same part of New Mexico at the same time of the year, because that's where most hail storms seem to be.

We have to coordinate and rationalize between and among the departmental budgets, and we must do more to eliminate unnecessary duplication of research. Cooperative activities should be encouraged. Some programs contribute to more than one goal. For example, computing for Defense can be valuable to the nation's pursuit of other goals, such as commercial technology and economic growth.

And there is a special situation with regard to fundamental science and technology. The NSF mission is to support basic science and engineering. In allocating its budget, the NSF has to be aware of scientific opportunities in what other agencies and the private sector are doing, then try to exploit the gaps in research.

This balance wheel function is troubling to some. In general, it appears to me that the NSF has to strive for continuity and balance, trying in all areas to respond to the highest quality proposals and to produce the people we need for the country.

The Stever taskforce of the Carnegie Commission on Science, Technology, and Government pointed out that the science and technology base must be built for the future (National Research Council 1992). We have to support: general science and math education; the science literacy of the public; higher education in science, engineering, and the social sciences; human resources; facilities; and institutions. These are long-term, national needs that must be supported by the federal government. Therefore, we need to have moderately stable science budgets. We also need to ensure that young scientists are trained well, and that the institutions that train our scientists are healthy.

Most scientists agree that they need money, but very few scientists believe that their institutions need money. This is why agencies must think about building the institutional base even as they carry out their missions.

So, what is wrong with this decentralized system? The problem is that our national goals change rapidly. If particular fields – and physics is one – are linked to specific goals, then the nation as a whole can be in trouble if the goal changes or disappears.

Our goal on health research has not disappeared. The NIH is moving along, though perhaps not as rapidly as we think. But biomedical research consistently gets more money from Congress than the administration requests.

Fields such as physics, computers, and communications are important for our economic competitiveness. Congress has been willing to support them bountifully for defense purposes, and commercial industry has reaped the gleanings from that harvest. Without the harvest, there would be no gleanings. These fields are suffering as defense budgets decline.

In principle, if we agree that defense spending is going to decrease but these other sciences are very important to other goals, and the NSF could provide the balance. In practice, in an era of tight budgets, NSF will be hard put to do very much extra. Making the best budget decisions in this complex situation is extraordinary difficult, and I do not believe there are simple criteria or simple answers.

We have to focus on major changes. Sometimes we will keep on supporting things we shouldn't for political reasons. In many cases, I don't believe that fighting to be number one makes any sense. I do think being competitive in all major fields is very important.

Within agencies, program managers have to balance one field with another and the present versus the future. After they've done this balance the best they can, I believe that the White House Office of Science and Technology Policy and the Office of Management and Budget should review the situation as a whole.

These reviews must pay attention to what is happening internationally and in industry. And I endorse completely the method of experimentation, review, and seeing what works. Adjustments may have to be made. I wish there were simpler ways of dealing with this, but we have a very complex system. And I believe that we have to look at it in its complexity before we can make any useful decisions.

COLE: Thank you, David. We have two distinguished panelists, and we'll move on to hear their initial comments, and then we'll hear from the floor. Dorothy Zinberg will speak first.

ZINBERG: As I moved back so as not to interfere with David's perfectly terrible slides, I had an absolutely paralyzing thought, because I realized, yes, I am here to discuss the federal aspects of funding. But my livelihood and that of most of my colleagues is based on private foundations. Who was I to comment on David Robinson, the Carnegie Foundation, the Carnegie Commission on Science, Technology and Government, and Ralph Gomory of the Sloan Foundation?

First of all, my memory was when McGeorge Bundy became the director of the Ford Foundation, John Kenneth Galbraith said to him, "Mack, you'll never hear another honest word." So, if some of my comments are orthogonal, it's Darwinian. And accordingly, part of it is made very easy, because I totally agree with what David Robinson has said, though he'll be shocked to hear that.

But I did have some disagreements with Ralph Gomory's comments. And I thought I'd try to put them as a null hypothesis rather than in a disagreement, because if you look at things differently, you might begin to come up with some different solutions or even ways of beginning to think of how you resolve a dilemma.

And it would be very valuable at this conference to look at this issue: what if there is not as much linearity in the future as there has been in the past about funding and about thinking about the role of science in society? And for those of you who were not present at the creation after World War II, it is within memory for many of us of what a very, very different world it was and how the changes now are not small but major.

And let me just name a few. One is a reigning value or a reigning opinion that was given voice by someone like Bruno Brunofsky: "Look, we scientists are simply terrific. We are honest. We are smart. And the public doesn't understand us, nor do they have to. Just give us the money, and we will give you great science. We need no outside interference." That could have been written in the Middle Ages, in terms of where public attitudes and expectations are now. And yet it's only 40 years ago.

To put it in its wildest sense, I think it was in the ‘60s that Dan Greenberg, who's been the gadfly of the scientific community forever, invented a character he called Dr. Grant Swinger. It came out of a wonderful ad for Scotch whiskey, which said, "As long as you're up, would you get me a Scotch." But Greenberg said, "As long as you're up, would you get me a grant."

Dr. Grant Swinger had created the Center for the Absorption of Excess Federal Funds. This was a sense that there really was so much money floating around that all you had to do was ask for it. In a more serious vein, that was echoed by Jim Watson at his 60th birthday discussion, where he said, "In those days, science was fun. If you wrote an alpha proposal, you got funded. Now even the alphas are not funded, or only a small percentage of them."

So, we're beginning by the late ‘70s, early ‘80s, the disjunction between talent and available funds. And this has only been exacerbated in the numbers of scientists we have been educating and some of the dilemmas that come from this.

If we had had this meeting in 1990 and talked about where we need funds, I think there would have been very few voices talking about the radical shifts that are coming about through the Internet, through all the interactive media and where that's going to take us with funding and emphasis.

So I think that is one way we have to look at how quickly things are changing. And perhaps in terms of taking this conference into its next phase, it might be worthwhile to think about what is afoot if it isn't going to be linear funding from the government with some kinds of tinkering up and down.

Now, let me just say briefly, when Ralph asked how much science is enough, I think that is essentially an unanswerable question. (laughter) Some of us did get an answer in part to a question like that. A few weeks ago, George Soros gave a lecture in which he said, "After I made $12 billion, I decided that was enough." And so we finally learned what is enough money.

What is enough federal funding or what is enough science obviously is not answerable. I think this is a big issue, and I hope we'll get to that in the university part, because there is where federal funding can play, has played a major role. And yet we haven't been able to support the institutions that are employing these people relative to the jobs that have been implicit in this federal funding. And I think that speaks a lot to what a science education is and what science is about, if it isn't a career.

Ralph Gomory raised several questions about what was happening to the international scientists who came to this country. Did they go home? Did they stay here? Those were just the questions I wanted to answer in the proposal that was turned down by the Sloan Foundation. (laughter)

But what we do know is that the investments that Taiwan and South Korea have been making in their scientific infrastructure are really beginning to pay off in the numbers of first-rate scientists and engineers that they are producing. Their numbers who are coming to this country are beginning to drop, not precipitously, but they're beginning to drop. And the numbers who are returning, we're seeing very large numbers. More than 25,000 have already returned, because of enticements, because of cultural comfort, so that we’re beginning to see the kinds of shifts that we really never anticipated. I throw all of this in to say that I'm not sure the assumptions of linearity are going to persist.

I also feel that much of what has been going on here, the questions are essentially questions of social science. If you look at federal budgets and what is most likely to go right under the axe, it's social science.

And I would say that we are asking questions that can only be answered by social science. And that if we wanted to look at the NSF budgets and the attacks on the NSF and other sources of funding, we should be thinking very hard about how we are going to protect the very groups that have the expertise to answer some of these questions which we think are of such importance.

There are books now being written about the end of science. I don't believe them. We have to take that very seriously. Does that reflect something larger that's going on? And I was struck at the beginning by Jonathan Cole's comment that what we have is a crisis of public understanding in science.

I would say that is matched by the crisis of the scientists' understanding of what's going on with the public and the Congress. And that perhaps much of our time in the near-term future should be directed, not only to public understanding of science, but of scientists' understanding of what the Congress and the larger society are about now and why federal science funding may not be as linear in the future as it has been in the past. Thank you. (applause)

COLE: Thank you, Dorothy. Our second commentator and panelist is Rita Colwell.

COLWELL: Realizing that I essentially stand between you and lunch, I will be brief. Also being an anomaly, an administrator still doing science, I came prepared for a slide-assisted lecture. But seeing the room and the visuals, I have weeded out all the tables and just have a few illustrative slides.

I forgot to bring my technology with me. I had intended to be prepared to be combative and to be disputatious, but I find that I'm in much agreement with the speakers and my fellow commentator. I'll give you my bullets first. And then I will amplify very briefly.

I'll make a bold assertion. I had originally intended to say, Ralph, that the first point I wanted to make is that physics no longer rules the world. However, in rethinking, I think the laws of physics do still apply. But physicists will not be there in charge in the 21st century, which is only a few years away. The best and brightest and largest numbers have been going into the life sciences where "the action" is.

And so the cadre of personnel in the 21st century is going to be in those life sciences. And I would also say that science, as it is being practiced now, is interdisciplinary science. And I will use an example from molecular biology of how physics and chemistry, mathematics, computer science, and biology come together to bring these advances that are occurring in the life sciences.

My third point is that the basic-versus-applied dichotomy argument is really irrelevant, because in fact, what is happening is that the basic research that ends up in science is applied immediately by companies. I'll give an example of that as well. And I do think that we are in a paradigmatic shift. That's a buzz word. But how we do science is much more in partnership with the public. We are doing science in a more visible way, because a decade or two decades ago, no self-respecting – how shall I say it? – hubris-carrying scientist would speak to the media, would be involved in public debate.

But now, we know that it is part of our responsibility. But more than that, our responsibility includes educating the public. And again, I will amplify and give an example.

Finally, I would say that the virtual university is here. I think the discussion about whether we shall be international or not: just talk to a couple of scientific hackers, and you know darn well that they're interacting with their colleagues all over the world.

In fact, we were recently doing a study where we discovered seasonality in a diarrheal disease. We put out a call to the Web, "Does anybody out there have such a result?" And sure enough, back came from New Zealand that, indeed, they observe seasonality. It was in September, October. Ours was in March, April. It meant that spring was the factor. But this call to the international scientific community immediately brought a response.

So that resource allocation as we discussed today, from my perspective, must be in those areas where we are leaders, but also those areas of social relevance. Now let me just very, very briefly go through the biotechnology as it's presently defined as an applied biological science. It's old and new technologies, any technique that uses living organisms or parts of organisms to make or modify products, to improve plants or animals, develop microorganisms for specific uses. All of this comes under biotechnology.

The seminal work in genetics was done in 1865 by Gregor Mendel, an Austrian monk whose studies on the pea plant elucidated the inheritance of traits by hereditary factors. His work was ignored until about 1900. But once rediscovered, his findings fit very well with what by then was known about chromosomal activity during cell division or mitosis.

The early to middle portion of the 20th century was a very exciting time, with major gains in knowledge of genetic inheritance. Thomas Hunt Morgan of this university (Columbia), working with a fruit fly – drosophila melanogaster – showed that genes or the units of hereditary were the constructs of chromosomes. His student, Alfred Henry Sturtevant, who later joined him when he moved to Cal Tech, made breakthrough discoveries showing that genes were linked, comprising chromosomes. Thus began the science of genetic mapping, a technique essential to the new genetics and employed, of course, in the human genome project, one of the biological megascience projects.

In the 1930s and the 1940s, genetics research was inextricably moving in the direction of the upcoming explosion of knowledge at the molecular level. People such as Barbara McClintock – she was ridiculed until eventually she proved prescient and was awarded the Nobel Prize. And also the work of Marcus Rhodes, who studied linkage and mutable characteristics in maize and corn and provided a new view of genes as being more mutable and variable than the Mendelian genetics allowed.

Meanwhile, research into what comprised genetic material moved forward very rapidly. In 1928, Frederick Griffith found that a "transforming principle" was able to alter traits in a bacterium – streptococcus pneumoniae, as it was then known. By 1944, Avery, MacCleod and McCarty of the Rockefeller University identified the transforming factor as DNA, deoxyribonucleic acid. And from that moment, the scientists in many laboratories labored to determine the chemical structure of the DNA molecule.

And finally in 1953, James Watson and Frances Crick's short paper in Nature was the breakthrough everybody was writing for. Well, since then, the applications of biotechnology have simply exploded. The ability to clone genes into plants, for example – what was a pyrotechnic kind of experiment, to clone the luciferase gene into the tobacco plant. Probably the only good value of tobacco: it lights up by itself, instead of being lit up and smoked by someone. But in any case, by cloning that gene into the tobacco plant, we were able to utilize that as a tool, as a reporter for the functioning of genes.

And since then, we have all kinds of applications in diagnostics and medical treatment. I will use one example, that of protoplast fusion: the ability to take two plants that do not form hybrids but by manipulation, by fusing protoplasts, producing, for example, the "pomato," where one has both your french fries and your ketchup in the same plant. The applications of genetic therapy, as we well know, are enormous, and they come from this huge capacity to use genetic material as the new manufacturing.

So, what has happened? The first U.S. biotechnology company, Genentech, was founded in 1976. Now, barely 20 years later, it is joined by more than 1,300 companies in the United States alone. In 1981, the first U.S.-approved biotechnology product reached consumers, a monoclonal antibody base diagnostic test kit. The following year, the pharmaceutical, Eli Lilly's recombinant

DNA human insulin, was approved for sale in the U.S. and Great Britain. And the Humulin sales just a few years ago were $560 million.

From 1981 to 1987 was a watershed period for the United States in biotechnology. An average of 90 companies were formed every year, for a total of 600 companies just during that six-year period. Although most biotechnology companies still are not consistently profitable, an increasing number of products have entered the market. The market value just in the last year or so of biotechnology companies was somewhere between $40 billion and $50 billion, with R&D expenditures of $7 billion and more than 100,000 employees. This is an industry that did not exist 20 years ago. In comparison, the U.S. pharmaceutical industry, which is heavily invested in biotechnology, had R&D expenditures in this area of only about $13 billion in 1994, so that we see a very rapid movement of the discoveries from the laboratory to the field.

Now, I would like to speak about the interdisciplinarity. This slide is the three-dimensional structure of the T-cell receptor. The molecular structure of the T-cell receptor – the T-cell, as we all know, is very, very important in AIDS immunology – has preoccupied immunologists for a long time.

Now, recently, researchers at the Biotech Institute at University of Maryland have made contributions to the understanding of the three-dimensional structure of the T-cell receptor and its correlation with function. The three-dimensional structure here shows the beta chain of the T-cell receptor as revealed by x-ray diffraction. In this particular photograph, the complementarity-determining regions at the top are shown in yellow and green and orange. And this is the part that comes in contact with foreign antigen and the histocompatability antigens. This is one of the component chains of the heterodimer that makes up the T-cell.

More recently, the researchers at the Center for Advanced Research in Biotechnology (part of the University of Maryland Biotechnology Institute) determined the 3D structure of the variable part of the alpha chain from a T-cell receptor. And so with this model and the one that I just showed you, the complete receptor could be modeled as shown here. Next, the researchers crystallized and determined the 3D structure of a complex between a bacterial superantigen and the beta chain. And the 3D structure of this complex, which will appear by the way in *Nature* in a few weeks, explains how superantigens elicit non-specific, useless immune responses to allow infecting microorganisms to proliferate in their invertebrate hosts.

What's the message? The message is that all of these studies use physical techniques and concepts such as x-ray diffraction, thermodynamics, data from x-ray diffraction experiments stored in the computer. The computer graphics are essential for the representation and study of the 3D structures. The material for the studies comes from biological systems and samples. The molecular biology techniques, such as expression of vertebrate genes, are all part of the experimentation.

Where does one discipline leave off and the other begin? It all weaves together to make the discoveries that are so important, which then very quickly go into industry. For example, the researchers at the same laboratory have been working with Proctor & Gamble on the structure of the protease that breaks down protein molecules. They were able to genetically engineer it, so that it would be more functional at high temperatures.

The basic research appeared in *Science*. And within the next week, the Proctor & Gamble scientists were incorporating the protease in their detergents. What is basic? What is applied? It is meaningless. It is irrelevant. We are moving so quickly in this area that the science we pull from the laboratory today as basic, funded as an RO1 at NIH or as an NSF grant, moves quickly into application and for the betterment of the human condition.

Let me speak to the social issue. Now, one of the areas is marine biotechnology. In 1800, the world fisheries were about a million metric tons. By 1980, the fishing accommodated about 100 million metric tons. The maximum sustainable fisheries of the world oceans are 100 million metric tons. Our harvesting has begun to decline, and that is because we are fished out, as *Newsweek* put it a year ago and as *The Economist* put it in an article recently, "The tragedy of the oceans."

To feed the 1.6 or the 1.8 billion increase – that is, from 6 billion to 10 billion, whatever the population increase will be – in order to provide the protein, we cannot look to the oceans and wild fishing, because the maximum has been reached. The expected need is somewhere between 135 and 165 million metric tons by the next century, a few years away.

That impedance mismatch, that gap can be closed only by aquaculture and biotechnology applications, producing transgenic fish that reproduce rapidly, grow to a larger size. The first transgenic fish with a growth hormone introduced from one species to another has already been accomplished. This technique will provide us with a capacity to meet the needs – so the social relevance is important in establishing our priorities.

Now, one other aspect I would like to amplify is that we have just opened a new laboratory, a new research laboratory in Baltimore. It's a new kind of laboratory, and I believe it's a laboratory that is of the future. Two-thirds of it is basic research, with scientists doing research in marine biotechnology, molecular biology. But the other third is for education of school children and kindergarten children and also a public exhibition area that allows the public to see exhibits that explain what is going on in the laboratories. And the building is built such that the public, as it goes through the exhibits, can see scientists at work.

The scientists in the laboratory doing the basic research are the docents. And the experiments, the hands-on research, are actually the kind of simplified research being done in the laboratory. This is the kind of interaction that is necessary to demysticize science. It is necessary to build the scientific community, the voter support of what we are trying to do, because they will understand it. And we can translate it for them.

Well, I'm running out of time, and as you can tell, once I get started, I begin to proselytize. But I do believe the point of the virtual university is a critical one as well. We are already, most of us who are involved in university education, involved in distance learning.

At the University of Maryland and I know other universities – Cal Tech and USC – we are involved in partnering in curriculum development with Norway and Sweden, with real time sharing of lectures, developing programs whereby students in Norway and Sweden and Maryland take courses together, take lectures together. And the faculties are combined as one university with a program in marine molecular biology. And I know this is happening everywhere.

It makes the argument about whether we should internationalize, whether we should be productive. The point is knowledge, like microorganisms, does not carry a passport nor does it respect borders. It is a shared resource, and globally. So I will close by saying that we have gone from assembly lines to fermentation vats. We have plants as factories. We are able to utilize the tobacco plant to produce compounds.

And I know that the Boyce Thompson Institute has recently cloned antigen genes, that is, the antigens for cholera and some other diarrheal diseases, into potato and into bananas, so that the new form of vaccination will not be by injectables. It will be by ingestion. And for little children, it will be a very quick and simple way to vaccinate.

I close then by saying the future is very exciting. We must be leaders, because scientific discovery and application moves much too quickly for us to lag behind. Thank you. (applause)

COLE: Thank you very much, Rita. And thank you all.

This is the moment in which I must engage in executive decision-making, since we were due to begin lunch about 20 minutes ago. Why don't we have one or two questions, and then we will break for lunch immediately thereafter.

MALE VOICE: David, I guess I'd like to ask you the following question. I agree with you that we should orient our funding paradigm according to missions. The question I have for you is, how do we arrange that the educational system will produce people that have a passion to contribute to those missions?

ROBINSON: I think the problem – how you get the education system to do that – is a complicated one, because it's a societal effect. I think we are an entrepreneurial society. And I think what you've seen in the software business, for example, is how the Japanese trying to develop a fifth generation computer from the top failed against our decentralized system, because of our society.

All I can suggest is that you work from example. If you have the professors interested in the outside and interested in the kind of thing you could do, then the students will be that way. But if you don't have the professors interested, you won't get the students.

MALE VOICE: But as you indicated, the NSF sort of sets the tradition, in terms of the support of research and education within the universities. And so if they are to remain detached from the missions, then the professors are also likely to be encouraged to remain detached.

ROBINSON: Well, I think we will move only slowly, and it will take a revolution to make a really big change.

COLE: Other questions? Paul, did you have one? Paul David.

DAVID: I want to try to ignore the very good and sound advice that Dorothy Zinberg gave about not biting the hand that feeds you. My experience with the Sloan Foundation is that they fund the proposals they like regardless of what the proposers say about them. And so I want to go after one-half of Ralph Gomory's proposals. And this is what appears to be a very attractive standard of measuring the level of science effort in a given field – not against other fields, and therefore trying to avoid internecine and unprofitable struggles within the science community, but to refer ourselves to what is happening in the world around us.

I think this is a very interesting and perhaps very practical sort of rule, algorithm to follow for the United Kingdom, but not for the U.S. And the reason that I'm led to that view is that in its absolute size, in its present, preeminent position in the world, the U.S. is not a small player. Decisions to not be preeminent in a given field can be signals that can be taken by funding people in other parts of the world to justify decisions, to cut back levels of effort in those areas.

In this kind of situation, what we can easily wind up doing is finding assurance in our reflection, our judgments reflected in the mimetic behavior of other countries who are also struggling to cut budgets and free up resources for projects with shorter term and more certain payoffs. That we will in fact enter in a race to the bottom. That is, mutual abandonment of certain areas of research. In positive feedback situations without any damping, you have a potentiality for unstable movements in either direction.

Therefore, one has to accompany this kind of program with some checks, some set of references to scientific and engineering judgment about where possible, unexploited, future developments may exist, even though the people aren't working in those areas. Without that, it seems that we would be throwing away a major part of the advantage that we've created by becoming the country whose scientific and engineering research establishment is the envy of the world.

We would decide not to listen to the judgments that issue from that community about what science is worth doing, but rather to place our faith in the political decisions that are made in other countries whose complex science policy processes are not less Byzantine or less bizarre in their outcomes than our own. Perhaps, Ralph will respond to that.

GOMORY: I'm not completely confident in modeling the world response to all of these things. I think it's complicated. But I do think that we need some rationality here. And my concern is really the opposite, that we do need to protect basic research against the assault that everything needs to be useful. And that's really the thrust of my remarks.

For example, let us take the spending of the United States on astronomy. Now, is that in balance? I think not. Much of that is NASA spending. And many billions a year are being spent. Now, of course, the manned program is a peculiar one, because it is masquerading as scientific spending. But even if we go beyond that, if we take this $2 billion or $3 billion a year that is spent on mostly planetary astronomy, I would say we have clear leadership at the cost of several billion dollars a year in that field, and we don’t need it. And I think this kind of thinking will help us to sort out where we should spend our money.

I am aware of the kind of dynamics that you describe as a possibility. I have to wait and see whether that really will happen. I think that the actual mechanics of the world are far more complicated than that. And I don't think that we should give up rational thought because of an elaborate cascade of events that in fact are unpredictable. Thank you.

COLE: Thank you, Ralph. Why don't we now adjourn for lunch.

Design Area Three:
National Security and the National Innovation System

Craig Fields
Kenneth Flamm
Sidney Winter
John Pike

Moderator
Michael M. Crow

COLE: Michael Crow, who has been very, very important in designing these conferences, is going to usher us through the afternoon sessions. The first one deals with national security research and national innovations. So it's a pleasure to turn the mike over to Michael.

CROW: Thank you, Jonathan. There is some method to our madness. For those of you who had the pleasure or non-pleasure of being at the first couple of meetings, we are methodically and slowly working through, as Jonathan mentioned this morning, first the history of how Vannevar Bush and *Science: the Endless Frontier* came about.

The second session that we focused on a few months ago was the impact of that design. And now we're looking towards the future. This afternoon, we will turn to defense and health science policy. And then tomorrow zero in on national laboratories, as well as on the design and structure of how decisions ought to be made in the future, or at least throwing out some suggestions towards that end.

This afternoon, we have a distinguished panel with broad experience here to talk with us about design issues and design parameters associated with science and technology policy issues, as they relate to national security. You'll recall from either your recent reading or historic reading of Bush that Bush guaranteed, or his panel guaranteed – they've used language nearly as exacting as that – that if science could be invested in over the long term from a basic science perspective, that scientists would be able to deliver national security in a military sense, national economic security and well-being in a health sense.

And so we're following that same logic as we work our way through what we hope in this session will be design parameters and design ideas for the future.

Our first speaker is going to be Craig Fields. Craig has a career where he has touched on all of the worlds, the academic world, the government world, and the industrial world, leading not only the Defense Advance Research Projects Agency, but serving as a faculty member at Harvard in his career; as well as leading one of the experiments, if you will, in new industrial organizations, the Micro-Electronics and Computer Technology Corporation for several years. And he remains active in industry, leading a new company in the edutainment technology arena. So, Craig.

FIELDS: Well, it's a pleasure to be here today. I received exactly two pieces of instruction on the presentation. The first was that it be no more than 20 minutes, and I can guarantee that I'll be able to abide by that rule. And then the second was that the topic was national security and national innovation. And I took that to be one subject, rather than two subjects, for purposes of today's discussion.

I want to make two disclaimers before I get into the substance of the remarks. The first is that these are personal views. I'm Chairman of the Defense Science Board now, but nevertheless, these are personal views. Although I should note that I decided as a matter of good practice to submit these remarks to the department, and they were returned "Cleared" and unchanged.

And then secondly, while the guidance was to be prescriptive, in fact, I found I couldn't do that. So my remarks are going to be descriptive and predictive, but not prescriptive. Perhaps we can get into some prescriptive notions during discussion.

I'd like to start with some background. We all know the background, but I think it would be helpful to go through it, four or five decades of national security support for science and technology. Basically, two reasons. Accelerating the creation of knowledge that was needed for national security, main reason. But then a second reason, namely what I'll term a benign conspiracy among a number of players to, under the umbrella, the aegis, of the Defense Department and the national security community, simply support science technology and education.

Some folks call this the Defense Fig Leaf. You can use whatever notion you want. But it's actually worked very well. With rising budgets and adequate resources, there was not a lot of complaint, and I think a lot of good has been done. You all know, as a partial consequence of these investments, much of the computer technology that's been developed, a lot of mathematics, oceanography, quite a lot of aerodynamics, materials science, the list goes on and on.

Now, what was done, and I want to characterize this for reasons that will become clear towards the end of the remarks, fits into certain buckets. Basic research and advanced technology, not only laboratory research but the creation of very large scale prototypes, building things to see what would work. Sometimes that's the only way. Manufacturing and process technology, and then education of scientists and engineers, as I spoke of earlier.

And then who did it? Who is doing it? Well, certainly universities and companies. But also there's grown up over the last four or five decades what I'll term our national security R&D infrastructure. The bargain in this infrastructure was that the DOD would be loyal to certain organizations, and they would be loyal to the DOD. These being national laboratories, like Los Alamos Defense Laboratories and the so-called FFRDCs, Federally Funded Research and Development Corporations, like RAND and MITRE. So that's the background, again, something we all know.

But the environment's changing. And when I wrote the notes for the presentation today, I ended with the comment that things have certainly changed between 1986 and 1996. It's one of those changes that's so slow, you don't quite notice it until you've just looked back over ten years. And I wanted to describe the changing environment in, again, two sets of remarks. One is what's going up? And then what's coming down?

What's going up? Well, globalization of science and technology, spurred in part by things like the Internet, but spurred by everything. Things spread out, and spread out fast. Globalization of corporations and universities. What company isn't an international company? Universities, you understand the circumstance. Growth and free trade. While we absolutely don't have absolutely free trade, probably never will, the trend is probably in the direction of free trade.

And then, lastly, something that's regrettable for the Defense Department, generally a slow down, an increase in the acquisition cycle getting longer. A lot of efforts today to shorten it. A lot of anecdotes about things getting shorter. It's not so clear that the trend has been reversed. So I'll call that in the up category as well.

What's going down? Well, the national security budget is going down. The national security R&D budget is going down, as folks have to trade off spending money on R&D, spending money on modernization, just buying stuff, and spending money on readiness, training troops, paying salaries, health care, and so on.

The national security influence on the nation's science and technology infrastructure is going down. DOD, other parts of the national security community are not quite small potatoes, but it's definitely shrinking. The international will power for blockades like COCOM. The willpower to just stop somebody from getting something is going down. This is not only a practical matter, but it's also a psychological matter.

And then lastly going down, I think the national willpower, U.S. willpower for subsidizing industrial segments, for boosting something you want to boost, to put it in a slightly more positive sense, is just going down. So, willpower.

Now, with all that said, we have management in the national security community and DOD, CIA, and so on, whom I think are really first rate, about the best I've ever seen. And, they're always asked to operate more like businesses, that's a litany that has some base. And in fact, they are acting more like businesses. They've made a decision that we're going to go for quality, not quantity. We want the best stuff rather than the largest number of things.

They're saying how can we – and now I'm going to put it in industrial terms – have better products than the competition? And in approaching that kind of question, they have to approach questions that businesses approach. What are going to be our strategic differentiators, what are going to be our strategic necessities? By strategic differentiators, I mean something that's real important to you, and you want to be ahead of everyone else. By strategic necessities, I mean something that's real important to you, but you're actually not expecting to be ahead of anyone else, or everyone else.

They're making make-versus-buy decisions that are explicit and I'll say with more an industry-like thinking. Now, what are the consequences of this change in attitude, a more business-like attitude, and changing environments as characterized by the ups and the downs?

Well, sort of a three-part change in the investment strategy, in my perception and view. One is procuring – please note the word – procuring more things that are off the shelf. Dual use systems, dual use subsystems, commercial off-the-shelf products, and so on. Buying things. The notion of a command and control system as a Microsoft application just isn't as silly today as it was ten years ago. That's the kind of stuff going on.

These things, the things that are bought off-the-shelf are strategic necessities. You expect everyone else to buy them, including adversaries. But what you're trying to do is to ride this wave of global investment so that you get the best stuff because lots of people are paying for it.

Number two, is developing, not procuring, developing more militarily unique technology. And where necessary, the underlying science. So here the development is increasingly focused, not exclusively, but increasingly on the things that can be strategic differentiators – namely, you'll have it and no one else will. Or you'll have it way ahead of other people. It's a business strategy.

And then thirdly, employing more full and open competition, away from special relationships. Again, not 100 percent, but that's the trend. That refers back to the community I spoke of, this special national security R&D infrastructure, which actually is sort of shrinking.

From the point of view of the infrastructure, the comment I hear – and I'm going to try to keep this not sexist – is, "I'm not going to be a kept woman, or a man, if I'm not kept lavishly." And so that community would like a little more freedom, since the budgets are going down. And from the point of view of the government is the discovery that capitalism and the free market are actually a good thing when resources are constrained. So there's a little push and pull on both sides.

Now, what are consequences of this? Well, there are a lot of consequences, but the one that I think is most note worthy for this meeting is a lower fraction and a lower amount of national security resources devoted to science and the development of dual use technology, because of this desire to differentiate in order to have better stuff than the competition.

I'm not saying this is good or bad, that's a separate discussion. Again, descriptive and predictive. But it's at least brings some interesting questions. And I will just note two before I close.

First, will the public's appetite for science increase faster than its appetite for national security, and hence the investment from national security into science decrease? Basically, can NSF go up as DOD's budget goes down? Well, I'm not too sanguine on that point. But that's certainly a question.

Secondly, will private investment in technology increase as fast as public investment in technology because of the shrinking national security community goes down?

Well, there my main worry is not so much the overall levels, because the venture capital community is quite healthy, although corporate R&D you can argue about. But I'm concerned with the particular kind of activity, which I think is very damaged by the change – namely building large-scale prototypes, doing grand, big things. That's just not something that's real easy to do in any single company.

I know back in the 70s I was right in the midst of the arguments of, should we build an Internet or think about it? And, you know without building it, you wouldn't have it. And large projects like this just won't happen unless somebody has the wherewithal and the will to build large things over lots of years. And that, to me, is the thing that's most hurt by this kind of change.

When I ended typing my remarks pretty early this morning, I felt discouraged because I wanted something real positive and prescriptive to say. And, frankly, I was embarrassed that I couldn't think of any great insight on what to do, because actually these trends are not all that positive for science, while probably true.

And the reason, in a nut shell, was that everything I could think of was less than five percent likely to actually happen, because of all the forces against it. And maybe I've just gotten to a point in life where things that are less than five percent likely to happen don't quite bubble up. But, in any case, those were the remarks I wanted to make just to get us going. And perhaps I can turn it back to you now. Thank you. (applause)

CROW: Thank you, Craig. Next we will turn to Ken Flamm. Ken is a Senior Fellow in Foreign Policy Studies at the Brookings Institution. Prior to that he has served in the Defense Department, and has spent most of his career as an analyst looking at issues associated with defense-related policies and some of them as they relate to science.

Right now, he's also working on a study of trade-offs and policy issues that affect the restructuring of the U.S. defense industrial base. And so Ken will be giving us that perspective.

FLAMM: I've spent most of my career looking at high-tech industry. And you could argue that certainly the histories of high-tech industry in this country and the Department of Defense are closely intertwined, in years after World War Two.

On the other hand, the sort of close-up, ant's eye view that I got of the Defense Department over the last couple years was an experience I hadn't had before and certainly taught me a lot of things, some of which are probably transmissible, and others which are not.

I'd like to start out by thanking you for inviting me to this symposium, and to thanking the organizers, and also thanking all of you for giving me 20 minutes of your time. The topic I was given was to look at is "National Security R&D and Its Interaction with the National Innovation System." It's a pretty broad topic.

I think I'd like to do two things. When I stand before you today, I actually have two diseases. I have a cold, which I'm sort of getting over, but since there's no water here I may sort of cough from time to time. The other disease is from when I was in DOD, where it's impossible to go through any kind of meeting or presentation without preparing some transparencies. I see Craig has basically kicked the habit, but I'm still suffering, I'm hooked. So I have a few slides I'd like to show you today.

Before we get to the slides, though, let me sketch out what I'd like to do. All right, first of all, we were explicitly challenged to be prescriptive. That was my understanding of what this meeting was all about. And, like Craig, I ended up sort of throwing my hands in the air a bit. I think I'm going to try to be a little bit more prescriptive, but you're still going to be radically disappointed in what I have to say in terms of solutions to the problem.

I think you can't really talk about where we are today without understanding a little bit about where we came from. Certainly, the context for national security R&D and the Cold War was, as Craig pointed out, rising DOD budgets, a benign conspiracy to basically fund a broad, large-scale investment in science and technology infrastructure in this country under the guise of national security. Yes, it certainly contributed to national security, but it also had broader designs. I think you can't look at the history of investments in any of the technologies without concluding that.

And basically the role of the government sort of reached its nadir in the early1960s when government spending actually at one point approached two thirds of total national investment in research and development. Basically, the national innovation system and the national security area in the early 1960s, certainly, had two major purposes. One of those was to deliver the military hardware that would give our forces a qualitative technological advantage in the event of any conflict.

And the second function was this broad view that the DOD basically had this major responsibility for S&T infrastructure, that it had to invest basically in the broader U.S. high-technology industrial base. And in implementing that broader view, in the early post war decades, a broad variety of players were brought into the game. Companies that were primarily commercial companies did a lot of DOD R&D in the 1950s and 1960s. And that's a situation that's changed a bit, and I think an important thing to talk about.

But in the intervening years, there have been some important changes. And the most important change, I think, is that in many of the high-technology areas where DOD was initially the driving force of fueling and funding much in the investment research and development, those commercial industries burst forth from those early seeds. And in flowering and growing and becoming important and growing quickly, ultimately the DOD influence receded and receded and receded until, particularly in the information technology area, DOD is basically a bit player today. Except in certain leading edge niche areas, DOD doesn't drive the thrust of information technology today.

I'd like to put up slide one, and I'm going to bore you with some historical slides, because I think there are some points that are worth making. This is in current dollars, by the way. If you go back to 1960, the amount of spending on national security by the government on research and development was about 50% greater than the total of industry. Essentially, not just DOD but national security was 50% more funds than all of industrial spending on R&D.

The key point today: not only is the shoe reversed, but industry spends about three times what we spend on R&D and national investment. That's a huge shift. And the era in which that shift occurred was the 1960s. And I'm going to argue today that the 1960s – in preparing for this talk, I went back and I actually tried to pull together some numbers, because I think there are some interesting lessons in the numbers – that the 1960s are really a critical decade for understanding what happened in our policy towards investment in technology.

This is an old slide from a book I did some years ago. But I think there's an important point here. This is an index, in constant real dollars, inflated by price index. There was this huge build-up in research and development by the DOD over the period from the late 1950s through about the mid to late 1960s. And then there was another build-up, of course, in the 1980s with a big trough in between. If you extended this figure off to 1996, it's basically been flat.

But there's an important point here. And the point is that relatively more of this huge bubble in funding that went into DOD research and development in the 1960s went into research, whereas in the 1980s, with the second epic of big build-up and DOD spending in R&D, a lot more of it went into systems stuff, development of systems. And it's not

(CUT IN TAPE)

on the sort of basic and applied research end of the R&D spectrum that occurred in the 1960s. If you think back on all the little stories we know about DOD influence on development of certain parts of the high-tech industry – computers, semiconductors – really a lot of the things that we talked about today were projects that really had their roots in the 1960s, in this period when we had broad-based support. So the '60s were a crucial period. But clearly, over time, there's been a real shift in the relationship between DOD spending and industrial spending in R&D.

The other thing that happened in the 1960s is that the rest of the world caught on to the fact that all this spending in R&D was having concrete economic effects. If you go back – and the data here are really terrible, by the way – in 1960, the U.S. was outspending the four principal competitors in high tech by about four to one. That is, we were spending roughly four times what they were spending in R&D.

Ten years later, in 1970, that number was more like 1.5 to one. The rest of the world caught onto the name of the game and made a determined effort to catch up, at least in terms of spending resources. And if you then switch to 1993, we have to change our base, because I'm going to look at seven countries. And it drops, not a huge amount, but down. The U.S. has receded in terms of its overall share of the pie, but not so much.

But the point I'd like to make is that the 1960s were clearly the era in which we were spending an awful lot on general technological infrastructure out of the DOD larder, so to speak. And it was also the era in which other folks made a determined effort to catch up and really increase their spending in R&D a lot. The research and development enterprise became a lot more globalized during the 1960s. And it was really the key decade for that.

The third kind of change that occurred is the distance between the defense market and the commercial market. And I'm not going to dwell on it at great length. But clearly, if you go back and you trace out the history of our procurement system in the Department of Defense, it’s quite interesting. We have a very elaborate, structured procurement system, with very rigid review sessions and meetings and different decision points. Well, this didn't come into existence until the early 1960s.

And over time, it’s become even more rigid. Basically, these barnacles have been building up on the procurement system since about the early 1960s. At the same time, the set of suppliers who are willing to work with this encrusted procurement system has shrunk because, while commercial markets have been growing and the DOD's relative share has been sinking, the difficulties of working with the DOD have multiplied. The folks who do business with the DOD have shrunk relative to the rest of the economy.

Which brings us to the present day. And we have to ask ourselves, if we just look back at the '50s and '60s from the standpoint of the 1990s, what is it that's really changed? What is it that we have to cope with?

Well, the problem with going after Craig is that I have to parrot some of the observations Craig made. But clearly, they’re correct. The Cold War is over. The Department of Defense budget is going down. The military customer is much less important to many high-technology industries, and in particular for information technology. And it's also clear that the military customers, as they did in the 1970s in the face of diminishing budgets, are not going to fund broader general infrastructure kind of stuff. The military departments are simply uninterested. Their priority is modernizing their equipment, pure and simple. And they’re not going to be interested in these broader issues of worrying about the technology base. That's somebody else's problem. That's not a problem they're interested in solving. And this same phenomenon of course happened in the 1970s.

So, if we ask ourselves what is it exactly that the system of national security research and development has to do today, and what are the problems it's going to face, it seems to me it boils down to four problems that have to be solved.

The first of these problems is what I would call defense-unique technology development. That is, there are a certain set of technologies and products that are unique to the Department of Defense. There simply isn't going to be a big commercial trade for tanks or armor. There's not going to be a big market for Stealth, there simply isn't a commercial demand for that.

The main policy issue wrapped up in developing defense-unique technologies is the linkage to commercial industry. On the one hand, if you try to use commercial suppliers in an era of globalized industry, any kind of effervescent strategic differentiating advantage, as Craig described it, you want to build has got a chance to defuse out.

So, if you want to build yourself a qualitative military advantage that is yours and yours alone to keep, you're going to try to build walls around the suppliers who are providing that capability to you. And if you build walls around that supplier providing that capability to you, you're also building walls between them and commercial industry and the way commercial industry functions.

So, in defense, technology development is what DOD is naturally going to focus on. At the same time, it's going to accentuate the distance between it and the commercial industrial sector. And it's also going to hinder development of linkages with commercial industry. There's just no way around it.

The second major responsibility of the national security innovation system today – given that the Department of Defense is going to be increasingly reliant, as everyone accepts, on commercial suppliers for broad segments of equipment and services, simply because they're done so much more cheaply – is to try to push the technology a little bit further. It may be so important to the performance of your systems that you want access to the latest and greatest a little sooner, or a little faster than the commercial industry is otherwise going to deliver it to you. Or you may be worried about your guys being able to deliver the same quality of technology that someone else has access to.

And in those dual use areas, you're going to want to make investments to make sure that you have access to the latest and greatest, where it's critical to your military systems. The problem here, of course, is selecting what those areas are, in an analytically sound fashion; and deciding how many resources to allocate to those areas.

The third kind of task that the national security R&D establishment faces is occasionally investing in what I would call the management of industrial vulnerabilities. Now, in the old days, you'd have to worry about ball-bearing factories being sabotaged or blowing up, and therefore not being able to deliver your gears to your airplanes and tanks. That's not so much a problem anymore, but there are other new kinds of industrial vulnerabilities.

For example, the kinds of problems people worry about today are somebody from outside coming in with a new information technology and taking down the national power grid, for example. Or taking down the telecommunications system. And there may be, even today in the radically changed world that we live in, certain kinds of scenarios and vulnerabilities of the industrial base that DOD's going to want to make some investment in remedying or doing something about in the name of national security. Again, it's a question of selecting those areas and allocating resources, which are the problems that DOD faces.

In the fourth area, which has historically been something DOD was not shy about stepping up to the plate and doing, is essentially sharing in the costs of maintenance of the sort of broad science and technology infrastructure that we've heard talked about several times today.

And it's the human resource part, training people, maintaining basic research in universities. Historically, DOD did its share, particular in the fat days of the 1960s when it was putting money into research, as it since hasn't.

But there's a real free-rider problem here. DOD increasingly, in an era of scarce resources, is cutting those sorts of generic, general kinds of investments in the infrastructure, figuring that, you know, let somebody else worry about it. Let the other parts of the government, let the private sector worry about it. And unfortunately, I don't think that's going to change either. DOD has limited resources. It's going to focus on what it needs, specific to DOD, and it's not going to worry about the other stuff.

It's going to assume that industry is able to supply it the latest and greatest without worrying explicitly about how industry is going to be able to do that in ten or 15 years. That's just the direction it's going in now. And perhaps Craig disagrees, but I saw that.

So, given these four basic tasks, and given that at best you're going to do task one fully, I come to the same point Craig came to, which was throwing up my arms in despair here. I'm going to go a little bit further. I'm going to pull 'em back down a little bit.

First, in a narrow way, what kinds of things can remedy some of the problems I've just described to you? DOD simply isn't going to be funding these things if we continue on the current track..

On funding defense-unique technology development and dual use technology, everyone realizes that we have to do some fundamental changes in the way DOD budgets. The budget process in Washington DC is a formidable obstacle to doing things efficiently. And in my view, even those people within the DSB (Defense Science Board) infrastructure for example, who are thinking carefully about this subject, realize that ultimately you have to budget on the basis not of individual programs but on functional capabilities. And you've got to have programs competing against one another, competing against multiple vendors, to be able to do things efficiently. And that process just doesn't exist right now. Certainly it's not the root of all evil, but the root of much of the evil is the fact that the Congress of the United States allocates programs on a line-by-line, program-by-program basis.

I can assure you that if there's one single intractable problem that has bedeviled every part of DOD that I've seen, it has been this ability of contractors, suppliers, special interests to work through the Congress and, in a very micromanaged way, to decide what gets built and what gets procured. The right way to do it, clearly, is to have a functional kind of budgeting process, which the Congress approves in broad strokes. And then the customer – i.e., the military – has the right to bring in the forces of competition between programs, between contractors.

Now, do I see any way this is going to happen politically? I'm afraid the answer is no. The vested interests involved and the procurement systems are so strong politically that, even though I can stand here with a straight face and tell you that this in my view is the single toughest problem, I see no solution. I just don't. And that is one of the reasons, despite all the rhetoric about acquisition reform that you hear, it's going to be damn hard to see all the concrete results that we wish to see coming out of the process.

Anyway, this is the problem with the first two items on my four-item task list there. What about items three and four? That is, working on the industrial vulnerability issue, working on the S&T infrastructure issue, in which DOD is one of the beneficiaries, along with industry and other parts of the government – as well as that second problem, that is, promoting dual use technology development in areas where it's going to be important to DOD.

It seems to me that there are more feasible solutions that are available potentially. Those solutions revolve around the players who are the beneficiaries of those investments – i.e., both industry and other parts of the government – coming together in some new kind of coordination mechanism that actually shares the responsibility, the budgetary resource responsibility, among all the beneficiaries of those programs.

And I think some of the experiments we've seen in joint government/industry programs say that it's not impossible to work out coordinating mechanisms to insolate it from the down side, which is political influence.

A few years back, I served on a National Research Council panel that recommended something like a civilian technology corporation that these government agencies could have their programs administered through – some impartial body that wasn't going to be immediately responsive to Congressional political pressure. Is that feasible politically? I don't know. It's an idea, and it might work, but clearly we need to do something.

I think we need to talk about some kind of analytical process and external review for some of these decisions in areas two, three, or four. That is, what is essential to the Department of Defense? What is a vulnerability the DOD should worry about? How much should DOD pay towards the maintenance of certain types of infrastructure? How important are those infrastructure investments?

In DOD, there really wasn't any analytical or externally reviewed process. And these decisions were, even though not explicitly political, nonetheless intensely political. And I think getting some dispassion into that budgetary process again is important. How to do it? It's a tough question. I'm not sure I see the solution. More broadly, it seems to me, there are two fundamental public policy issues that we're talking around the edges of today, which we really haven't resolved.

First of all, it seems to me that the non-national security rationale for public support of science and technology investment clearly has not been articulated in a way that's going to allow DOD to pass off this responsibility to another organization. Unfortunately, unlike the Yale motto, which I saw on your shield there today, the light hasn't quite shed on me yet, as to how we're going to do that.

The second problem, which I just want to bring to your attention, is that in some sense there's an internal conflict in the way we look at our interaction with the global system, within DOD. That is, on the one hand, a certain amount of not just DOD but the entire government sees research and development as a global public good, and they're pushing for sharing the resources for investing in this public good. At the same time, institutions like DOD, even if it is an international public good, are trying to build walls around it, and make it a national asset so we have our strategic differentiating technology advantage. So I think the tension between a global public good and DOD's efforts to seek strategic national advantage are an inherent tension that we still don't have an obvious solution for.

And having left you with a list of problems, I will depart. (applause)

CROW: In this room, we'll pretend we're in New Haven. And in the other room, we'll pretend we're in New York. (laughter) Thank you, Ken. I think it's worth noting, following Ken's remarks, that he served for two years as President Clinton's principal deputy assistant secretary of defense for economic security, which is probably the first time that position ever existed.

We have two discussants today. Our first is Sid Winter, who is a professor of management at the Wharton School of the University of Pennsylvania. Prior to joining the Wharton school, he served for several years at the chief economist of the General Accounting Office of the United States. And prior to that, he served in a number of economist roles at the RAND Corporation, the Council of Economic advisors, and others. So, Sid.

WINTER: Thank you. I'm going to take as the premise of my remarks a common message that came from Craig Fields and Ken Flamm and was also implicit in much of what was presented this morning. And that is the assumption that the era of benign conspiracy, the defense fig leaf, is over, or at least drawing to a close. And that poses a question about the future federal role in supporting, in particular, basic science and perhaps, as Craig has emphasized, some parts of technology.

I would go a little further than others in talking about the change in the environment. I think it is not just the fact that the Cold War is over, the defense budget is coming down, and the R&D budget in particular is coming down. I think that the general environment of budgetary stringency, plus some other visible trends in the system, is creating a situation where accountability and compelling rationales for federal expenditure are becoming a more and more prominent consideration. So if indeed the future is going to take the form in which there continues to be substantial federal support for basic science, then it seems to me that would imply the need for the construction of some alternative rationale and mechanisms of accountability.

So I do not believe that the past mission-oriented organization of federal participation in basic science is really viable in the present context. I think there are too many areas where people properly ask questions about what the taxpayer is getting for his or her money.

Now, Ralph Gomory put forward a very interesting suggestion, which we translate as saying, we want to win at least a bronze medal in every scientific event. But I think an interesting, important question to raise about that proposal is whether that is a politically viable line of argument. "Gee, let's support this federal participation in this area." I'm going to address, as a prominent candidate much mentioned, the idea that the alternative rationale is the contribution of science to the economy. And if that is the rationale, then we face a problem never faced in those same terms before. The mission-level goal definitions will not suffice. We would need some kind of a coherent economic case about the benefits of basic science. I recognize this is not the only alternative to this rationale, but it is a prominent one.

This idea of trying to do an analysis of the impact of science on the economy is a very difficult problem indeed. One necessarily has a great deal of skepticism about the possibility of anything that might be convincingly labeled as an objective answer to such a question. As a mental experiment, you can ask yourself about DOD basic science spending or about all federal basic science spending. And you ask how compelling a case could you make that what we're doing is the right number, compared to a number that's five times higher, or alternately, to a number that's five times lower. And I would say that, while there are a few areas where you can make that kind of case, overall we have very little guidance on what the answer would be.

Now, the fact that it's hard to get an objective answer doesn't mean that there won't be an answer. There are devices – legislatures, courts, juries, churches, various institutions in this society – that are well specialized in answering questions in practice where in principle it's very hard to lay out exactly what the answer should be.

So another thing that we should have in mind as we're thinking about this problem is the very important role of considerations of legitimacy, of the defensibility of processes, as opposed to the defensibility of outcomes. And the importance of the accountability issue that I had mentioned. Somebody saying "Your money was well spent," able to show money was spent on a particular undertaking. So, against that background, let me move with a little bit of boldness here towards the challenge of prescriptive analysis. And you don't have to worry that I'm going to provide the answer. But I'm going to put forward some alternative perspectives on how one could think about this issue, supplementing comments that have already been made.

These are four different ways of thinking about the issue of what is going to replace the benign conspiracy as a source of support for basic science. The first one, as I've already alluded to, would take on the economic question. One possible course of action would be to seriously address the issue of the economic justification and try to then base the defense on that.

Following Ed Steinmuller, what is needed is a proper cost-benefit analysis. And if you think about it, you realize that, although really compelling answers would be very hard to come by, there presumably are ways to apply some human intelligence to this issue, derive some conclusions from the experience of the past, and pick out things where there's a case and where there isn't. And one could only imagine taking it seriously if you have not merely the capability of doing an intellectually respectable job on the problem but that you also create institutions that have this required legitimacy – which would permit them to make judgment calls on allocation and not be shot down immediately in the political process.

The major obstacle here is, as I have suggested, the fact that we simply do not have great stores of existing expertise for addressing an issue of that scope. Certainly very little that has been done in the area of, say, evaluating rates of return on science would prepare you for the exercise that is involved there. What would be required is a time-consuming effort to build new capabilities and imbed them in new institutions.

Just as an aside here, although the prescriptive challenge is severe, the 50-year horizon ought to give us some freedom to think about directions and possible change. Fifty years is a long time. It's not nearly as long a time as it seemed 50 years ago, but it still is quite a long time.

The second general perspective I would offer on the prescriptive issue is, we could try to muddle through on something that is very close to the present path. And that would mean we made incremental shifts in spending towards research that had a reasonably arguable economic rationale, and funding shifts away from projects where the economic rationale was notably absent.

In favor of that alternative, it's relatively easy to implement. In fact, it would be difficult to avoid implementing it. That is the way the system would tend to go by itself. And it has the additional advantage that it is a mechanism that supports some kinds of diversity in the national science effort, in spite of its irregularities and flaws. And that is a desirable thing. But against it, one what would have to say, there are very important biases in the existing system.

The most central of these biases is the bias that favors what is against what could be. The political process responds to interests that have been developed, not to interests that could possibly be developed. And in many cases, that's going to be a significant portion of the picture. I think it has a bias of large versus small. It's easier to get a political constituency for a Star Wars or for a Superconducting Supercollider than it is for a lot of paper and pencil, theoretical research. And that is a part of the political environment.

I think you will see that if we continue on this path, and economics is emphasized more, that recent achievements will get excessive weight. That something will happen, will come out of a particular program, be of some economic interest, that will get some coverage. The Congress will respond and it will go off in that direction, rather than looking at longer term track records and longer term prospects. So I think those are significant shortcomings, and a common theme throughout them is the point that jobs matter an awful lot in the political context.

And I want to read from the introduction of Vannevar Bush's book: "What we often forget are the millions of pay envelopes on a peacetime Saturday night, which are filled because new products and new industries have provided jobs for countless Americans. Science made that possible, too." Now you notice that it wasn't the relief of household drudgery. Or the improvement in entertainment possibilities. It was the interest of the producers, the jobs that were created, as a result of the science and innovation. Now you might think that was just a reflection of the Great Depression. But on Capitol Hill, that is still a very dominant mode of economic thinking.

A third perspective is to think of the grassroots-politics approach to the problem of basic science in the country. And that would mean a long-term effort to try to mold the public towards a better understanding of science and its meaning. Not just as a source of economic benefit, but also its meaning as a source of meaning. Of interpretation of your place in the universe.

That is a course of action that could be pursued and probably to positive effect. I think very little has actually been done. I doubt that we know very much about what the public really thinks about science, or what you would find if you went past a superficial quick questionnaire about attitudes. And there would be some possibility at least of understanding how one could start to shape a program of that kind. Maybe in an off year, it would be a good idea to hire some political consultants, who in my opinion probably take the prize as the most commercially successful example of applied social science. It's a significant industry, it's hidden from view, but they are very effective and very sophisticated. And we could probably learn something by getting them to ask the public what they thought was worth supporting in science.

Those are issues under the heading of how you could build a new framework of some kind that would provide a basis for a continuing federal role in funding. As a fourth perspective, I want to raise the question of the role of private philanthropy in areas where the economic argument is weak, where by anybody's account, curiosity is the primary driver.

I am not optimistic about the prospects of convincing the average voter that he cares about whether the universe is closed or not. There is a role for that kind of research – I'm not sure that public funding is appropriate there – and we do have private philanthropy as an alternative. We could do many things to strengthen that alternative, changes in tax treatments, changes in the rules, and so forth.

Those are my four proposed perspectives. You can think–

(BREAK IN TAPE)

Just by mixing and matching to some degree, they're not mutually exclusive.

But as we sit here and contemplate this 50-year horizon, we should remember that our attention is going to be jerked around in the future by a bunch of considerations extraneous to science. In particular, when the country gets around to confronting its entitlement crisis, the social security and Medicare crises, then the budgetary situation will be different. We don't know when we will have this awakening. We can imagine major events in international relations, wars, major terrorist actions. We can imagine major advances in science, directly affecting these issues.

So it certainly would be bold to try to predict where we're actually going and bold to try to prescribe for it. But I think there is nevertheless a compelling argument that we need some new arrangements, and some new institutions, some new arguments, to support the funding of basic science. Thank you. (applause)

CROW: Our last panelist before questions is going to be John Pike. John is the director of the space policy project at the Federation of American Scientists, where he coordinates research on military and civilian space policy and also national security issues. Then we're going to move to questions immediately after that.

PIKE: I'm normally the fellow who gets dragged out to tell why Star Wars is a bad idea and international space cooperation is a good idea. Having been doing that relentlessly for the last dozen years, I figure you already are familiar with those issues, and so consider that I've already delivered my rants on those subjects.

And since I'm a discussant rather than a panelist, I feel some obligation to respond to the presentations that we've had here this afternoon. I was really struck by the fundamental point that particularly Craig and Ken were raising about the relationship between national security, Defense Department spending on technology, and the question of preserving unique American advantages.

In Craig's prepared remarks, he noted the question of placing priority on the use of defense funds for the development of required military-unique technology, which otherwise would not be developed at all – the question of undercutting the notion of gaining differentiated U.S. military advantage from basic science and fundamentally dual use technology.

And then Ken was raising the issue of how DOD can be confident that its commercial supplier base will always provide it with early, assured, and affordable access to the very best technology, globally, in areas critical to military advantage. Now, in the few minutes that I'm going to keep you from getting something to drink, I'm not going to be able to address the full range of these technologies and institutional considerations. But I would like to briefly conjure with them in two areas of particular interest to me and, I believe, particular importance to the United States: what I would term for simplicity outer space and cyber space, the question of our national security space program and the question of our intelligence program, including most particularly information systems. Collectively, these account for approximately $50 billion a year, about one-fifth of the entire defense budget. When you go to the high-technology center and ignore all of the soldiers with dirty boots and everything, they probably account for nearly half of military spending.

If we look back over, for instance, Operation Desert Storm and ask what were the things that differentiated Coalition forces from our unfortunate Iraqi adversaries, it would have to be the unique advantages that the United States was displaying in outer space and cyber space. Having said this, I think that it's important to caveat this discussion by recognizing that a lot of the high-tech advantages that the United States enjoyed in confronting a high-tech adversary during the Cold War may be of declining relevance in confronting low-tech adversaries or low-tech situations in a post-Cold War environment.

The Soviet Union was after all a worthy adversary and basically attempted to have one of everything. But when we insert out troops into a situation like Somalia, where we have an adversary that's using drums and whistles to communicate, the fact that we have millions of dollars of signal intelligence satellites in the sky probably isn't going to provide us quite the advantage that it did with respect to the Soviet adversary.

I'd also caveat these observations by suggesting that in contrast to the confrontation during the Cold War, where it was possible, or at least arguable, to equate national security with military forces, it's clear that in a post-Cold War environment, national security must have a much broader definition than merely that which the Pentagon does when it gets into the office every morning. Obviously, environmental considerations, economic considerations, and so forth impinge on national security in a way that extends beyond simply what the Defense Department does. I would suggest that how we are succeeding and not succeeding in adjusting to the opportunities afforded by commercial technologies in a post-Cold War environment, in outer space and cyber space, says a lot about what can be done. And a lot about what should be done.

For the sake of argument, I would submit that we have a singular set of success stories in the national security space program, in leveraging commercially available technology. In fact, this is probably one of the great underreported success stories of the Clinton administration's acquisition reform initiatives, in terms of attempting to leverage commercially available technology for national security applications.

I would suggest that what we have been doing, or rather what we have not been doing, on the cyber space front in the area of intelligence reform is probably the most glaring set of examples of failures to adjust to the new national security realities and the opportunities for leveraging commercial technology. When I go down the list of what is going on in our military outer space programs today, I think that this is probably the place where we have seen the largest convergence between military and commercial space systems, the greatest emphasis on leveraging the synergy between national security and civil technologies – the availability of the global positioning system and the convergence of the military and civil weather satellite programs. The emerging availability of high-resolution, commercially available reconnaissance satellite imagery, that's going to be of interest to folks like me, to the Defense Department, to terrorists in North Korea, and all kinds of other people.

Certainly, the most singularly underreported success story in the convergence of military and civil space technology is the Air Force's Evolved Expendable Launch Vehicle (EELV) program, which is basically encouraging the development of an expendable launch vehicle that's going to be useful to the military, civil government, and commercial interests in the United States. That's going to result, I believe, in a significant reduction in American launch costs and a significant improvement in the competitive position of the American launch vehicle industry, with respect to those of other countries.

There are still some problems in the national security space arena. We continue to be far too focused on technology push and not nearly focused enough on demand pull. We're still focused on coming up with new pieces of hardware and not nearly focused enough on satisfying user requirements. And unfortunately, some of the organizational innovations that we've seen recently, I think, have moved us in the wrong direction. Instead of focusing on satisfying the identifiable demands of existing users, I think that we've gone in the other direction, focusing on a technology push.

But whatever shortcomings we continue to have in the national security space arena, I think, unfortunately, are utterly dwarfed by the problems that we've had on the front of reforming our intelligence community in the post-Cold War world. And if anyone doubts that the American intelligence community is broken and in need of some serious repair, I urge you to take the time to read the Intelligence Community 21st Century Staff Study, prepared by the House Permanent Select Committee on Intelligence, which is available on the Internet, since they've sold out of all of the hard copies. It makes extremely sobering and provocative reading. If you're only going to read one thing this year on the intelligence community, that should be it. I think that it makes a devastating case about the difficulties the intelligence community has had in adapting to the new information environment.

And if anyone doubts that improvement is needed, just go and look at the way the Central Intelligence Agency's website was hacked by people from Sweden a couple of days ago. Or the Department of Justice website, hacked a couple of weeks ago.

Where to begin cataloging all of the problems? Well, the National Security Agency would certainly be a very good place to begin. The staff study makes a very compelling case that the sort of signals intelligence activities that the NSA has traditionally done during the Cold War are fundamentally broken and cannot be fixed.

Unfortunately, we continue to have a cryptography policy that is predicated on the proposition of making the world safe for the National Security Agency, while leaving the rest of America vulnerable to other people who would be interested in conducting attacks against our information infrastructure. There's no clearer case where we're going to have to choose between national security in terms of what the national security apparatus does when it comes into the office every morning and national security considered in terms of the national well-being of our society as a whole.

Given that the National Security Agency worries about this full time and other people worry about it part time, we continue to have policies that are far too focused on making the world safe for our national security apparatus rather than making the world a safer place for America generally.

I know that you're all getting thirsty, and you're all looking forward to putting your own two cents' worth in. I would conclude by saying that I think we have fundamental advantages in terms of leveraging commercial technology, if for no other reason than the United States is big, and all of these other countries are small. The American intelligence community budget today is larger than the Italian defense budget. The reason that the evolved expendable launch vehicle program is going to put the European Ariane launch vehicle program into such a world of hurt is because the overall space program is about five time bigger than the European space program, providing a captive market for our commercial products that simply cannot be matched anywhere else.

So I think the challenge, rather than focusing on attempting to preserve defense-unique technologies, is attempting to advance our national well-being as a whole and use that to leverage the advantages that we can secure in national security. Thank you. (applause)

CROW: Thank you, John. We have time for a few questions at the microphone. If you just go to the microphone, identify yourself, ask the question generally, and one or more of the panelists can come up and grab one of these microphones, or come back up to this one, and give an answer.

GOMORY: I'd like to make a comment. There's been a very well developed and, I think, excellent theme. Both Craig and Ken pointed to the decay or absence of the fig leaf, which has been so important for this country. And then Sid went along to suggest a number of replacement leaves, of which I think the most plausible is the economic one.

But I want to raise one point in connection with the economic one, which may sound perhaps esoteric or too detailed, but I think it's actually right. And that is, do you think that economic justification comes in lumps or in microscopic increments? Let me explain what that means. Sid said we'd need a big analysis, perhaps to show some sort of return on investment. And I think that would be necessary were it true that the contributions to the economy are the sum of a number of microscopic improvements. But I think one can argue that this is the kind of event in which a few big ones make all the difference.

For example, the creation of the semiconductor and the new biotechnology industries. A great deal of the value spent on science is reaped from a few visible events, not through extensive, microscopic examination. The creation of jobs – and I completely agree with Sid's emphasis on that – in these industries is probably not only one of their great contributions, but it is one that can be communicated whereas a detailed analysis would not be.

So I do think there is a case to be made along the lines that Sid has recommended. I think it can, however, be made relatively easily and not by a sense of analysis. I might add that my own proposals are tuned to that model, and that's why I think it's important, as he put it, to be in line for all the bronze medals. And to get an occasional big winner, called the gold medal. Thank you.

CROW: Yes, panelists, and by the way, for those of you not familiar with the back of Ralph's head, that's Ralph Gomory of the Sloan Foundation. Sid, or anyone? Craig, Ken?

MALE VOICE: I want to turn the question around, back to you again. And that is, it seems to me that there is a demonstrated track record on what happens when you try justifying government technology programs in terms of economic benefits. The comeback is that there is an ideological attack: how can the government make judgments about the economic benefits and programs? You're picking winners and losers.

The point you're making, by the way, I would interpret a little bit differently, that there are a few very huge winners and lots of losers. But that gets at the nature of research. Research is finding out about stuff that doesn't work, too – that has positive social value, even if it doesn't work.

The question I want to throw back to you is, the minute you start going out there and saying, we're going to make some bets – which is what you're fundamentally saying – that we're going to put resources in areas where we think there’s a positive economic payoff, unless I misinterpret you, you're going to invite that attack.

GOMORY: I don't want to monopolize this microphone. Whatever anyone says, I promise not to return. But your remarks were so interesting, that I would like to respond.

I think it's exactly that element of the unpredictable that I am trying to talk about, and talked about this morning, which is, yes, it's hard to pick winners and losers. And that has been very effectively characterized as a futile activity, though it may not be. But anyway, it's dead from a political point of view.

But you see, those sorts of programs that you described, which I think are also not viable, are really a selection of small things. And I think a much better defense, and a much more realistic one, is to say fund basic research and related things across the board, because the historical record shows us that unpredictably there are some very big winners.

EISENBERGER: Peter Eisenberger, formally of Princeton, presently of Columbia. I'm always amazed in these discussions that we struggle with the issue of industrial policy, the role of the government, and economic development, and we don’t go directly to what everybody I think would agree is the responsibility of government – that is, to provide for the well-being of their citizens. And it seems to me that we have a lot of problems, whether it is infrastructure, environment, or education. And it seems to me that would provide a very natural focus for many research efforts.

Now, we don't have to be organized that way. And I suggest what we’re trying to do is take the existing structure that developed in the old system, and twist it to try to find missions that are acceptable rather than ask ourselves, well, if we look in the future, what do we really need? And clearly we need to address the social, infrastructure, educational concerns of society. And the environmental concerns of society. So I guess I'd be interested why we’re not talking about that. And why that's just a natural obvious solution. Who could argue with that politically? That doesn't violate anybody's religious tenants about the proper role of government. It is the role of government. And again, is this because the people are not at the table? I'd be interested in people's comments, as to why that's just not automatically occurring.

WINTER: I think that's a very good question. It was remarked this morning, however, that social science among all sciences is particularly vulnerable in the current environment. Because of ideological issues, addressing social problems by means of science is not considered legitimate by many parts of our population. Were that not the case, I think the question raised would be extremely compelling. And it would be obvious that we should turn in that sort of direction. But at least under current political conditions, it's far from an easy thing to do.

LUBELL: I'm Michael Lubell, professor of physics at C.C.N.Y. and director of public affairs for the American Physical Society. The question for Craig Fields: the labs in the Department of Energy, in the Galvin report, were criticized for competing with each other and not cooperating as much. This was also said about the defense laboratories. Obviously, some political decisions have been made to keep the defense laboratories more or less intact. But still, I have a suspicion that it was not the competitiveness issue that drove it. And I'd your reaction to that.

MALE VOICE: I think I've said, or at least meant to say, that I think the department's leadership is acknowledging more and more value of competition. You know, vis-a-vis competition versus cooperation, Karl Marx thought he had a pretty good idea – it sounded good, but didn’t work so well. And in the trade-off between what I'll call competition with duplication and cooperation without duplication, one is more aligned with human behavior than the other. So I think that there's really an inexorable push in this government, irrespective of the outcome of the election, towards more competition. And that also applies to the national labs. I don't view the national labs as more likely to cooperate with each other, since in fact they’re organizations of human beings, with normal human motivations.

And furthermore, I think there's likely to be an increase in the desires on the part of the administration to have processes and procedures that choose the best way of getting work done, quite irrespective of the categorization of the actor that might do the work. When I was at ARPA, we sent quite a lot of money to Los Alamos and Sandia and a number of other national labs. And in every single case that I was aware of, it was because they were the best to do the job. And we informally saw to it that the right kind of competition took place. So I think that is the direction that's going to work. And while there are statements you can make for and against competition, I think history has proven which one actually works.

CROW: Before we break for something to drink, I'd like to encourage you all to be understanding with us in the sense that we realize that the way we have set this conference up is an endurance test for the participants. We're trying to be true to Vannevar Bush's design. That is, we're trying to take it apart and look to the future, piece by piece. And so we have packed all of this into two days to accomplish that. So why don't you get something to drink, and come back in about ten minutes. And let's thank our panel. (applause)

Design Area Four: Health Research, Health Cost Explosion and Quality of Life

Kenneth Shine
Nathan Rosenberg
Annetine Gelijns
Herbert Pardes

Moderator
Jonathan Cole

COLE: Well, we turn now to the fourth design area, "Health Research, Health Costs, Explosion and Quality of Life." And this is a central concern to the national innovation system. While public support for biomedical research shows little sign of declining – and I think for that we do have some interesting data produced by Research America and various other polling and survey agencies – there is growing concern for health expenditures, which until very recently were exploding and which seemed to be putting great stresses on the system. And those rising costs seemed to be calling for some possibly significant design changes. For those who work in research universities, we're all familiar with the transformation and the relative costs of building up the health, science, research enterprise, relative to some of the basic sciences.

It used to be that outfitting a physics laboratory was considered highly expensive. Now we turn to the health sciences, and we see multiples of that very often. The size of the budgets at the research universities that are associated with the health sciences complexes have become enormous in relative terms. And their growth has been relatively faster than other areas of research universities.

We are fortunate to have very distinguished people with us this afternoon. The first design presenter will be Dr. Kenneth Shine, who is the president of the Institute of Medicine of the National Academy of Sciences, and professor of medicine emeritus at the University of California, Los Angeles. And he is UCLA's School of Medicine's immediate past dean and provost for the medical sciences. He is surely an expert in this area, and he is going to share his thoughts with us this afternoon. It's a pleasure.

SHINE: The fall of the Soviet Union called for a new paradigm for science policy in the United States. We still don't have that paradigm, but I believe that there is a critical need for a coherent concept, shared broadly by the scientific community in the health and non-health sciences, in order to make the argument for federal funding. The economic prosperity argument is useful but incomplete, and it will make us vulnerable when it comes time for cost accounting in particular areas. Moreover, it leads to the potential risk of deciding which areas of fundamental science are most likely to produce that economic prosperity, something we don't know how to do and which we must clearly avoid. Poverty, poor jobs, pollution, and disease are every bit as dangerous as the evil empire. Creating a healthy population and prospering in a sustainable environment is every bit as good a goal as dealing with military preparedness.

Whatever the new paradigm for science policy may be, we as a scientific group have to come to some closure as to what the message is. Economics can be a piece of it, but to stake the whole argument on economic prosperity means that there will be many, many members of society – the environmentalists, those who don't share in the results of the stock market, and a whole variety of other people – who are not going to buy in.

The health science enterprise is, relatively speaking, very successful. At the federal level, of the $70 or $75 billion invested in R&D, perhaps half of that is truly basic research, and over a third of that is in health. That portion is going to grow. The pharmaceutical and medical device industries are increasing their investment. Discussions with pharmaceutical houses clearly demonstrate that well over 90 percent of their investment is in drug development, with ten percent or less is in what anyone would call basic science. Even now, they're using new nomenclature, which I've heard from Pfizer, Bristol Myers, Squibb, and others: they say "directed basic research," or "targeted basic science."

The notion that these industries are going to provide support for basic science on an industry level is naive. Moreover, in the health care industry, it has been possible to say that good basic science policy is good industrial policy. There are many reasons why the National Institutes of Health receives large increases in its budget. One is that many in Congress identify with health. In terms of public understanding of science, health and health sciences are areas in which they haven't the foggiest idea about what goes in terms of molecular biology, but they do think they know something about cancer.

I am always amused by the discussion about the disease orientation of the health sciences community in raising money. Note that 78 percent of the electrical engineers in the United States were trained under a budget designed to deal with war, which is as good a disease as any. And in fact, the conquering of that disease created a problem for the Department of Defense.

Coming back to the NIH budget, Congress has some understanding about health. There is a broad constituency in science that works hard with letter writing, testimony, meetings, and contacts. But, interestingly enough, one of the key determinants of the budgetary increases for NIH is that representatives of the bio-technology industry have gone to Congress and said, "Our development as an industry occurs in basic science laboratories, funded by the NIH. Fund the NIH." That kind coalition is critical in all areas of science, and the challenge is to develop a means to put together those kinds of coalitions in other areas.

I'm going to make a couple of general observations about health science. Then I will outline some of the major developments in the health care environment, and what I believe their implications are for universities and academic health centers.

I think the 20th century, which began with things like x-rays and Einstein, and went through the atomic bomb and space program, was a century of physics, physical sciences, and engineering. The 21st century is the century of the life sciences. Not just health, but also agriculture, fisheries, and chemistry, where the chemical industry will be producing through biological organisms many of the compounds formerly made by chemists. The work to clean up the Exxon Valdez is but one example of the usefulness of biological strategies to solve problems once left to the chemists.

That momentum, both in terms of funding and of intellectual direction, is imperative for finding ways in science to bring physicists as well as behavioral scientists together with health scientists and biological scientists to solve new problems. By physicists, I mean people who have a concept about the way physics can interface with biological systems. There is plenty that will happen in the life sciences that will do that. Moreover, the social and behavioral sciences will emerge as the health care system matures, because there will be money in it. I will return to this point later.

The message that I want to convey is that the role of the health science enterprise will increasingly become the role of the university. And the separations between faculties in physical, chemical, and behavioral sciences and those in the academic health center will have to be overcome. In some cases, these changes will come by force, by changes in the health care system.

In broad sweeping terms, the health care system is going from a cottage industry in which individual practitioners did for patients what they remembered in the last successful case they treated, with limited numbers of records and no capacity to analyze in the aggregate the impact of their work (with the exception of certain surgical procedures), to a system of organized health care delivery plans. In fact, health is becoming, and has become, an industry. The impact of this has been to create remarkable consolidation among providers, insurers, and others. In the early 1990s, I predicted that most major metropolitan areas in the United States would, by the end of the decade, have between two and six principal networks of providers for about 80 or 85 percent of the population. I had the direction right but not the number: six is too many. Even in New York, it may be closer to four.

In any case, there will be a limited number of systems of care. Those systems of care will continue to consolidate, in terms of trying to deal with excess capacity, and take advantage with regard to issues of scale and information systems. Those systems will, for the first time, offer some real opportunities to practice health scientifically, because it will be possible to collect data about what happens to both individuals and to groups of patients.

For the first time, it will be possible to think about the health of populations, and come to grips with the most difficult scientific question in health, how to adjust for risk. In an environment in which you want to pay for health, and you'd like to pay for as many people as possible, how do you figure out ways to pay the right amount for people who are at very low risk for illness, versus those at very high risk for illness?

These systems also will provide opportunities to do serious research on the outcomes of care and to develop improved quality of care. Our own research suggests that quality of care is not improved by individual providers, it's improved by enhancing systems of care. That requires organization.

That sounds good, but there are a few problems with this scenario. First, the driving force in all of this activity is cost. None of these organizations wants to pay any more than it has to, particularly those that are providing a return on investment to shareholders. The biggest single challenge in this system is how to prevent these organizations from doing too little, too late, by not making information available and not providing the kind of services that ought to be provided.

I predict that states will pass extensive health care regulations, and that the federal government will have to get involved in order to rationalize the different regulations begin imposed by the states. In an environment in which cost is the driving factor, there is very little opportunity to support research and education. That is where the biggest challenges exist for our research enterprise.

There are other challenges as well. For example, consolidation in both medical schools and hospitals. Administrators, understandably, want to achieve economies of scale. There may also be changes in what the federal government will fund. Where once it funded a particular unit, one per institution, what happens to the two federally funded activities when two units are merged?

There is a whole series of questions that arise, but none is more important than the culture of the institutions. Moreover, for many of these institutions, there is a fundamental need to identify their true core competencies. Many of these institutions are spinning off, consolidating, and changing the health care delivery side of the operation. Don't think they aren't going to change the science side, as well. In some cases, it will involve consolidations of basic science departments with basic science departments in the general campus.

Consolidation models are beginning to percolate around the health care system. That's what I was referring to when I said there was going to be juxtaposition of science and the health sciences on the university campus to a far greater extent than anyone would have imagined a few years ago. As funding sources shrink and reorganizations take place, those kinds of reassessments will occur.

Health care dollars have contributed between $800 million and $2.5 billion a year to research in the United States. This funding supports between 15 and 30 percent of biomedical research. It supports clinical studies and basic science.

What are the policy implications? I strongly support instituting an assessment on health care premiums to support research – something on the order of one to 1.5 percent, and an all-payers plan in support of research and education. I also want to emphasize my belief, which is not shared by all scientists by any means, that those funds ought to go to clinical research. That is, research involving disease states.

My reasons are as follows: First, I think insurers, patients, and health care providers understand that putting money from the health care dollar into experimentation and trials can improve care directly. Second, public policy in this country has been such that Congress has supported the basic science budget of the National Institutes of Health. If a stream of money from the health care system is used to support basic science, I believe Congress will stop providing direct appropriations and turn to the health care system for the money. Third, what I hear from the managed care organizations, both for-profit and not-for-profit is, "Why should we support research? We pay our taxes, and the taxes go to the National Institutes of Health." My answer is, "You're absolutely right. Your taxes to go the National Institutes of Health for fundamental laboratory research. But we're talking about clinical research, which you need to improve the quality of services in your organizations. And finally, if you're all paying one percent, nobody gets a price advantage." Under those circumstances, I believe one can encourage such a policy.

Let me then conclude by indicating some of the likely changes affecting academic health centers and the research enterprise. First, there will be an increasing emphasis on core competencies in research. I predict that in the next eight to ten years, the number of truly comprehensive academic health centers doing research in all areas will shrink dramatically. Increasingly, they will have to decide what areas they want to be preeminent in, what are the critical masses required, and how to make investments in them.

Second, there will be increasing differentiation of faculty in these institutions. Some of them may even spin off research institutes with faculty who get full compensation from funding agencies for their salaries and cannot expect to get clinical dollars for this purpose. At the same time, there will be other individuals in the health care delivery business who will be primarily involved in the care of patients.

A relatively small number of individuals will be needed as bridges, clinical investigators who will have to submit protocols for research. These proposals can be within the National Institutes of Health, but if the investigators are using money from the health care system, the proposals should be peer-reviewed by the institutions themselves. Today, if you have human subjects approval, you can do research in most institutions. That cannot continue. Institutions must look at the quality of the research being conducted with health care dollars, decide what is the most important research, peer review it, and make sure the resources are used in a significant and important way.

Outcomes and research and technology assessment will be key in this cost-oriented environment. Here academic centers have a great deal to contribute. However, in the area of drug trials, for example, there is a budding industry in the private sector to evaluate drugs. For those pharmaceutically-oriented activities to continue in academic health centers, the centers will have to develop a methodology as competitive as the private sector's. Some are trying to do that. Others will decide that is not central to their scientific mission.

The ultimate effect of such change will be to take the health care delivery portion of the enterprise farther from the university, and the research and academic health center portion closer to the university, with the exception that health services research, outcomes research, and technology assessment must be a part of both.

In sum, we need a coherent message. I believe that the message must relate to a public understanding of what we do in terms of its outcome and not necessarily a public understanding of the details by which we do it. We need a funding stream that will allow expansion of the life sciences.

I believe this is feasible. It will take a number of years, but it is possible. Making the case for the need can produce support. We must maintain the alliance with industry. In the health area, this alliance is clear. In other areas, it needs to be developed and nurtured. In areas outside of health, such alliances have been developed already.

We must make sure that the effects of consolidation of the health care system on research are very carefully monitored. This needs to be studied, and we need to develop policies to respond to what are almost certainly going to be negative impacts. That doesn't mean there won't be positive impacts, but undoubtedly there are clearly going to be negative impacts as well. We need to monitor the changes closely.

Academic health centers must be more responsive to those who use them. This relates to how technology and care are evaluated, as well as the kind of clinical research they do. If we do that appropriately, and if we deal in a realistic way with these changes, I think the health care enterprise can emerge stronger than ever.

COLE: Our second presenter will be Nathan Rosenberg, well known to many of you, who is the Fairleigh S. Dickinson Professor of Public Policy in the Department of Economics at Stanford University. Professor Rosenberg has served as chairman of the Stanford Economics Department, one of the great departments in the country. He's a member of the board of directors of the National Bureau of Economic Research. He's been chairman of the advisory board of the UN Institute for New Technology and a fellow of the Canadian Institute for Advanced Research. Nate's primary research activities have been in the economics of technological change, and his publications have addressed both the questions of the determinants and the consequences of technological change. It's a great pleasure to have him back at Columbia. We welcome you, Nate, and are looking forward to your remarks.

ROSENBERG: I start out with the intention of playing the Devil's advocate. I collected a series of propositions that the Devil might state on the topics of health research, health cost explosion, and quality of life. It has been a rather disconcerting experience: I found out that I personally believe, or at the very least half believe, most of the Devil's observations.

“We have the idea of a health cost explosion totally out of perspective,” says the Devil. The rising cost of medical care is a phenomenon that the United States has been sharing with most other affluent nations. In fact, if we go back a few decades to 1960, it turns out that our medical care costs have not been rising much more quickly than that of other OECD countries. Then why all this breast-beating over a health cost explosion?

Indeed, if we look at the annual rate of increase in real per capita health spending for OECD countries between 1960 and 1990, the Devil has a point. The U.S. is by no means at the top of the list. Our rate of growth at 4.8 percent was not very much higher than that of Germany with 4.4 percent. It was well under that of France and Italy with 5.5 and 6.1 percent, respectively, and far below that of Japan, which headed the list at 8.2 percent. And although there may be many features of the Canadian health system that are admirable, cost containment is not one of them. Although their health spending did not grow as rapidly as America’s 4.8 percent, it was, in fact, as close to the American figure as you can get; it was 4.7 percent.

These figures, extending over a period of three decades, strongly suggest that there are some widely pervasive common forces at work driving up expenditures on medical care. Technological change in medicine, the product of our huge past expenditures on health research, is one such common force. I will focus on that connection.

What really distinguishes U.S. health care spending among OECD countries is not its rate of growth, but its level, roughly14 percent, substantially higher than other OECD countries.

Here the Devil – if he's a Devil, and if the Devil is a he – has an incisive and powerful riposte. Why should that be a cause of national concern? What is wrong with the richer country choosing to spend a larger share of its income on medical care? Our population is aging, largely as a product of some of the spectacular successes of earlier generations of health researchers. In view of these demographic changes, what could be more appropriate than committing more of our affluence to healing the sick and alleviating various discomforts and disabilities of the aged? Indeed, the Devil here can cite very powerful econometric scripture for his purpose. A number of careful econometric studies have shown that there is a high income elasticity of demand for medical care.

The truly disturbing thing is not how much we spend, it's that the U.S., with its huge spending on medical care, does not rank very high internationally on the basic measures of health care status: life expectancy, infant mortality, et cetera.

We seem to be in the position of spending more and benefiting less; we are getting very little bang for the marginal medical buck. Experimental studies by the Rand Corporation have confirmed this at the family level. The Rand Health Insurance Experiment studied two groups of families, one with full medical coverage and the other with a large deductible. The families with full insurance coverage spent 40 percent more on health care than did the families with a large deductible. However, the researchers were unable to detect any measurable health benefits associated with the 40 percent of additional spending for the families with full insurance.[1]

Now here again, the devil has a powerful response. That is, there are obviously many determinants of health that have little or nothing to do with medical care. While everyone or almost everyone besides the devil is opposed to purely wasteful expenditure, it is naïve, says the devil, to expect a close association between spending on health and health status. Consider the startling mortality differentials, he points out, between two contiguous states in the United States, Nevada and Utah.

The states are quite similar in many respects: access to medical care, climate, and schooling. Nevada's income is actually slightly higher than Utah's. Yet infant mortality in Nevada is 40 percent higher than in Utah, and comparable differences in premature mortality exist for both males and females and higher age levels. Victor Fuchs pointed out that it is difficult not to attribute much of the difference to the fact that the population of Utah is 70 percent Mormon.

[1] Funded by the Department of Health, Education, and Welfare, the RAND Health Insurance Experiment was a 15-year, multimillion-dollar effort that to this day remains the largest health policy study in U.S. history. The study's conclusions encouraged the restructuring of private insurance. For more information, please visit RAND's Health Insurance Experiment at www.rand.org/organization/health/researchnav.html.

Mormons abstain from tobacco and alcohol, and have a much higher level of marital stability. It is not surprising to find that Nevada has the highest incidence of smoking related deaths among U.S. states and Utah the lowest. I've done a little further research of my own on this intriguing topic. I discovered that Utah has the highest birth rate of any American state but is the lowest in terms of unwed teenage mothers. Somewhat outside of the immediate range of our present interests, it also turns out that Nevada has the second highest student loan default rate in the United States, while Utah is very, very close to the bottom. Nevada also has one of the highest incarceration rates in the United States, whereas Utah has one of the lowest. I could continue.

I'm not quite sure what the devil would have to say about this Nevada/Utah comparison, but it seems obvious that conducting one's life so that it is constant with certain behaviors may make a great difference to health status.

Finally, there can be little doubt that a great deal of the justifiable American concern over health care is that its high cost makes proper medical care much less accessible to the poor. Even the devil has to concede that. More equitable access to medical care is both highly desirable and, I believe, politically inevitable. But even here our devil has one final parting iconoclastic shot: one should not expect universal access to health care system, whatever exact form it may take, to make very much difference in terms of measures of health status. The devil cites the powerful counter-example of the British National Health Service introduced in 1948.

The main rationale for its introduction was to remove the financial barriers to access to medical care in the belief that this would drastically narrow the huge inter-class health differentials that existed in Britain at the time. Although the NHS did indeed provide universal access to medical services and although mortality rates in all social classes subsequently declined, the gradients in mortality across social classes did not narrow. They are as wide now as they were in 1948, suggesting at least the persistence of strong socioeconomic and behavioral differences as dominating determinants of health status.

So the devil walks away with his tail between his legs, but he's heard to mutter something about the inevitability of unfulfilled expectations over any future reforms that provide universal access in the confident expectation that such access will eliminate inter-class health differentials. Well, so much for the devil.

I will now narrow my focus to the connection between medical innovation and the cost of medical care. We do not need the devil to inform us of the mixed nature of our blessings. That, for example, the genuine wonders of modem medical technologies come with higher price tags attached to them. Although it is not impossible to find new medical technologies that are cost-reducing, there can be little question that the vast majority are used in such a way as to increase costs. One of the most careful students of the subject, Joseph Newhouse, estimates that more than 50 percent of the growth in medical care costs has been due to technological change.

The rising costs are fairly obvious in the case of medical imaging technology such as magnetic resonance imaging (MRI). An MRI machine costs about $2 million to purchase, another half million dollars to install, and another million dollars or so per year to operate. Surgical procedures such as coronary artery bypass surgery are now performed hundreds of thousands of times in this country each year. But the rising costs also come in more subtle forms such as antibiotics, certainly one of the great glories of 20^{th} century medical research. Antibiotics may be thought of as wonder drugs that provide low cost cures for infectious diseases, but they also keep elderly people alive long enough for them to require lengthy periods of costly treatment for some chronic or incurable conditions.

Sixty years ago, they would have died quickly and cheaply of pneumonia, which was once known as the old man's friend. So death, to put it brutally, makes little demand on medical budgets. The availability of AZT and other drug treatments for HIV means that the lives of HIV victims are prolonged. But from a purely budgetary point of view, it also means that they now become candidates for extremely costly treatment regimens. In short, when the medical profession acquires the competence to do things it could not do before, medical costs are likely to go up and not down.

Now, the way this occurs is sometimes rather subtle, and therefore worth looking at a bit carefully. Think of laparoscopic cholecystectomy, one of the most widely practiced forms of laparoscopic surgery in America. The percent of gall bladders removed by laparoscope in 1987 was zero. By 1992, it had risen to 83 percent of the total and currently it's over 90 percent. This procedure is widely acknowledged to offer many advantages including cost reduction. It involves only small incisions rather than opening up the abdominal cavity, it causes less discomfort, more rapid recovery and consequently, much shortened hospital stays and a more rapid return to work for the patient.

According to an article in the Journal of the American Medical Association that reported on the experience of a very large HMO in the Philadelphia area over a five-year period, 83 percent of its patients with diseased gall bladders were opting for the laparoscopic procedure by 1992 (Legorreta et al. 1993). According to the HMO, the cost of each operation had decreased by about 25 percent over the period under review. Nevertheless, the HMO's total expenditures for gall bladder surgery rose by 18 percent. The reason was simple: associated with the 25 percent reduction in cost per patient was an increase in the number of gall bladder removals of no less than 60 percent. How do you account for this? Apparently, the less invasive procedure has made it possible for doctors to remove the diseased gall bladders of patients who, due to the frailties of age or the existence of comorbidities, had previously been regarded as too high a risk for the traditional operation. Moreover, the laparoscopic procedure led to an increase in cholecystectomies in younger patients who are only mildly symptomatic. Since the new procedure was not nearly as big a deal as the old one, the doctor or patient or both interpreted the risk/benefit ratio in terms that were more favorable towards surgery.

Indeed, it appears as if some of the increase may have been prophylactic in nature; that is to say, gall bladders were removed from some patients who were totally asymptomatic. In these patients, it was accidentally discovered while exploring for another problem that the gall bladder problem existed.

In economic language, this experience suggests a greater elasticity of demand for medical services than is commonly believed. But this is because the nature of the service being delivered has undergone substantial change. In the case of gall bladder surgery, a downward shift in the supply curve and associated lower cost brought with it an outward shift in the demand curve for the removal of diseased gall bladders. The critical point is that the large increase in demand was a reflection of a significant qualitative improvement in the surgical service that could now be supplied. So that cost savings on a per patient basis – and there are cost savings on a per patient basis – have been more than offset by the increase in the use of the new medical technology.

This experience is far from unique. Indeed, I suggest that it may provide a prolegomenon to the future economics of medical care in affluent societies, reinforced by the aging of their populations. Expectations of new technologies offering the prospect of expenditure reduction are likely to continue to be disappointed for the excellent reason that the quality of medical care is also likely to continue to improve.

Very similar stories could be told in the category of coronary medical care. Angioplasty was once hailed as a cheaper alternative to coronary bypass surgery. In fact, what seems to have happened is that subsequent improvements in bypass surgery led to an extension of the procedure to both angina pectoris and congestive heart failure. Moreover, many patients were also given both procedures since the rate of failure of angioplasties due to rapid restinosis has been very high so that the total expenditures for both procedures rose very rapidly throughout the 1980s.

By the late 1980s, both angioplasty and bypasses were being performed in significant numbers in the over-80 years of age population. Again, this was partly due to significant improvements in the new technologies. Nevertheless, difficult ethical as well as economic concerns have emerged. It is estimated that 20 percent of this age group suffers from some form of coronary heart disease, but when subjected to either of the two procedures, death rates are several times higher than when those procedures are performed on people in the 65-69 years age bracket.

At the other extreme of the age spectrum, neo-natalogists have made quite remarkable progress in saving the lives of extremely premature babies, even those weighing 2 pounds or less. The availability of lung surfacants now offers protection for immature lungs, which had been a leading killer of premature infants. But the evidence is now compelling that such infants will go on to suffer a much higher incidence of mental retardation, chronic lung disease, cerebral palsy, and severe visual disabilities than less premature infants.

Recent research suggests that two-thirds of such infants will never emerge from an extreme state of dependency and will require life-long treatment at enormous financial cost. Putting aside all financial considerations for the moment, a medical technology that is improving but still highly imperfect poses profoundly disturbing ethical questions of the kind I think we have to worry about. Is the most aggressive therapy, even therapy that borders on the experimental, always justified? When formulating a course of therapy in which the prospects are so uncertain, how is it to be decided when aggressive therapy is justified? What are the appropriate criteria? And not least, who is to decide?

I have deliberately cited situations from the extremes, extreme old age and extreme prematurity, in order to underline a general point: improvements in medical technology, however welcome, inevitably bring with them difficult ethical questions, questions that previously did not have to be confronted and from which there is now no escape. Once you know how to do something, should you do it? The questions are difficult not only because they require that momentous decisions be made in situations characterized by poor information and a high degree of uncertainty, but also because the downside risks are so devastating when unfavorable outcomes occur.

However ironic it may be, the conclusion to which I am drawn is this: a major reason, perhaps *the* major reason, for the so-called explosion of health care costs is a steady upward drift in the technological capabilities of the medical profession, combined with strong economic incentives, at least until very recently, to utilize these capabilities in a highly aggressive way. It remains to be seen whether the growth of managed care will change these incentives very much.

In the meantime, is it plausible to try to control this explosion by setting new priorities for the National Institutes of Health peer review process? One suggestion that has received some attention is that technology assessment might be systematically introduced in the early stages of the development of new medical technologies so that judgments of the probable cost implications of the emerging technology can be formed at an early stage. While this suggestion has some merit in principle, I think it founders on a single observation, which is that the history of medical technology ought to make us very skeptical of our ability to anticipate the eventual uses and eventual impact of new medical technologies. The uncertainties that dominate this realm are so great not only at the level of fundamental research, but even at the clinical level, that such an assessment approach will be quite simply unworkable.

Nevertheless, I do believe some form of technology assessment is inevitable and that if a high priority is attached to cost containment, it may be of use in determining what fields or what disease categories warrant a high research priority. Consider the fact that in 1993, the cost of caring for Alzheimer’s patients was estimated to be $90 billion a year, consisting mostly of nursing home costs. Should not the possibility of reducing such a huge financial burden through geriatric research raise the priority of Alzheimer's disease within the nation's medical research budget? Because in fact, geriatric research remains a small research specialty and the National Institutes of Health currently spends about ten times as much on AIDS research than on Alzheimer’s disease. I've become more convinced with each passing year that our criteria for allocating resources to health research devotes insufficient attention to the problems of the elderly.

COLE: Thank you, Nate. Our first panelist is Annetine Gelijns, a doctor who is the director of the International Center on Health Outcomes and Innovation Research and an associate professor in the Department of Surgery of the College of Physicians and Surgeons and at Columbia’s School of Public Health. Her current research focuses on the factors driving the rate and direction of innovative activity in medicine, technological change and its relation to health care costs as well as measuring the outcomes of clinical interventions. It's a pleasure to have her with us today.

GELIJNS: I would like to begin by complimenting the speakers on their very thought-provoking papers and in turn compliment their remarks by adding some thoughts on the future of biomedical research policy. The current restructuring of the health care system, with its shift towards managed care and its new emphasis on cost reduction, is dramatically changing the incentives for medical innovation. Moreover, these changes, as well as broader societal concerns such as the federal budget deficit, are adding new pressures on the biomedical research enterprise, which makes today's session very timely indeed. In my response, I will focus on three major players in the biomedical research community – the NIH, industrial firms, and universities – and will raise some further questions about research funding and the need to set research priorities.

First, the NIH. Following four decades of unprecedented growth, during which the budget increased 40-fold in real terms, the NIH may now be moving into an era of steady-state funding. Even the more optimistic forecasts would not predict a return to the rate of growth in the 1960s and 1970s, and the prediction of many is that the budget will remain constant in real terms. Thus, the NIH must address the question of how best to support research at a time when resources are increasingly tight.

I agree with Dr. Rosenberg that the centrality of uncertainty makes it very difficult to anticipate the health benefits and the costs of individual research projects. Who for example would have anticipated that aspirin now takes a central place in the management of cardiac disease or that the laparascopes that were first introduced as diagnostic tools would later become the central components of minimally invasive surgery?

Despite these uncertainties, however, a cost-conscious health care system does raise some difficult and I think very contentious issues about the need to set priorities among broad categories of research. And I believe that Dr. Rosenberg touched on some very important issues here that we might want to return to in the discussion.

Of course, pressures on the federal research budget not only raise questions about setting priorities within that budget, but they also raise questions about what the appropriate role should be of the private sector versus the public sector. Since the pharmaceutical and medical device industries now invest far more in R&D than the NIH, about $15 billion, the patterns of R&D activity in the private sector are of fundamental importance to the nation's medical research effort. Public policies, such as those concerned with patents or FDA regulation as well as the health care financing system in general, obviously have a very great influence on the incentives of the private sector to invest in R&D. And indeed, if we look at the current restructuring of the health care system, we can already see an important redirection of industrial R&D activities.

Some of these changes I think are highly beneficial. Let me just mention two. First, the direction of research has shifted towards more emphasis on cost-reducing technologies. For example, less costly alternatives to widely practiced clinical procedures such as radical prostatectomies are becoming preferred R&D targets. Second, cost pressures are encouraging efforts to increase the efficiency of the R&D process itself. A case in point is the introduction of so-called combinational chemistry techniques that allow for the rapid, automated synthesis of thousands of experimental substances for drug screening. Similarly, the development of new statistical methods – for example, those that allow large-scale, low-cost clinical trials to be conducted – are likely to improve the efficiency of the clinical valuative process.

Not all of the effects of the current health care restructuring, however, are desirable. For example, small firms in the medical device and biotech industries are confronting greater financial and regulatory uncertainties and might be driven out of the industry. This may be of particular concern because these small firms traditionally seem to generate a disproportionately large share of major breakthrough innovations. But perhaps more critical in the long run are the pressures within health care and within managed health care for the greater standardization of medical practice and the exclusion of experimental technologies.

Historically, much innovation has taken place within or in close symbiosis with actual medical practice, often in academic medical centers. This important source of medical progress is in jeopardy, and I think we need to discuss creative policies to preserve it. Academic medical centers, as you all know, have traditionally garnered the majority of NIH research funds, and they are well recognized for their major achievements in basic biomedical research. At the same time, these centers are involved in a wide variety of other research activities. They develop new procedures and new products. They also have been the sites for pre-marketing and other clinical testing. And finally, they actively reshape and refine emerging technologies. In fact, it is probably their unique position at the boundary between the laboratory and the clinical setting that makes them so well situated for these kinds of research.

I think that it has sometimes not been sufficiently well recognized in policy circles that to achieve medical progress, we need to support both clinical as well as basic research. Obviously, basic biomedical research is the source of new clinical interventions. However, introducing them into clinical practice and studying them at the bedside often leads to unexpected discoveries that generally pose new questions for basic research. For example, the unexpected discovery of new indications of use after a drug has been introduced into clinical practice is a very widespread phenomenon.

This means that realizing the payoffs to basic research involves acknowledging and resolving uncertainties that may first emerge in the clinical context. In this sense, the payoff to basic research is not independent of our commitment to clinical research.

Now, as Dr. Shine indicated, much clinical research – and I'm here including outcomes research and technology assessment – has traditionally been heavily dependent on cross subsidies from patient care revenues. But with the major current changes in the health care system, the margins for such cross subsidization are diminishing. Now, how will we support this critical part of the research enterprise? Dr. Shine suggests that all payers, including managed care organizations, be taxed one to two percent of health care premiums to support clinical research and education. I believe that some such proposal is an important step in the right direction.

These payers, with their large populations and vast databases, are in an excellent position to participate in evaluative research. Moreover, they should have a strong interest in the results of clinical research because it facilitates the timely adoption and cost-effective use of medical technology. I believe that this ultimately, although it's not the case right now, will be an important competitive advantage in their industry. But because clinical research is to a certain extent a public good, payers now tend to underinvest in it. Dr. Shine's proposal circumvents this problem by taxing all payers.

However, I believe that there are some very major issues of infrastructure – such as who shapes the research agenda, who conducts the research, who sets standards – if we're going to set any standards for evaluative research that require further discussion in this room and outside of this room. The most important question of these probably is who will shape the research agenda. In other countries that have created a fund for evaluative research, its management includes government, payers, and industry. This is because all these three actors have very different perspectives and would select different technologies to study based on their own interests.

As Alan Garber recently observed, government agencies, for example, might sponsor a study of the use of aspirin in the prevention of heart attacks. But industrial firms probably would not because aspirin is a generic product and the results of the research would not accrue to these firms. Similarly, payers might not sponsor such a study because their expenditures for aspirin are insignificant – except of course if they expected that aspirin would significantly change their expenditures for heart disease.

In closing, let me emphasize that the achievements of biomedical research throughout the course of the 20th Century have been truly spectacular. Nevertheless, one consequence of the expansion in medical capability has been to drive up health care expenditures. Rational decisions regarding health care resources will increasingly need to depend on research that determines what works and what doesn't work and at what cost. One of the major challenges in the coming years, therefore, will be to design a system that adequately supports clinical research. However, investing in such research will not eliminate the need to make choices at an exceedingly painful level, as Nate Rosenberg just discussed. That is part of the price that is exacted by scientific and technological progress. Thank you.

COLE: Thank you. We're rounding into the home stretch, and we've got a person who is a very good stretch runner to finish for us. Dr. Herbert Pardes is vice president for health sciences and dean of the faculty of medicine here at Columbia, as well as chairman of the Department of Psychiatry. He's was director of the National Institute of Mental Health, and he was president of the American Psychiatric Association. He's the current chair of the AAMC, and he's a member of the Institute of Medicine. More importantly, for me, he's been a fantastic colleague here at Columbia. He has produced a renaissance within our school of medicine. He's a prototype of a person who does understand the links between disciplines, between arts and the sciences, between the professional schools and medicine, and acts upon that. He is probably the most effective lobbyist I know of in Washington for health care and for biomedical research. A great, great privilege for me to introduce Herb Pardes.

PARDES: Thank you very much, Dr. Cole, and thank you for the privilege of being on a panel as distinguished as this with Doctors Shine and Rosenberg and Gelijns. I must say that when I saw I was the last speaker today, a twinge of anxiety hit me. I recall the episode in which a speaker walked in to give a presentation and found one person in the audience, and debated internally for some time as to whether to proceed with the talk, and finally felt that he owed an obligation to that person. So he gave the hour and a half talk, and at the conclusion, went down into the audience, approached the man, thanked him profusely and said, as an expression of appreciation, he would like to take him out for a drink and for dinner. And the man responded, sit down, I'm the second speaker. (laughter)

I don't think that it will take long for the audience to detect slight differences in perspectives, and maybe that will create a kind of nice, warm, and lively discussion period. I will start by saying that this topic is introduced in the program with the notion that while health research still enjoys vast public support, high costs, weakened institutional capacity, and increased focus on effectiveness of clinical interventions may mandate a reconsideration of the system. And a question too is raised in the program as to whether research necessarily improves the quality of life.

I associate myself with Dr. Shine's comments, that it sounds remarkable to suggest we change dramatically something that has been one of the nation's outstanding successes, the NIH and the associated academic medical centers and research institutes around the country and the world. All we do should be under constant scrutiny and subject to reconsideration, I agree, but the issues are complex, and they involve the whole of academic medicine.

And I would therefore like to put forth some propositions and then elaborate on those propositions. First of all, high-quality health research is productive and a valued social good. Second, medical research has reduced costs in the past and may be one of our best options for containing ominous cost increases associated with an aging population and the diseases that afflict that population. I agree with Dr. Rosenberg that there are many instances in which technology fuels higher costs, but I would suggest that there are other instances that go the opposite direction.

Third, medical research is a primary contributor to the quality of fife. Fourth, the research setting undergoing maximum stress is the academic medical center, where much of the basic research and research on causes and mechanisms of disease are conducted. And fifth, these same academic medical centers make other major contributions to the social good, including the training of outstanding doctors, the setting and sustaining of a level of quality of care, and the rendering of more than half of the nation's care for indigent populations.

Sixth, these centers are experiencing declining revenues, due to managed care, state and local government financial cutbacks, and the general contraction of revenues available throughout the nation for discretionary programs. These declines threaten the existence of some of these centers as well as their collective ability to sustain these public goods.

And seventh, explicit actions can and should be taken and are being considered to prevent the unraveling of the collective group of academic medical centers. Were that unraveling to occur, it would have in my opinion a devastating impact on the nation's medical research, quality of life, and economic benefits secondary to academic medical centers. Let me elaborate.

Medical research has vastly changed the nature of human existence. Infectious diseases such as polio, diphtheria, pneumonia, which once caused havoc, have been brought under control. New technologies for the treatment of heart disease have afforded countless people additional years of useful and productive life. Neonatal techniques have saved hundreds of thousands and possibly millions of babies who were born prematurely and at low birth weight. Diseases for which there was little treatment and little hope such as cancer, serious psychiatric disease, and others, are being met with increasing success by a variety of therapies. While non-medical factors contributed heavily to the improvement in life expectancy, medical research has played an important role in changing life expectancy for the average individual from some 50 years at the beginning of the century to something in the neighborhood of 75 to 80 years toward the end.

The American people viewing these results tell us how high a priority they assign to finding relief from diseases that affect them and their families and to attempts to find answers and treatments. If you ask the American people in what areas of research would they want an increase in support, they overwhelmingly select medical research. In one illustrative survey, 66% chose medical research, 18% environmental research, and the remaining 16% were scattered amongst a variety of other areas of work. Some 50% would even endorse higher taxes to pay for medical research. I submit that the notion that medical research is a productive and valued social good is a proposition that is rather widely supported.

There are many examples of reduced costs due to medical research. Senator Harkin, with a flourish, showed an iron lung machine at a recent hearing, pointing out that expenditures for that industry had been eliminated with the introduction of the polio vaccine. Fluoridation has had a massive effect on expenditures related to dental care. Lithium saved more money than all the money ever invested in the research budget of the National Institute of Mental Health.

Some, as Dr. Rosenberg indicated, claim that new technology in medicine costs more because more people use it. The problem is that all costs are not necessarily measured in the same context. Thus, new methods of ambulatory surgery, laser treatments, and other more effective treatments may cost more because more people use those treatments, but the result and impact in reduced hospitalizations and reduced numbers of second procedures is substantial and may offset the increased costs secondary to more widespread use.

The use of lithium kept millions of people out of state hospitals. The recent introduction of Clozepine saves tens of thousands of dollars for every individual with schizophrenia placed on this medication rather than requiring multiple hospitalizations, which vastly increase costs. Furthermore, as we face the explosion in medical costs related to the aging of the population– and here I think Dr. Rosenberg and I come together – research offers answers.

The Census Bureau estimates that Americans over 65 will expand from 31.2 million in 1990 to 37 million by the year 2005. That means larger numbers of people with Parkinson's disease, Alzheimer’s disease, arterial cirrhosis, et cetera. Any advance in our ability to delay or perhaps even eliminate Alzheimer's disease could save billions of dollars, as Dr. Rosenberg pointed out, in the economy, secondary to reduced use of nursing homes and other institutional settings. Such a possibility is increasingly likely because the remarkable advances in brain research work on memory and work on the contributing factors and treatments for Alzheimer’s disease. In fact, it was just a few weeks ago at the Columbia University that studies showed that estrogen treatment may delay or prevent the onset of Alzheimer’s disease.

Medical research has developed so many interventions that it would seem hard to imagine there would be many questions about its importance for the quality of life. Just think of some: sedatives, pain killers, anxiety reducers, antidepressants, vaccines, antihistamines, and countless others. Patients with AIDS are coming to hospitals less in this city and living longer because of recent development in AIDS treatments. One can decide whether that's good or bad, I guess my bias is, it's good. Further, the quality if life has also vastly improved due to hearing aids, cataract surgery, other techniques for improving vision, the general ability to transplant hips, hearts, livers, kidneys, lungs, and increasing number of other body parts.

The goal for medicine, I think, has been articulated by Robert N. Butler, as living one's full life whether that be 85, 90 years or whatever number of years with little in the way of dysfunction followed by as rapid and as comfortable a passing as possible. I am sure we can all think of an endless number of people who have survived an enormous array of diseases and gone on to have many more productive years and others who while having the illness have received tremendous relief from the symptoms.

It's hard for the population, however, in general, to understand what an academic health center or an academic medical center is. The nation’s 125 such centers are confusing entities to the general public. People understand doctors, hospitals, they know that medical schools educate physicians. They know that research is done not only in pharmaceutical company labs, biomedical companies, intramural NIH, but also at medical centers around the country. But not well articulated nor understood by the general population is the unique fabric understood as the academic medical center.

In such a setting, students, residents, fellows, and others, learn; patients are treated; and research on disease and the basic sciences of biology and behavior as they pertain to the normal and abnormal function of human beings is conducted. What is poorly understood is the extraordinary value secured by co-mingling these functions. By training a student in a setting where clinical care is rendered, a student receives concrete hands-on examples of theories expounded in formal didactic settings. Medicine comes alive. Concurrently, studies show clinicians are more excited and pleased to work in a clinical setting where education takes place. The joy of passing one’s knowledge to the next generation makes the doctor a happier clinician and in turn elevates the quality of clinical care in that setting.

Further, the student trained in a context where an investigative perspective is present is a student more likely to be alert to that which is new. We don't want practitioners whose level of medical knowledge becomes fixed at the date they graduate from medical school or residency. A good physician is compassionate and knowledgeable, constantly asking questions and educating herself or himself regarding the best of medicine so in turn they can provide that to their patients.

If we dismantle these efforts, the fabric which has established American medicine at its best, at the very best, and I'm not saying there aren't problems with it, would be severely if not mortally wounded. It is critical for the nation to understand that the accomplishments of medical research in this country are critically dependent on the vitality of the medical schools and academic medical centers in which much of this research is done. Other entities play important roles, too. The pharmaceutical industry, the biotech industry. But the gene for colon cancer was found by

Johns Hopkins academics. The virus for Karposi sarcoma was found by Columbia scientists. The pioneering work on liver transplants was done at the University of Colorado Medical Center and subsequently followed up at Pittsburgh. There are many other examples.

Put simply, if the United States values its medical research and high quality of medicine, it must likewise value its academic medical centers. Beyond serving as agents for medical research, however, these centers also train some of the best physicians in the world. Some 40,000 applicants seek the 16,000 positions in American medical schools. It's not surprising that students and patients from all over the world seek education on the one hand and care at the other at American institutions. This is not to say there are not good physicians in many other countries, but collectively, United States physicians are excellent.

Take, for example, the consultation being requested of Dr. Michael E. Debakey by Mr. Yeltsin. It is the medical schools in the United States that have produced the doctors who serve in those institutions. Beyond the research and the training, academic centers do more indigent care than any other group of institutions in the country. One of the most attractive social policies brings together the best of academic and American medicine in urban academic health centers, with members of the population whose means are meager if not non-existent. For-profit hospitals cannot afford this. The record shows they do trivial amounts of such care by comparison to that provided by academic centers.

Academic centers also set a quality standard for care in their geographical area. Continuing education programs, specialized experts available to health care practitioners in the community, increasing use of information technology, all mean the presence of an academic health center in the area generally increases the quality of medical care. Beyond that, the prominence of academic medical centers in the United States has important spin-offs of new knowledge, patents, products, and the like.

One of the derivatives of the superior nature of American medical research is the explosion in biotechnology. Nobelist Joseph Goldstein in the talk given at the AAMC one year ago, took note of the fact that there were 1,311 biotech companies in the United States employing 103,000 individuals with product sales totaling some $7.7 billion. Their market capitalization was $40 billion to $41 billion, with an estimate that in the year 2000, biotech companies will be spending in excess of $50 billion a year for research and development. A survey in New York showed $2.3 billion total annual spending impact from the academic medical centers in New York, and a similar study of all the members of the AAMC, the Association of American Medical Colleges, revealed that they boost the economy of the country by some $185.6 billion annually.

So whether measured in terms of medical research, training of excellent doctors, providing of a quality standard, rendering of care for the indigent, benefit to the economics of the nation, academic medical centers clearly provide a social good. It's not easy to secure current financial appraisals of academic medical centers. No academic medical center is excited about being portrayed as an institution in trouble. Such a public perception can have negative effects for fund raising, recruitment and retention of scientists, solicitation of investments. Thus, we are only beginning to see indications that academic medical centers are in trouble.

One can divide academic medical centers into many subgroups, and I will focus for the moment on public and private institutions. Each receives money from clinical care, medical research, and tuition. Beyond that, the public institution receives support from state, local, and federal administration sources with less in the way of fund-raising and tuition. Private institutions receive less state and local government support and more in the way of private philanthropy and higher tuition. Managed care is reducing the amount of income coming to academic medical centers, and the degree of the reduction appears to be in part a function of the degree of penetration of managed care in that area. Costs of research increase by virtue of more sophisticated technology. Recent increases in NIH are not quite keeping up with the increased costs of research.

Further, the NIH by virtue of fiscal squeezes is doing more cost shifting to medical schools. There have also been marked changes in indirect cost policies so that medical schools and universities do not fully recover the administrative costs of research. For graduate students, NIH now is expecting medical schools to pay more of the overall costs, and yet graduate students are critical to the nation's pipeline of scientists as well as the needed hands in the laboratory for conducting research.

The necessary institutional financial support to make the center run, to help retain scientists, buy the new piece of equipment, is being squeezed by a retreat, too, of foundations from supporting medical research, by the elimination of general supports of medical schools, such as the biomedical research support grant which provided for these purposes in the NIH, and a tightness in pharmaceutical company spending. States have cut back support, the Veterans Administration is contracting support, hospitals by virtue of constrained finances are finding themselves less able to support the academic and medical school mission.

Securing more revenue from tuition is unlikely. Students face accumulating debts of $75,000 to $100,000 and over, with anticipated sharp cuts in physician incomes in the future. There are medical schools already in jeopardy by virtue of the inability of their clinical systems to sustain positive financial bottom lines. In other instances, part of the health care enterprise of an academic center are being offered for purchase to outside investors for the apparent benefit to the university rather than to the medical center.

In a report on August 18 in *The Washington Post*, the hurt to medical schools was recounted by multiple individuals including NIH director Varmus. Professor John Eisenberg stated, "Either another source of funds has to be identified or the research and teaching missions will be compromised."

So I have stated that health research is a productive and valued social good, that such research reduces costs, and it makes a primary contribution to the quality of life, that it is carried out in large part in academic health centers, that the centers make many other contributions to the social good, and yet they face dramatic reductions in funding, which will undermine the functions mentioned earlier to the disadvantage of the entire society.

What's to be done? Everything should be done to facilitate collaborations between academic medicine and industry. Giving industry special tax breaks dependent upon contributions to academic medical centers is worth considering. Certainly the kind of policy which allows universities to take advantage of the knowledge development emanating from government-sponsored research and work with industry to gain revenue streams from licenses – is a critical policy to be sustained into the future.

Methods by which industry could partner with academic medical centers and perhaps with federal government to support the training of researchers are being considered by the NIH Committee on Clinical Research. The Hatfield-Harkin Act or something like that, which would provide for a stream of money from a source other than annual appropriations, is worth considering. This must not jeopardize the support for increases and appropriations. Senators Hatfield and Harkin would forecast a substantial boost in medical research budgets through a separate stream of money, which might be garnered from a gasoline tax, tobacco tax, and not be subject to annual review.

Another approach is that of Senator Moynihan, who has introduced a bill suggesting a trust fund to support education and training in academic medical centers. This would require an assessment, as Dr. Shine pointed out, of all health care premiums to medical education. Senator Moynihan said he wanted to protect – in his own words – the jewels that academic medical centers represent. Most likely, this trust would come about as part of an overall Medicare bill, which will be on the docket for the next Congress.

The idea that managed care companies will voluntarily supply substantial dollars for medical educational research is illusory. Only by mandating that all health care systems shoulder some responsibility, can one bring everybody in. It's noteworthy that Congressmen Archer and Thomas, Republicans, suggested similar revenue streams in 1995, and the prospects with Democratic and Republican leadership in the two houses makes the possibility of such an effort more attractive and more possible.

This support would help even the playing field for academic centers with their extra functions as they go head to head against health care institutions with no extra function. It is worth considering providing special protection for academic centers of excellence such that they receive an appropriate amount of clinical activity through managed care. Also government, business, and other leaders should encourage foundation leaders to follow the example of Howard Hughes. The Hughes Institute, recognizing the current stress on medical schools, has provided special financial grants for continued recruiting and developing of young basic scientists, the same as you've heard before are needed for clinical research. The NIH Clinical Research Committee again may come up with suggestions along those lines.

Other enterprises, such as the managed care industry, the pharmaceutical industry, the insurance industry, should be encouraged by leaders from President Clinton on down, to help support medical research and education efforts of this country. Those who provide philanthropy for private institutions and public institutions should be given far greater attention and commendation by government and media. They should be held out as examples for other individuals of means who can help medical schools. The Woodruff Foundation just donated $297 million to the Endowment of the Emory Medical School, as an example.

Medical schools should interact even more with other partners in the overall academic and scientific enterprise. This includes other parts of the university, the community, other academic medical centers, other hospitals – the richer one makes the scholarly fabric, the better the resulting products. The pipeline, both of young basic and clinical scientists, has to be protected. Thus, either the NIH has to contribute more for the training of such young scientists or other sources, perhaps by virtue of collaboration with industry, have to be found to help the medical schools that are being increasingly called upon to cost share, while they're also being asked to carry all kinds of additional expenses.

An attention to seed money, discretionary money is critical. These should not be denigrated as slush funds. It should be recognized as the necessary glue that enables medical school leadership to clinch the recruit, make the retention, sustain the individual, during times of interrupted funding, purchase the extra piece of technology, and help renovate the labs to make them modem so one can keep scientists and bring in new ones. The BRSG (Biomedical Research Support Grant) Fund I mentioned before is one fund which might be considered for new funding.

Finally, this has to be seen as a collective responsibility of more than medical school faculty and leadership. This is a crisis in the making with ominous ramifications for all citizens. The United States has been too ready to relinquish leadership of other enterprises. Other countries are more than willing to take over leadership in medical research and education. They are already making greater investments in some instances than the United States. And certainly the academic medical centers, as Dr. Shine indicated, have to make their own efforts to reengineer, reduce expenses, and pursue as diversified a funding base as possible. Schools should find efficiencies as have so many other societal enterprises.

Often, in this country, we wait for a problem to intensify in order to secure widespread consensus that something has to be done. Academic medicine will not survive that stance. If we wait until the problem is pervasive, it will be too late. Allowing these institutions to unravel cannot be offset subsequently by some rapid action or fix. There may be no way to prevent a few medical schools from closing or perhaps in a positive way from consolidating with others. The more worrisome possibility from the perspective of the country is a pruning of sufficient cream off the top of every one of the most distinguished of the academic medical centers to convert A-plus enterprises into C-minus enterprises.

Johns Hopkins has a superb Urology Department, Columbia has an outstanding Neurology Department. These programs are sustained by faculty, residents, fellows, and other staff, a critical mass that focuses on advancing our knowledge and treatment of a given disease area. If you take away 20%, 25%, whatever percentage funding from each and every one of the institutions, one contracts each of these critical masses. As a result, we may convert pioneering research enterprises with quality clinical and educational components to pedestrian programs that merely supply clinical services, do some teaching, but do not have the resources to push the frontier. Nor may they have the resources to create very much in the way of a quality educational experience.

I don't believe the American people want this. I don’t think they recognize yet that it's happening. The interconnected issues of quality of life, improved health care, attention to the needy, contribution to the economic good, and elimination of disease with cost savings to research all argue for a dedicated policy. This policy should begin with the next Congress and with the society as a whole to reverse these dangerous trends and to reestablish our academic medical centers and medical schools on the firmest possible footing. Thank you.

COLE: Thank you, Herb. If you want to see an encore performance from Herb tomorrow, he'll be playing middle linebacker for Columbia against Harvard, and we expect him to make many tackles. I notice a few people lining up, and we will have a few questions entertained.

DEVINS: My name's Sam Devins from Columbia. Nathan Rosenberg sort of followed the devil as the devil's advocate – well, I'm going to follow Nathan Rosenberg. The question I pose is, why is it when one talks about health, whether it's research or it's treatment, one always talks about it in terms of cost and deficit and never in terms of what it produces? Any numbers against health are red ink whereas numbers against, say, the burgeoning cost of electronics is always – well, it's a burgeoning production of electronics in the country. Now why is the reason? Is it because the people who pay and the people who receive the benefits are different? Is it because there's no option? One usually thinks of medicine as an expenditure beyond one's choice, although I believe medicine does have electives these days. Is it because it's not exportable? Where is the value of medical treatment recorded?

COLE: Thank you, Sam. I think Nate Rosenberg wants to respond.

ROSENBERG: If you ask an economist to come and talk about a particular subject, you mustn't express surprise when he talks about what something costs. I did not say it wasn’t worth it. You will recall that the words I put in the devil's mouth were that richer societies are spending higher shares of their gross national product on medical care.

COLE: Let's have a couple more from the folks who I know are anxious to speak.

MALE VOICE: This is such an important issue, I do want to make a comment about it. Everybody's right. We spend too much. It's clear that the bypass rate in Texas is one-and-a-half to two times that in New York, with no evidence that health is any better in Texas than New York. On the other hand, up to now, we've had no way of measuring value, and the commentator is absolutely right. This is not about cost, it's about value. The biggest development, and I made some reference to it although time didn't allow, was indexes – quality-adjusted life years, disability-adjusted life years – in which you can begin to talk about function, disability, and performance, and create a denominator to go with the cost.

The problem is, up to now, we've never had the information systems by which to do it. And I'll also point out that our great academic health centers had none of this data. Our great academic health centers can't even cost-account the equipment that they put in when they put a new monitor into the coronary care unit, so that we've got work to do on both sides.

MERRILL: Steve Merrill, National Research Council. First, a wild assertion and then a heretical question.

The assertion is that the changes underway in the health care market, the way the stakeholders are behaving, seem to me dwarf the changes underway in the public military equipment market, which has preoccupied the discussion for much of the earlier part of the day. If that's the case, what we heard today was a whole variety of ways in which that is going to ripple through the innovation system, affecting companies, research performers, the National Institutes of Health, biomedical research policy at the federal level.

My question is, if that's the case and the changes are driven in part by attack on what is perceived to be excess capacity, why don't we have to confront the question of whether steady-state biomedical research funding isn't an appropriate response?

MALE VOICE: I have two observations. I'm triggering on Mr. Robinson's comments this morning when he talked about allocation of R&D resources, basically using the biomedical field, and gave what seemed to me to be a manual on how you actually got more from the government. We ought to remember that since the Second World War, certainly in civilian R&D, it could be argued that biomedical research in the life sciences has gotten a disproportionate amount of the public resources available.

The problem you face is that every interest group comes before the government, saying that this investment will pay off in some way down the road, and so everyone has this idea that this is not the cost but cost for benefit down the road. And I would suggest just gently that in a time when we're facing a decreasing federal R&D budget, it seems to me that biomedical research is particularly vulnerable in terms of the past history.

Secondly, just one caution: there have been a lot of discussions about set asides, taxes, dedication to the system, that have to be looked at in terms of how the tax system works and how the R&D system works. It is a very dangerous thing, it seems to me, to start dedicating particular taxes to particular areas because what you're doing is siphoning off that particular area from what I consider healthy competition for R&D funds in a limited budget.

ROSEN: My name is Steven Rosen, I'm head of the Scientific Careers Transitions Program funded by the Alfred P. Sloan Foundation, working with the career problems of scientists, physicians, and attorney. This is a blip on the radar screen, but there's been a very significant increase in the number of physicians who are seeking to move outside of medicine or within medicine and change their career directions. Among the scientists, it's because there's a surplus. Among the physicians, it seems to be career distress or dysphoria.

MALE VOICE: Just a very brief comment. I want to express my disappointment with the tenor of some of the discussion in this last panel. I'm disappointed because we're basically discussing the problems of a system where the traditional funders of R&D are not going to be funding that R&D anymore. It looks like we're going to be cutting back in R&D on the order of 15% to 20%, depending on whose numbers you look at, over the next few years.

As Dr. Robinson pointed out earlier, the health sciences in particular have a relatively privileged position within the hierarchy of R&D in this country, that was purchased through a very astute and clever political strategy, but one that worked.

And so what's the response? What do I hear for the last 45 minutes? Well, don't cut us back... you know, those other guys, their funding is going away, but don’t cut us... not only don’t cut us back, send more money, it's really worthwhile.

Now, I have no objection to the idea that there may be considerable social value to doing these things – although I would observe that many economists say the fundamental problem is that the people receiving the benefits are different than the people paying the bills, there’s a disconnection between the benefits and the bills. But it seems to me that if the U.S. scientific community is to disintegrate into a bunch of interest blocks each arguing, don’t cut us, cut the other guy, in fact, send us more money, then we're not going to make much progress towards solving the problem that this conference is ostensibly concerned with.

LUBELL: This is Michael Lubell, I'm professor of physics at C.C.N.Y. and director of public affairs of the American Physics Society. This last comment leads directly into what I would like to raise. Twice today, it has been suggested that the 20th Century was the century of the physical sciences and the 21st Century will be the century of the life sciences.

I think the entire characterization is incorrect. If you look at what has happened during the 20th Century, our theoretical understanding has brought us down to a microscopic level, quantum mechanics, quantum chemistry, molecular biology, biochemistry, biophysics, chemical physics. We have made tremendous progress. The tools we use, even in the life sciences in the medical area, lasers, electronic microscopy, fiber optics, computers, MRIs, spectroscopic analysis, and so on and so on.

I think science is becoming one, and what we need to do is talk about science in that fashion. It was suggested that in fact, if we don't, the political system is not going to be responding to it in a rational fashion. We will slit our own throats, and the country will be much the loss for that.

PARDES: That's quite a rich array of comments. First of all, I want to associate myself with Sam's anguish. There is a tremendous value there, and I think that the American people will vote that way.

Second, somebody talked about the privileged role of biomedical research. I agree with the comments that we should not fight a bunch of scientific disciplines; that's not the point I was trying to make. I would associate myself with improvement in funding for science across the board. But privileged position for medical research? The country spends almost $1 trillion in health care costs, and the federal government investment in the NIH is a grand total of about $12 billion. I think my calculation comes out to that being about 1.2%. What industry do you know of that would be satisfied with a 1.2% interest in R&D?

So this is not an argument to denigrate the extraordinary value of other sciences. It's to say that there's been a value in medical research over the years, and I don't think the bite should come out of our scientific colleagues. I do feel, however, that I'd rather have my dollars go to a new medication rather than to a new bomber. It's as simple as that. And I would associate myself with the 50% of people in the survey who say they'd even be willing to pay a few more dollars for research monies.

We can easily reduce ourselves to squabbling within the scientific, academic, medical, and concerned populations. I think there's a broader fight going on, and it's simply to what will this nation ascribe? Either catering to those who would walk away from the problems of other people and be happy to see the federal government reduced to nothing in the way of capacity, or to those who feel the government can do something and there's a reason to try to make our civilization better for everybody. And my association's obvious. Thank you.

SHINE: I started my remarks by trying to talk about the notion of an overall science policy. That was deliberate because I don't believe that we're talking about one versus the other. What I'm saying is that I believe that the traditional way that many elements of the scientific community have made the case for the support in their area is weakening for a variety of reasons and needs to be reconceptualized.

Secondly, I don't accept the principle that there should be or that there even will be a progressive decrease in the federal investment in basic research. I simply don't accept it. I believe the issue is, how do we make the kinds of arguments so that that doesn't happen.

I will remind you that both the administration and the Republicans projected decreases in the NIH budget last year and this year. It didn't happen. I believe there are other examples of areas in which that investment can be maintained or increased, and I think the notion that the investment in basic research is critically important for the country is one that we can't walk away from. The question is, how do we articulate that, and I think it's the articulation part that's important.

I'm not going to get into an argument about the importance of the various sciences to the biological sciences – of course, it's been enormous. The message I'm trying to convey is that – in spite of the notion that in a variety of areas, people will appropriately try to produce intellectual advances, whether it's in particle physics or in a whole variety of other places – the forces that drove 20th Century science were the kind that we've described. And I'm arguing that we have to develop a different agenda. I'm not deprecating the role of, for example, physics in contributing to that. I'm saying, is there a way to make it part of the overall argument rather than the intellectual argument that each of us tries to make for our own discipline. And I think that's where both Herb and I would want to see something happen.

A lot of the technology that's been developed has been developed by engineers. And I'll remind you that every health care expenditure is somebody else's income and the industries we're talking about are important industries – when you get past Boeing, our balance of payments is profoundly influenced by what we export in terms of pharmaceuticals, devices, and a variety of other things. So one could make the economic argument that this portion of the economy is helped in a very substantial way in terms of this investment.

COLE: Thank you. I want to thank the presenters, the panelists, and I want to thank the participants. It's been a long day, I think it's been an interesting day, we will adjourn now.

Dinner Speech: A New Framework for R&D

Peter Eisenberger

COLE: Let me say a few things about Peter Eisenberger since he's new to our community but has undertaken an extraordinarily important role in terms of the university's initiatives and its own sense of its strategic advantages.

Peter joined us only about a month ago, although it seems that we have known each other for the better part of a year because we have been discussing the prospects of him coming to Columbia and to become the Vice Provost of the Earth Institute and the Director of the Lamont Doherty Earth Observatory. We were very delighted that Peter has accepted that invitation, and I'll say momentarily why I think it's so important that we have someone of Peter's stature, his imagination, and his vision to hold that position.

But let me first go through what might be called the formalities of giving Peter Eisenberger's background. Peter holds a Ph.D. in applied physics from Harvard, he has served in the corporate sector as senior director of Exxon Research and Engineering Company's corporate research laboratories, and in the academic sector as director of the Princeton Materials Institute at Princeton University from which he comes. Early in his career, he was department head at AT&T Bell Laboratories, and he brings to Lamont Doherty and the Earth Institute a unique and vital set of skills and experiences.

I recount these items on Peter's vitae because they illustrate the many strings to his bow, and those strings we believe are all necessary in the way we are trying to organize the growth of knowledge and technology at Columbia as we look to the 21st Century. The Earth Institute is an effort to combine the knowledge that is being generated in many fields. It starts off with a fantastic base in the work that has been generated from the Lamont Doherty Earth Observatory, which has done so much pathfinding work. And it combines with that work from not only the physical sciences but the social sciences and the policy sciences.

We're trying to experiment with the language of these different disciplines, trying to bring them together, to think of problems like the future of this planet in terms of whole systems analysis, in which solving scientific problems without solving some of the social, anthropological, or cultural problems will be of limited value in practical terms. But if we can integrate multiple disciplines and bring together people with very, very different intellectual backgrounds with very different interests in terms of their degrees of specialization but with a common purpose to solve problems related to the Earth, we think that we can begin to organize knowledge in a significantly different way.

I know that Peter will represent in his position an exciting new presence at Columbia, because he has a vision of the way knowledge permeates boundaries from traditional disciplines and can usefully interact to solve some of the major problems that we face in our society today.

So it is with great, great pleasure that I give you Peter Eisenberger, who will be our dinner speaker.

PETER EISENBERGER: Thank you. I'll begin by reviewing the thoughtful efforts of others to suggest a new framework to replace the Bush paradigm. After that, I will outline the parts of a new framework that I believe need to be emphasized.

Before doing that, I would like to identify the factors that have created the need for a new framework. These are not new, but they are the drivers for the efforts of others and the factors which have influenced my suggestions for a new framework. These major drivers are:

1) the replacement of defense by civilian and commercial objectives for research and development;
2) global competition and growing concern over global constraints on resources;
3) the difficulty of wealth generation and the fast pace of innovation;
4) the information age and changing organizational and management practices;
5) the increased complexity of important scientific problems, emerging technologies and societal problems; and
6) the related increasing importance of education generally and the growing gaps in understanding between the science and technology generators, the decision makers, and the public.

In response to these six factors, there have been three reports that attempted to provide input to developing a new framework for R&D. One was the work of the American Association for the Advancement of Science's (AAAS) Committee on Science, Engineering and Public Policy (COSEPUP) in 1993, *Science, Technology, and the Federal Government: National Goals for a New ERA* (COSEPUP 1993). The second one was the National Academy of Science's Committee on the Criteria for Federal Support of Research and Development, chaired by Frank Press, in 1996. And the third was the recently issued report by the Council on Competitiveness, entitled, *Endless Frontier, Limited Resources: US R&D Policy for Competitiveness*.

While each of these reports was written with varying degrees of participation from the university, government, and industrial sectors, one can loosely associate the COSEPUP report with academic concerns, the Press report with government concerns, and the Council on Competitiveness report with industrial concerns. One of the points I will return to later on is that this historic separation of the three sectors which was built into the Bush paradigm is fragmenting our R&D efforts. This needs to change.

Starting with the COSEPUP report recommendations, the first goal is that the United States should be among the world leaders in all major areas of science. They reasoned that achieving this goal would allow the nation quickly to apply and extend advances in science wherever they occur. The second goal is that the United States should maintain clear leadership in some major areas of science. Finally, the comparative performance of U.S. research in a major field would be assessed by independent panels of experts from within and outside the field.

The Press report had as its main recommendations, first, that Congress should create a process to examine the entire federal science and technology budget before the federal budget is disaggregated into allocations to appropriations committees and subcommittees. Furthermore, the President and Congress should ensure that the federal science and technology budget is sufficient to allow the United States to achieve preeminence in a select number of fields and perform work at the world class level in other major fields. This clearly supports the COSEPUP report's recommendations.

The Press report also recommended that federal science and technology funding should generally favor academic institutions because of their flexibility and inherent quality and because they directly link research to education and training in science and engineering. This recommendation has elicited a firestorm of response. As a complement to this support for academic institutions, the Press report recommended that the federal government should retain the capacity to perform research and development within agencies whose missions require it. They argued that the nation should maintain this flexible and pluralistic system of support.

The main findings of the Council on Competitiveness report are first that R&D partnerships hold the key to meeting the challenge of transition that our nation faces, and second, that the United States has an urgent interest in resolving the current polarized debate over the proper federal role in R&D. The Council included very thoughtful, detailed suggestions for each sector.

Those three reports provided expert input on how to develop a new framework for R&D. Now I will present my perspective on a new framework, unconstrained by current political correctness considerations or vested interest considerations.

I have been thinking about the need for a new framework for over a decade, ever since I had the responsibility to downsize and redirect Exxon's corporate research laboratories in 1986. I began by asking the question, how serious is the need for change? I concluded that the need is great, not only because of the current forces of change, but more importantly, because all three R&D sectors developed some very bad habits during the golden age of Vannevar Bush. Like any human or natural system, a long period without real stress makes the individual components and the overall system less prepared for real challenges. One is, in a sense, most vulnerable at such a transition; yet one has the strengths created during the period of abundance to bring to bear on the new challenges. This is related to the conventional wisdom of, if it ain't broke, don't fix it.

But I believe significant departures are required from past practices in each of the sectors and, most notably, in the system as a whole. The changes should be made carefully to protect the real strengths of the current system, as these will be useful in facing our new challenges. However, an indication of how much change I believe is needed is that I can only come up with three major items that need protection.

First, echoing the recommendations of the three reports, I believe our investment in university education and research infrastructure needs to be preserved and even strengthened. This recommendation does not trace its roots to my own place in academia; I said this even when I was in industry. In making this recommendation, I am not endorsing all university practices in education and research, which need to change; nor am I saying we need as many research universities as we currently have, because we don't.

The second major category I believe needs to be preserved is the national research facilities, like those which provide high magnetic fields, photons, and neutrons. Many of them are housed in our national laboratories. Expensive, state of the art capability will certainly be needed as we address the complex future in both scientific and technology terms. Here again, I don't want to imply that all the facilities are well run or that all the ones we have are needed.

Finally, I certainly want our industries to maintain a vigorous research effort. Here I am less concerned than others about their short term orientation. In a better coordinated research and development system, others can perform the longer term research. In general, I believe industry has already taken major and painful strides to address long term issues. Among the three sectors, it is currently best prepared to address the future.

Now I will turn to my framework for R&D for the 21st Century. My focus is on the parts of the framework that will help achieve the goals of excellence and effectiveness. By effectiveness, I mean contributing to developing the knowledge base and technological innovations that are needed in a timely and cost effective manner. First, I recommend a periodic, comprehensive review of federally funded research programs. Each program should be required to prioritize current activities in terms of excellence and strategic importance. A national committee of wise persons, those with experience in science but without current vested interests, should review the assessments and choose where to make the cut in excellence. We have a lot of excellent programs, but one consequence of our golden age is that a lot of low quality, unimportant work is being publicly supported. Many of my colleagues suggest that the number may be as high as 50 percent in their field – not in somebody else's field, in their field.

Most importantly, we must initiate this process ourselves rather than having a more political process imposed on us from the top in reaction to budget reductions. Industry made a mistake in this regard. They put themselves in a reactive position rather than preparing themselves. As a result, their research efforts suffered much more than they had to.

Next, I would follow some version of the COSEPUP recommendation to determine a national research portfolio. This should be done by mission, not discipline, and it should both question existing missions as well as add new ones, like exploring the frontiers of science, excellence in education, improving quality of life, and the environment.

The savings achieved by the excellence assessment should provide resources to preserve and strengthen the needed infrastructure and create new programs in areas needing additional effort based upon our new portfolio analysis. In the parlance of today, this should be a balanced budget exercise.

Even more important and of greater difficulty is the goal to achieve greater effectiveness. This is where the Bush golden age has taken its greatest hold. There are many reasons for this, a notable one being that in some sense, the Bush framework as it finally emerged was an ineffective design. The experience of developing the bomb was more profound on the science community than we admit. The community wanted to avoid national coordination, which at that time meant military control.

There are many other reasons for our current poorly-coordinated innovation system, including self-interest. No one likes to be given direction. Also, the cultural and political consideration is that central planning and/or industrial policy is bad. Here again, I agree with the three reports, which to varying degrees call for a more coordinated approach to a national R&D.

The changing nature of the innovation process and the global nature of economic competition require that we function effectively as a team. Many feel threatened by this, and there are the standard arguments of how planned efforts fail, but doing it right is the challenge we currently face. An uncoordinated approach certainly is a good defense against mistakes, but I don't know any field of endeavor that has serious outcomes and operates under constraints that has an uncoordinated approach as its method of choice. We certainly should build some degeneracy into the system, and use the strength of our current bottom-up approach to avoid any central planning disaster. But we must use our existing investments much more strategically than we are now doing.

The second major area needing change to achieve effectiveness concerns the internal practices of our universities and government laboratories. This will be the most painful. Here, we should follow the lead of industry but avoid making the mistake they made in relation to their employees. Our objective should not be downsizing, but rather, to get more productivity out of our existing assets. In this process we must preserve and even strengthen the environment which nurtures creativity and is supportive of the many excellent researchers in our university and government laboratories. The goal is to get more, not less, out of the best and brightest in these institutions.

This institutional effort to achieve enhanced effectiveness should be comprised of two components. First, there should be a top-to-bottom review of existing practices and procedures, asking each one whether it meets current needs and whether it can be done better in a different way. There have been several tentative attempts in universities to address this issue, but they have been too constrained by the culture to produce the needed changes. The reason for this is related to the second aspect, and the most radical from the perspective of my university colleagues. Put simply, the political center needs to reassert more control over its institutions. The social contract needs to be redrawn, especially in our universities, to reflect greater concern for, and contribution to, the goals of the institution. Not surprisingly, since it involves the same people, the balkanization of our institutions is similar to the fragmentation at the national level that impedes a coordinated effort in support of our country's objectives. Without discussing the intellectual consequences of disciplinary balkanization on our research efforts, I will state that these also need to be addressed in the proposed review.

Clearly, many other areas need to be addressed as well. For example, do we need to promote the formation of new kinds of institutions between our universities and industry to provide improved effectiveness for our national innovation process?

In sum, the design parameters I would recommend for a new research system should include the following features:

1) it should promote the assessment of fields and programs in addition to individual efforts;

2) it should facilitate the termination of programs which are not performing adequately;
3) it should promote the development of a national portfolio which reflects both scientific and strategic priorities and in particular, and should enhance focus on quality of life concerns;
4) it should strengthen the ability of institutions to direct resources towards achieving institutional goals; and
5) it should promote greater institutional responsibility to initiate reforms in their practices and procedures so that they can more effectively contribute to national goals.

Design Area Five:
Basic Research and the American Research Universities

James Duderstadt
Donald Kennedy
Paul David
Eduardo Macagno

Moderator
Jonathan Cole

(MISSING TAPE)

COLE: –through some of the aspects of the Vannevar Bush framework and as we try to think about the issues, the questions, the problematics that are part of any effort that might be made to redesign that framework in light of changing times and changing conditions, as we look forward to the next decades in American science policy. I, for one, found the presentations and the discussions extremely interesting and fruitful in generating ideas for additional work that will follow this conference.

So, we turn this morning to the session that is very dear and close to my heart and that has to do with basic research and the American universities. I think it is fair to say that Vannevar Bush, in *Science: The Endless Frontier*, had this very much in mind as he talked about the answers to the four questions that were put in the letter from President Roosevelt and then later delivered to President Truman. When he talked not only about national security but the public welfare, he talked indeed, as it was mentioned yesterday about full employment, how basic scientific research is scientific capital.

In answering the question "How do we increase the scientific capital?" he said, "First we must have plenty of men and women trained in science. For upon them depends both the creation of new knowledge and its application to practical purposes. And second, we must strengthen the centers of basic research which are principally the colleges, universities and research institutes."

Indeed, over the past 50 years, that linkage has been an extraordinarily important one, both for the research universities, of course, as well as for the production of new knowledge. He goes on to make the very important additional point that there ought to be a coupling of the education of graduate students and professionals with graduate education at these universities, supported through fellowships and scholarship programs. Where education and research are really joined has been one of the hallmarks of the period that we review at these conferences.

So, I look forward enormously to the comments that we have from our presenters and our panelists. It’s a great pleasure to introduce the first policy design presenter, and that is James J. Duderstadt, who is now president emeritus and university professor of science and engineering at the University of Michigan, one of the nation’s true leaders in higher education over the past decade. Jim Duderstadt received his bachelor's degree in electrical engineering from Yale in 1964 and his doctorate in engineering science and physics from Cal Tech in 1967. After serving a year as the Atomic Energy Commission Fellow at Cal Tech, he joined the faculty of the

University of Michigan in 1968, as professor of nuclear engineering. He became dean of engineering in 1981 and then provost and vice president for academic affairs in 1986. He was named president of the University of Michigan in 1988 and served in this role until 1996. It's a great pleasure to have Jim Duderstadt with us today. Jim Duderstadt –

DUDERSTADT: Let me add my gratitude and express my surprise for this turnout on this remarkable fall day. I should point out that this is the first fall Saturday in ten years that I don't have to worry about something else happening involving 22 young men. I can join my colleagues, such as George Rupp, to focus on intellectual matters on Saturday.

Actually, I do have another worry. This afternoon, for the first time in history, another college football stadium, that of the University of Tennessee, will presumably set the new record, surpassing Michigan's record of 106,900 in attendance. Hey, not to worry. Not my problem. Not my watch.

Let's focus instead on the remarkable social contract that has existed between the universities and the federal government as laid out in Vannevar Bush's report, *Science: The Endless Frontier*. It does reflect a unique character of higher education in America, in the strong bond that exists between our institutions and the society that shaped them, supported them, and so forth.

The partnership between the federal government and the universities for the support of basic and applied research really did carry on that theme. It's had a remarkable impact. Shaped the universities, a remarkable institution, such as the one we're meeting at today. It's made America the world's leading source of fundamental scientific knowledge. Produced well-trained scientists and engineers. And addressed many of the most critical issues facing our nation in a broad range of areas from national defense to health care to agriculture to economic development.

Largely as a result of this policy, the American university today, the research university, is regarded as the strongest in the world, at a time when the investment returns in research have never been higher. Many of you remember two or three years ago when The New York Times, in an editorial, referred to our nation's research universities as the "jewel in the crown of our national economy," going on to assert that university research is the best investment taxpayers can ever make in America's future.

And yet, the 1990s are a time of great stress and concern for many of this nation's most distinguished campuses. There is a sense of a breakdown of mutual trust that all too often has led to an adversarial relationship between the federal government and our institutions. A certain level of skepticism, even hostility exhibited by the media and government, which has badly eroded public trust, as revealed by a deluge of attacks on the academy. Forces upon and within universities, pushing them to rebalance their missions, to shift away from research toward teaching and public service. Signs that the morale of our scholars, our researchers on campus, has deteriorated significantly over the last decade for many reasons – the time-consuming and difficult nature of obtaining funding, the disintegration of a scholarly community, concerns about the future.

Many things are going on here. To some degree, it represents a cyclic view of our society about the importance of scientific research. There may be a certain level of populism, of suspicion, of distrust of expertise, elitism, of excellence. But I would contend that something else has happened, a very fundamental change is underway, both in the nature of the relationship between our universities and the societies they serve, and in the character of those universities.

Let me make some brief remarks on both. First, the research partnership itself. A topic that, I suspect, you have considered in some detail yesterday and in many other forms is the shift that has occurred in national priorities over the last decade, to put it simply, from guns to butter. For almost half a century, the driving force behind many of the public investments in our national infrastructure has been the concern for national security in the era of the Cold War. In the wake of extraordinary events that have occurred in recent years, our nation is, instead, drifting to define new driving priorities. Far from a peace dividend providing new resources in the post-Cold War world, what we find is an ambiguity.

Although numerous societal concerns are now mentioned to succeed national security – national health care, crime, K-12 education, economic competitors – none of these has yet assumed an urgency sufficient to set new priorities for public investments. And therefore, much of the existing intellectual infrastructure developed to underpin national research is now at risk: the national laboratories, industrial R&D laboratories, and, of course, the research university itself.

Second, there's been a change in the character of the relationship between the university and the federal government. *Science: The Endless Frontier* stressed the principle that the government had to preserve freedom of inquiry. That is, to recognize that scientific progress results from the free play of free intellects working on subjects of their own choice in a manner dictated by their curiosity for the explanation of the unknown.

Since government in years past recognized it did not have the capacity to manage effectively research in the universities, the relationship was a true partnership. The government provided fairly unrestricted grants to support a part of the research on campus, with the hope that wonderful things would happen. And, of course, they did.

Unfortunately, in recent years, the basic principles of this extraordinarily productive research partnership have begun to unravel, so much so that I think today one might more properly characterize the relationship as evolving from a partnership to a procurement process. The government is increasingly shifting from being a partner, a patron of basic research, to becoming a procurer of research, just like other goods and services.

In a similar sense, the university itself is shifting to the status of a contractor, regarded no differently from other government contractors in the private sector. A grant is viewed as a contract, subject to all of the regulation, oversight, and accountability of other federal contracts. Speaking as a has-been university president, but a very recent has-been university president, this particular view has unleashed on the research university an army of government staff accountants and lawyers, all claiming that their mission is that of making certain the university meets every detail of its agreements with the government.

Third, there is a significant shift in public attitudes. What is at risk today is that a national consensus, decades in the making, that saw universities and faculty as fundamental investments in the scientific, technological, and scholarly preeminence of the nation is beginning to unravel.

This particular consensus, which married research and scholarship, which gave rise to the concept of the faculty as teacher/scholars, is now beginning to change. And it's pushing many of our institutions to regard their faculty as simply teachers. I think one of the interesting premises to arise, and it was pointed out in some of the early discussion papers associated with this particular session, is the challenge to the conventionalism that research and teaching should go together.

This is, in fact, much of the tradition behind many of the particular policies of universities and federal programs. I would quote in specific a policy statement of the National Science Board in the most recent issue of the *Science and Engineering Indicators* – that the integration of research and education is in the national interest and should be a national objective and, to advance this goal, federal science and engineering policy should strengthen efforts to promote the integration of research in education at all levels, supporting innovative experiments in this area.

And yet, in contrast, we're beginning to hear signs that perhaps this is not the right direction to go. My former boss, Harold Shapiro at Princeton, one time noted a growing sense that the competitive demands of specialized scholarship have created an irreparable rift between graduate and undergraduate educations. And it may have impaired the capacity of research universities both to remain centers of modern scholarship and to fulfill their broader educational functions. As President Shapiro put it, "the predicament today is that the faculty is indeed transmitting what they know and love, but with little awareness of what the student needs to learn."

Bob Atwell, in his final letter after stepping down from the American Council of Education two weeks ago, focused on graduate education at the root of much of our problems. We all, of course, view doctoral education as the crown jewel of American higher education, certainly the envy of the world. But Atwell argues that there's a mismatch today between doctoral education and the higher education marketplace, which is much more central to our difficulties.

He argues that too many faculty in our research universities are out of touch with the main stream of higher education, not to mention societal changes and fiscal realities, and so they go on trying to clone themselves in the persons of their graduate students to assist in their research. And yet, even those few graduates who are employed in higher education are unlikely to be employed in research universities.

Rather, they'll be employed in that far larger body of four-year comprehensive institutions, community colleges, in which education is the primary mission. And yet, because of their training, often frustrated, these new younger faculty pressure these institutions toward becoming research universities themselves.

In a sense, these kind of changes reflect the profound nature of the challenges and changes facing higher education itself. University presidents grapple every day with the current political economic crisis, the imbalance between revenues and expenditures, that characterize governments at the federal, at the state, and at the local level.

While much has been made of the new mantra in Washington these days, of balancing the budget within the next seven years, whether via the Contract with America or through reinventing government, it is the case that domestic discretionary spending, and therefore spending on research in education and federal support of our institutions, is at great risk.

I should point out the states are also in serious trouble, not simply from cost-shifting from unfunded mandates from federal government, Medicare, Medicaid, ADA, occupational safety and health, and so forth, but because of the effort to deal with K-12 education concerns. Many states have actually earmarked off the top the funding of those, and they've made massive investments in corrections in order to respond to public concerns about crime.

Well, one of my colleagues pointed out that a decade ago, when I assumed the presidency of the University of Michigan, my state had 15 public universities and eight prisons. Today, we still have 15 public universities but 35 prisons. And this past year, the amount we spent on corrections in our state, about $1,500,000, passed the amount we spent on higher education. That's happened in many other states, including the state of California. It raises, in a very real sense as well, whether we are increasingly beginning to regard education as a private good rather than a public good – moving away from the public principle that since education benefits all of society, all of society should support it.

A related concern, having to do with dollars but also with regulation that university presidents at institutions like this have to worry about, has to do with the rock and the hard place. In the life of many university presidents, the rock is intercollegiate athletics. The hard place is the medical center.

The great deal of time and attention is focused on how to help these extraordinarily valuable enterprises survive in a world in which health care delivery has shifted cost from third party payers to hospitals and now directly to doctors – that means to our medical school faculty.

The third concern: forcing change is politics itself. Across this nation, we're seeing once again an effort, sometimes well-motivated, sometimes quite misguided, by politicians to influence everything involving our universities, from the prices they charge to what they teach to who teaches to whom they teach. We see that in federal policies. We see that at the state level.

But I think what's the most concern of all is that in our society today there seems to be a new brand of politics – indeed, I might say populism – abroad, almost a post-modernist, deconstructionist variety that not only challenges but actually tries to destroy social institutions and social commitments. I think of the effort made in the state of California today to unravel three decades of support of affirmative action, which is now appearing in most states across this country.

Related to that are the particular pressures on a university presidency. A week ago, the Association of Governing Boards (of Universities and Colleges) released a major report of the

National Commission on the Academic Presidency, which concludes that the greatest danger to higher education today is that, in an era of growing doubts and demands, colleges and universities are neither as nimble nor as adaptable as times require.

Why? Because the academic presidency has become weak – "anemic" was the term they used – the authority of university presidents having been undercut by trustees, by faculty members, by political leaders, and, at times, by the president's own lack of assertiveness and willingness to take risks for change. I think the fact that right now the presidencies of the ten major public universities in this nation are open or will soon be open and are going to be dreadful to fill is a sign that we do have certain problems.

There are even more fundamental challenges forcing change on our campus. Our fundamental mission of creating, preserving, integrating, transmitting, and applying knowledge is not changing, but how we do each of those activities is changing very, very dramatically, driven by other changes in our society. The pace, the nature of change, is so fast at times that we have trouble recognizing what's really driving us.

It could very well be that our present institutions, our structures for the conduct of intellectual pursuit such as research, are as obsolete and as irrelevant to our future as the American corporation of the 1950s is. I love the quote by Don Langenberg, chancellor of the University of Maryland, who said, "it is probably about as safe to assume that the dominant higher education institutions of the 21st century will stem from the small but powerful group of present day institutions," referring to the research universities, "as it would have been to assume that today's dominant life form on Earth would stem from Tyrannosaurus Rex."

What is the university of the future? How are these forces going to force change? Well, let me just throw out several possibilities. It is clear that our institutions, at least our faculty, are quite nimble and are already beginning to redirect their efforts. They're moving increasingly away from the public sector to the private sector for support. Beyond seeking corporate support for R&D, they're beginning to market far more aggressively educational services and put into place far more realistic price structures.

Beyond that, there are fundamental changes occurring. The American research university was a faculty-centered culture. After all, the faculty was the source of the research that drove the intellectual vitality of the institutions. Today, however, we're moving from provider-centered to customer- and market-centered institutions, to use a business term. In a sense, to enterprises in which the people whom you serve come front and center as driving the institution.

But there's an even more subtle shift that I believe is occurring, a shift in public attitudes now underway, placing less stress on values such as excellence and elitism and more emphasis on the provision of cost-competitive, high-quality services. That is, shifting from prestige-driven to market-driven philosophies.

This relates very much to *Science: The Endless Frontier* and the partnership it represents, because, in a partnership with relatively unconstrained patronage, to get the highest quality research, you provided that patronage to the highest quality faculty, to the most prestigious institutions. Academic excellence and prestige were valued.

Today, society seems reluctant to make such long-term investments. Rather, it seems interested in seeking short-term services from universities. Of high quality, to be sure, but with cost as a consideration. In a sense, it’s shifting away from value and prestige in an effort to seek low-cost, quality services. That is, it's asking, "If a Ford will do, why do we want to buy a Cadillac?"

That suggests that the research university as we know it today, rather than moving into some new paradigm, may in fact return to a paradigm of the past, a century ago, with institutions, such as the land-grant universities that tended to focus much more on responding to the here and now needs of American society.

What drives this is a very interesting feature of the American university that most realize once they think about it but is rarely discussed. The modern university is a holding company for entrepreneurs. In a sense, we are indeed a collection of highly entrepreneurial, highly talented, and driven faculty members, each of whom is trying to optimize their own particular objectives and move toward that – and that, in turn, drives the evolution of the university.

That's why my university, for example, has become a $3,000,000,000 a year conglomerate. We do teach 50,000 students a year, but we also treat over 1,000,000 patients a year. We have campuses from Seoul to Hong Kong to Paris to London. We’re too big to purchase insurance, so we run our own insurance companies. We have a big-time entertainment industry called the Michigan Wolverines, which generates about $280,000,000 a year worth of licensing.

What does that have to do with the core learning of the university? Well, it's sometimes hard to find the relationship, but that's what we've become. A highly adaptable knowledge conglomerate. And I would dare say most of our research universities have become the same. Driven by the interests and the efforts of our faculty because we've provided them with the freedom, the encouragement, and the incentives to move toward their personal goals.

It's that point I would like to conclude with, because I believe, having been a part of it, having essentially grown up in it, that the American research university is probably more important to our society and our world today than ever before. I'll simply remind you of a statement made by Erich Bloch a number of years ago, when he was director of the National Science Foundation, in testimony, that the solutions to virtually all the problems with which government is concerned – health, education, environment, energy, urban development, international relationships, space, economic competitiveness, defense and national security – depend on creating new knowledge and hence upon the health of America’s research universities.

As important as these institutions are in our everyday lives today, it seems increasingly clear that, in the future, they should play an even more critical role. And yet, even as I speak, our institutions are evolving away from that particular paradigm.

The individualistic, entrepreneurial nature of our faculty, the fact that they do sense the pressures of our society, is moving them in new directions. They hear loud and clear the message that

America no longer believes in the importance of basic research and questions even the relevance of the research university. Whether they like it or not, the faculty is remarkably sensitive to the criticisms voiced by critics of the academy about too much emphasis on research. About too many Ph.D.s and not enough jobs. About whether we should shift towards more applied activities.

And they are responding quite rapidly to adapt to this brave, new world. I, for one, fear that unless we sound the wake-up call not simply to America but to sound it sufficiently loud and clear that our faculty can also hear the reverberations, the American research university will already have evolved into something else. Perhaps responding to other societal needs, but no longer with the capacity to respond to the intellectual needs of this nation and society.

The world and the structure of academic research have changed a great deal since this report and the principle was put out. But those principles, I think, still merit re-affirmation. Now, more than ever, the national interest calls for an investment in human and intellectual capital. In a sense, that's our challenge. To continue to make that commitment. To provide that, even during a time of extraordinary change. Thank you very much.

COLE: Thank you very much, Jim. I'm sure there will be a lot of people who will want to not only comment but ask questions on that. We will now shift over to the comments of our second policy design presenter. And it's a personal privilege to introduce Donald Kennedy, who is currently the Bing Professor of Environmental Science and president emeritus at Stanford University. His research interests originally were in animal behavior and neural biology, in particular, the mechanisms by which animals generate and control pattern/motor output.

In 1977, Don Kennedy took a two-and-a-half-year leave to serve as commissioner of the U.S. Food and Drug Administration. Following his return to Stanford in 1979, Dr. Kennedy served for a year as provost and for 12 years as president, a time marked by renewed attention to undergraduate education and student commitment to public service and successful completion of the largest capital campaign in the history of higher education.

As I think about the Bush period and the aftermath, the 50-year period since the publication of this report, there are a number of American universities, both public and private – two of which are represented here in the panel today – that have made extraordinary, extraordinary strides. And Stanford has become, as we all know, one of the great, great institutions in the world of higher education.

I think Stanford and the country owe a great debt to Don Kennedy for being one of the real leaders in that growth and development. So it is great pleasure to have Don here today to comment and to present on this issue. Don? (applause)

KENNEDY: *Science: the Endless Frontier* as metaphor represents a momentous decision that decanted the mechanism and the resources for supporting science into the institutions responsible for training the next generation of scientists. It was a bold step that no other industrial democracy took, and the others have reason to regret their choice. There is no question that the decision was good for science. The question I want to consider is, was it also good for the universities? That is a harder question.

It may not be the right moment to answer it, because for America's universities it is not unfair to say that it is the best of times, it is the worst of times. Surely it is the best in a number of important respects: scientific vigor, desirability, international respect, and others. But it is also the worst of times, and for a whole array of reasons. This awkward sense of doing better but feeling worse resonates with a historic public ambivalence about higher education. On the one hand, we are the escalator of upward mobility and the agent of personal improvement. On the other, we are seen as elitist and stuck up.

Our public, while clamoring for their sons and daughters to get accepted, resents the fact that in little more than a decade the lifetime earnings gap between high school and college graduates has increased by 50 percent. Our research accomplishments are recounted breathlessly in the newspapers, but in conversations among parents, the central theme is that Susie's calculus teacher can't speak English as well as Susie. Some of this disaffection is aimed at a utilitarian academic research culture that in some ways is a collateral descendant of *Science: the Endless Frontier*.

That report introduced a new role for America's universities. As keepers of the national scientific flame, they came to be seen also as the driving force for a whole suite of economic and social objectives. At first it was the general argument that basic research would empower a more innovative society. By the late 1970s, international competitiveness was already being invoked as a challenge for university science and as an argument for funding it more generously. By the 1980s, higher education was being seen as an engine for improving regional economies. And every valley with a university in it seemed to be made of silicon.

Somehow, though, the American public has held on to a more distant version of the university: one that today sounds almost quaint. It is a place where young people get in touch with great ideas through introductions conducted with sympathy and understanding by thoughtful older scholars. It is a place where they learn to analyze and reason, and develop the habits of inquiry. It is a place where intellects can wander freely over ground that may or may not have immediate application, but where the culture is examined and advanced.

At the core of this image is the passage not just of knowledge but of the capacity to gain more knowledge from one generation to the next. When Americans look at their universities, they sense that the new utilitarian obligations have somehow triumphed over this older and deeper vision. That disparity, the gulf between new reality and old expectation, lies at the heart of our present public discontent.

In what follows, I want to try to map that more precisely. But lest the rest of this seem too discouraged or critical, let me begin with a quick accounting of the benefits that *Science: the Endless Frontier* has left with American universities. They are boundless.

First, doctoral training has been made richer and more effective, to the benefit of science and, presumably, to the benefit of the trainees as well. Revenue accruing to the universities from sponsored research has not only made possible a new level of intellectual activity in scientific fields, it has permitted internal reallocations that have helped the non-scientific disciplines as well. Educational programs generally have been enriched by closer contact with active investigation: not only graduate students but undergraduates have been the beneficiaries of new opportunities for well-equipped independent study. There has been a closer coupling between university research and societal need. Support from federal mission agencies and from industry has extended the domain of research application. It has helped to keep faculty closer to the most dynamic locus of research activity, and in that way has enriched teaching at all levels.

But in other respects, the very success of the idea Bush launched has, as so often happens, produced some second order problems. First, the postwar research surge has altered the balance both between undergraduate and graduate education and between research and teaching. Although in some respects there has been expanded opportunity for engaging in supervised research, and undergraduates have benefited from the change in those ways, their greater distance from faculty and the absorption of the latter in their own work has probably weakened the undergraduate experience. Undergraduates spend far more time today with para-faculty and teaching assistants, and less with senior faculty.

Second, the expanded opportunity for graduate students in their own specific research areas has been accompanied by some real restriction of opportunity. The growth of research assistantships and the lengthening of time required to complete the doctorate are side effects of the need for graduate students as labor in the university research enterprise.

Perhaps this need accounts for the unwillingness of our science departments to limit graduate enrollments even in the face of evident oversupply. This is a tragedy of the commons that is producing a morale crisis for some of our best and brightest young people. If you want to glimpse the depth of this discontent, talk to doctoral candidates at west coast universities who refer to the "I-5 route," the series of substitute teaching assignments they may be forced to take at the several dozen institutions spread along the Interstate 5 corridor.

There is a litmus test for detecting when sectoral problems assume enough significance to begin the transition into publicly recognized issues, and it happened to this one: Garry Trudeau made it a long-running theme for *Doonesbury*. The agent of President King is outside the gates of Walden College recruiting gypsy faculty from an eager crowd of candidates assembled there in a mob. He's shouting through a bullhorn. “Intro Bio. I'm looking for an Intro Bio.” A well-dressed respondent says, “I'm a Cornell Ph.D. I don't expect tenure, obviously, but I would like a two-year contract with medical benefits.” The agent looks around, then asks, “Any other candidates?” From the back, “I'll work for food.”

The crisis of confidence is made worse by a disjunction between what the students are trained to expect and what they are likely to get. Little or no effort is made to prepare our doctoral candidates in the sciences for alternative careers, or to be more effective teachers, or even to confront some of the professional and personal challenges – ethical and other – that they may meet in academic careers. We do more for MBA and law students in this regard than we are doing for those whom we are preparing for our own profession. It is extraordinary.

In fact, our graduate students are being prepared to lead lives exactly like those of their research supervisors, and for that only. Naturally, they expect they will find work in elite universities. They are almost invariably disappointed.

The growth of dependence on federal funds among state-supported as well as private research universities has blurred the distinction between public and private. The University of Michigan and Stanford might both be described as quite similar federal universities. This blurring has been accompanied by a subtle but steady increase in government ambitions for control, which are justified under the all-purpose principle of accountability. In the past ten years, government agencies have made determined efforts to regulate access by particular groups to unclassified university research; restrict access of foreign nationals; place restrictions over academic researchers publishing their own data; and pursue newly-claimed regulatory authority over something vaguely defined as academic misconduct. We have also watched the growth of legislative pork-barrel appropriations.

I am not suggesting that universities should do no government work or take no government money, but we need to be realistic. When institutions serve utilitarian purposes, they invite political intervention. Absent the growth in federal control we have seen, I suspect that state university governing boards might not have become as ambitious as they have, and might have stayed in their traditional oversight roles.

Today, three great public university systems – Michigan, California, and Minnesota – are in desperate disarray over efforts by political regents to assert control over traditional academic functions. It is a very serious situation, so far without significant opposition or public outcry.

The success of the research venture, spectacular though it has been in many respects, has been mixed. Where there are small production units and tightly bounded problems, the returns have been extraordinary. Perhaps the success of biomedicine is the best example.

For the big problems that societies have to solve – violence, poverty, environmental deterioration, the economics of health care – university research has been much less successful. I suspect that two explanations for this may be valid. One is that the funding system has strengthened departments, making interdisciplinary work more difficult. The other is that the very system of making grants defines areas too tightly.

Finally, the legacy of *Science: the Endless Frontier* has been to alter life irreversibly for faculty members, especially in the sciences. The new order has added immeasurably to their productive capacity, but it has also attenuated their institutional loyalty. Faculty are more peripatetic. Their membership in the invisible international academies of their disciplines is far more weighty in their lives than their attachment to their own university and their students. It is this disengagement that caused Henry Rosovsky, concluding his second term as Dean of the Faculty of Arts and Sciences at Harvard, to speak of the secular decline in civic consciousness of his distinguished professorate.

Now I return to the problem of the American public's troublesome disaffection with higher education. First I will summarize it, then suggest some resolutions. The problem is that we are seen as not occupying a central role in solving the big problems, as overbalanced in our emphasis on esoteric research, and worst of all, as failing in our duty to educate our sons and daughters. In short, even considering the benefits we have gained in the past 50 years, we need to worry about the costs.

Can those costs be reduced without giving up the benefits? I think that the resolution depends, in the end, on a pretty simple principle that rests on a notion about intergenerational equity, a notion very much built into the Bush proposition as it was originally put: we need to return students to the center of our institutional concern.

The argument for this is not a kind of moral abstraction, it is intensely practical. It is very difficult for me to think of an academic scientist, even among the most distinguished colleagues I have had, who has not contributed more through the students he or she has produced than through his or her own work.

That is how we progress: by finding people with capacities greater than our own, filling them partway with what we have to offer, and then watching them go off and go farther than we have been able to do. They, more than the innovations begun in the labs where they were trained, are the mainstream of technology transfer.

It is people, not things. In practical terms, shifting our gaze does not require a disengagement from research. But it does require abandoning the idea that advanced students are there primarily to serve contemporary ongoing research programs. On the contrary, they are there to develop their own capacities, which they will do best if permitted much more choice and control, and given a broader education than is now the case in most university science departments.

William James once referred to the Ph.D. octopus. Times haven't changed much. We are requiring a degree well-designed for one set of things, giving it to people, and watching them go out and do another set of things. It is remarkable to me how we could have gotten into the situation we have with respect to the market crisis our graduates are confronting.

In the 1960s and early 1970s, those of us with active research programs thought we could turn out a Ph.D. every year or two, totaling up to maybe 15 or 20 over a long career. Did we really think that this employment sector was going to increase by 2000 percent in one generation? Had you posed that question then, anybody would have said, "of course not."

We created that excess with the encouragement and enthusiastic support of government policies and funds. Now we have to rethink our rate of production. One of the difficulties is that replacement is happening too slowly. Universities are in a kind of academic gridlock in which resource constraints and retirement disincentives are combining to block a generational transition. That is unfortunate, because the young people who are surviving this experience and getting the few positions that are available in the research universities are extraordinary. They are the best we have ever seen.

That brings me to a concluding recommendation. The most promising route to constructive institutional change is, in fact, to change the players. We are confronting an alarming problem in an aging science faculty that will not quit. In the past two decades, the average age of faculties of most research universities has increased by somewhere between six and eight years.

I promised you another meaning for the endless frontier metaphor. It is this: my cohort of academic scientists, this group of aging buckaroos, has been riding through the golden age of the frontier. We have passed through the fence that was called mandatory retirement until the Congress busted it, and we're headin' for the sunset, defined contribution retirement plans in hand. There's every incentive to stay in the saddle. So happy trails, partners. The frontier may be endless in more ways than one.

To rescue our successors from discouragement and broaden the influence of science in the larger society, we need to change graduate education for our best students. We need to open up some different opportunities for the very good others. Above all, we need to put the next generation at the center of our concern. And the best thing we may be able to do for 'em, partners, is to get out of the way.

COLE: Paul David is our first panelist and also one of the extraordinary contributors to issues of science policy. Paul is professor of economics at Stanford and, since 1994, a senior research fellow at All Souls College at Oxford. Dr. David is known internationally for his contributions in a variety of fields, including economic history, economic and historical demography, and the economics of science and technology. I can tell you personally that I have spent time exchanging ideas with him. And hopefully someday Dr. David and I will get to collaborate on some work that we are doing that is very similar. So it's a great pleasure to have Paul David here as the first panelist in this session. Paul?

DAVID: (applause) Thank you very much, Jonathan. I accept the offer of collaboration.

The billing of these sessions as design sessions put me in mind – especially after listening to our distinguished presenters who have been through the fire of recent developments in American research universities – of the problems of designing the future of a transatlantic transport system from the deck of the Titanic. There is some need, in the midst of the scramble for the lifeboats and attempts to figure out where the nearest landfall might be, to maintain some perspective on the larger set of questions.

And what I want to do is to comment on the context within which the problems of the American research universities might be considered. To look at some of the system-level implications of the possible remedies that are being suggested for the problems and future of the research universities. And to suggest that, in the spirit of the remarks of both Don Kennedy and President Duderstadt, there is a set of unique roles that the universities have played and are uniquely positioned to continue to play. And those roles are becoming potentially more important for national well being, even at the time when the ability to perform these roles is increasingly under challenge. And one needs to think about the ways in which the universities can do what they uniquely are able to do, rather than transform themselves into some other kind of institution.

The first point of perspective that I want to insist on is that, when we talk about the universities and research universities, we ought not to be thinking only about the 60 universities that form the upper quintile, the 20% of the research universities. There are about 200 research universities. The upper 20 percent, it's true, do about 50% of the total research and something between 50% and 60% of federally funded research. Within this top half, there has been a history of increasing spread of the research-style university over the course of the past three decades.

And we can ask, what was the logic that drove an increasing number of institutions of higher education to get into the research mode? And by reflection, we can ask, what will be the implications of an alteration of the environment? Where will the pressures for change be greatest, and where will the alteration, the readjustments be most dislodging for the larger university and non-university research system?

When we look at the fiscal pressure, the cuts in public funding that are being scheduled, it's important to bear in mind a point that has frequently been touched upon. And that is, we do not have a consolidated research budget. The nature of the cuts is a function of what's happening in a number of diverse agencies, which are responding in different ways to cuts of different magnitudes.

And therefore, if we look at the pressures that have been felt in the Department of Defense and the Department of Energy, it's important to note that relative to the other major funding agencies of NIH and NSF, those two agencies were most elitist in their approach to funding. They had specific missions. They had a need to fund research to achieve a certain research product. The effect of cutbacks in that section tended to impinge particularly on the upper echelon of the research establishment. And other agencies, such as NSF– which has an explicit mission to build up a very broad-based research establishment – have tended in the face of cutbacks to spread these out across the whole system. To maintain for various reasons some political protection, because of the nature of their mission. And to maintain continuing research activity even at a lower level across a much wider range of institutions.

Now, the effect of that difference has been that the agencies have tended to increasingly at the margin ask the universities to bear the cost of some of the research. And the effect of that has been, as many people are aware, to reduce the effective ability of the system to cross-subsidize a wider range of activities, including teaching.

Research cross-subsidized the growth of a large part of the university system and, to a large extent, was an unexpected byproduct of the approach which Vannevar Bush foresaw – an indirect cost aspect to the funding of research that would put resources at the hands of research performers to be used for a wider range of activities.

The shift from a patronage mode to a procurement mode has led to the abandonment of that implicit contract. As a result, one of the things that's happening at the margin is that the effective amount of indirect cost recovery has contracted. A recent set of estimates suggests that, although the average negotiated cost recovery rate is something like 50% or 48%, the actual rate for private universities is 30% and for the state universities is 20%.

Over the course of the '80s, the universities responded to this by using indirect cost recovery to stay in the research business by matching these federal funds. And the result has been the disappearance of effective margin for the support of a wider range of university activities. It is in that context, it seems to me, that the set of pressures for redirecting the activities of the elite universities back towards teaching acquire a greater force and a greater appeal, because the former fiscal complementarity between teaching and research has disappeared.

There was an important set of organizational, intellectual, and pedagogical reasons for the coupling of research and teaching. And I think we have a general agreement that those arguments still obtain. And they obtain particularly strongly at the level of graduate teaching in the sciences. This goes without any dispute, certainly in this audience.

But what has happened is that the generalization of the research-teaching complementarity as an institution-building strategy, as an organizational strategy, as an internal fiscal strategy, has disappeared. And the question I want to raise is the extent to which the fiscal decoupling of research and teaching goes into a steady-state first and then contracts – and what can be done to prevent that from being translated into an intellectual and organizational decoupling of teaching from research.

I think that we need continually to understand the larger systemic implications of allowing such a decoupling and of allowing a reorientation of major institutions of higher education away from the performance of research. Because the implications of such decoupling are potentially very serious not only for the universities but also for society at large. There are three areas of dysfunctionality that I think we should bear in mind.

One concerns the implications for the research activity in the national research system. The second has to do with the implications for the training of future generations of researchers. And the third has to do with the ability of universities to fulfill their role as open nodes in a international global knowledge production and knowledge exchange system.

And the spirit of all of these points, it seems to me, lies very close to the set of concerns that Vannevar Bush had in mind when he proposed that the university should be a vehicle for an expansion of research activities to replenish a stock of knowledge that had been drawn down, as he saw it, rapidly in the war mobilization efforts.

There are three archetypal issues. The first of these is, what portion of public funding of science and technology should go to research that is driven by the internal logic of the subject – that is, the development of conceptual schemes for their own sake?

This is not a question encountered only in the public sphere. There are some large R&D-intensive business corporations that also have sufficient resources to make it pertinent for them in their strategic planning.

But research is an uncertain proposition, as we know. And such planning is insufficient to dispose of the underlying issue completely. Many important fundamental science advances have derived from problems that were first encountered not in a blue sky or curiosity-driven program but in practical research contexts.

And so the second practical choice point in the management of research is to ask how far beyond the immediate need for a solution to a problem at hand, a particular research mission, should you encourage researchers to go in exploring the more general conceptual problems that they've opened up – thereby creating potentially new demands for curiosity-driven research that otherwise would not have been discovered and couldn't be planned exactly.

One microlevel form at which this second practical matter occurs is how tightly specified is the budget. How close does it look to the grant, to the procurement contract?

We're living increasingly in a mode in which pressures on the universities from funding agencies – in particular the federal government but also to some extent in business collaborative research – have resulted in what might be called grant tracts.

A grant tract is half a grant and half a contract. It has a short term. It has deliverables. It has a lot of monitoring and regulation. It prevents mid-course corrections in research projects. It prevents people going off and chasing things that weren't in the original work plan.

So the issue is how much top-down as opposed to bottom-up research direction there is going to be. The traditional university mode allowed within the context of grants an enormous amount of flexibility and chasing things that were not ordinarily planned. And one can say that in the system as a whole, you want to have some of that. And you don't want to see all of that go away.

The third choice point involves the question of what is to be done with the findings. The opening up of a new demand for investigation by seizing on the accidents of research often entails the mobilization of concepts, research talents, and techniques imported into new and emerging fields from areas whose relevance was not immediately perceived.

We can think of laser science and its applications in microsurgery or molecular protein chemistry in relationship to plant genetics. Computer science in relationship to gene sequencing. A different range of questions, thus, is posed.

We want to ask how widely, how quickly, are the findings of research in a new area going to be disclosed. How fully are they to be disclosed? Who is to have access to the data? And at what levels of completeness? How should the new fields be best explored? By complete disclosure, which throws it open to all commerce on an equal footing, or by policies of information management designed to keep direction control in the hands of the original discoverers? Or the institutions supporting them?

This is the important issue of access and openness in science that was quite central in Bush's thinking. It remains very central. But it is increasingly under pressure.

If one considers the pressures to adapt to new sources of funding within the university, one can look back over the 1980s and see what has been nicely documented in a Ford Foundation-financed study carried out by Wes Cohen, Richard Florida, and Richard Go, all of Carnegie Mellon, who looked at the growth of university-industry research centers. We did not have a national industrial policy. We did not think about it. The Reagan administration foreswore having an industrial research policy. And nonetheless, encouraged by the administration, the number of university-industry collaborative research centers expanded rapidly. And now there are more than a thousand of them, spending in 1990 approximately $2.5 billion on R&D and another $1.6 billion on research-related activities, including education and training.

The one area of the Cohen-Florida-Go report on which I want to draw in this connection is one that concerns the changes in policies towards openness of information within these units. Nominally, they look like university activities if you ask about their internal and fiscal management. Between 88% and 91% say that industry has little or no influence over their fiscal management or their internal administrative operations.

But if you ask what has happened to the norms regarding control of information within these organizations, you see that at first subtle and then not so subtle changes have taken place. If we ask in what fraction of these institutions are participating companies allowed to restrict the flow of information both inside and outside the centers in which they are involved, you find that in 57% of these units in the sample, the firms have information control policies that permit restrictions. In 53%, communication of information about research projects to non-participating companies was not permitted. Well, we think, okay, this is reasonable. We don't want so much free riding. In 40% of the centers, information flows to participating companies were restricted even if they were not involved in a particular project.

Now we want, at a lower level, to enforce collaboration in particular projects if you want to have full access. Now if you ask about indirect leakage via the academic research communities, you find that these centers also have in place restrictions to prevent that. In approximately 29%, there are restrictions on communications with faculty members at large. In 21%, communications with faculty of the center's own university was restricted. And in about 13%, there were internal restrictions on information flows to faculty working within the center itself.

One more point might be made about this. If one asks about the effect on permitting delays in publication, I think we are quite familiar with the fact that most universities have now allowed for delays to allow time for patenting. In many cases, the offices of technology licensing are themselves the patenting entities. But what about suppression of research results? I found it very startling that in 54% of the institutions, when the mission was regarded as very important to the company who was sponsoring it, they were allowed to suppress research results. In 20% of the centers, firms were allowed to suppress research results whether it was important or not. Thus, in a substantial number of these centers, we have a reversal of the prior norms of open research and their supplantation by essentially the rules of proprietary research.

Now, what these developments portend, at least for me, is the prospect that if one moves towards the decoupling of research into affiliated institutes and separates the teaching and research from the general context of the university, one can predict that the alternative system of incentives and norms regarding scientific cooperation and disinterestedness and openness in research will be supplanted by the other system through which research is organized. That is, we will move from the system of relationships among researchers and their views about information that characterize a patronage system, in which there is the implicit bargain of patronage in exchange for full disclosure, making into a public good new knowledge. The whole system of production of reliable knowledge as a collective distributed process will be replaced by the alternative system, which is a property system.

That is, you have intellectual property. You are rewarded by being given an element of monopoly power over the exploitation of knowledge – and therefore have incentives to keep it private or semi-private until you can secure full property rights or to use secrecy as the alternative means to appropriate the benefits.

For me, a problem in research management is to get the balance between these two modes correct. We have a system in which we have managed to balance a very large and active industrial research activity with a very vibrant research activity conducted under the open science mode through the universities.

If we allow the pressures of the vice within which the universities have been placed – between the cutbacks of the agencies and the pressures of OMB and other organizations to make them lean competitors – lead to the shucking off, the shedding of research activities into semi-independent organizations, because they are no longer a source for cross-subsidization of the other missions of the university, than this decoupling will have a serious impact upon national research activity.

It will affect not only the character of the research but also the character of the training and the ability of the universities to provide to industry the enormous subsidy it provides through the training and evaluation of researchers. It's true that university professors are producing graduate student Ph.D.s like sunfish, at a rate of about one to fifteen (that's approximately their lifetime reproduction rate) faculty members per university Ph.D.s in science and engineering. But these Ph.D.s do not have to be, and have not historically been contained, within the university system. They move into industry. Since we are also a specialist in training people in this system from overseas, they also move out of the country.

Our ability to train people in an open science mode to carry on this kind of research has had important effects on the attitudes of firms in the industrial sector. There are large firms – and I'm talking not only about Bell Labs or the IBM campus or Xerox Park, all organizations from which enormous technical advances have flowed – that conduct research and publish their research in order to stay in touch with the open science mode.

And then when that goes, there will be consequent changes in the style of research carried on in the corporate sector. And the effects of this I think are deleterious. They will also lead to changes in the ability of American researchers to interact on an open basis with research in the rest of the world at a time when people recognize that increasingly the science base is internationally distributed. That there are lots of areas where the expertise at the frontier needs to be tapped in order to absorb knowledge for application and for furthering research in this country.

So while I believe that the major universities have to think about their core missions, and that their core missions include teaching, and that one can take the redirection back towards a concern for teaching as an important set of pressures to which the universities must attend, it is equally important to keep in mind that those needs and that part of the university's mission was always important. That the arguments for being more attentive to our undergraduates and to the needs of graduate students to develop their own autonomy as researchers and to be able to pursue a career are significant.

We are being tempted to decouple research as competitive with those goals – not because they are necessarily competitive from a pedagogical viewpoint, not because they're competitive intellectually, but because the complementarity on the financial side has disappeared. And it's important not to be driven by that into a decoupling of research from the other modes of the universities, which will be both damaging for the students and damaging for a much larger set of concerns nationally.

I think the universities have to think about their other important mission, which is to be centers of open learning. To be the points of contact with the international open science network. That universities should be perhaps less worried about lack of civic loyalty and more interested in collaboration and in reducing research costs by shared access to facilities.

We have an increasing number of tools through the information revolution that will permit remote access, sharing of facilities, tighter coalitions among universities both nationally and internationally. And the American research universities, given their present position in the world, are in a unique position to exploit this. That is the direction a healthy set of responses might go along, taking the point that we also need to consider the origins of the university institution as a teaching vehicle. Thank you. (applause)

COLE: Thank you, Paul. Our second panelist is Eduardo Macagno, who as an undergraduate worked on the team with Professor James Van Allen on the early exploration of the earth's radiation belt. He began his studies in the physics department, actually at Columbia in 1963. He has now become the dean of the Graduate School of Arts and Sciences at Columbia, appointed to that position in 1993 by George Rupp when he assumed the presidency. And he holds a co-title of associate vice president for research in graduate education. Also, he made a shift from physics into neurobiology after studying in the post-doctoral position with Cyrus Levinthal of Columbia's department of biological sciences. It's a great pleasure to have Eduardo here this morning as our second panelist. Eduardo?

MACAGNO: Good morning. I'd like to offer a few reflections on issues that have been raised both by my fellow speakers this morning and by others at this conference and elsewhere, as they relate principally to my current interests.

I would like to reflect on some issues, principally as they relate to graduate training and its role in research, that I have to deal with as both the dean and as a scientist with the laboratory that is still quite active. There are certain contradictions that I face almost daily between the two things I'm trying to do, and I thought I might bring some of those up.

As opposed to the more general views of my fellow speakers, I'm going to try to deal with more short-term concerns. To quote a favorite saying of my colleague Jonathan Cole, "The devil is in the details." I think we need to think in those terms about what kinds of things we might be doing to move towards the future. I think our system of coupling of doctoral training with research and also of projects that are proposed and carried out principally through faculty-initiated, individual-initiated grant proposals, has many things that are very positive and need to be remembered. But there are a number of problems that arise, inevitably, from the system.

As has been pointed out by the previous speakers, the system is fundamentally dependent, as we've conceived it in the past, on an ever-expanding base of resources and positions. The faculty trains many Ph.D.s. The number of scientists clamoring for opportunities increases exponentially. It is not surprising, therefore, that the competition for grants has become fierce and that hundreds of applications are received for each faculty position that is advertised.

It has been proposed that we practice birth control by limiting the number of Ph.D.s we train as some professions, in fact, have done with their own degrees, with their own production. And some departments, in fact, have begun to reduce the size of the graduate programs. But I think there are some significant problems with doing. As has also been pointed out before, who is going to do the research that is now performed by graduate research assistants if we reduce the number of students going through the pipeline? Certainly, one possibility is that post-docs and technicians do that, but of course that will cost more money.

I think one needs to consider as well as whether there is a supply of post-docs and technicians who can, in fact, do the work and in a creative way, like graduate students do it at this point. I think we need to rethink how we do research if, in fact, we move seriously away from that mode.

A second question that arises is, how do we select who gets into these programs? To paraphrase Dr. Gomory's unpredictability principle, we cannot predict who is not going to be a good scientist. We have an idea of who might turn out to be a great scientist. They're easy to recognize. The very few who are even as undergraduates extraordinary but whom we say no to is very hard to determine, and I'm not sure that we can.

Another question is, given the way that all the problems have been discussed so publicly, how do we keep the very best possible candidates for scientific careers from going into other careers? That is, by proposing that we are going to be very selective and reduce the size of our graduate program, I think we're also driving away potentially very good scientists from considering our program.

Our emphasis on individual-driven projects has also some negative consequences that we need to think about. For example, collaboration to reduce duplication and inefficiencies is hard to introduce. The faculty is used to operating very independently and to essentially having a great deal of hegemony over their own operations. I think that has translated, to some extent, at the federal agencies into a lack of funding for common facilities for supporting, for example, technicians that maintain equipment and so on.

It is not that difficult these days to get equipment for shared facilities. It's virtually impossible to get funding for the technical personnel who will keep those going. It was the case, 15 to 20 years ago, that the NIH provided a certain amount of general funds to a university that could be used for a variety of purposes, one of which was this. But as the funds have become somewhat less adequate, the pressure to put all the funding into individually driven grants and proposals has, essentially, pushed towards the disappearance of such funds.

A point that I think is worth considering is that we have undervalued post-doctoral and research positions. In fact, in many publications, such positions are referred to as underemployment, because the tenure-track faculty position has become the supreme goal of the people we train.

Another issue worth discussing is that the system, as it has evolved, has led often to distrust and in fact conflict between university administrators and faculty. As I said at the beginning, I see that kind of conflict going on as a scientist, and as a dean I'm having to work in different directions at times. The faculty often feels that the overhead is excessive and that it should be maintained within their operations as opposed to being given to the university, because they feel that it cross-subsidizes too many things. The faculty feels that tuition that we charge for graduate students is unwarranted, because they are in fact in the lab and there should be no tuition paid. The administration suspects that the faculty is always trying to gain the system and keep all their money for themselves. This kind of interaction has had some bad consequences, which, I think, have repercussions outside of the university.

So, those are just a few ideas that have come up from discussion that I've heard thus far. I'd like to suggest a few possibilities for how we might deal with some of the problems that have been created by our system in the short term.

I'm not talking as much as the previous speakers about the long-term evolution of the university. As both the scientist and administrator, I'm trying to think of ways in which we might evolve in the short term, to deal with some of the problems that I just mentioned.

I think we need to reduce our dependence on graduate students for carrying out research, by shifting more to post-docs and technicians. And that requires that we improve both the status and benefits of post-doctoral trainees and professional researchers, who are in fact post-docs perhaps in a holding operation but maybe just in a situation that they'd like to be in. Of course, the status and the benefits are not commensurate with staying in that position for very long. If we reduce the number of graduate students, we have to at the same time make those positions better considered by both the individuals who have those positions and by the university itself

We have to focus on the training of Ph.D.s rather than on the output of the students as they do their research, by including more teaching opportunities as well as opportunities to broaden the range of expectations. If there's a lot of pressure to reduce the time to a degree, it’s because it has at least in some areas gotten rather long. And so broadening the education should not mean increasing time to a degree by having minors or a lot of additional courses.

But we have to develop ways in which we can broaden both the interest and the expectations of our trainees. Internships in nonacademic positions are something that has been suggested by many and that we ought to look into. I think it is important for grants to provide some support of students while they are actually doing the research, but we've tended to depend too much on the grants too early on in the career of the student.

This, by the way, is exaggerated by the fact that lots of our students are foreign-born and cannot, in fact, be put on training grants. So, by and large, the support of the students who are also perhaps not capable of taking on a TA-ship (Teaching Assistant) has become a role for research grants.

Another proposal that I think we need to discuss is enhancing the value of master's degrees. As part and parcel of becoming more selective in whom we might admit to Ph.D. programs or in reducing the size of Ph.D. programs, we still need to perform our function within society of training the people who are going to go into industry and do their research in industry.

I have heard and I suspect that many of the jobs that our Ph.D.s take in industry could very well result from a master's degree rather than a Ph.D., although not across the board. So, the gradual decrease in the status and significance of the M.A. at the research universities has to be reversed. Those degrees, which are much shorter, much more flexible, have been a way of bringing in some revenue to the school. We have to make those degrees really be perceived as having both a status and a significance, better than they have at this point.

In a way, perhaps, the hardest problem is to get the faculty to change both their expectations and their set of values that they impart to graduate students. My own experience, here at Columbia, has been that by and large faculty is so directed in the sciences, towards the research, that educational questions are really rescinded. Not all – there are many sitting in the audience whom I recognize, who are very concerned about these matters – but many, particularly the lab scientists, are not willing to really change their way of thinking about their educational role. It is important for us to begin to bring them into this kind of discussion.

My next-to-last point has to do with an issue that you see in your programs. It is the moving, the separation, of education and research, which Dr. David has just raised in the context of the restriction of information flow. I think that we ought to fight this separation from another point of view, that students who end up doing their degrees in institutes, away from the campus, tend to have much narrower experience. It is much harder to give them the breadth that I think we need to give them. They don’t interact with, for example, social scientists or humanists in thinking more broadly about the societal consequences and other issues of the work they're doing. They become too isolated. They also don't interact with other scientists in other disciplines.

You know, I've started an interdisciplinary seminar here. I'm finding a lot of trouble in bringing students who are in what I would call the institutes within Columbia – like Lamont-Doherty Earth Observatory and the medical school and even the Nevis Laboratories – to the university even every other week to have a discussion because they are so focused on their own endeavor within their laboratories and their disciplines. I think that it's detrimental to the vision that students have of their own opportunities and the issues that they should be thinking about.

In addition, separating graduate training and research from the undergraduate campus, at least from our experience here, would be very, very negative. A lot of the best undergraduates whom we can call away from the law, business, and medicine into the sciences come into the sciences because of that interaction with the research with the graduate students, with the professors. Even if the interaction with the senior professors is more limited, it is not nonexistent. And there's a lot of enthusiasm for such careers, which is derived from that interaction.

My final point is, while all the issues about the evolution of the university that have been raised are important, I think we have to be careful not to overreact. I think oftentimes we see criticism as being adversarial, as opposed to being really a fundamental questioning, which we should welcome. I think the good things about the research university as conceived in *Science: The Endless Frontier*, as well as by its practitioners, the good things are there.

We have to make sure that in the process of considering utilitarianism or the economic drive or the fact that the university may change its conception vis-a-vis the society, we don't lose some of those wonderful aspects such as the interaction between research and training. Such as the openness of interactions and conversations that have made these places, the research universities, places that we all want to be in. Thank you. (applause)

COLE: Thank you, Eduardo. Please identify yourself as you raise the question or make a point.

GUSTON: I'm Dave Guston from Rutgers University. In Bush's day, the question was asked, "What is to be the federal role in funding university research?" One of the subtexts of that question that's missing here is the federal role as opposed to the state role and that was very important in that period.

There were three implicit arguments in *Science: The Endless Frontier* that favored a federal role as opposed to a state role. One, Bush thought there was a lack of sufficient talent to run research programs among the states. Second, the states lacked the ability to fund research in the university with stability over a long period of time. Third, the federal government would have to be less intrusive than the state governments would be apt to be.

And over the past 50 years, it's arguable that the states have become more talented, relative to where they were, and that the federal government has not proved so superior, at least in the current time, in not being obtrusive and being able to provide stable funding. So, not to jump on the devolutionary bandwagon, but the point is to suggest that maybe the argument for a federal role might be a little bit weaker in some respects now than it was 50 years ago. And the design question is, are there ways that we can creatively incorporate a new state role in university research that might expand the constituency for university research?

MALE VOICE: I guess that's the one I get. Actually, I think the point is well taken, if you go back a century. But I think it has some serious difficulties in 1996. A century ago, the states were actively involved in supporting highly applied research through agricultural experiment stations, through engineering experiment stations, that many land grant public universities have.

What has happened in the 1980s and 1990s, though, is the same structural budget imbalance that afflicts our federal government has also manifested itself at the state level, in which universities and their activities tend to be funded from a shrinking pool of discretionary resources with most of the real public revenue going into either entitlement programs, into corrections, into earmarked K-12, which limits their resources.

Second, there is very definitely a narrowing view of public support of universities at the state level, being used primarily for the support of undergraduate education. And, indeed, more and more states are moving towards formula funding, based on undergraduate instruction, which totally disregards the support of either graduate education or including in the professional schools and certainly research. And so I do not look with any great optimism to seeing the states play a significant role in the support of basic research on our campuses.

QUESTION: Just a quick addendum from a state more ambitious than most in trying to do things. The State of California has had a couple of modest research programs, funded by special initiatives. I think they have been poorly managed. I think that, you know, whatever bad things one can say about the federal bureaucracy, one can double in spades for state bureaucracies.

EISENBERGER: Peter Eisenberger, Columbia University. I guess what I find interesting in this discussion is that people talk as if there is such a thing as "the university" and there is such a thing as a single and simple definition for "education" and "research." What I'd like to suggest is that the enterprise has become much more complex than the original embodiment, as represented by how large it has become and the enhanced role it is playing in the society.

It might be helpful to leave the simpler models, where we start from the premise that there was such a thing or there is such a thing as a simple definition of the university, and think about the problem from the following perspective. On the education side, we are interested in educating citizens, we're interested in educating professionals, and we're also interested in educating future faculty members or intellectual leaders to join our academy. And on the research side, we want to certainly do research that is of value to society as well as research that pushes the frontiers of knowledge.

And if you take that complexity of possible missions and allow a diversity of institutions to arise that can specialize in meeting one or more of those missions, we might end up with a more robust enterprise than everybody trying to look like Harvard University, as I think the current system is trying to do. I raise that as a question, whether diversification is not an appropriate response and specialization is not an appropriate response to the current situation we are confronting.

MALE VOICE: Let me respond, briefly. Clearly, higher education in this country is highly diverse, although there is an unfortunate tendency to all look to mother Harvard as the model that they aspire to and intend to evolve in that direction.

I think part of the difficulty is that while there is good reason for diversity, unfortunately, we tend to send out signals and put out incentives that force some degree of narrowing of vision and expectations. I look at, for example, the single investigative grant paradigm, which has characterized the last 50 years, as creating this entrepreneurial university and destroying the concept of a community of scholars. I mean, in reality, research and teaching are both different variants of something we call learning. But unfortunately, we've created a system that puts very strong incentives out there that break it apart.

Maybe one of the issues that ought to be on the table is whether sponsors such as the federal government should make serious efforts to try and rebuild scholarly communities by shifting away from the traditional single or team grant approach to block grants, like it's done in many other countries. I do think this loss of a scholarly community is one of the great reasons for the problems that we have today.

MALE VOICE: If you're speaking about institutional diversity in the broadest sense, that's happening by itself, and it's healthy and it's terrific. We shouldn't, obviously, focus just on the research universities, except the topic of this conference put us in that pigeon hole.

If what you mean instead is what we were given in the case design, and I quote it here: "A case can be made that in a number of areas, there ought to be more of the decoupling between research and teaching, with research and perhaps advanced graduate education going on in specialized institutions with undergraduate and lower level graduate education, including in the sciences, going on at universities where the faculty may not be doing much research" – I think that's not a good strategy.

I think one of the benefits of the decision that we've been discussing here has been the extension of serious research opportunities to a wide range of undergraduate students whose talent may not have yet been recognized in their careers. We regularly recruit terrific students to the sciences and institutions like that. I'd regret the loss of that kind of opportunity, owing to a kind of premature segregation.

EISENBERGER: Just two quick comments. I really think that the thought of getting some institutional grants to help build community is something that should emerge from this exercise.

The second comment is, all I'm arguing is that we're trying to think about the problem from where we started, trying to make everything fit. It's just a logical construct I'm trying to address. That we're trying to approach the problem as if there's a single way of approaching it, rather than looking at the outcomes. We want to have students emerge who are not going to become academicians with a certain set of skills and abilities. We want different people to go into professions, and we want some people to come back and become faculty members.

And I'm just suggesting, if we ask what each one of those groups needs and then design a system to meet those needs, we might come up with a different answer than starting with a unified system and trying to stick them all into that same system.

MITCHELL: Hi. I'm Tyrone Mitchell. I'm with Corning, Incorporated. I have a very short question, but I have a long comment. (chuckle) I've been with Corning for close to six years now. Had a long career with General Electric. I'm a scientist by training, a Ph.D. in polymer chemistry from RPI.

I spent many years in the trenches working in industry doing product development, at General Electric, specifically, where I hold about 25 patents in various areas of technology. So, I'm really amazed at this conference. This is a very, very good conference. I'm really impressed with the credentials of everyone, all the speakers and the experience, the very broad range of experiences.

But my question is, where is industry's perspective at the conference? Now, I'm very much concerned about that. I work in a group called technology transfer for Corning, Incorporated, where we try to find new technologies for the science and technology organization. And I am the manager of technology assessment. In addition to that, I also monitor our interactions with universities. And one of the things I've tried to do is attend a lot of conferences where there's discussions about university-government-industry interactions, because any future research model has to include those three components. We are all here today because of one reason. That's because the Cold War ended, and the government is trying to figure out what to do with all of the resources it has.

Muddying the picture was the Bayh-Dole Act. We have run into a lot of problems at Corning in trying to work with universities because of that and the need for schools to own the intellectual property. Corning owns a lot of intellectual property, and a lot of these patents really aren't used. I think we need new models for how we're going to do science in the future.

Another thing I'd like to mention is that Corning was involved in a benchmarking that was done by some students from Cornell, where they did a project under a visiting professor who is a former vice president of Corning. That project was to study barriers to government-university-industry interactions. This happened out of the Cornell Business School, and these students in about three months did a very good study, comparable or maybe better than some studies I've seen that were done on the NSF grants that took a couple years.

I think the thing that made it work was the fact that these graduate students were pretty much turned loose on this. It was sponsored by Xerox, and they gave funds to these graduate students to travel all over the country, talk to different universities, different centers of excellence, different industries. And they wrote a tremendous report on some of the barriers to university-government interactions in technology transfer.

So, I think one of the models should include trying to get more use out of graduate students to do some of this policy making or determining exactly what the new policies are going to be. I think that model ought to come out of some of the graduate business schools, schools of economics.

I'll finish up by saying that recently I was part of transferring some new technology into Corning that came out of Los Alamos National Lab through a small company. I have to give a talk on that in a few weeks. One of the papers I came across pointed out that all of the fundamental understanding of that technology was done by two graduate students (one was an American graduate student and one was out of Japan), where the research professors just turned those students loose to go and do this work. They published some critical papers in the area, one in 1971 and the next in 1976. The technology was developed by a person out of Los Alamos. I guess it was around 1982 or '83 that he found these papers and developed this technology, which has started a small company that is doing quite well.

My point is, we really have to come up with some new models. You know, I've heard a lot of history today. I've heard a lot about what the problems are, but I think that there's some deeper thought needed, and we really need some innovation. Thank you very much.

MALE VOICE: Thank you for your comments. I do want to point out that later there will be a session that will be dealing with civilian technology policy. I hope that will address some of your concerns.

BERTZENBERG: Carol Hertzenberg, Argonne National Laboratory. I'd like to speak to a point brought up by Donald Kennedy and I imagine some other members of the audience might want to also. And that is his suggestion that those of us in the older age group should retire and (chuckle) make space for the younger members of our profession to come on board and become active and take over positions. Great idea. Hard to effect.

A couple of points. Those of us who are scientists, most of us are doing it because we love science, irrespective of our condition. Nation of birth, race, age, gender, whatever, we love doing science. We don't want to give it up. There's another point, though, that is a little less obvious, probably to this audience, since I would imagine most people here are well to do. It probably isn't showing up too much in the universities. But it is showing up in other areas of our society. Scientists are no longer enabled to work as autonomously as was the case in the past. And salaries of many scientists are nowhere near what they expected when they entered graduate school. Many of you who have excellent positions at universities and industry and corporations probably don't feel this. But there are a lot of scientists out there who really feel the effects of this proletarianization. They can't afford to retire and let the younger generation in. This is not just a problem of this generation. But the way things are going, this is going to be a continuing problem of future generations. And I think we need to think about that.

MALE VOICE: Thank you very much. I'm not prepared to take names and to urge individuals to make this particular sacrifice. You would have every right to ask me about my own plans, which I will allude to briefly in a moment.

Let me tell you what I think the problem is. First of all, we do need to make arrangements so that academic and other scientists can have the opportunity to be intellectually active in retirement. Part of the problem is that we don't have an adequate set of arrangements for that.

The second problem is called defined contribution retirement programs. It was predicted that when people hit 70, despite the uncapping of mandatory retirement, they would take their pensions and go away. When they sharpened their pencils and started making the calculations, the actuarial advantage under a defined contribution retirement plan like TIAA-CREF (Teacher's Insurance Annuity Association – College Retirement Equities Fund) is overwhelming.

The result is that this past year, of the faculty members at major research universities who hit 70, in my sample over 50% are staying on past 70. That's a consequence of the design of retirement plans. We can't change the personal incentives to stay active. We can change the financial situation they encounter as they hit 70 or whatever.

GEIGER: Roger Geiger, Penn State University. I think that many of the pathologies that the speakers have alluded to are related to a single larger phenomenon. In the last 20 years, the research universities have performed approximately one-half of the nation's total basic research.

And they've done that not by expanding the kind of research that they were doing in the 1960s but by changing their research portfolio considerably. In other words, they've kept pace with the changes in the research frontier and the frontiers of advanced technology.

So my question to the speakers is, do they wish to continue, or do they wish to see universities cede a significant portion of the nation's basic research to other institutions, and probably cede some of their educational mission as well?

MALE VOICE: The comment I would make is one everybody seems to be stepping back away from for one reason or another.

The national labs are clearly struggling to find new missions at great risk as efforts are made to consolidate. I think most statistics governing industrial research labs indicate that the share of their activities devoted to basic research has dropped precipitously over the last ten or 15 years. Universities are one of the few games in town with respect to basic research. And if we pull back away from it, it's probably not going to happen – at least domestically. So in a sense, we do have that mission of great importance to the nation. And I don't think we can turn our back on it at this point.

Whether as we move into the future other kinds of entities will evolve in order to take up more of that, it's hard to say at this point. But I think there has to be a recognition that in the research enterprise in this country, the triad is already changing very, very significantly, making perhaps the role of research universities even more important for the future of the country.

MALE VOICE: I'll try and take a bite of that. I think it's clear that an implication of this question is whether the nature of the shift in the publicly funded research program is to continue towards mission-oriented, applications-oriented, near-commercial kinds of activities, sort of predevelopment but oriented towards that kind of work. And the question you have to ask is, is the university research environment one that is well-suited to that kind of research activity?

If the universities adapt to make themselves suited towards that, will it be possible to preserve the universities as the environment to perform a different kind of research? – more basic, fundamental, less top-down directed, more able to pursue different ideas and programs that arise possibly in an applications process but that lead into new directions and open up new areas.

There is a question as to whether within a given institution you can contain two essentially different and competing ways of organizing the research effort. I'm rather skeptical about that ability within one institution with one set of internal sets of incentives and priorities and research culture, one or another of which tends to dominate. And I don't think that you can maintain them in a healthy balance by doing both proprietary research and open research within the same four walls.

And so the question is, do we want to let go a more commercially-oriented, problem-solving style of research? If that is what the nation wants universities to do, then I think this is a real area where perhaps we need some new specialized institutions separate from universities to do that.

And that the struggle to hold onto that may be more damaging to the universities' other missions than is worth just keeping them afloat.

HUANG: Alice Huang, NYU Several speakers have spoken about the size of the Ph.D. cohort that we ought to train. And I think this deserves some attention. And I would like to know what the panel thinks about limiting the numbers of Ph.D.s

I for one would hate to see that limit come about because I feel that in an open society such as ours, by limiting the numbers of Ph.D.s, we will be limiting the opportunities for those individuals who can aspire to those positions. And also by limiting any profession we fall into a state of protectionism, thereby limiting the competition in that profession.

KEISER: Bonnie Keiser, Rockefeller University. I direct a program for pre-college students and teachers. They come into the graduate research laboratories and perform research. High school students are well capable of cloning genes. They really understand what they're doing.

My question is more of a comment. When we talk about graduate research and teaching I think we all have a shared mental model, an apprenticeship model that is the delight of the world. But when we talk about education, I think many of us are unaware that there are wonderful things happening in education that are coming closer to an apprenticeship model.

And I think we shouldn't be so scared when we talk about research or teaching because there is evidence that teaching is some cases is moving closer to a graduate model. Rensselaer, undergraduate physics, cost-effective total laboratories, City College workshop chemistry, cost-effective higher retention rates. Smaller group sizes, discussion.

SCHWARTZ: I'm David Schwartz, former president of Schwartz Bioresearch and a vice president of Becton Dickinson. I have this question that has nothing to do with what has been said so far, but I think is extremely important.

I recently gave a talk at the New York Academy of Science entitled "Science: From Hero to Villain in One Generation." We need friends. And the place where friends and understanding of science is optimally obtained is in the research university. All the pieces of the future body politic are there in a formative stage.

And I think it's up to the research university to see to it that scientists mix with nonscientists, that the future lawyers and the future economists understand each other. We're not doing that. There's a lot of isolation. It's bad for science. It's bad for the body politic. And I don't know of any other place. And here are two ex-university presidents and a provost, and I can't think of speaking to this issue to any better people. Thank you.

MALE VOICE: We've had an extraordinarily rich presentation, which gives rise to a lot of thought. I just want to bring one thing up. You talk about the number of graduate students and the small number of jobs in science. And my own professor produced 80 Ph.D. s in his active life. But that pathology of the imbalance has existed for a long time in the humanities departments. And the humanities departments have dealt with it, say, in French by letting in lots of graduate students so they can get a lot of teachers, and then having lots of barriers to the Ph.D. so they produce very few Ph.D.s. And they take an awful long time. So that the costs per Ph.D. production in French is greater than the cost of Ph.D. production of a chemist because of the small ratio between the entrance and the exit. The only jobs they're looking for are professors, And I guess the question I would ask is, do you see chemistry departments becoming like French departments in the future?

MALE VOICE: We can kind divide up these very quickly. Let me handle just a couple of comments on the Ph.D. and also the merging of scientists and humanist and social sciences on our campuses. I'll be somewhat more radical and suggest that rather than having birth control or limiting the number of Ph.D.s, I think the Ph.D. needs to be dramatically restructured.

In the COSEPUP (Committee on Science and Public Policy) Report from the National Academies a couple of years ago, there was a preliminary draft that proposed consideration of a two-plus-two-plus-two-to-infinity model. What we call the Ph.D. would be a four-year fixed-term degree: a master's degree of two years, a four-year Ph.D. Probably not a degree appropriate to put people into the academy, at least into the research university.

Last year, of the 3,000 graduates of my liberal arts college at the University of Michigan, over a thousand went on to law school. Thank God, not because they want to practice law, but because they feel they need an advanced degree that gives them breadth and further opportunity.

A Ph.D. could do this quite well. Although it's based upon specialization, it gives individuals the capacity for intellectual adaptation. And if we made that a fixed-time degree, it would handle many of these issues and broaden it out.

Second, I agree completely that we need to recreate the dialogue between science and humanists and others on our campuses. But let me suggest the place to recreate that dialogue is in undergraduate education. The same kind of specialization that we prize so highly right now in our scholarship has led to a compartmentalization in how we approach undergraduate education. It does deep disservice to our undergraduates and breaks faculty apart. If once again we were to create a totality, a certain coherence in undergraduate education involving scientists, humanists, artists, and social scientists side by side, maybe that's the place we'd create the dialogue. Don?

KENNEDY: On Alice's thoughtful observation, enlarge the expectations. And change the style of training rather than limit the numbers. I want to thank Bonnie Keiser for bringing up the interesting subject of inquiry-based instruction and its capacity for remarrying some of the notions we have about research and education even at K-12. It's a terribly important revolution. And it's one that relates, I think, to Dr. Schwartz' thoughtful plea about needing understanding friends. I think there is a way to teach science that engages people much more deeply and systematically with the way science is done, and that's the kind of understanding that we need out there.

MACAGNO: I think that for us to proliferate Ph.D.s poses a lot of problems, because I don't know how we are going to support them. At least at this point in the sciences, we have a certain amount of funding that comes from federal sources, training grants, and so on meant to support the research phase. And with a small additional amount, we can actually support the students fully. But if we were to go into that mode that you're talking about, either we would have to develop other funding methods or the students would have to pay – which is, by the way, what they often do in the humanities and social sciences – and get a tremendous debt that they can't deal with later.

DAVID: I wanted to come back to the sense of loss of community within the university, as driven by specialization and by the external orientation of research faculties. I think this is clearly an important issue. I think it's essential, if it's going to be addressed, that one recognize that although the creation of the single investigative grant and the development of the mobile professor as part of the American university scene could be something laid at the door of the research foundations, it is also the case that university administrations responded to the incentives created by the research system in allowing a reputation-based system to drive the entire process. That a set of incentives was created for faculty to invest in what they did for their invisible college, for the people who reviewed their grants, for the people to whom committees wrote for appraisals for outside review.

I don't think that it's possible in the logic of a collegiate reputational system to substantially do away with that. A question that does exist is whether it is possible for universities to try to internalize by forming either stable, collaborative groups with other universities to have faculty members have the feeling that there is a connection between their reference group and their research groups and the set of institutions whose joint interest can be promoted. If we have shared facilities, if you have more remote access, if you have more interuniversity cooperation, groups of universities can internalize some of this. We have seen this happening in the Research triangle. It can happen in other places. But it requires both an external change and a change in attitudes on the parts of university administrators.

COLE: Thank you. I have been terribly restrained in my own remarks. I would love to say a good deal about some of the observations made by my colleagues and others here.

I was particularly interested in the extent to which there exists public illiteracy about science and technology, and tried to begin to understand – and I underline begin to understand – why that is so. And I began to conduct a very unsystematic study with the help of a colleague of mine, Dr. Eleanor Barber, who is here. I asked leading American historians of the postwar period to tell me what they thought were the best books that had been produced on the subject, the leading textbooks used in colleges and in secondary schools, with the aim of seeing what young extraordinarily intelligent students who are not going into science are learning about the process of discovery and the actual discoveries of American science since Bush's report appeared in 1945.

And we did that study. I was going to report on some of the results. But, as you can imagine, to say there was a paucity of references to not only the achievements of science and technology but to any aspect of it whatsoever in the works of our leading American historians wouldn't be a matter of hyperbole – it would be understatement.

I can tell you there are more references to Madonna than there are to DNA or to Watson. And Crick does not appear in any of these books. The Atomic Energy Commission, the nuclear explosions are referred to on occasion. Occasionally, there's a reference to computers and computer technology. But it is almost totally absent from the books that are read by people who will go on and become members of Congress and who will be the people we'll be asking for support for science leaders in the society outside of the scientific establishment. And they do not get very much of this, as you review the curriculums of not only undergraduate American history but graduate programs in American history.

Part of it is that the people who are teaching these courses, extraordinarily able people, are totally untrained and ignorant themselves of the achievements of science and technology. That is something educationally we can begin to deal with at universities like Columbia and others, and one that I think requires more attention.

I note that the Sloan Foundation has given a very substantial grant to MIT to bring together some quite extraordinary American historians to try to create a history of science in the postwar period – to integrate the story of the growth, the extraordinary expansion of science and technology during this period into the institutional, the political, and the cultural histories of this nation since 1946.

We'll reconvene here in the proverbial ten minutes. Thank you. (applause)

COLE: I'm going to turn the podium over to my friend and colleague Richard Nelson, who will do the introductions for this next session. Why don't we take our seats if we can. Thank you.

Design Area Six: The Organization, Management, and Funding of Federal Science and Technology

David Hart
Michael Crow
John Holmfeld
Alice Huang

Moderator
Richard Nelson

NELSON: The arena under discussion certainly is an important and contentious one. As we all know, the system of governmental research finance in the United States turned out to be a highly decentralized one, with a number of different government agencies looking after their own needs and their own research missions associated with those needs.

Beginning as early as the 1950s, a number of commentators have suggested that that system was extraordinarily messy. And that the possibilities of coordination of various mechanisms were limited and inadequate. Several times over the course of postwar history, proposals have been put forth for the establishment of a broad-gauged department of science and technology or something along those lines. And always, counter-arguments have held the day.

That's the background for what our design presenters are going to be dealing with this early afternoon. David Hart will start. David is now an assistant professor of public policy at the Kennedy School at Harvard. He is just coming out with a book. And I think a tiny bit of advertising is appropriate, since I had the chance to read an early version of it, and it's terrific. His book is called *Forging the Postwar Consensus: The Governance of Technological Innovation in the United States 1921-1953*. It'll be published by the Princeton University Press this coming year, and it's going to be a terrific book.

HART: The U.S. faces a perennial challenge that is growing more acute – how to deploy its limited resources to best achieve the very large goals that we hold as a people. There are a lot of these goals: military security, environmental quality, insurance against infirmity and poverty, and so on. My view is that like money, science and technology should be seen primarily as means to achieve these ends, rather than as ends in themselves. My concern is the confusion between means and ends.

The debate over science and technology policy has begun to resemble too much the debate over fiscal policy. In fiscal policy, the nation has gotten caught up in rhetoric about deficit reduction. This has become an end in itself, and we no longer talk about the deficit as a means to achieve economic growth and stability.

Science and technology policy, too, has been marred by confusion between means and ends. This problem can be seen most clearly in discussions about a Department of Science, but I don't think the confusion has been confined to that proposal. This confusion of means and ends distracts us from grappling with the more important problem of choosing well among means.

That is really what ought to be engaging our attention. I will return to that later, but first, let me begin by discussing the Department of Science and related ideas.

My argument draws upon a debate among the giants in the history of science policy that was carried out on the pages of *Minerva* about 30 years ago.[2] Michael Polanyi and Alvin Weinberg were some of the participants in this debate. This was a time that budgets were growing by something like 15 percent per year. On that note, we have to marvel at their foresight, to foresee this day and age when we would come to what Derek DeSolla Price called the steady state.

In this debate, Stephen Toulmin proposed what he called the chalk and cheese principle. In a well-structured administration, Toulmin argued, decisions have to be taken among commensurable alternatives, comparing in each case chalk with chalk and cheese with cheese. This principle, Toulmin said, holds in the administration of scientific affairs as forcibly as it does in the rest of public service. His point was not that R&D projects should be compared against one another, but rather that they should be compared against other ways of achieving the goals laid down by political authorities. Although both chalk and cheese are solids that crumble differently, one is for writing, the other for eating. The goal of policy analysis – if I can stretch this metaphor – should be the best writing and eating, not optimizing crumbliness. The latter demonstrates confusion of means and ends.

This confusion of means and ends appears on the contemporary scene in a number of different guises. Take the analysis of total federal R&D spending. Perhaps because so many scientists are recipients of federal funds, I think we have grown into the habit of judging the budget in terms of its year-on-year growth. This mode of assessment appropriately prompts a couple of criticisms. Scientists and engineers are perceived as arrogantly assuming an entitlement that the representatives of the people have not voted, or else they are seen as a classic Washington interest group clutching at the federal purse for no other reason than their own material benefit. In either case, the ends of the spending are not specified.

Another way to analyze federal R&D spending is to add it to private R&D spending and then compare the sum – that is, total national R&D spending as a share of our gross national product or gross domestic product – with that of other nations. When the nation falls behind its competitors on this indicator, the federal government is presumed to have some responsibility to make up the difference. Unlike the first approach, this method typically relates total national R&D to some goal: in the past, military security; more recently, economic growth.

But even though a national goal is specified, I would argue that this approach of taking R&D as a percent of GDP still violates the chalk and cheese principle. If the national goal is economic growth, R&D spending ought to be compared against other policies that might achieve that end, like deficit reduction or demand expansion, rather than comparing it with the fraction of R&D as a percent of GDP spent by other countries. The question is, to maximize economic growth, would the marginal increment of federal spending best be spent on R&D as opposed to other

[2] For a sample of this discussion, see Michael Polanyi, "The Republic of Science," Minerva 1:54-73 (1962); Stephen Toulmin, "The Complexity of Scientific Choice: A Stocktaking," Minerva 2:343-359 (1964); and Alvin M. Weinberg, "Criteria for Scientific Choice II: The Two Cultures," Minerva 3:3-14 (1964).

ways of spending, or not spending it at all? I admit this is a difficult calculation to make, but I think it the way we ought to pose the problem.

The Department of Science concept is equally confused, in my view. The idea of a central institution to manage the nation's science and technology has been traced back to the Constitutional convention by Hunter Dupree. The idea for a Department of Science has been offered up more than a hundred times just since Vannevar Bush, although Bush didn't make exactly the same proposal.

The latest of these proposals was put forward by Representative Robert Walker when he was chair of the House Science Committee. The proposal excluded the bulk of R&D funding, that of the Department of Defense (DOD) and the National Institutes of Health (NIH), but it did include such disparate elements as parts of the Department of Energy (DOE), the Department of Commerce (DOC), the National Aeronautics and Space Administration (NASA), the National Science Foundation (NSF), the Environmental Protection Agency (EPA), and the U.S. Geological Survey (USGS).

Walker argued that the main mission of these entities is the promotion of science for its own sake. Of course, a brief look at their authorizing legislation, with the exception of NSF partially, shows this is not the case. NASA is supposed to explore space, EPA to protect the environment, and so on. It seems to me that unless Congress has accepted science as an end in itself to a much greater extent than it has, Walker's Department of Science would be little more than a holding company and a juicy target for budget cutters.

The travails of the National Endowments of the Arts and Humanities in recent years suggests that the cultural argument – this is the label that Alvin Weinberg applied to the argument of science for its own sake back in 1964 – is no more politically persuasive now than it was in the past, and perhaps less so.

The last example that I offer as the confusion of means and ends in the contemporary debate is *Allocating Federal Funds for Science and Technology*, a report of the National Academy of Sciences that was chaired by Frank Press (Press 1995). The Press report's central recommendations include the establishment of a federal science and technology budget, as well as executive and legislative institutions to manage it. The main goal of this budget is to assure U.S. world leadership in science and technology. To this end, the budget provides a mechanism to trade off R&D projects across agencies. The Press report's vision is in some ways more ambitious than the Department of Science, since its federal science and technology budget includes NIH, as well as about eight billion dollars of DOD.

It is also more contemptuous of the chalk and cheese principle. The Press report's budget process would deliberately force chalk versus cheese choices while making chalk versus chalk and cheese versus cheese choices harder. For instance, an EPA research program on the diffusion of effluents in ground water would have to compete not only with EPA enforcement spending, as it normally would in the budget process as it is now constituted, but also with hydrologic programs in other agencies such as NSF. The criteria that the Press report endorses for making these kinds of comparisons – that is, between the two research programs – include not only the program's contributions to the missions of these agencies, such as safer drinking water or knowledge of hydrology, but also the processes and instruments by which these funds are dispersed. As I understand it – the criteria aren't exactly transparent in their application – the budget-makers could cut EPA's research funding in favor of NSF in this area with little regard for the EPA's larger program if they determine that EPA failed to consult adequately with the external scientific community, which is one of their procedural criteria.

This is not the proper way to go about these things. The proper way is to apply the chalk and cheese principle. It begins with the specification of federal missions by the President and Congress, the setting of priorities among them, and the establishment of budgets.

Working within these budgets, the agencies determine the appropriate level of investment in science and technology for achieving their missions compared to other kinds of spending, such as direct services, procurement of more conventional goods, and so on. This is essentially the system that we have. It is a system that has evolved some instruments, like the Federal Coordination Committee on Science and Technology (FCCST) cross-cuts under the Bush Administration, and the National Science and Technology Council (NSTC) working groups under the Clinton Administration that help deal with the duplication that might arise in such a system as well as facilitate interagency programs and deal with international joint ventures, which are becoming more important.

It is not a perfect system, and it has particular failures, most notably the jurisdictions of certain appropriations subcommittees, which can force perverse tradeoffs. Nonetheless, the fundamental design is sound. We ought to continue to work for incremental improvements and select out those experiments like the cross-cuts that adapt the system well to new circumstances, rather than pressing for the kind of wholesale change that Representative Walker or the Press report's proposals would.

In our decentralized system, a major challenge is to get agencies and their political masters to take a long-term view of the mission: when and how it might be achieved, so that, on the margin, R&D spending might be more favored than it is now. In other words, those who believe that science and technology provide powerful means to give the public what it wants must make the case in those terms.

The supporters of biomedical research have done this extremely well, as the NIH budget curve shows. The cold war Defense Department is another example of successful advocacy of mission-oriented R&D, for better or worse.

It isn't always an easy case to make, since the time horizon of most politicians extends only to the next election. It invites the application of dangerously rigid standards of evaluation, even ridicule. Senator Proxmire used to hand out the Golden Fleece award for projects he deemed especially unworthy of federal funding.

Some of our efforts have to be devoted to ensuring appropriate efforts to measure the contribution of science and technology to agency missions. One argument we might make is that these kinds of evaluative measures should be applied to whole programs rather than individual projects, since we do not know what the outcomes of projects will be in advance. And we might argue that such programs ought to be evaluated in qualitative terms. Perhaps we should also engage the users and beneficiaries of these programs in these evaluations, not merely peer reviewers.

But however difficult these metrics are to devise, and however disadvantaged long-term thinking might be in our political system, these are the terms in which the case ought to be made. We should not exaggerate the difficulty, because, as Senator Domenici has documented, R&D budgets have done pretty well in recent years. Most of the pain is still prospective from the point of view of aggregate R&D spending, though that doesn't always translate down to the individual level of scientists.

What does it take to make this kind of case? It begins with a community with a deep commitment to its cause, that can be mobilized in its support. I think the scientific and technical community has this commitment, although many in it may lack the time for a lobby day in Washington. Scientists, engineers, and science enthusiasts tend to be reasonably wealthy and sophisticated, and they tend to be widely distributed geographically. These are all highly-prized attributes from the point of view of mobilizing a political constituency.

Second, and perhaps most important, the political leadership of the community has got to know how the budget process works, and have a sense of the tactics and timing appropriate to each stage in that process. It must also possess the organizational capacity to carry out these tactics – that is, to turn out its supporters when they are needed.

Finally, the case for mission-oriented science and technology can draw on a deep sense of faith among the public that these investments in science and technology will pay off. I think the fear of an anti-science trend has been greatly exaggerated. In fact, if anything, that audience is too gullible when it wants to believe that something is possible, like the Strategic Defense Initiative.

That's not to say that the nation has been sold a bill of goods by scientists and engineers. Even in the case of biomedical research, according to the NSF, the total public and private spending on biomedical research in 1994 was $33 billion dollars. That is a lot of money, but remember that the total enterprise is a trillion dollars. About 3.3 percent of this doesn't sound bad to me.

Rather, my point is simply to remind the lobbyists, if I can call the scientific and technical community that, to try to keep expectations reasonable. Convey the promise not of spectacular leaps forward but of broadly diffused pay-offs. I do not believe science and technology can solve every problem, no matter how well funded it is.

A second challenge in a decentralized system of mission-oriented R&D is to achieve an adequate balance between dedicated expertise and flexibility. For missions that are deemed very important and long-lasting, there is no substitute for specialized institutions that cultivate unique knowledge and capabilities. It is impossible to imagine the post-WWII rate of progress in weapons technology without the national laboratories. It is equally impossible to imagine the rate of progress in medical technology without the academic medical centers.

Unfortunately, when public priorities change or when the mission is achieved, like winning the Cold War, these institutions become a burden. The benefit of specialization becomes the burden of rigidity. I think the people in these places can be reoriented, and perhaps some of the equipment as well, but I do not think the institutions and culture that they nurture can be. Rather than try to save them, the proper policy is to reduce or close them in accordance with the new level of mission need, and to facilitate the reemployment of those resources elsewhere on other missions.

If the communities where these facilities are located are mobilized as I have described, closing the facilities can be a pretty difficult job, as we have seen in New Mexico. In these instances, I think it is incumbent upon the S&T community to break ranks, rather than to circle the wagons.

The Press report does a good job of this. It calls for reductions, for instance, in the DOE labs. What it does not do is provide enough of a rationale to articulate new missions to which those resources might be better put in the future. This is especially true for transferring resources to the universities, which I take to be one of the main objectives of the Press report. Its main argument for funding academic scientists is that they are flexible. This calls to mind the Bush report's metaphor of a reservoir of knowledge that can be put to use as new needs emerge. But flexibility is not a mission.

The Press report likewise under-emphasizes the role of academic scientists in education. This mission was fully articulated in the Steelman report (Steelman 1947), which has tended to take second seat to the Bush report in our histories. It's a mission that resonates with the nation right now. The proper role of the federal government in education is far from settled, but that is all the more reason for the community to be mobilized and to advocate on this point.

The advocacy of education for its own sake comes dangerously close to what I referred to before as the cultural argument. And while I think that that argument has limited appeal – although it may appeal very much to those of us who are academics and would like our students to become broad-minded human beings for their own good – it should not be abandoned. There actually is a reasonable amount of public support for areas of research that don't necessarily have a mission application, such astronomy and cosmology. But I do not think that we should make too much of the federal role on that cultural argument.

I want to offer an argument that I think has broader appeal, and that is to link education with the economic needs of the next century. It may be conventional wisdom that the economy is based increasingly on technology and innovation, and therefore requires an increasingly skilled and creative workforce. However, the nation has not done very much to act on that conventional wisdom.

Adopting this kind of argument has serious implications for science policy, and we ought to recognize that. It means that the expected future demands of the job market, rather than the opportunities perceived by academic researchers for science, ought to be the major criterion for allocating funds. It means that teaching ought to be accorded more emphasis and respect.

We can hope that these things will line up – that is, scientific opportunities and job opportunities and teaching excellence and research excellence – but they may not, and all too often in the past, they haven't.

An argument that connects research funding with education for the sake of economic growth – i.e., an economic management mission of the federal government – creates political opportunities for the science and technology community. The Clinton Administration entered with plans to make an array of investments that included R&D but extended also to infrastructure, education, and other sorts of programs. Much of this was abandoned in the name of deficit reduction, and perhaps appropriately so, if I can refer back to the chalk and cheese principle. But in the long run, I think that macroeconomic management is going to be an inadequate substitute for the provision of public goods that make markets work. These kinds of goods, like research and education, are complimentary.

Science for science's sake can be achieved with R&D funding alone. Science for the economy's sake will not pay off without other investments besides R&D. In this respect, the scientific community might be able to join a coalition with labor and business organizations that believe in making these kind of investments for the sake of the economy. We have to remember that there are going to be enemies made along the way, and the process may divide the community. But nonetheless, I think that it is a plausible rationale.

The chalk and cheese principle is not easily applied in the U.S. Our political system is prone to overlapping jurisdictions and turf wars. I submit, however, that this ideal is a more sensible guide for efficiently carrying out the will of the people than simply maximizing federal R&D spending or ensuring that federal R&D spending is done in accordance with the wishes of the scientific establishment.

And it is carrying out the will of the people and participating in the formation and refinement of that will that ought to be the object of the science and technology policy community.

NELSON: Thank you very much, David. Our next presenter is Michael Crow, who has been for the last several years vice provost as well as professor of science and technology policy here at Columbia University. As all of you know, Michael has from the beginning been a major force in the design of this conference. And it seems to me quite fair that someone who has designed the conference has the chance to make a presentation at it. (laughter)

CROW: I'm going to present recommendations for specific changes in the original design that Vannevar Bush gave us 50 years ago in *Science: The Endless Frontier*. This may put me a long distance out on a limb, and I do it with some trepidation, realizing that those who have done this before who are not eminent Nobelists usually have been butchered shortly afterwards.

My premise is that Vannevar Bush's design is not flawed in any serious way. Rather, it is so seriously outdated that it appears completely flawed. To update Bush's design, I have approached Vannevar Bush as if he were a software engineer who laid out the program for the conduct of science in the United States some 60 years ago. I tried to consider it from the perspective of what the design principles were that Bush put into his software code.

There were seven such principles. One was political autonomy. Bush's design parameters separated the scientific enterprise as much as possible from political processes. In practice, there are varying degrees of separation, but autonomy was one of the design parameters.

A second design principle was self-regulation by scientists. Scientists, like the Marine Corps and major league baseball, and any elite group for that matter, were supposed to discipline themselves, set up mechanisms to control their culture, and so forth.

The third of Bush's design principles was a focus on science for science's sake as well as for problem-solving. This principle has been distorted by many people who think that Bush's principal design parameter was science for science's sake. These critics are wrong. Science was both for fundamental discovery and for specific problem-solving.

Fourth, because of both his background as a professor at MIT and his time as dean as well as president of the Carnegie Institution, all of Bush's design parameters are built around a strong academic model of individual achievement. The focus is on the individual – both the individual discipline and individual scientists.

In his last three design parameters, Bush called for scientists to be accountable for achieving national security from an economic, military, and health perspective. Rather than specific accountability, project by project, discipline by discipline, or field by field, he called for scientists and their outputs to be measured in terms of general accountability. Success was to be determined by national achievement.

Sixth, Bush called for a national science organization. That is, he proposed concentrating basic research in a single area. He didn't call it the National Science Foundation. He and his panel had other names for it, but it was to be a single, major, basic research agency.

Lastly, he called for amazingly small budgets. I'm not sure if this was a political calculation on his part and those that were working with him, but the budgets that he called for were very small.

I've taken each of these seven design parameters and, thinking like a software engineer, I have looked at each of them from the perspective of how it could be improved, enhanced, or in some way made better.

Design parameter #1: political autonomy. It may sound like a strange response to this recommendation, but we should establish an institutional mechanism for forecasting our long-term national science and technology needs. This should be a rigorous, ongoing, continuous process that fills a current void.

One of the reasons that political autonomy isn't working for the scientific community is because nobody in the general population knows where they're going or why they're going there. And if they get there, how or when they got there. That is why we need a process that would generate a science and technology roadmap so that everyone can see where scientists are headed and why, and what that means in terms of implementation.

I am not suggesting that we replace the Office of Technology Assessment, which had its own problems. Rather, Congress should establish a means by which a national science and technology roadmap can be developed. A good example of this process has been carried out the last few years at the National Institute for Future Technologies in Japan, which conducts an exercise to plot the direction of national movement.

Second, the Office of Science and Technology Policy, regardless of the administration in power, must look to this roadmap and either follow it or explain why they're not following it. If done well, mission agencies can and should build their agendas around it. I know this sounds a little bit foreign, but I am looking for something concrete that people can think about.

Design parameter #2: self-regulation by scientists. On this parameter, I have three specific recommendations. One, spend a measurable percentage of all national science assets on educating the public about science and research. We are doing only half of this now. We're trying to educate about science, but we are failing to educate about research.

Second, and this is very controversial, develop a science court for internal discipline and conflict resolution. Bush made no account for this. The numbers of conflicts, questions, and debates, are only going to increase in the years ahead. If we do not develop some type of a mechanism, we will not be able to deal with the political backlash that will occur because we don't have the kind of checks and balances in the system that one would think we ought to have.

My third recommendation is to broaden the criteria for peer review to include the potential for considering broader social profit. Social profit is a poorly-defined term, but suffice it to say that it's an amalgamation of all those things not related to science. If peer review processes on a project and program level do not find a way to begin to include social profit as part of the decision-making process, the notion of self-regulation by scientists will have to be significantly modified at some point. It's under attack right now.

Design parameter #3: science for science's sake as well as for problem-solving. I think one of the barriers that we have to this is incessant fighting, discussing, and arguing over the definition of basic and applied research. The National Science Foundation is a basic research agency. The Environmental Protection Agency is not. We ought to do basic research here and not there. It's the old adage that my work is basic, and so therefore I can't explain it; and you just ought to fund it, because you're too uninformaed to understand it anyway.

We are going to have to define these terms once and for all, and there has been a major attempt to do this in the Press report (*Allocating Federal Funds for Science and Technology* Press, 1995). Second, we need to evaluate projects with regard to their purpose, realizing that the type of research – basic, applied or what-have-you – relates to the function of the mission agencies. I suggest that all government agencies have the possibility of doing basic research, applied research, and technology development in support of their missions. This is something that should be better understood and better organized. That is, we should bring discipline to an undisciplined process. Lastly, consider all projects and program areas as equal, regardless of their scientific focus or technical objectives.

One of the ideas that permeates the American university setting is that if you go through a Ph.D. program and you're then hired by another academic institution, that's great. If you get a job in industry, that's good. If you get a job somewhere other than those two places, that's not so good. There is a hierarchy in which basic research is the highest order function and all other functions are somehow lesser. I suggest that we find a mechanism wherein all research, all projects, are equal. This goes back to Bush's design.

Design parameter #4: a strong academic model of individual achievement. This parameter has led to a number of problems: barriers between disciplines, difficulty moving in new directions – a whole range of things.

We should develop new, team-funding mechanisms and expand the recognition mechanisms for team participation. We don't have that in the national labs. We have that in industrial labs, but not in academia. We should work toward the evaluation of scientists by discipline and by group. For example, what is the field of chemistry contributing, and to whom?

There is another consideration that goes beyond individuals and individual departments. These are what I call star groups, groups that have the capacity to make significant achievements. We need to find a mechanism beyond the individual model of trying to disperse resources to a large number of people in equal amounts. We should find a way to provide significant funding to these star groups.

Design parameter #5: general accountability. I think there should be a significant evaluation of agency research programs based on their success or failure to attain particular pre-defined goals or objectives.

If we know why we're moving in a particular direction, people should have some understanding of our logic. They will be able to see how or if an agency's programs contribute to moving toward a defined goal or objective. This may sound a lot like central planning, but it's not. I do not aim to differentiate projects based on an artificial modality. I'm talking about a way to determine, down to each and every individual project, the ability of a project to make progress towards a pre-determined goal or objective beyond merely the scientific goal or objective.

Looking at general accountability, this means that the White House Office of Science and Technology Policy and not the Congress – which would probably do this separately – would have a map. They would establish annual, five-year, and ten-year objectives for national science and technology investment. We don't do that now; we just talk about it. We put together the Council on Science and Technology, which has not been that effective.

We have to drive the process by the precursor step, which is constructing the scientific and technological map by asking where the science might take us. Then, following that mapping activity, decide upon a strategy or plan. Instead, what we do now is spend about 90 percent of our science budget on implementation and ten percent on planning, thinking, strategizing, and so forth.

What does this mean in a research agency? It means that U.S. government research agencies that are funding research projects to industry, academia, or laboratories and that don't have an elaborate mechanism for evaluating the progress of their research programs according to a national strategy and national R&D map are wasting money, since they don't have the means to evaluate whether or not they're making systematic progress.

They certainly can know whether scientists have won the Nobel Prize, but otherwise, progress is difficult to determine. We do not have sufficient or appropriate measurement tools today, but we need them. Developing tools of assessment is going to require some new mechanisms, some new thinking, and some new cooperation between social scientists and others that have the capacity to interact with scientists.

Design parameter #6: a single basic research agency. This is a bad idea because there are basic research questions that are linked to all of the agencies' missions. What you can have is a single basic research agency like the National Science Foundation which has a specific role. This agency is in charge of building the foundation, knowledge, and research tools to support the research activities of the other mission agencies of the government.

What does that mean? At a research agency, it means rethinking budget and planning models to define their roles as producers of foundation knowledge, basic knowledge, or specific solutions to problems. Some agencies are working on specific solutions to problems. One of the agencies might be working on foundation knowledge. Those planning and budgeting processes need to be linked together.

Design parameter #7: limited resources. Bush emphasized both in the words and their undercurrent, and in the class of individuals he had participating in the process to build *Science: the Endless Frontier* as a report, that limited resources should only be allocated to the best science.

It has been argued that one of the reasons to spend resources at as many institutions as possible is to enable a bell curve distribution of scientists, such that somewhere in the middle or on the right side of the curve, someone is going to be very successful. And therefore you need to have as many participants as possible.

I don't think that is a logical argument to sell to the public. Instead, one has to argue for two things: first, concentrate resources in the fields of greatest importance, linked specifically to their individual mission. Second, and perhaps controversially, dramatically increase the size of average grants – more funding for fewer groups – making the competition even more intense, in order to separate groups that have the capacity to compete on a world class basis from those that do not.

Clearly, we have moved beyond the parameters of Vannevar Bush's science policy design. The complexity of interactions in today's arena calls for equal complexity in the design of our policy apparatus, analysis, and planning. I have suggested science and technology roadmaps to address the outdated notion of political autonomy. Pubic education, science courts, and peer review reform will help to modify scientists' self-regulation. Looking more closely at the purpose of research and developing tools of assessment will increase accountability. We need to increasingly work towards linking scientific research to societal outcomes and Vannevar Bush's design does not facilitate this goal.

NELSON: Thank you very much, Michael. Our first discussion panelist is John Holmfeld, who recently has been senior advisor for science policy at the Washington office of The Dana Alliance for Brain Initiatives. For many years prior to that, John was a professional staff member of the House Committee on Science, Space and Technology. So, John more than just about anybody else I know is in a position to respond to the question: How will proposals like that play out in Washington? (laughter)

HOLMFELD: These two papers do what I had hoped all of them would do at this conference. They're radical. They don't accept the status quo, and they ask us to think about some of the underlying assumptions that our science policy has been based on for these last 50 years. And I welcome that.

I think some of the themes that they deal with are ones on which I find myself thoroughly in agreement. They raise the question of the governance of science and the relationship of science to the political community. They raise this question of autonomy. They raise this question about whether or not the policy should be based on the relationship between means and ends. I think that's something we haven't done enough of. And they raise, both of them, the issue of the extent to which we can measure and quantify the benefits that this massive investment yields for society.

So, I find them very useful. I want to go on from that and make some comments on the rationale that has been the basis for this enormous build-up of the federal participation and the question about whether that should be re-structured, as Mike has suggested.

It is my observation over some years that federal officials and scientists who came to Washington and testified for a Congressional committee have presented a sort of three-pronged rationale for that build-up.

First is the technology rationale. Scientific research will produce a payoff in the form of technology, cures for diseases, new processes, whatever. Second is the education rationale. The funding of scientific research will also contribute to the education of the next generation of scientists. Third is the cultural rationale, the prestige rationale.

I would like to make some comments on the last two and then go into the Bush rationale in some detail. But before I do, I think it's important to indicate the extent to which the environment has changed, certainly in Washington, and I think to a certain extent in society as a whole. Not only as a result of the change in the funding situation, but also in the attitude towards science.

There's a story going around Washington – I have it probably third-hand and I can't verify it exactly – but it's illustrative of that change in attitude. You recall that this summer a move was made in the House Science Committee to reduce the role of the social sciences at the National Science Foundation, to eliminate the separate directorate for the social sciences.

And a number of scientific organizations wrote to each member of the committee, emphasizing the importance of the social sciences and saying that this move should not be pursued – including the AAAS, the representative of all of science in this country. And Congressman Frank J. Sensenbrenner, Jr. (R-WI) wrote back to the AAAS and said, "If the social sciences are so important, why is it that you hardly ever print in your journal a social science article?"

Now, that is mildly amusing at one level, but at another level, I think it is indicative of the attitudes of both parties to that exchange. On the part of the congressman and I think many of his colleagues, it indicates this change in attitude of questioning the statements coming out of the science community. They no longer are willing to simply say, "Oh, Mr. Nobel Laureate, Oh, Mr. AAAS, if you say so, it must be true."

And on the part of the scientific community, I think it represents a lag in being prepared to accept that. I think the scientific community has not yet reached the stage where they're much more careful in analyzing what they say and substantiating the kind of claims that are made. So let me go into a discussion of the three-part rationale.

The cultural or prestige rationale I think is real. It is with us. It's been discussed on several occasions. And the only point that I would emphasize in this is that it in no way can justify the level of research. It can justify a level of research funding by the federal government probably on the same order as the humanities endowment or the arts endowment. And we're talking about $200-$300 million a year, nowhere near the $15 billion in basic research.

The first thing I think that's worth saying about the education rationale is that I find myself in the uncomfortable position of having to disagree entirely with Dr. Cole's observation earlier today, that that rationale appeared in the Bush report. I have not found it nor have I found it in the Moe Report.

You recall that Bush commissioned three studies in preparation for writing his own report. And the Moe Report was the science education report. Both that report and what Bush had to say about science education was focused on replacing the generation of scientists that had been lost during World War II when very few Ph.D.s were graduated and on the issue of allowing more undergraduate students to enter college from families that could not afford it.

There is a one-sentence mention of the relationship between research and education in one of the two other consultative reports. But it is significant I think that Bush elected not to lift that out and incorporate it in his own report. The justification of saying research is good because it contributes to education in my estimation emerged only in the post-Sputnik era.

And it became prominent, because what we were trying to do in the post-Sputnik era was to increase not only science, but also scientists, the number of scientists. One of the things that we are facing today is that we don't hear very much about that. There are serious problems with that rationale. There's this intense debate on the oversupply of Ph.D.s in the sciences. And there's beyond that the whole question of the role that scientific research and the whole grant system is playing within the university structure.

Is the grant system and the associated reward system that encourages and provides incentive for working scientists at the university not to do undergraduate teaching? We all recall President Don Kennedy's call to his faculty to redress that balance, and it's proving to be very difficult to deal with.

But we are not hearing about that rationale. And we could probably spend two days in conference discussing that, because it needs careful, detailed analysis. So, let me go onto the Bush rationale itself – the rationale that science should be supported because it leads to technology.

What Bush essentially was saying – and I apologize to those who resent the discussion of historical developments, but I think it will be relevant to my recommendation towards the end – is that the findings of science go into a large reservoir, and technologists come along and open the tap on the bottom of the reservoir and pick out those science findings that they find can be used in the development of technology that they're interested in.

Now the federal government should pour lots of research findings into that reservoir, but with three qualifiers. The first one is that you cannot predict which of those research findings that you develop with federal funds will in fact produce a payoff. Some of them will, and some of them won't. The second qualifier is that for those that do produce a payoff, you cannot predict when. Some, it may come next year; some of them, it may come 30 years from now. And the third qualifier is that research in one field may produce a payoff in a totally different field.

That is a rather sophisticated rationale. And it's interesting that the Congress and the public accepted that and it served as a rationale for many years and still really does. What I would argue is that we came out of World War II with a second rationale, based on the observation of what happened in World War II.

In World War II, we gathered together lots of scientists, mainly physicists in five laboratories, and they produced technology. They produced radar at Harvard. They produced the atomic bomb, as we all know, at various laboratories. And they produced the proximity fuse and a number of medical innovations.

And the support that we saw in the postwar period was based on that model, namely established agencies or parts of agencies which like our war-time agencies would dip into the reservoir and pick out those research findings that would help us solve problems. And I think there are a number of models there. The Atomic Energy Commission was to do that. They were to develop civilian nuclear power. They were to develop nuclear medicine and a number of other things and perhaps do a small amount of basic research.

The big example in this field is the NIH. The postwar period is full of examples of additional institutes being established at NIH: National Eye Institute, National Dental Institute (now National Institute of Dental and Craniofacial Research), et cetera, which the Congress expected would similarly do research that would lead to solutions of these diseases or the prevention of them.

And that is still very much with us. We still see initiatives in the Congress based on the notion that you would establish groups or agencies, especially in the Defense Department, which would take the findings of basic research and translate them into technological application.

What has happened in the postwar period is that a number of these agencies have gradually – for reasons that are complex, I suppose – increased substantially their support for basic research. And I'd like to give a couple of examples of where we are today in the NIH because there has almost been a flip-flop there.

The NIH does a certain amount of clinical research, but the leadership of NIH and of many of the institutes are committed to molecular biology. Last fall, a House subcommittee held hearings on Parkinson's disease. The chairman was interested in Parkinson's disease, and he called a number of witnesses to discuss it.

The director of the Neurology Institute towards the end of the hearing was asked, "How much money do you really spend on Parkinson's?" And he said, "We don't do research on Parkinson's. We do basic biological research on how the brain and the spinal cord work. And we expect that some of those findings will, in fact, produce a solution to Parkinson's. We don't know when."

Another example is what happened when Christopher Reeve appeared before the convention in Chicago, just a few weeks ago. You recall, Mr. Reeve pleaded for support of spinal cord research. A few days after that, the NIH director felt called upon to issue a long statement, three or four pages, in which he said, we should all realize that we don't do research on spinal cord. We do basic biological research. Some of the research may be labeled Parkinson's, but it'll probably pay off in the field of spinal cord. And some of our research that's labeled spinal cord will probably pay off in Alzheimer's, etcetera, etcetera.

So, we have a situation where the expectation of the political system that research will be in fact targeted is being responded to by the scientific community and the federal administrators, largely in this case made up of scientists following the Bush model. And I think in the long run questions are going to be asked about that. That is where this question of quantifying the output will come in, and I think increasingly the younger members of Congress will ask questions about that.

You recall very recently, in the last Congress, the same kind of question was applied to the National Science Foundation by Senator Barbara A. Mikulski (D-MD) when she chaired the NSF Appropriations Subcommittee, and she directed NSF to do (quote) "strategic research." She was not satisfied that $3 billion worth of Bush-type rationale research – that is, totally untargeted – was really the thing to do. She has actually said that a large fraction of NSF funding should be targeted. And so I think that's what we're up against.

In some agencies, that transition has taken place in a formal way. I can remember in the late '70s being on loan from the science committee staff to the agriculture committee staff at the initiative of Congressman Raymond H. Thornton, Jr. (D-AR), who served on both of those committees, to write the section of the 1977 Farm Bill that established the competitive grant system at the

Department of Agriculture. But in most of the other agencies, it has been a transition that has not been formalized.

Let me point out an interesting development in this field. There have been a few examples quite recently where compromises have been arrived at between the targeted research approach and the Bush untargeted approach. The prominent one and the one we've all read about is AIDS research. When AIDS achieved a certain visibility in the '80s, the AIDS community, following the pattern of the postwar world, argued strenuously that the NIH should have a new institute on AIDS research. That's the way you dealt with this kind of thing.

And the leadership of NIH and many in the scientific community argued strenuously against that. We don't want another institute. We have had enough institutes established in our organization to do targeted research. And the compromise was to establish in the office of the director an office of AIDS research, which would be funded but which would provide its funding to each one of the individual institutes within NIH, which would in turn give out the grants.

Now, that compromise is still being fought over. Within the last few months, the House Appropriations Committee has declined to fund the Office of AIDS Research. They say it's a wonderful office. They should coordinate things, but they should not be funded. We'll give the money directly to the institutes. The Senate Appropriations Committee provides funding separately for the Office of AIDS Research. And so in the Congress, that debate is still going on.

Well, let me conclude with another anecdote. And it comes from the memoirs of Vannevar Bush. In 1944, the proximity fuse was a success. American forces were on the ground in Europe, and the military became concerned that if a proximity fuse was fired and was a dud and landed on enemy territory, they might copy it and begin to shoot down our airplanes.

So the joint chiefs issued an order saying the proximity fuse would only be used over open water where duds could not be recovered. Well, Bush's experts told him that it would take at least two years, if the enemy found a dud, to copy it and get it into mass production. So Bush went to see Admiral King, an admiral of the fleet and a member of the Joint Chiefs.

And King, who was sort of a crusty character, opened the meeting and said, "I have agreed to meet with you, but this is a military question. And it must be decided on a military basis to which you can hardly contribute."

And Bush, who was a sort of a crusty New England Yankee, himself came back and said, "It is a combined military and technical question. And on the latter, you are a babe in arms and not entitled to an opinion." (laughter)

And Bush concluded, "It was a good start. And the discussion went on from there and went well." (laughter)

The lesson I would draw from that little story is that we should still take Bush's advice, but that the roles today have been reversed. Today, it is the public and the political community coming to science, and it is science that too often says, this is a scientific issue that only scientists should be entitled to decide.

I think the time has come to pay serious attention to the fact that this is a joint decision, that we should be willing to pay more attention and be more open-minded, including about some of the very controversial issues of geographic distribution, ear-marking, and all these other issues where the scientific community has been very rigid and unwilling to compromise.

My final remark is a comparison between the scientific community and the military. You all probably recall the enormous prestige the military was held in during the immediate postwar years. The GIs came back from the war and were given the GI Bill. Dwight Eisenhower was made President, and the military was held in the highest possible esteem.

And then over the years, contractual problems, the divisiveness of the Vietnam War, et cetera, et cetera, a long series of things happened – the military is still held in high regard, but people have in the back of their minds reservations about it. They're always asking questions: What's going on here? I am concerned that the scientific community will similarly go through a long series of missteps of that same kind and begin to decline in the perception of the public.

And to conclude, there is another reservoir that is still with us in addition to the Bush reservoir of research funding, and that is the enormous reservoir of public good will towards science. And we should be very much aware that we should not do things to reduce that. Thank you. (applause)

NELSON: Thank you, John. Our second discussant is Dr. Alice Huang who is a biologist by training and now is dean of sciences at New York University.

HUANG: Let me begin by thanking the organizers for getting all of us together to focus our attention on the important issues facing science in the United States today.

Now, of course, after Michael's talk, I'm ready to move in for the kill, but instead I'd rather join him. I'm going to make some proposals and give you some opinions, hang them all out there, and see whether they can be shot down or not.

I am going to take very much the roll of a discussant, from the point of view of someone who is in the trenches, who doesn't administer science on a daily basis. And I have focused on four points. One, I would like to put an end to the discussion of a central Department of Science.

I would like to suggest some mid-course corrections that scientists themselves can make. I would like to issue a warning about our loss of autonomy and how we can get it back. And I would like to give you a chance to vote, all of you, so you can participate on what we can use as a justification to support science.

I believe that, since the end of the Cold War, we as a group have been very often rudderless, without the strong captains that we had looked to in the past. People like Bush, Morgan, and Shannon. We have not always spoken with one voice, and we have become quite defensive.

The idea of organizing science funding under one Department of Science is to me less in vogue today, although it continues to be brought up. The driving force for that suggestion was due to the frustration of not having a Presidential science advisor for some time under Reagan and the hope that science would have a national voice of a cabinet member. We have all seen the central planning and the failure that has occurred in the old Soviet Union. And we are now seeing problems of central planning in both The People's Republic of China and The Republic of China.

I don't think that is the route we ought to go. Michael's suggestion of a science analysis office for the Congress is an important one, and I support that, to help guide decision-making processes and forecasts. Better yet, for our excess number of Ph.D.s, we should broaden their training, so that we can prepare all of them for public service as congressmen. (laughter)

Also, defining scientific missions and drawing better boundaries between federal agencies, getting rid of redundant support and organizing under one monolithic Department of Science is not a good idea. Boundaries as we have heard are not easily drawn in science these days, as we become more dependent on interfaces between scientific disciplines to solve our many problems.

We are a big enough nation and we are a rich enough nation that some overlap is probably to the good. I am reminded of a young scientist, an assistant professor, who was refused funding by both the NIH and NSF, to later get funding from the American Cancer Society. His work subsequently led to the important isolation of oncogenes and to the understanding of their regulatory mechanisms, without which the basis of any cures for cancer would be impossible.

Therefore, this kind of plurality serves science well, because no one of us is so infallible in predicting what will or will not be successful science. I cannot accept the elitist statement of funding only the best science. The corollary to that, if we really think about it, is to forget about all the others, creating much more of a have versus a have-not society. We have seen too many institutions get rich and bloated and arrogant, producing less and less value.

Flexibility in funding choices without neglecting to pull up the institutions at the bottom of the heap occasionally will help us to maintain competition among research universities and institutions. In addition, by concentrating research in only a few top institutions, how can we guarantee that the finest of our human resources will always find their way to these institutions? We all know about the Mississippian or Montanan who if they had not had access to a good research environment in their home states would never have made it on to the national scene.

Again, I would argue that diversity in the kinds of science supported, even at the expense of some duplication, is not to be avoided entirely. To deserve all this, we do have some self-examination to do. Here I echo and expand on what Dorothy Zinberg said yesterday. I run the risk of hanging out some dirty linen, but I think that these issues are fixable. So let's discuss them.

Support of science is not an entitlement. Let me expand. We need to continue to explain what we have accomplished. We also need to avoid the practice of the self-destructive, Chicken Little behavior each year crying out that science is in a crisis. We have also created schisms among ourselves, between big and little science, between pure and applied science, all of which reflects a creeping parochialism.

We have not always used our funding most effectively. To cite an example, the initial funding in AIDS research. Too much money came too soon. Industrial liaisons with their huge financial rewards have eroded some of the collegiality and openness inherent to our university campuses. All these we need to address and change for the better.

Most importantly, we should be aware that science is dramatically changing how we live. Legitimate fears and ethical issues brought about by these changes must be addressed by us. We have become addicted to federal support, forgetting that there was once a time before World War II when federal support accounted for a very small percentage of science dollars. The rest came from a mixture of state, private philanthropy, foundations, and industry.

As we see the growth of foundations like Howard Hughes and more recently the Burrough's Wellcome Fund and the Ford Foundation, it may be that these institutions will permit some decrease in federal support. Our dependency on the federal dollar has blinded us to creeping political influences on how we do science. We need to watch carefully the intrusions of the political process and the distribution of even our peer-reviewed support from NIH and NSF.

Already, congressional hegemony over some of the federal agencies has resulted in funding of science by geography and not merit. I welcome the suggestion of a science court for settling scientific disputes, leaving out the use of congressional hearings for this purpose. Lost of autonomy by scientists will not only degrade the science that we do, but will poorly serve the nation, not improve the human condition, and definitely waste tax dollars.

Even at the loss of some funding, we should be clear that the scientific autonomy is worth preserving. Do not misunderstand me. Autonomy does not mean total freedom. Whether it be tax dollars, foundation dollars, or any other dollars, we will always have to be accountable and be able to justify what we do and why we want to do it.

That brings me to the final big issue. Without the justification of national defense, we have been searching for other means of justifying what we do. Several suggestions have been made in the course of these two days, and let me reiterate them. One, science contributes to the economy, national competitiveness in the global market. Two, science contributes to human welfare and a sustainable environment. Three, the process of science, the doing of science is important to the educational training of our young, whether they enter science or not. Four, knowledge for knowledge's sake, which translates into an understanding of our physical world, the space around it, and our place in it, providing us with the power to dispel myths and better control our environment.

We have a very experienced and distinguished audience here, so I invite you to think about these issues and raise your hands for those arguments you think we should use. All right? Do you remember all of them. I'll just do it quickly. Economy. Human welfare and environment. Education. Knowledge. Or all of the above. (laughter)

Economy. How many of you think that is a useful and good argument for us to use? Okay. Human welfare and the environment? About equal. Education. You may vote more than once. Education. Knowledge? All of the above. (laughter) All right. Let's use whatever we can and use them all.

When the first of these meetings was started here at Columbia, science did seem in considerable disarray, under siege from many directions. Within the last two years, new leaders like Harold Varmus have begun to have an effect. New allies in Congress and the executive branch have been found. Judged by the output of science – more papers of high quality, better scientists who are trained – we are doing pretty well, while technology as many mentioned is a growing successful industry.

We promised in the '50s that the studies of bacteria-phase genetics would lead to an understanding of cancer and old age. And that is happening. The "Decade of the Brain" is already showing tremendous progress in how we discriminate and process incoming signals, leading to retention and memory. We have just turned the corner on HIV research and have the first realistic hope that a vaccine or cure is possible. I hasten to add that the last two examples, the brain and HIV, are part of current research ongoing at NYU.

Rather than bemoaning the fate of science in the United States, I would suggest somewhat audaciously for this meeting that we all just celebrate science. (applause) By whatever means we can, we should communicate that success as well as that excitement. That is why I support outreach programs, new technology in teaching and learning. I'm working with Leon Lederman to get his primetime series on scientists and how we do science on to the TV.

So, on that positive note, I hope you know that we all have a great deal of work to do. We cannot rest on our laurels. And I will end there. Thank you. (applause)

NELSON: Thank you very much, Alice. The issues that have been posed by our presenters and by our panelists are important and fascinating and controversial. Why don't we take about up to 20 minutes for discussion of these issues, and then break for lunch.

FOWLER: I'm Alan Fowler. I'm a former employee of IBM. I'd like to challenge Professor Crow's ideas about what I regard as a bureaucratization and a rigidifying of scientific administration. It seems to me that the last speaker very ably made the point that diversity in funding is very important in our system, that a centralized way of doing this sort of thing is bad. I would also specifically attack his idea that you can lay out a road map for science.

Now, I worked for 35 or so years for IBM. Every year, we would have a one-year plan, a five-year plan, a 12-year plan. Now in general, those plans were very good, when the development of science was evolutionary. They were total failures when there was a paradigm shift as there was in the 1980s. IBM totally failed to recognize the possibilities that existed that smaller companies were able to take advantage of. And it seems to me that there's the same danger in laying out these plans of research on a national level. Thank you.

CROW: Let me try to deal with the second question first, which is the comment on the issue of what I called the road map as opposed to the plan.

I'm not suggesting a planning process of the same kind of contextual complexity that someone like the IBM corporation might develop. I'm talking about a different kind of process. I ask you to look at the Institute for Future Technology's five-year report, which comes out of a complex assessment of all of the principle scientists in Japan.

This is not a bureaucratic road map of, you know, this is what we have to do. It's an analysis of where science might go within context of national issues or national problems. And it's a very different kind of process than any traditional planning process that you might be familiar with in either government or industry.

And it's something that helps to guide thinking as to what are the problems out there that we might address. What are the findings that science might move us toward in the future? And it's basically something that helps to orient or to give a compass bearing to the masses, who in a sense have no compass bearing, have no way to orient themselves, have no way to understand where science might be taking us overall. That's the second question.

The first question as to bureaucratization. I couldn't agree more that increased bureaucratization would be as negative as anything that we might undertake.

MALE VOICE: If you're commenting as to R&D evaluation as being the means by which bureaucratization would be implemented, one certainly has to develop more of a means to evaluate what we're doing, so as to understand where we are on that path. One can perhaps do that with less bureaucracy.

FLAMM: Kenn Flamm, Brookings. Just a couple of comments I wanted to make. First, I wanted to offer a neo-revisionist critique of the proto-revisionist hypothesis advanced by Hart in the crypto-revisionist view of the Bush report from Crow et al. In particular, I think both of you missed the point, although you have very different sort of interpretations in some respect of the Bush report.

I just wanted to offer another interpretation. David, in particular, makes the point that there's this issue of confusing the means and the end. And that has afflicted U.S. science policy and discussion of U.S. science policy. And I think you can make a real argument that that in fact was one of the objectives of the Bush report, confusion of the means and the ends.

In fact, the logic of the Bush report in some sense is that the means serve unforeseen ends. And therefore, the means become the end. And that in essence is what all the endless frontier stuff was about. It is that the process, the means, becomes the end.

Now, Dr. Crow is quite right in pointing out that Bush also speaks of the need for work on applied problems as well. But a significant portion of the Bush report, it seems to me, is advancing the idea that the means in and of itself has become a goal. To support this particular means, because it serves unforeseen, unpredictable, and therefore undiscussable ends. I think that's another interpretation of the Bush report, which kind of goes at odds with your fundamental chalk and cheese discussion.

HART: Why don't I just make a comment on that. Maybe our revisionism isn't too far apart. I didn't mean to represent my views as being those of the Bush report. And when I referred to the reservoir metaphor of the Bush report, I think that is exactly the kind of approach that he had in mind, as you described. I want to disaffiliate my comments with Vannevar Bush's.

FLAMM: That's okay. But then we're coming back to the point that in essence Bush was a chalk and cheese-ist, if that's the right way to put it.

And I also wanted to talk about the Bush report from the perspective of Dr. Crow, because I think you raised a couple of interesting points. It seems to me that if you look at the Bush report and the real innovative proposals in it, there is one key thing: in addition to setting up that the means is the end, there is also this idea of creating this institution insulated from the political process, which is going to fund this means, which is also an end, i.e., science.

And the insulation from the political process is purchased in two ways, one of which you thought was a bit mysterious, but which I don't think is mysterious at all. And that is by offering up a small budget for the thing. The surest way to attract political attention, I think, in Washington today and clearly probably also in Washington circa 1945, although I can't swear to it obviously from personal experience, is to have a significant sum of money out there.

And so that was one form of insulation from the political process, having this little thing with a little budget. No one's going to take that huge an interest. And it can actually get going. And you can actually partition this money off from the legislators on The Hill who are otherwise going to take an intense interest in where that money goes, by making it a small pot of money.

The other means of insulating it from the political process was this whole idea of non-mission specificity. I think the write-up for the next panel describes how, in fact, there's this huge commercial civilian technology agenda that's been sitting out there for some time that really doesn't get talked about in the Bush report.

You could argue that this idea of non-mission specific research was a way of separating what was going to become the National Science Foundation's activities from the entrenched interest – other agendas have already existed in Washington and were bound to come into existence after the war.

So, these two key features – a separate institution insulated from the political process and the small budget without a specific mission – were both devices to keep it away from the meddling of the politicians, if you will, in Washington, it seems to me. That's the neo-revisionist interpretation of the facts you're talking about there. And I offer it on the table for your comment and reaction.

The one other point, and I think it's interesting, is this idea of a national technical road map or a national scientific road map. And it's a particularly interesting idea, because I think in recent years industry, despite the gentleman from IBM’s comments, industry has found this a useful way increasingly to deal with some of the increasingly expensive infrastructure that has to go into some of these technology areas.

But I just wanted to comment to Dr. Crow that I think you've got it located sort of in the wrong place. I think if you're going to have a road map with any kind of lasting power to shape policy in Washington, it can't be a road map that is fundamentally shaped by political institutions, in the sense that those institutions have very short time horizons. Two years at best in the Congress. Four years at best in OSTP. They're always reshuffling, rejuggling.

You can foresee, for example, that we could define national technical goals in each of the separate areas that Professor Huang identifies, where in each of those areas, you'd have industry come together along with academia and perhaps define, without choosing between those different objectives, define the three or four most worthy technical goals that the government should consider funding. Because what actually sways the Congress, what makes Congress stick to the game plan? The bottom line is some kind of political constituency out there in industry.

Perhaps Mr. Holmfeld would disagree with me on that. But it seems to me that the longevity of any kind of technical plan is going to depend on a significant group with real clout with the Congress, with industry and academia together being the ones who formulate the goals that the political process supports.

DEVONS: Sam Devons, Columbia University. I'm going to comment on something that surprisingly was not said. We live in an age that was certainly not spelled out by Bush or anybody before him, the age of cybernetics and the World Wide Web, which may change the character of the way in which we teach science and do science. In fact, one of our lively economists here, Eli Noam, published some papers that were quite widely read, in which he predicted that the electronic age would make the university superfluous or economically non-viable.

So in a sense, the whole electronic age, which came out of academic research – the university has forged the tool that will make its own destruction, make itself obsolete.

It certainly opens up whole new vistas. For example, you can do space science sitting at your desk at a computer, without going anywhere into space at all. And many do. Much of high-energy physics is computerized. Many of the conferences take place over some sort of a network or web.

These things seem to me a mixture of social and technical factors. But they obviously portend great changes. Whether they will make the university obsolete or change its character, they're bound to have a big influence. And it's just beginning. As a small footnote to this, particularly as I see from our slogan here, "Learning From The Past, Designing From The Future," what I'm referring to is very characteristic of the past.

The electron, it so happens, was discovered just about 100 years ago. In fact, next year is the official centenary. Nobody on any panel could predict what the electron would do to our society.

It was an academic venture, mostly in Germany and England, in Cambridge. Nobody planned it. Nobody foretold it. In fact, for 10, 15 years, it did nothing.

But it has transformed science, our society, our way of doing things. It may even have rung the death knell of academic science in some sense. But I'm surprised that nobody has mentioned the impact of this particular technology.

MALE VOICE: We have three more people in line, and we have three more minutes before we break for lunch.

EISENBERGER: My name's Peter Eisenberger. And I have the gall to ask the shortest question. I've heard us talk about objectives, and I've heard us talk about process. Would anybody like to talk about kinetics? What I mean by that is, assuming we know where we want to go and we have some good ideas about potential process, how do we get these very diffused, unorganized communities to come together and try to do something?

I have a feeling in all these conferences, we've been talking about these things forever. A freight train's coming at us, and we're talking about it. But there's no way to start the kinetics of doing something about it.

MALE VOICE: Twenty seconds. I would locate the key place in your interest groups in Washington. Those people have to become more sophisticated politically, and that's where the leadership has to come from.

MANDULA: I'm Barbara Mandula, and I have a short comment and a short question. These are both from Michael Crow's talk. I thought he talked about changing the NSF to developing tools for other kinds of research. At least a few years ago, there were some fields of academic basic research for which NSF was <u>the</u> only funder – you know, 90 to 100 percent of the researchers, I think one of them was anthropology, were funded by the NSF.

And so if one does decide that the NSF will not fund basic research per se, there are some fields that are going to have to be taken care. They don't naturally fit somewhere else. At one point, I actually tried to document duplicated research. And no matter whom I asked, everyone knew it happened, and nobody could give me a specific example. So, again, when this duplication argument gets made, does anyone have

END OF TAPE
(BREAK)

FUSFELD: Herb Fusfeld, RPI. The previous speaker had referred to the idea of getting industry's input into some of this planning. Let me give you a Bush anecdote. The Industrial Research Institute has most of the big companies as members. Their members do $80 billion worth of research.

The organization tried to get started in the mid-'30s, and it wasn't going anywhere. And about the late to mid-'30s – '36 or '37 – Vannevar Bush was working with, of course, the military and the top political people in the country, very concerned about what was happening in Europe. And he wanted to be sure industry was ready to help in what he saw was coming. And he heard about this, and he said, if we form an Industrial Research Institute, we can have a mechanism by which industry can participate with government in its R&D planning. And Bush was a force behind the start-up of the Industrial Research Institute.

NELSON: I would like to thank the panel for a fascinating and provocative set of presentations. As always, the time for discussion is too short, but it's lunchtime.

Luncheon Speech

Former Congressman Bill Green

COLE: We're fortunate to have with us today former Congressman, always Congressman to me, Bill Green, who represented Manhattan's East Side and Midtown in Congress, and is really one of the more actively interested members of Congress, former members, in almost every aspect of science and technology. He's going to give a brief talk and is then willing to entertain questions from this group on re-designing the science structure of the US Government.

I could go through a very, very long list of credits and background information on Bill Green – but that has actually been handed out. I think you will find it an extraordinarily impressive list of accomplishments. Let me simply say that he served in the US House of Representatives for eight terms, from February 14, 1978 to January 3, 1993. And from 1981 on, he served on the House Appropriations Committee and was ranking Republican member of its VA-HUD-Independent Agencies Subcommittee, which covered the VA, HUD, NASA, EPA, the National Science Foundation, the Federal Emergency Management Agency, and numerous smaller agencies. He always served on the Foreign Operations Appropriation Subcommittee during 1991-92.

From personal knowledge of the activities that Bill Green has been involved in, few congressmen, however favorably predisposed toward science and technology, really understand the process of discovery and the way federal resources can and have been used to improve the social economic welfare of our citizenry through those investments. Bill Green is absolutely one person who as a member of Congress really did understand it. He understood the virtue of the partnership between the federal government, the research universities, and industry.

And since he has left Congress, I should say he continually comes to Columbia, and undoubtedly other educational institutions, and follows the new developments in science, technology and other areas of education and continues to be a very, very articulate voice for the values that were originally expressed with Bush and which have been articulated really as recently as Alice Huang’s comments that immediately preceded lunch.

It's therefore an enormous privilege for me to have Bill here. And I give you Congressman Bill Green, who will speak on re-designing the science structure of the US Government, all in 20 minutes or so or thereabouts. And then he will then be more than happy to answer a few questions before he has other obligations. So, Bill, good to have you here. (applause)

GREEN: Probably not since the period after World War II, the era of Vannevar Bush’s *Science: The Endless Frontier* (1945) and William T. Golden’s report to President Truman recommending the creation of the post of Science Advisor to the President, has there been as much discussion as in recent years on the structure of science within the United States government (Golden, unpublished). Examples of works that have played significant roles in that discussion are the collection of essays edited by Golden in 1988, *Science & Technology Advice to the President, Congress and Judiciary*, the reports of the Carnegie Commission on Science, Technology, and Government, and the National Research Council's report, *Allocating Federal Funds for Science and Technology* (Press 1995), which was produced by a very distinguished committee chaired by

Frank Press, president of the National Academy of Sciences from 1981 to 1993, and before that President Carter's science advisor.

For 12 of my 15 years in the House of Representatives, in my role as ranking Republican on the House Appropriations Subcommittee on Veterans Affairs, Housing and Urban Development, and Independent Agencies, I faced many of the issues from the current debate. In addition to the two cabinet departments, the independent agencies for which we originated appropriations included the National Aeronautics and Space Administration (NASA), the Environmental Protection Agency (EPA), the National Science Foundation (NSF), and the Federal Emergency Management Agency, as well as a host of smaller entities with some science and technology responsibilities, such as the Office of Science and Technology Policy (OSTP) and the Council on Environmental Quality (CEQ), both part of the Executive Office of the President, and the Consumer Product Safety Commission.

Thus, in originating our appropriations bill, which annually accounted for approximately 30 percent of the nation's domestic discretionary spending, we had to deal with both competition for funds among scientific disciplines and the claims of science and technology versus those of other parts of the federal government. Those problems were aggravated by the fact that during the 12 years that I served on the subcommittee, the Consumer Price Index rose by 59 percent while our allocation of funds from the full Appropriations Committee rose by only 17 percent. The pressure on our allocation did not represent any hostility towards us by the full Appropriations Committee. Instead, it represented the crowding out of all discretionary federal spending by the entitlement programs, most notably Medicare, Old Age and Survivors Insurance, and Medicaid. That was a decision made annually by the full Congress in its budget resolution.

One idea that has gained prominence in recent years as a means of restructuring the government's science enterprise is the creation of a Department of Science and Technology to encompass all the science and technology functions now spread about the executive branch. That would be followed by science and technology authorizing committees in the House and Senate that would take over all the science and technology jurisdictions of the other committees and similar appropriations subcommittees.

The idea has obvious appeal. It is, at least on initial contemplation, simple, and appears to improve accountability. It may, however, fall into the category which I think H. L. Mencken once described when he said that every problem has an answer which is obvious, simple, and wrong.

Moving things around is always a temptation. At one point in my Congressional career, I was vexed with NASA for seeming to give priority to putting people in space rather than maximizing the science return from space. I contemplated introducing legislation to move NASA's science responsibilities and funding to the NSF, leaving NSF grantees the option to hire NASA, private sector, or even Soviet cosmonauts in furtherance of their research.

My staff ultimately persuaded me that such a shift was unlikely to change the political and public relations pressures that drove the manned space program, and was just as likely to result in less space science as in more. The only sure outcome was that the shift would have disrupted space science for at least a year as the change was made. But there are larger reasons why I am skeptical of a Department of Science and Technology. The fact is that federal agencies do science and technology for many reasons, reasons that may be important to an agency mission though they would not be to a science department. Should an EPA, for example, have to justify to a Science and Technology Department funding research on the clean-up of a particular kind of hazardous waste site that the agency feels is a major problem?

The fact of the matter is that the two agencies would have different criteria in making decisions and there would be no reason to expect a Science and Technology Department to have expertise on all the issues EPA must consider in setting its priorities.

Let me give another example from our subcommittee. The VA runs a medical research program, funded at around a quarter of a billion dollars. Though to some degree it focuses on rehabilitation medicine and obvious VA interests, that is far from being the exclusive focus of the program. Even without a Science Department one might ask why this program stands alone at the VA instead of being folded into, and subject to the priorities of, the National Institutes of Health.

There is a reason. The funds are used as bait for medical schools to affiliate with VA hospitals. Since studies have clearly shown that VA hospitals with medical school affiliations perform better than those without them, this inducement to medical schools to affiliate with VA hospitals is an important element in maintaining quality in the VA's $17 billion a year medical system, the nation's largest single health care system. The quarter of a billion dollars is modest science funding, at least by Washington standards, and certainly if measured by the scale of NIH, but for the VA, it has a very large payoff for the department's mission, a payoff that would be totally lost in a shift to a Science Department.

That is not to say that there are not places where consolidation or revamping of federal science activities might not be beneficial. One place that comes to mind is science education. Both the Department of Education and the NSF have programs in this area, and other federal agencies also see it as a responsibility. Thus *Science in Air and Space: NASA's Science Policy Guide* notes that, "throughout most of its history, NASA has explicitly undertaken a major role in the support of graduate education and the education and training of graduate students." The report goes on to propose that "NASA and its research community must become more actively involved in pre-college education." If there was any coordination among these various education programs, it was certainly not evident to those of us who were in Congress, and it might well make sense to have some sort of coordination.

The real issue facing the United States government in relation to its science and technology effort is how much money science and technology are to get as a whole, and how to divide up that money among the various claimants in the science and technology community. Creating a Department of Science and Technology would not by itself resolve those questions any more than the existence of NSF today tells us what its overall appropriation should be or how to divide that appropriation among its several directorates.

Another example of the difficulties in deciding how to allocate funds among claimants, even in a narrow range of disciplines, is the report *Setting Priorities in Space Research: An Experiment in Methodology* (National Research Council 1995). The group involved in the effort was unable to arrive at a consensus on procedural instruments to be used to make allocations in this field.

At the outset of my recommendations on these issues, let me note that I have been very favorably impressed by the mechanisms that are in place to get advice on priorities and funding needs within individual disciplines. The agency advisory committees and peer review mechanisms, the OSTP and the various White House advisory committees, and the National Research Council system seem to me in general to do an excellent job, and I found their work very helpful when I was in Congress.

Candor requires me to state that the National Research Council has appointed me to its Space Studies Board, but I can assure you that I reached my conclusions well before that appointment. In my view, both initial budgetary decisions (how much to propose *in toto* for science and technology) and at least the first cut as to how funds should be divided up by disciplines, must be determined by the White House. In that respect, science and technology are not different from other areas of federal activity.

For example, it is the White House that decides how much to propose for transportation infrastructure and how to divide it up among the several transportation modes. Who at the White House should have primary responsibility for recommending those decisions to the President? The two obvious players are the OSTP and the Office of Management and Budget (OMB). I would see the internal White House process as a joint effort of the two, as indeed I believe it is now and has been for some time.

Because the head of OSTP, the Science Advisor, is something of an advocate for science to the President, I would see OMB as having the larger role in balancing science's claims against other claims on the federal list. Once that choice has been made, I should think that OSTP, because of its expertise, would have the larger role in making the decisions among the science and technology disciplines.

Still, to decide who is responsible for decisions does not tell us how it should be done. White House budgeting will function within a larger process, such as zero-based budgeting or management-by-objectives, which are examples of approaches that the White House has used to operate its overall budgeting system. How are the specific science and technology choices to be made? I have found the recommendations in the National Research Council's Press report, to which I have previously referred, an excellent start. Though by its own terms more suggestive than prescriptive, the Press report recommends that a science and technology budget be an integral part of a federal budget. That contrasts with the current system, in which the science components spread throughout the many agency budget requests are pasted together after the presidential budget recommendation is completed.

Under the system proposed in the Press Report, science and technology funding levels would be decided by determining what was necessary “to maintain a world-class position in fundamental science and technology and a leadership position in select fields.” OMB calls to agencies to start the budget process, and agency responses would reflect that premise. Congressional budget procedures would be changed by having the Budget Committees track the extent to which individual appropriations bills meet administration requests.

Finally, having such a device to provide a rationale for the administration's science funding requests would strengthen them in competition with other claims on federal funds. But to be candid, it would not truly tell us what to do when there just is not enough money to go around – another way of saying when the political system decides it has other priorities.

In the end, given our democratic system, whatever process we choose for making government science and technology decisions is always going to be messy. As E. M. Forster put it, two cheers for democracy. We are, after all, dealing with a perfectly normal situation in which the useful things on which we can spend money require more money than we have to spend. For those of us with an economics background, it was always difficult for government entities not subject to the marketplace to make such decisions in the absence of a means of determining the marginal benefits and the marginal costs of various alternative programs.

There is, of course, another way of looking at the problem. It is Robert Browning's observation in his poem “Andrea del Sarto”, “Ah, but a man's reach should exceed his grasp, or what’s a heaven for?”

ADAMS: Dorothy Adams from Columbia University. Please pardon my naivete. But is there a possibility that in the future we might take the funding for science out of discretionary funding and make it regular and just give us a slice of the pie that rides with the GNP?

GREEN: Well, that's always a possibility. And I think every group that comes to Congress for funding tries to become an entitlement program, if it can. I'd have to say to you that as Congress has seen its control over the budget process deteriorating because of the growth of the entitlements and the crowding out of the discretionary spending that I described, there has been increasing resistance within the Congress to that kind of approach. But if you want to try, good luck. (laughter)

SILVER: I'm Howard Silver from the Consortium of Social Science Associations. Good to see you again. As you sat across the table from me, when I was trying to make the case for science in the Appropriations Subcommittee hearings – as you did many times – what from your point of view as a member of Congress were the most effective arguments that were being made?

GREEN: Well, first of all, the groups that were really savvy always had a witness from the chairman’s home state (laughter) and preferably sitting by his side was a witness from the ranking minority member’s home state. There is some of that, I must say, in the way Congress operates.

Seriously, I think different members had different interests. Perhaps to me, it was easy to sell the idea of knowledge for its own sake. Others were very, very interested in the competitiveness issue, particularly in the early ‘80s, when it was perceived that we were wonderful at pure science but that somehow the Japanese were perceived as being better at taking the pure science and turning it into salable products. That mystique may have eased somehow, but certainly at that time, that was a major, major influence.

There are those who are primarily military oriented, the Star Wars types. And I guess they would see that as their Holy Grail. So, I don't think there's any one issue that appeals to every congressmen. We all have different interests, different priorities. And the way you put together a winning coalition in Congress is by trying to bring enough of them together to get a majority.

QUESTION: Mira Tadari, City University of New York. Some of my colleagues pointed out that some of their projects are long-range and that they have a very short time for review and re-funding by Congress compared to people in other countries. From your vantage point, is this a real problem that should be addressed?

GREEN: It is. I'm currently, as I mentioned, on the Space Studies Board, and the project at which I'm particularly working is with a group of Americans and a group of Europeans, trying to figure out what has worked and what hasn't worked, by way of European/American cooperation in space science. And certainly, the US annual funding process, as opposed to longer term funding of the European Space Agency, is an issue in those discussions.

I should point out that multiyear contracting, multiyear appropriations are certainly permitted under the federal system. Congress again is not eager to give away that annual review, but the administration has asked for an increase in that kind of funding. I know there are a couple of accounts in NASA where the administration is newly seeking multiyear funding. So it is not impossible to achieve.

Also, while the funding for an account may be on the basis of an annual appropriation, the agency may still be allowed to make a multiyear grant or a multiyear commitment. Now, obviously, to the extent that it makes a multiyear commitment, it makes fewer commitments than if it makes a lot of single-year commitments. And there's always that tension, in terms of the agency and its constituency. But there are ways to deal with what I acknowledge is a problem.

QUESTION: As you recall, a couple of years ago, a very remarkable debacle, a big project we worked on for ten years – I think more than $2 billion was invested – was cancelled by a vote of Congress. Is there any political lesson that you would draw from this about how scientists behave or misbehave, or should behave?

GREEN: You're talking of the superconducting supercollider, I assume. Well, that was an interesting case. It was a very popular program while 40 states were competing for it. Once one state won, it didn't have quite as many friends. NASA had the other so-called "big science project" – although I've never really thought there was much science to do with the space station – and was able to come to the floor and point to subcontracts in 40 states in trying to woo votes for that one.

So I would have to say that the mood of the Congress and the pressures of the budget in the year that was killed – '93, was it? – were such that it was clear to me at any rate that it was either going to be the station that went down or the superconducting supercollider. There weren't going to be two of these mammoths occupying the budget stage.

And, you know, I think that's unfortunate. I think they got the wrong one, because I always thought the superconducting supercollider was true science, even if it was going to be in Texas and not New York (laughter) – whereas I didn't see the space station, even though at the time Grumman, a New York company, was coordinating the work, as offering as much in the way of true science as the collider.

(OFF MIC QUESTION)

GREEN: Well, I think the science community was unwilling to face the fact that it had to make a choice and that the political climate was such that it was not going to get both. Now, in fairness to the space station, the micro-gravity work both in terms of the life sciences and materials is important to a group of scientists. So I don't expect that it would have been possible for the science community to achieve unanimity. But I think if the science community had chosen to back the superconducting supercollider and say, we'll give up the station, that would have had a powerful impact.

COLE: Why don't we have three more questions. And maybe we should have them together and then Congressman Green can answer.

QUESTION: International cooperation in large science and medium-sized science projects has been discussed as a possible route to reducing the costs of big science. Looking at the prospects of that from a congressional viewpoint, do you see that there are some viable political mechanisms for which the executive branch and the Congress can cooperate so that the U.S. can take a consistent and credible commitment position in order to enter into a larger number of those agreements?

MITCHELL: Tyrone Mitchell from Corning Incorporated, the glass company. My question is around the dual-use concept out of the Defense Department, which was probably unilaterally determined by the Defense Department as a way of staying military-ready and having industry make whatever they needed. Don't you think a better model might be to just let the Defense Department do business as usual at the reduced funding level that's required to maintain whatever defensive readiness that we have and use those extra funds to put back into programs like maybe the infrastructure of the country, something that benefits everybody, bridges, roads, tunnels, or mass transportation?

QUESTION: Jay Hauben. In the unfolding of society, the little person has very little say, except through the vehicle of his government. And so the government should be as big and powerful as possible, so that the little person has a chance to make an input. But how does the little person get to his Congressman and have the Congressman represent the need for as much science and technology as possible, so that life can be better in the future?

GREEN: Let me go in reverse order. I'm not sure that everyone would agree that the little person is protected by having government as big and powerful as possible. I don't think that's what the

Founding Fathers quite had in mind. But the question, of course, is: as government does get big and takes jurisdiction over more and more areas of our life, how does the individual deal with that? One answer that political scientists would give, I guess, is to try to devolve as much as possible back to the state and local level. And that's the theory of many who favor essentially a block grant type of assistance for the federal government or going back to the Eisenhower days and the Kastenbaum Commission, actually having the federal government give up some taxes.

And some of you may remember that President Reagan had a similar proposal early in his administration. Of course, the tax that he proposed to give back was the windfall profits tax on oil, which phased out over a period of years, so the states weren't very eager to grab it. On the other hand, what he proposed to pick up from the states was Medicaid. And I bet most of the states wished they had taken the bargain for that reason.

So that's one way to do it. If you're asking how do you lobby effectively, I think it is inevitable that a letter to your own congressman and your own senators has weight. Obviously, your participation in national groups that can speak collectively for a lot of people also has weight. And I think it's very much in the American tradition, the de Toqueville theory, that when we perceive a problem, we get together in groups. And it's those groups that energize our government.

As to the question of the allocation of the funds to DOD and whether the dual-use funds might not be better taken away from DOD and spent elsewhere that Congress proposes to use them, I guess I would have to say it depends on what funds you have in mind.

I remember some years ago, the Defense Department had a program for the garment industry, in which they were trying to keep our garment industry competitive, so that if they suddenly had a mobilization and needed lots of uniforms, we wouldn't have to bring them in from Hong Kong, in order to dress our army. So at the Fashion Institute of Technology downtown, they had demonstrations for the local garment manufacturers on how to use lasers to cut cloth uniformly. And maybe that's something that is within the Defense Department's purview and will have to stay there, even though it may have beneficial uses in civilian life.

Finally, the issue of international cooperation. I'm never totally comfortable with the large science versus small science dichotomy, although obviously the problem of funding individual investigators is an important one. On the other hand, I'm also reminded that the National Science Foundation set up supercomputing centers, which you might regard as big science, and yet a significant part of the purpose was to give access to supercomputers to individual investigators, who obviously couldn't afford one full time and didn't need one full time, but for whom some time on a supercomputer would be useful.

I think international cooperation is a way of sharing costs on projects that are going to have international benefits or are going to broaden the world's knowledge. I think international agreements have probably been useful in selling Congress on projects, at least under some circumstances. Certainly, in votes in '91 and '92 on the space station, the fact that we did have international agreements and that we would be subject to international criticism if we terminated the program, that was a telling point in the arguments of those who favored funding. So, it's no panacea. It does depend on funding and the will to proceed. But I think, by and large, it is useful and I think Congress recognizes that use. Again, thank you all very much for you attention. (applause)

COLE: Again, I want to thank Congressman Green for joining us on this gorgeous autumn afternoon and for talking to us about his thoughts on various subjects involving the science structure in the U.S. government. We now move on and I hope that you'll stay with us, because we have some very good stuff coming up, where we will be talking about civilian technology policy. It is an issue that was raised earlier today. May I ask that the participants join me. Michael Crow will be doing the honors of introducing our panelists and our designers.

Design Area Seven:
Civilian Technology Policy

Richard Nelson
Herbert Fusfeld
Irv Feller
Patrick Windham

Moderator
Michael M. Crow

CROW: At one time as we planned this meeting, there were going to be 40 design areas, but we didn't think that you could quite get through that, so there's actually only eight. We're on number seven, and you can deduce from that that we're coming into the home stretch here.

What we have been doing, as I mentioned yesterday, is systematically approaching each of Bush's points. Now we come to the first area where Bush was relatively silent if not totally silent, which is the area of civilian technology policy and the role of the government in specific ventures related to the development of technology itself.

We've got a distinguished panel. I'll just briefly give you an overview and then introduce each of them individually. Dick Nelson, who's here at Columbia, has been thinking about the role of the government in research since the 1950s. In a sense, he's the father, I think, or the parent of R&D economics.

Herb Fusfeld, who has been in industry, served as an R&D vice president, served as the president of the Industrial Research Institute and now, for quite a while, has been in academia.

Irv Feller, who's been attacking – I use that word, Irv – the value of technology-oriented policies for some time at both the state and federal levels in the most systematic fashion of anyone who's out there, I think.

And Pat Windham, who's up from Washington, and who's on the subcommittee on science, technology, and space of the Senate Commerce, Science and Transportation Committee, has been the principal author, architect, and champion, on the Democratic side of things, of the technology policy of the United States, and particularly over the last several years.

First Dick Nelson, who's one of our co-conveners of this meeting. He holds professorships here at Columbia, it's worth noting, in business, law, political science, international affairs, I don't know, aeronautical engineering, what else? (chuckle)

NELSON: I'm a member of the medical school.

CROW: Medical school. So, first, Dick Nelson.

NELSON: I intend to treat this topic of civilian technology policy relatively narrowly. By that, I mean I am not taking up the broader set of questions and issues that were raised yesterday and this morning, regarding whether a new, broad rationale that is persuasive for government support of basic science can't be tied to economic benefits or economic growth rationale.

Rather, I'm going to focus specifically on government programs that are targeted at various areas of civilian technology, particularly programs aimed to help the technology in particular industries. As the write-up of this particular session signals, and as our discussion has gone on over the last couple of days, there really are two rather peculiar aspects of the Bush report that bear on the topic under consideration here in this session.

One of them is what Michael just mentioned, others have mentioned before, and that is the rather peculiar focus of the Bush report on government policies specifically aimed to advance the technology or the science underlying the technology of particular industries or sectors. This is peculiar, because, as several have noted, if you look at the structure of federal research and development spending prior to World War II, the largest program is not defense. It's agriculture. And even in those days, the program funded by the feds was matched by funds coming from the states.

The other aspect of the Bush report that we've commented on, on a number of different occasions, which I think is highly relevant to this particular discussion was the articulation of the linear model. That technology in a sense comes from drawing from science. You have the pool analogy, where science provides the opportunities for advancing technology.

However, there is no serious way identified in the report to know what are the areas of science that are going to be contributing or have a high probability of contributing to the possibilities of technical advance in different fields. And, as I think several have commented earlier, a very plausible reason for taking that position by Bush is that if you articulated that and you got the political structure to buy it, you would damp down tendencies on the part of the political structure to try to engage in relatively fine-scale allocation of scientific resources, in a sense, targeting research.

But, of course, as we have also discussed, much of the lion's share of the programs that materialized for basic research support in the years after the Bush report were most emphatically targeted. The big funders of basic research haven't been the National Science Foundation. That's been a relatively small part of the story.

Rather organizations like the National Institutes of Health, which as the discussion went yesterday and today, clearly have targeted a variety of different broad areas of science, in anticipation that these are the ones that are going to yield the payoffs in particular areas where the rhetoric has gone back and forth. And, of course, the Department of Defense, playing active roles in developing whole new fields of targeted science, like materials science, computer science. The Atomic Energy Commission, the successor Department of Energy.

Yet, as Donald Stokes has argued, I think, very, very well in his wonderful manuscript called Pasteur's quadrant, for a long, long time, forever, that much of fundamental scientific research has been done in areas where the areas of application are successful or moderately clear and the people doing the research knew about it. That's Pasteur's quadrant, as Stokes called it. In fact, the result of the Bush report was that so far as broad citizenry was concerned, he didn't think about it that way.

Now, both of these particular aspects of the Bush report increasingly have come back to haunt us, I believe, and their conjunction now haunts us. Beginning in the middle 1960s, you can see a string of proposals for the federal government to mount research and development support programs, specifically aimed to help particular industries, from housing in those old days to flat panel displays in a more recent era.

That particular thrust has waxed and waned and was certainly strong in the early days of the Clinton administration and has been damped down recently, but it will certainly come back again. Particularly if you have a strong Democratic results in the next election. This argument of focusing federal programs to help certain civilian technologies is going to be with us.

At the same time, I'm going to propose that probably the most fruitful kind of such federally supported programs is a targeted science support kind of a program, like funding materials science and computer science if you're trying to help the technologies and the industries that draw significantly from those technologies. Yet, in a sense, because of the Bush report no-no on that type of thinking, that’s not the way that the policy discussion regarding civilian technology policy has proceeded.

To begin to put my own particular views of this on line, I have been bothered for some time by the fact that most of the discussion about civilian technology policy has proceeded as if the most fruitful way to conduct such a policy is to identify particular product fields or products that really ought to be developed and to provide funding that goes into industry in particular, to achieve those kinds of objectives. It is this kind of policy that, I think, correctly has generated so much of the argument about governmental ability to pick particular product areas or particular commercial opportunities and support that – where the record, both in the United States and in other countries, is very, very poor, indeed.

But as I've looked at a number of such policies, it seems to me that there is a variety of other kinds of difficulties with programs conceived and designed in that particular way. In particular, they are politically very, very fractious, indeed. Government programs that proceed by funding certain firms and not funding other certain firms get to be really very dicey, politically, down the road a little bit, particularly if the firms that are provided with government funds do well.

Therefore, it seems to me that an effective government civilian technology policy, or a policy in a particular area, rather has to be conceived and designed along quite, quite different lines. First of all, the funding in the program should not go to individual business firms or to small coalitions of business firms but rather to research institutions that serve the industry as a whole or large portions of the industry. I'll come in a moment to my proposition that in many cases the most plausible way of setting up such a program looks to me like associating such facilities with universities, but that's not necessarily so.

Think, for example, of Sematech, which meets the criteria of being an institution separate from individual business firms and serving the industry as a whole, at least those firms that are willing to contribute to and interact with it. But, most emphatically, it is not a university-affiliated enterprise.

The second ground rule I would propose is that the program not aim close to the market at all, but rather stay a considerable distance away from the market, and focus on funding those kinds of research, those fields of science and engineering, and those areas that in Ralph Gomory's terms of yesterday look as though they have very considerable promise of enabling technological advances to proceed. The issue is how to design such programs and how to govern and structure them. And my proposition here is that the proper mode of organization and governments, subject to those two broad guidelines, is going to differ a lot from field to field and from targeted industry or targeted-industry problem to targeted-industry problem.

On the other hand, two of the most successful civilian technology programs that the United States has had have been affiliated with universities. They're very different types of programs. One of them is the old agricultural research support program, which in a number of instances got relatively close to nonproprietary practice but which also supported and very well, in a number of cases, the underlying sciences that would enable one to understand problems of plant nutrition and things of that sort.

The other success is, of course, many of the programs of the National Institutes of Health. These programs, at least some of the subprograms, have to be understood as research support targeted at enabling – through advances in science at a number of different times – technical advance in medicine to proceed, in many cases through the roots of development of new pharmaceuticals, in turn left as the province of the companies in the industry as a whole.

My strong belief is that it is a fool search to hunt for a broad-gauge civilian technology policy. Rather, one ought to be much more open than we have in the past but more sophisticated than we have been in the past in identifying various areas of civilian technology and various industries that, at particular periods of time, are calling out for and really could benefit from a targeted research support program funded, in part, by the federal government.

The governance of such programs seems to me to require a structure that involves people from industry, but not people from industry alone – also a heavy infusion of academic scholars and scientists in the field and people from government. We began to have a discussion this morning of the notion of mapping exercises and setting out relatively long-run plans. A good example of that is the long-run research planning exercise that the semiconductor industry has been going through in recent years, which maps out various areas where funds ought to go, at least if the bet is right, and changes from year to year but provides some guidance. And my belief is that probably it is the National Academy of Sciences, National Academy of Engineering structure, that ought to be the locus for that sort of a planning and mapping exercise.

If I had time, I would discuss what strikes me as one of the more interesting new cases of opportunity and demand for a civilian technology policy now. This is the problem associated with a phenomenon that several of you have noted, which is the collapsing of corporate central research in the great electronics companies of the United States, which has been worrying very significantly people in the industry itself.

It has been worrying Bill Spencer a lot, who knows a lot about that industry and originally thought that he might be able to deal with that type of problem through the structure at Sematech. He now is strongly of the opinion that a research organization like Sematech can't do that – that what you really need is a set of laboratories affiliated with universities but with heavy industry involvement in research planning but not control of research allocation, with a significant infusion of industry people, among other things, to facilitate smooth transfer and communication between the project and industry.

But my time is up. (applause)

CROW: Thank you, Dick.

Next will be Herb Fusfeld. Herb is professor and chairman of the Advisory Board of the Center for Science and Technology Policy at the Rensselaer Polytechnic Institute. He has served as director of research at Kennicott Copper, and he's a past president of the Industrial Research Institute. Herb.

FUSFELD: In the early 80's, there was a report of the National Academy of Sciences on the competitive situation of American Industry which made the following point: that Japan was beating the heck out of us; and it was doing this for two reasons. One, they weren't doing any basic research, and therefore they had more resources to spend on other things. Among those other things was manufacturing, and they were very good at it. So they were beating the pants off us in manufacturing and getting ahead of us. And what was the recommendation of the report? One of the principal recommendations was that we spend more money on basic research in American universities.

Let's say a report that focuses on technology policy shouldn't necessarily attempt at the same time to solve the overall science policy question. They're related intimately and I'll make that point, but we should focus for a moment on civilian technology policy.

"Civilian" presumably means non-defense, but I take a narrow view. There are many areas where government is either the principal user of the technology or supporting a broad general industry structure. I'm focusing on that aspect of civilian technology where government efforts are supposed to in some way lead fairly directly to economic growth, employment, and so on. In those areas of civilian technology policy, the government makes policy.

If this were industry, the focus would be on strategy. In government, I believe the appropriate federal role in this area is to facilitate, strengthen, feed, and support industry and industrial research. Not necessarily through direct subsidies, but some sort of support.

The government has a problem here because American industrial research is extremely strong. Any government policy or university approach that is based on the premise that industrial research is weak and needs help is wrong. That doesn't mean industry can't be strengthened, but is it crying, is it running for help? No.

For example, IBM had some terrible marketing problems a few years ago, and losses of many billions of dollars. It cut its R&D budget from $5 billion to $4 billion – a 20 percent cut. A lot of people were hurt, unfortunately. Still, $4 billion isn't bad. And today, IBM is spending $5½ billion, ten percent more than at its peak of four years ago.

General Motors, which four or five years ago was spending about $6 billion, is today spending about $8 billion on R&D. The Industrial Research Institute just issued its annual report on industrial research. This year, industrial research spending is going to go up by about five percent. Of the companies surveyed, 50 percent were holding the line on new personnel. Forty percent were hiring more personnel. Ten percent were cutting a little.

So, keep in mind that industry is quite strong, and therefore a federal role in supporting a strong base like that is not easy, but there are many things that can be done. However, in order for the federal government to have good policy, it has to truly understand what industrial research is and what its real long term needs are.

There are four points about the conditions under which industrial research has changed from the period up to the late 1970's until today. In fact, there are many, but I want to point out four important factors that change these conditions. Number one is the shift in the ratio of federal monies to industry money. In 1960, the federal government spent twice as much as industry on research. Today, it's almost reversed: industry spends $102-104 billion for research funding. They conduct more, but they fund $103 billion. The federal government funds about $62 billion, which goes primarily to just two or three industries.

Until the late 1970's, the university research effort was focused on government, and industry was not always a welcome visitor at universities. That did not hold for Columbia University, Rensselaer Polytechnic Institute, or Carnegie Mellon University, but it did hold for a lot of our elite research universities. Today, we have many elegant papers on the importance of university industry reactions. Those papers didn't exist before the mid-1970's. The university approach has changed.

The second factor, which doesn't get much analysis, is interest rates on money. The golden age of industrial and university research took place in the 1960's and into the early 1970's. Then, during the Carter administration, interest rates rose to about 16 percent. No program that lasts more than two years is going to stand up under 16 percent interest rates.

In a way, industrial researchers shot themselves in the foot. We were doing quite well in the 1960's and early 1970's. Industry has to sell itself to management for research, just as universities have to sell themselves to the public. Industrial researchers told management, "We're a great investment. Just judge us the same way you would judge any investment and you'll put your money in R&D." That was great when interest rates were six percent and eight percent, but at 10 and 12 percent, we didn't look so good. In the late 1970's, companies began to take a wholly different approach to R&D, which was much more short range. That was the beginning of the change in industrial research.

The third factor that really changed things happened in the 1980's, when international competition came roaring in and it was critical to produce things competitively. During the 1980's, industry solidified the fundamental reasoning of putting more into short term R&D. I was once in the office of the Hertz company in Frankfurt, with the man who was the board member for R&D. I asked, "What is your driving force for R&D?" In response, he pounded the table and said, "Time, time, time. There's nothing pushing us harder than the need to get things out in a short time." They didn't cut their R&D, but they certainly changed the ratio of it.

Finally, the fourth major condition that has changed for industrial research is that because of increased competitive pressures and the increasing cost and complexity of making technical advances, companies began to have a declining technical self-sufficiency. Until the mid-1970's, if a company wanted to get into a field, broaden its area, take any kind of strategic approach, it was reasonably likely to be able to do so with in-house technology. Or else the technology could be acquired at reasonable cost and in a reasonable time. Steadily into the 1980's, that began to be less and less true. Companies could not do certain things because they did not have the people or they couldn't be done in a realistic time. Those were the changing pressures on industrial research.

Industry has reacted in a couple of ways to that, which has set the stage for what we're doing today. To some extent, it sets the stage for what industry needs to do. First of all, the most dramatic effect of all that change has been an explosion of joint ventures, what they call strategic alliances, reaching out to develop access to external sources of technology. Companies are not trying as hard to be self sufficient anymore. They have core competencies, but they are trying to identify where the sources of science and technology are that they need, not just in ten years, but this year and next year.

Reaching out to external sources is now bread and butter to industry. That is partly why the growth of industrial research has slowed: not because there's less emphasis, but when companies are working more and more with other groups, they don't have to increase their own resources as quickly as they did before.

Secondly, there have been major changes in the internal management of R&D in large companies. Specifically, a push to get as much as you can out of your internal resources. Years ago, there were (and there probably still are) a lot of turf battles. There was a central research group and divisional labs that were relatively self-sufficient. Today, more and more major technical companies like Hewlett Packard and Motorola have a major company-wide program where they pull in resources from other parts of the company to wherever they are needed in order to carry out a task, and then they return to where they were before. Through the management of R&D, companies are trying to overcome some of these pressures. So, today we see better management, reaching out to other people, and great pressures, but from a funding standpoint, industrial R&D is very strong indeed.

Is that sufficient? Does government go away? Not exactly. There are two or three things that industry is not doing. There has been a cut-back in what we would call the centralized R&D function. In a way, that's good management; R&D should be coupled as closely as possible to the business units. That is more effective for product development. But since the central group is the one that is concerned with long-term R&D, there's less of that today.

Another problem is with the ability to concentrate technical resources. Until the mid-1970's, companies could pull together 1,000 people in a central laboratory, if they had to. To some extent, they've lost that ability. Also, companies need to have access to longer term ideas, the reservoir. The important thing here is personnel. In these three areas, federal policy can help: the ability to concentrate technical resources, providing a reservoir for long term advances, and good people.

What are some logical ways for government to have a role in civilian technology policy that would strengthen industry? Number one is to support a strong technical infrastructure. You cannot maintain a strong industrial research base if you don't have a strong foundation under it, and that foundation comprises people and technical advances in science and engineering. Industry has always supported that and will continue to support it.

Secondly, government can facilitate industry's access to these technical advances. The best ways of doing are through support of engineering technical centers, which link universities to industries; and support of mission-oriented centers at universities. This is a more natural way for industry to link with universities. Industry is interdisciplinary. They don't just want physics or chemistry, they want a mission, whether it's solid state devices or something else. A mission-oriented center at a university gives industry a better coupling. Within the university, a mission-oriented center can couple itself to the research of the scientist, but strengthening both of those would help.

A third way is to encourage the external linkages of industry. These external linkages, which industry needs, are two-way streets. Our industries gains as much as they may give. Those who are concerned with export controls should think about this. We did a study for the Defense Advanced Research Projects Agency a few years ago. We concluded that, with regard to these linkages, the Defense Department has two choices: have absolute control over weaker companies, or negotiate relationships with stronger companies. We felt the latter was by far the best.

Fourth, the government should encourage federal technology intensive-programs. Don't ever underestimate the importance of defense research. I hope we don't need wars in order to justify these kind of things and hope it can be something else, but politically, that's the only thing we have that justifies $30 billion a year, year after year.

Not only because it pays for university research; it strengthens the training of people in universities, it allows technology to advance, mostly because it focuses money in specific areas. If you divided $30 billion equally among all universities, you would raise the level of all science a little bit. By focusing in certain mission oriented areas, you raise advances enormously in selected areas, whether it's electronics, communications or what have you. It's a very important function.

And finally, the last item, and perhaps the most important. To make technology policy, it is not necessary to spend money. Money is the most important aspect for many people, but in theory, government can have a policy without spending funds; before the 1930's, this was the case.

The most important policy action for the government to take is to provide an encouraging and stimulating economic environment. The tax and legal liability structures of the country, regulatory areas, et cetera. There are a great many things by which government influences industry. Many of them inhibit innovation, for example, maintaining different tax structures for foreign research done by American companies. It's an incentive for our companies to do research in other countries.

The most important thing is to coordinate the various government actions that affect policy, and made them coherent to stimulate industrial research. The final statement on that is a very simple one. If you have companies which are aggressive and eager to move ahead, make investments, and so on, they will take care of most of their technical innovation. If you have companies that don't want to make an investment, it doesn't make any difference who does the research and who pays for it, it won't be used properly. That's what the government has to keep in mind.

CROW: Thank you, Herb. Our first panelist is Irv Feller from Penn State. He's been there on the order of three decades. He is the leader of the Institute for Policy Research and Evaluation, and is one of the leading figures in attempting to build the tools by which research programs in technology areas and other areas might, at some point, be evaluated. Irv.

FELLER: Much of what I say will be to underscore and support what Dick Nelson has already said, and perhaps to give it a little more institutional context, but also to point out some of the stresses and tensions within his well-crafted analysis. And I want to begin by emphasizing some of the points that Herb Fusfeld talked about.

To me, the most striking thing about the discussion to this point is that it has not been focused on ATP or TRP, which I already see as a very positive aspect. That is, we are talking about civilian technology programs in a very broad framework. We are talking about (and I'm quoting Ira Fusfeld here) programs that lead to economic growth and employment, that facilitate and support industry. We are not talking necessarily about specific products. We are dealing here with a broader issue than a single federal program.

In trying to get a handle on civilian technology programs, what struck me is the breadth of programs that one could look at across agencies, especially if one followed Dick's emphasis on infrastructure and the general question of what our industry needs. This goes back to the kind of ends and means discussion we had earlier today, and that is what civilian technology policy consists of and what are the objectives. And my sense here is that you're going to find quite a range of programs implied by these discussions. I'll come to the specifics in a moment.

It also strikes me that in trying to deal with framing civilian technology programs, particularly in setting the criteria, that implicitly one is dealing with models of technological innovation. How does infrastructure and how do certain types of knowledge flow through the economic system?

So what I'm struck by is, as one tries to handle this in a policy sense, as in a programmatic sense, implicitly one is forced back to priors about how does knowledge supported by the public sector, federal government or state government, enter into new products and processes; and particularly, what would be the most effective means for government to act?

Let me do this in a series of specifics, each of which I think reinforces what they talked about. Some of this is drawn from our current research, some from experiences. I've been doing interviews with firms that are sponsors of NSF Industrial Research Centers Program. I think this would fit the generic kind of infrastructure, the type of knowledge activity, not necessarily to specific products.

The literature on the contribution of IRCs to industry includes many cases like the one I am going to cite. IRCs have to do cross-cutting research, which in some sense is generic, but is not too close to product development, although there have been a lot of pressures on the centers to move to short-term products.

The example I cite is a distinguished university's engineering department coming up with what the industry people said was a major breakthrough in laser optics. It fundamentally changed their production process, essentially, and provided great reductions in cost, and increases in reliability beyond their R&D frontier. It led to major contracts, increases in sales. It was clearly of value to the faculty engaged in this type of activity. It would fit very closely this concept of infrastructure. Doing research which is closely tied to industry but not necessarily product-oriented. The interesting part of this kind of vignette, which goes beyond the traditional justification of IRC's, is that the public nature of the findings. This firm also worked for the Defense Department, and other firms in the industry, in order to remain competitive, also had to quickly adopt this technology. It essentially became the standard for the industry, not for the firm, the standard for the industry, with spill-overs to many other product lines.

The other part of this is that the firm that was the initial sponsor of the IRC, soon thereafter went out of that line of business. It was simply as part of corporate strategy re-deployed. The benefits, or the beneficiaries of the IRC activity or other firms in the industry, and presumably other consumers beyond the initial consumer.

To me this represents the kind of civilian technology policy that captures much of what Dick has said and I think it clearly is a success story. It happens to come out of NSF, not out of Commerce or DOD. Again, I cite it to highlight the diverse sources of support for this type of activity. The downside of this vignette is that, as we conducted interviews with firms that belong to IRC's, they will site a wide variety of benefits, many of them considering that their investments have brought high rates of return, but are very uncertain about whether or not their firm will continue its investment, simply because of the fluidity of corporate strategies, and simply because of some of the cutbacks in R&D expenditures.

They'll also note that they place great value on the cross-disciplinary training of students who participate in IRC's. Again, the linkage between research and education, the ability of these students to work in team projects and conduct collaborative work. But essentially they say, "we haven't hired too many of them because we've been cutting back on our R&D labs." So, there is difficulty of maintaining the viability of a system which seems to be working quite well.

Let me cite another example, which in some ways hasn't been discussed at all, and that is the NIST manufacturing extension partnership (MEP) program. This, essentially, is to create a national network of technical assistance programs to small and medium size firms. The goal here is not product-specific or industry-specific, although there is some targeting. It is process-oriented, it seeks to provide information, knowledge to firms, in most cases, to make better use of existing knowledge. This is a Commerce program. It's modeled very closely on the agricultural extension program, with some notable differences.

First, by and large, these centers are not housed in universities. Some are, but the larger number are not, in part because the early experiences with these centers suggested that universities were not well suited to provide the very mundane, detailed operations that firms needed. And input, for political reasons, namely that where the land grant university was located was not close to the concentration of industry within that state.

Penn State is located in State College, PA. We are not exactly an industrial center of Pennsylvania. Another aspect of these MEP's which distinguishes them from the Agricultural Extension [Service] is the concept not only of shared funding between the federal and the state level, which is closely modeled on the agriculture case, but the assumption that there will be fee for service; that is, that there are private gains from these activities. Although I'm not sure if it's essentially market failure issues here, or simply the fact that the federal and state governments want additional revenue sources, which are leading many of these centers to impose fees for service.

Which comes to a very interesting kind of technical point in how do you fund these types of programs. The principle of leverage now permeates almost every federal industry, public sector/private sector program, and each cites the other's contribution as justifying the program to its own funds.

In competitive markets, a lot of the gains of these manufacturing technology centers may not be realized by their users or clients, but by other firms. The ultimate beneficiaries may not be the clients of the center, but the firms that buy from clients; and eventually consumers may reap the benefits of lower prices. We really don't have good mechanisms right now for funding these organizations on a long term basis. In the current budget climate, and with the view that civilian technology is close to the market and therefore the private sector should be paying, we run the risk of minimizing the needs for sustained federal funding of these programs.

For example, manufacturing technology centers (the Hollings Centers are now the larger program) were funded on the expectation there would be three years funding, a review, then another three years funding. There is, basically, a termination of the federal funding with the state and industry picking up the difference. In terms of the long term liability of these programs, I would suggest we need to rethink the funding again, in terms of maintaining the core principles.

Let me give you one third and final illustration of what I see as examples of civilian technology policy which provide for infrastructure and yet create new problems of their own. And that is the linkage between federal, state, and university or industry. I would assume if Governor Celeste has been. here, you have heard a good deal about this. I'm going to give you a very mixed assessment about these programs, but I think it's very important to get it on the agenda, because when we talk about a national civilian technology policy, we will increasingly focus on the contribution of state governments to civilian technology, and especially the need to coordinate the relationships between federal agencies or federal policy and state policy.

The example I cite, which I heard in an IRI conference not too long ago, was of a set of Pennsylvania State University researchers who received funding from the NSF Materials Research Program. They had, over a good number of years, developed great expertise, coming up with findings that were anomalous. They were rather marginal to the research agenda of the core faculty, but seemed to have some commercial interest, which linked up with a small firm in Pennsylvania.

This was a small high tech firm that was looking to broaden its product line. Using state funding from the Ben Franklin Partnership program, they put a graduate student to work to develop the technology contained within this finding. This in a sense, led to a new product which broadened that firm's product line. This is an example of civilian technology policy involving both federal and state partnership. Starting from generic research, being picked up and conducted through the university, again being picked up by a firm for further product development. I predict that we will see a lot more of this.

There are two risks inherent here. One is that the state policies come very close to being too close to the market. Several of my writings have been to challenge a lot of the state programs because they have been too close to the market, or I have argued the programs such as in Texas, or the New York State Centers for advanced technology. They have been effective in good part because they have been essentially state models of engineering research centers, but in fact have broadened the constituency. There's a great risk of state programs being too close to the market, and I've chastised my own state program for being of that type, which is why I tend to make most of my presentations outside of Pennsylvania.

There's another risk here, in addition to amount of euphoria or romanticism about the magnitude of the state programs. There are the beginnings of a new federalism linear model of technological innovation. That is, the federal government is to fund a basic research and generic applied research, while states are seen as the delivery arm of product development.

That is a caricature of what states do, and to do that would greatly diminish the federal contribution. So, while I am very supportive and while I encourage you to look at the state initiatives, I would suggest one do this with a certain amount of skepticism and not be caught up in some of the rhetoric.

Let me make one final point. I would take the title, the subtitle of this conference, "Learning from the Past, Designing for the Future," and also focus on learning from the future and for the future. These programs are highly politicized. They exist under exacting scrutiny, as all new programs do. How we evaluate these programs becomes a very important theoretical, methodological, and political issue. There have been many calls for scientists to become increasingly involved in educating elected officials about the contributions and the processes of science and technology.

I would also suggest there is great need for the research community to become more attentive to the means by which these programs are evaluated. I'm not focusing here on quasi-research designs. I’m talking about the very essence of these issues. What comes to mind is a session I organized for AAAS two years ago on implementation of the Government Performance Results Act. One of the speakers I had there was Paul David, who justifiably raised a lot of skepticism about the methods for this.

I am very much concerned about the bureaucratization of evaluation of these programs. We are tending to fall into the trap of having a set of very conventional measures – patents, licenses and the rest – which, while they have a modicum of economic content, economic relevance, can become so important at the bureaucratic level that we lose sight of the processes that we are trying to stimulate. Thank you very much. (APPLAUSE)

CROW: Thank you, Irv.

Over the last ten years or so, it could be easily said that if the topic was technology policy or the topic was resources for new initiatives related to technology, and it was Washington, all roads led to Pat Windham. Pat Windham, through his service to Senator Hollings and Senator Rockefeller, as well as to other members of the committee that he served for 12 years now, has been at the apex of the evolution of technology policy in the United States. And we thought it might be interesting to hear from him a little bit about where things ought to go in the future. So, Pat.

WINDHAM: Thank you. I'm very pleased to be with you today.

I need to emphasize a standard proviso that goes into all congressional staff speeches, which is I'm speaking for myself, not necessarily my bosses. In the interest of time, I'll be very brief and say that I'm going to focus on two subjects. One is a bit of the background, very quickly, on where Congress was coming from, at least many Democrats and some Republicans, in doing civilian technology policy since 1980.

And then with apologies, Dr. Feller, I’m going to focus on one particular program for a few moments, the advanced technology program, which is the one I know best, and talk a little bit about some of the design issues that we had as we put that together and that the Clinton administration faced as they scaled it up – what choices were made and the pros and cons of those choices.

The background, and I'm going to be a little provocative here, is that by 1980, for many people in Congress and some in the outgoing Carter administration, science was not enough. Or, rather, science policy of the traditional sort was not enough.

My boss, Senator Hollings, had a particular line that struck him as capturing a lot of this. He said, "We in the U.S. get the Nobel prizes. The Japanese get the profits." And so the question was, what was going on?

In terms of federal R&D spending, you could take a look at it and notice a couple of things right away. With the exception of some dual use programs – NASA aeronautics, electronics at the Defense Department, et cetera – the United States Government, for all of its R&D spending, was practically putting zero funding into general industrial technology development projects.

In fact, for many years in the official OECD documents, based on reporting from the U.S. government and others, one tenth of 1% of federal R&D went explicitly into what was called General Industrial Development. The corresponding figure in what was then West Germany, for example, was averaging 13% to 15%, fairly typical for most other countries.

This isn't to say that government spending, by any means, is the solution or silver bullet to industrial competitiveness, but we began to see something of an imbalance as the U.S. economic lead from the World War II days, economic hegemony if you will, was beginning to slip. So there was an interest in what was important in terms of priorities. Briefly, there was also the whole question of where were the problems.

We saw and continue to see – in fact, it's accelerating – what we think is something of a market failure in the generic, enabling, precompetitive, whatever you want to call it, areas of technology. Areas in which both entrepreneurs and even larger companies enter what some of them call "the valley of death," where they have a promising idea, but the rate of return is such that they're not going to be able to get major investment for it.

Starting about 1980, three types of civilian technology initiatives were undertaken, an arbitrary classification but one I want to mention. One was to try to build on existing investments and make them more useful to companies, frankly for job creation and economic growth. The engineering research centers and S&T centers that Bill Harris started at NSF are a sterling example of trying to make the university investment better linked to industry. And there is a lot of analysis on that, I think generally helpful. Some limitations we've seen in GAO studies and elsewhere, but a step. Federal Lab Tech transfer, that my friend Mike Telson works on at the Department of Energy, and others.

Second, dual use efforts of the Defense Department. Craig Fields was doing that until he was fired in 1990. Jesse F. Bingaman, Jr. (D-NM), who is one of the key technology policy architects in the Congress through his position on the Senate Armed Services Committee, played a role first in creating Sematech and ultimately things like the Technology Reinvestment Project.

And third, to try to actually create some new explicitly civilian programs. Ones that were more broadly based on industrial sectors, but not necessarily sector specific, like NASA aeronautics or agriculture. And some of those were the ones that Senator Earnest F. Hollings (D-SC) and Congressman George E. Brown, Jr. (D-CA) started, of which the ATP (Advanced Technology Program) and the manufacturing extension partnership, which is more for smaller firms, are the two main ones.

Let me briefly hit some of the questions we had, as I said earlier, when we started ATP, about design choices – what were we going to focus on? how were we going to run it? – and what the Clinton administration has done, because it illustrates, I think, some of the choices that people have.

The first issue was, what part of the R&D effort should we focus on? Again, there was the assumption that university research itself was not automatic. This is particularly true in those sectors of industry that are less, if you will, science driven.

If one is talking about biotechnology, clearly, there's a very rapid and, we think, fairly easy transfer from university research into the commercial area. For a long time in semiconductors, there was some of that as well. If you're talking about the auto industry, the textile industry, and many of the others, the question was, what do you do for those?

The feeling was that new innovations were not coming to fruition as quickly as for our competitors overseas and that, in fact, while we're now doing better relative to Japan in many areas, we have the problem Dr. Fusfeld mentioned, which is that all companies are pulling back from their longer term or mid-term R&D into more product development.

So, we wanted to focus on that part of the R&D enterprise. Who did we want to do this research? And here I want to be provocative. We did not want universities, per se, although with a caveat I'll mention in a moment.

The advanced technology program and the TRP, some of their programs, were focused on companies, either individual companies or joint ventures. The reason for that was quite simple. The companies are the ones that are going to be making the products in the end, creating the jobs, we hope, competing economically and making the country stronger. It's important to build up their core competence and, under proper guidelines, to trust them to decide where that research should best be done.

In many cases, the companies in the ATP have chosen to work with universities, which we think is great. In some cases, also with federal labs. The ATP is a battered program politically right now, but we've had 280 awards since my boss was able to get funding for the program starting some years ago. Of those, over 160 involved universities, which we think is great. The idea has been to let the companies decide how best to do the early stage development work on their technologies, where to source that help and so forth.

Secondly was a major question about whether we wanted to go sectoral or not. And here there is a political contradiction, with which we are intensely living with. The sectoral programs that we do have are, as I think Dr. Nelson said, exactly right. Successful if they attract and help all the major companies. If you're dealing with aircraft, for instance, aeronautical research at NASA.

That program's not under much attack, except a little by Congressman Robert S. Walker (R-PA). One reason is that all the major companies in the aircraft industry participate. It turns out it's four. Boeing, Macdonald Douglas on air frames, and GE and United Technologies on engines. And some of their suppliers. In agriculture, some of the same sorts of things.

The ATP though was designed partly because it had less money, could not take on something for everybody, but also because we thought we wanted to go with enabling technologies that would help a range of industries – breakthroughs in electronics or other areas – to deliberately not be a sectoral policy.

Furthermore, because it's a competitive program of limited awards, a bit like NSF, no one company knows that it's going to be a guaranteed winner in future years. In my world, that creates certain problems of building a political constituency. But it may still be the right investment to make with the limited number of dollars.

In terms of the governance structure, an issue that Dr. Nelson raised and got me thinking about after I read his notes earlier this week, there has been a move in the Advanced Technology Program at Commerce the last couple of years to go with industry-nominated, but if you will, peer-reviewed focused program areas. Not focused on a particular industry, in most cases, but on a general area of technology – composite materials, advanced manufacturing for electronic components, and so forth. Industry will nominate those areas.

There'll be an intensive review based on criteria to whether there are breakthroughs that are possible here and a relevant role for the government, as opposed to industry already doing it. Then those areas are picked, and specific competitions are held within those. So there's an attempt to get industry input, but also to put it through rigorous evaluation. Again, that sometimes means the industries don't entirely feel ownership, but we really get fairly few complaints from industry about how that's done.

Finally, there was this question that in 1987 and '88, when we did this, was relatively new, as to whether these kinds of programs should involve direct contracts or grants to companies, or whether they should be cost-shared partnerships. The decision was made early on to try to make them cost shared, partly to make the industry participants feel they have a stake in it and in making the results viable. We wanted more research then simply what a dollar of federal money could buy. And I think our experience so far is that that approach has worked fairly well.

I'm going to quit there, because of the time shortage. But the point is, we have been grappling with these design choices, which have not only programmatic impacts, but in this Republican Congress, have had political ramifications as to which ones are broadly supported and which aren't. We're continuing to experiment, not just with the Commerce Department programs, with others as well, and if any of you who are the real experts on much of this stuff have suggestions or inputs, we would genuinely welcome that. We're trying to learn as we go. Thank you. (applause)

CROW: We'll have time for a couple of short questions. Using the "sled dog" references we've heard a couple of times today, this is definitely the Iditarod of science policy meetings and so with that in mind, we're coming up on Nome here, on our race from Middle Alaska. So first question.

WORTHAMMER: I'm Richard Worthammer. And I want to compliment the speakers on what I thought were very informative and pertinent talks. I was listening for two things that I didn't here, though, and since I feel they are significant for the subject, I wanted to at least put them out on the table. First is that the government statistics focus, as has been said earlier in the conference, on R&D and the manufacturing sector, predominantly. And yet R&D in the service sector has been mushrooming tremendously.

You look, for example – this was an area that I've had some experience with in the last six months – at project development in the financial services industry. Banks, security firms, et cetera. Staggering amounts of money, billions of dollars are being spent on computer systems, telecommunication systems.

Now the purist may say that developing a very, very large-scale computer system is not R&D. I think that's a little hindsighted. With the extension of the definition of R&D to large-scale computer systems, there's an enormous amount of money in the service sector in that. So, this is an aspect that I think more attention needs to be paid to, as to the amount of R&D going on in the U.S. It's also highly competitive. It makes the U.S. very competitive in the world markets.

Second point relates to small business innovation and research grants, which creepingly, over the years, have gotten to be very significant sums of money as fixed percentage set-asides from agency R&D budgets generally – if 2% of an agency's R&D money must be spent on SBIR grants. That's quite a boost to the civilian sector in contrast to the ATP program, which is on Congress's radar screen taking a lot of flak. I think the SBIR program has been flying low under the radar, not getting a lot of criticism, and yet there's substantial bucks there going really into small business innovation, I think, very effectively.

CROW: Any brief comments?

MALE VOICE: I would just make one comment on SBIR. I just happened to hear a paper at a recent conference which suggested there may be significant displacement effects of SBIR. That is, firms that win SBIR awards cut back on their own internal R&D. Or, if you like, the product they produce is SBIR proposals. I think there is a question here of the net impact of SBIR to aggregate industrial R&D. It's a new finding, obviously, and not a politically palatable finding. It does show, I think, throughout everything we're talking about, the impact of distributive politics of civilian technology that we heard about from Congressman Green. I don't think the programs we're talking about are any different. I think it's one of the appeals of the manufacturing extension partnership program.

LAIRD: I'm Burgess Laird from Los Alamos National Laboratory. And I have a specific question for Patrick Windham. You pointed out that science is not enough, but competitiveness is not enough either. Such was, I believe, the guiding motivation behind support of the partnership for a new generation of vehicles. That is to say, the PNGV (Partnership for a New Generation of Vehicles) program was not trying to respond to the question, what is it that we can do for the auto firms to improve their competitiveness? But, what is it and how is it that we might collaborate with and provide incentives to firms that are under-investing in research and development for a generation of vehicles that global population trends and economic development would seem to make imperative?

My question is very direct. What are the prospects for the PNGV program?

MALE VOICE: Well, let me tell you a little story that illustrates how these go. This is a program that is largely at the Department of Energy but is supervised by Commerce. It involved EPA and others and is largely designed not for economic competitiveness, per se, although we hope that'll be an effect, but for fuel efficiency and improved environmental performance. We hope to go to, I guess, triple fuel efficiency of cars, at least in demo models.

That program was targeted early on, particularly by the main critic of Commerce and Energy and other programs, Congressman Bob Walker of Pennsylvania. It turns out Mr. Walker's favorite hobby is weekend auto racing. He was invited to do some test track work up in Michigan and was briefed on the program and, after that, he no longer criticized it. As a result, the program itself is not being targeted. However, because the Energy Department is the main partner here, the fact that they're civilian related programs is being particularly targeted by this Congress, and bodes not too well in the long run. Mike, do you want to add anything on that?

MALE VOICE: That particular Mike of the 47 million Mikes in the United States is Mike Telson, of the Department of Energy. David. (chuckle)

BECKLER: David Beckler of Carnegie Commission. I sense a divergence of view between Mr. Windham's feelings about the ATP program and the other speakers who were arguing for more generalized assistance to industry.

I must confess, my convictions are in favor of the more generalized support, because I don't see what the macro impact would be of support to individual companies for rather narrow types of product development. I do see something in between that you didn't discuss, and that's where my question's really addressed, where the project is of such scale that you really have to engage industrial companies, because of the nature of the work.

I have in mind, well, for example, the high-priority President's program for cooperation among automobile companies on energy-saving, low-polluting automobiles (chuckle), which would presumably be beyond the media targets of those companies. We have the good examples of the MCC and Sematech. One could think of the space program as another example, where, to get the satellite surveillance programs going, you really have to deal with large hardware contractors. I see, on the horizon, a lot of work in the superconductivity area, large-scale demonstrations.

So there is an area where I would like to ask the panel whether there is a legitimate role in working perhaps through consortia of companies.

CROW: Okay, Bill?

HARRIS: Bill Harris from NSF. A general question really, as you go through this, seems to me to be intellectual property rights. When I go around the country, that's a debate that I get involved with, whether it be a university or industry. I don't know if there's been a lot of thought to that by the panel members, but I'd sure appreciate some comments.

CROW: Those kinds of questions are very nice. (chuckle)

QUESTION: Mioto Dorage, CUNY. My question was already started. I'm just making it more specific. Is it the opinion of anyone in the panel, the industry think high kelvin superconductivity is something promising? Is the government putting sufficient money to pursue this idea?

SALVATO: Michael Salvato, DPI International. I was just wondering about the government and the industrial perspective on the impact of globalization. In particular, the international distribution and product development and manufacturing – any cautions or assumptions that should be built into civilian technology policy related to that?

CROW: So, any comments on any of those questions from panelists? Dick?

NELSON: Let me respond to several of the comments and questions. First, I didn't talk about ATP because I didn't want to get into a direct discussion of the kinds of government programs that put money into individual firms, pretty close to commercialization. But by talking about those kinds of programs in general and laying out the kind of desiderata that I thought was appropriate, I signal relatively clearly that I don't think that's the way to go. I think that that is not a good path to follow.

Regarding David Beckler's questions about large-scale research, opening up new technologies like in superconductivity that very appropriately ought to involve private companies as well as universities – yes, I think those definitely fall appropriately within the province of government technology policy.

I think, again, the ground rules there to signal my response to Bill Harris' question are that in general, while companies that were engaged in such work might be able to make some intellectual property out of it, the general stance of a government program with respect to an industry, is that what comes out of it is to be shared relatively broadly across the industry and among the various participants in the program. I want to come back to this double issue of governance. Regarding these kinds of programs, I think there is an important issue regarding mechanisms for designing the program, deciding the high-priority areas and choosing how to make allocations.

As I indicated, I think that has to be done by a body that involves people from industry and people who are not from industry. I have a conjecture that, though I'm not sure about this, the National Academy structures might be an appropriate one for that kind of operation.

What puzzles me a lot, however, is what government agency should be involved in some sense, as the conduit for, the lobbyist in Congress for, and the dispenser of these funds. Department of Commerce is, from one point of view, a plausible place and, from another point of view, a very implausible place. And those of you who have been involved in this discussion know exactly what I mean.

FUSFELD: Well, since Dick tackled several of these, let me address the one on globalization and so on. One of the interesting things to me is – and now I'm looking probably five to ten years ahead at least – there's a steady increase in the location of plants and markets that are outside of the industrialized OECD countries. There's a gradual accompaniment of those by at least engineering and, if history is any guide at all, eventually R&D. Today, probably 95% to 96% of all the industrial research in the world is done in the industrialized countries, but ten years from now that could be only 90%. Well, that means twice as much innovation will be coming from these other countries as they build up their own markets.

However, one of the effects of that could well be that as new products and new processes enter the world market, coming from nations outside the industrialized countries, it will step up the demand for more R&D inside. So I think in the long run, that type of activity will stimulate more industrial research in the United States among other countries. I can't answer the economic questions having to do with the labor markets. That's clearly the reason why many of the companies are going there, but there is a secondary effect of R&D also gradually going out and that will have an impact on the United States directly.

CROW: We're going to take two minutes to set up for the last panel. If you'd like, get something to drink and, so we can get out of here in short order, come back to this room. So let's thank this panel (applause) and if the next panel could come up, I would appreciate that.

Design Area Eight:
Federal Laboratories and Other Assets of the Last 50 Years

Barry Bozeman
Nick Samios
Michael Telson

Moderator
Michael M. Crow

CROW: Design area number eight focuses on federal laboratories and other assets that, in a sense, have been developed as a legacy to the Bush design. Just let me throw out a couple of numbers to put that into perspective. In the spring and summer of 1945, when Bush was putting together *Science: The Endless Frontier* with his panel, there were about 30 research universities in the United States, five of which he identified as mature. There were about 500 R&D laboratories in the entire country. And outside of the technician class of individual working as a part of the Manhattan project, there were fewer than 15,000 scientists operating in the United States, at that time. We now have 150 research universities, 800 federal laboratories, 16,000 or more other research laboratories. Ten thousand of those institutions receive some funding from the federal government.

That's an institutional legacy of immense proportions. As one thinks ahead about what one needs to do in terms of the design of science policy, it's one thing to change policies that affect the tax code or what have you – it's quite another thing, as we were learning in the biomedical area, to change public policy where it affects institutional structures that are numerous and large.

Our panel today, we have shrunk to three people, quite purposefully, because we know everyone's at about their limit. (chuckle)

Our first speaker is Barry Bozeman. Barry is the director of the School of Public Policy and professor of public policy at the Georgia Institute of Technology. Prior to that, he was the director of the Center for Technology and Information Policy at the Maxwell School at Syracuse University. And for the past 13 years, he has been the co-director of the National Comparative Research and Development Project, a multinational project involving more than 30 researchers, focusing on the structure and policy environment of R&D laboratories in several nations. And so Barry will talk to us about the future.

BOZEMAN: Federal labs came under siege when the Republicans won a majority of seats in the House of Representatives in November, 1994, and Newt Gingrich became Speaker. Already reeling from an outbreak of peace, the last thing the federal laboratories needed was political leadership devoted to the arcane 19th Century, nihilist political principle, "Let's blow it up and start all over again." As it turns out, very few labs have been blown up, the only significant one being, appropriately enough, the Bureau of Mines Explosives Testing Lab in suburban Pittsburgh.

However, federal laboratory personnel cannot feel too much at ease as long as influential members of Congress have the Departments of Energy and Commerce in their gun sights.

Federal labs have been victims of social and political forces over which they have no control. They have also, in many instances, been their own worst enemy. Federal laboratories have been accused by the General Accounting Office of waste due to poor accounting practices. The Department of Energy's Inspector General's Office accused the Department's federal labs of mismanaging cleanup of contaminated land. A whistle-blower who called attention to the vulnerability of a nuclear plant was demoted. Perhaps strangest of all, a ghost from the 1950s came back to haunt us as we found out about insidious nuclear experiments being performed on non-volunteers.

My personal favorite came from *The Consumer's Digest*. Amongst all the product reviews of leaf blowers and new cars, there was an article about the war on Washington waste. In it, *Consumer's Digest* complained erroneously that the Energy Department spends one-fifth of its budget on cooperative energy development programs, giving money to firms like General Electric and Westinghouse to support research so they can turn a profit.

I do not want to blow up federal laboratories. That may be an extreme position these days, but to me, it makes about as much sense as blowing up land grant universities because we no longer have a predominantly agricultural economy.

In my view, the fate of the U.S. federal laboratories is a matter of great consequence. Whether or not you agree with a former laboratory director that federal laboratories are "a reservoir of scientific and technological talent that can help to compete in international markets," whether or not you are impressed with the Nobel laureates working in federal labs, the resources devoted to federal laboratories have to command attention.

More than $20 billion per year is spent on R&D for the 627 federal R&D laboratories, which amounts to about one-third of all federal R&D funds expended. Federal laboratories employ nearly 60,000 scientists and engineers, a significant fraction of the U.S. scientific and technical resource base. In addition to producing tens of thousands of scientific and technical papers each year, federal laboratory personnel file nearly 1,000 patent applications. The range of functions performed by federal laboratories is remarkable.

The core functions of such mega-labs as Sandia or the Naval Research Lab are familiar. These labs are involved in a wide range of activities, many of which stretch well beyond the core concept of their missions. If the largest labs receive the lion's share of attention, 700 or so less visible federal labs undertake an even more diverse array of scientific and technical tasks, ranging from collecting and analyzing seed samples at the U.S. Department of Agriculture's National Seed Storage Laboratory in Fort Collins, to devising building materials that will resist terrorist attacks at the Army Construction Engineering Research Laboratory in Bloomington, Illinois. Federal laboratories are engaged in research at every point on the spectrum: basic, pre-commercial, direct, applied, development, and testing.

My objective is to assess and add to the list of ideas about policy change in the federal laboratory system. Before doing so, I am going to outline some of the characteristic flaws in policy frameworks that have been used to analyze R&D policy in the United States. My perspective on this has been developed during my work under the aegis of the National Comparative Research and Development Project (NCRDP), which was begun in 1984 and involved researchers in four nations on a wide variety of technical reports and papers.

During nearly 13 years of work in the NCRDP, we interviewed or sent questionnaires to more than 1,000 scientists, science administrators, and science policy makers in Japan, the United States, Canada, Russia, Korea, Germany, and England. We visited R&D laboratories of every sector and stripe: industry, government, university. Many of these include the largest R&D laboratories, including Lucky Goldstar in Korea, the National Institute for Metals in Japan, and the Brookhaven National Lab. We also spent a good deal of time in the hinterlands, the Fort Keough livestock research center and the Chalk River Atomic Laboratory.

There have been three predominant science and technology policy paradigms in the United States since the beginning of our science policy. The market paradigm for science and technology policy and its attendant economic development implications is based on familiar premises, that free markets are the most efficient allocators of goods and services, and that left to its own devices, an unfettered market will lead to optimal technology and economic growth outcomes. Most policy in the United States, not just laboratory policy or science and technology policy, is strongly influenced by the market paradigm. This paradigm is alive and well.

The mission paradigm has been particularly prominent. The earliest government involvement in science and technology policy was within its framework. The mission paradigm assumes that the federal laboratories' role in science and technology should flow directly from legitimated missions of agencies and should not extend beyond those missions in pursuit of more generalized goals such as technology development, innovation, or competitiveness goals. As such, the mission paradigm is not radically different from the market paradigm. Its roots can be traced to early government involvement in national defense, public health, and, to some extent, agriculture. The mission paradigm is alive and well – witness the Department of Energy's "Alternative Futures for the Department of Energy National Laboratories" (Galvin Panel 1995).

More recent is what I call the cooperative technology paradigm. During the economic downturn of the late 1980s and a perceived crisis in U.S. competitiveness, many of our core assumptions began to be examined, including the bedrock faith in the private sector as a source of all innovation. This was particularly the case as other nations, especially Japan, began to take a different tack and have some success in technology development.

During the 1980s, a number of policy initiatives challenged the preeminence of the market paradigm with a new model, the cooperative technology paradigm. As I use the term, the cooperative technology paradigm is an umbrella term for a set of values that emphasizes cooperation among the sectors: university, government, industry, and cooperation among rival firms in development of pre-competitive technology. Today, the cooperative technology paradigm is alive, but on support systems. It's not doing so well.

The time has come for a new paradigm, one I call the institutional design paradigm. It is oriented toward resolving three major problems that permeate policy making in the United States pertaining to federal laboratories.

First, and probably most important, is a poor basis of empirical knowledge about laboratories in the United States. Not many people even know there are over 16,000 of them. We are concerned about licenses that come out of the federal laboratories, but don't know how many came out last year. In the interest of managing laboratories, we might want to know the administrative intensity level, or the ratio of administrators to scientists. What is the average level? What would be a good level? Nobody knows the answer to questions like that. While we know a great deal about specific labs, we have a very poor empirical base of the system as a whole. We know a great deal about specific sectors, but very little about the system as a whole and its mechanics. That is problem number one.

A second problem is what I call the hazards of stereotyping. It is no longer possible to try to define a "government lab," versus a "university lab," versus an "industry lab." The truth of the matter is, there is as much variance within sectors as there is across sectors. Increasingly, assumptions such as universities are for basic research or industry is for development and commercialization of technology run at odds with the configuration of research resources that we have in the United States.

The third problem is too much ideology and not enough pragmatism. In many instances, the reasons that discussions of science and technology policy in federal laboratories seem to push people into ideological corners is that ideology becomes a sort of a shorthand for a lack of empirical knowledge. It helps us keep a handle on assumptions that we want to make in policy making, in the absence of any empirical knowledge about the outcomes and effects of particular policies. The institutional design approach was developed to try to alleviate some of those problems that are characteristic of policy making for science and technology.

The institutional design approach for science and technology policy is based on just a few straightforward principles. The *player principle* says that most R&D organizations in the United States should be ignored. Most R&D organizations in the United States, more than 10,000, are basically small engineering job shops run out of firms. They may be very helpful to the firms, but they are not particularly innovative and do not contribute to national innovation.

On the one hand, we can ask, "With 16,000 R&D laboratories, how are we ever going to understand enough to make empirically-based decisions about them?" The answer is, "We don't focus on all of them." Because, in fact, there are only about 500 or so that really have the potential to contribute to the national innovation system. This is particularly so if we exclude the handful of small firms that are producing most of the innovations.

The second principle, the *systemic principle*, is that we need to know something about the dynamics by which laboratories inter-relate and respond to environmental change. If we want to understand the impacts that public policies will have on laboratories and not just science and technology policies, but tax policies or labor policies, we have to understand more about the system as a whole.

The *never in neutral* principle says that when we implement public policies in laboratories, those policies are never going to be neutral with respect to existing functions. For example, if we provide a manufacturing extension function to federal laboratories, it affects the preexisting mission of the lab. The work we have done trying to assess the impact of industrial partnerships with federal laboratories has certainly made that clear.

The *comparative advantage principle* says that public policy should be differentiated, targeted, and based on a lab's capabilities and proven areas of effectiveness, not its particular affiliation with respect to agency or sector. Laboratories, quite simply, should be reinforced for doing what they do well. If we want to talk about downsizing or closing laboratories, the reason to close them is because they are not doing well what they are supposed to be doing well.

The *opportunity cost principle* has more to do with the way we should evaluate federal laboratories. It is actually a pretty complicated notion about evaluation, which is that it is not enough for a laboratory to show a positive marginal cost benefit ratio. It is not enough to be able to say that this money was expended in a certain way with a certain multiplier effect. The real question is, "What would have happened if the money had been expended in some other way, particularly ways in which money is already being expended by the laboratory?"

The problem with the institutional design approach is that there are a number of prerequisites, most of which are not now in place. One of the most important prerequisites is a greater knowledge of laboratory assets, capabilities, and performances. Most efforts to measure the assets of laboratories have met with little success. In our own efforts, we have focused on certain areas, but there are wide gaps in the kind of knowledge that we have been able to develop.

Another prerequisite is greater coordination and coordinating apparatus. If we are going to implement an institutional design approach, greater coordination is absolutely required. That does not necessarily mean coordination by bureaucrats, but a variety of stakeholders should be involved in coordinating federal laboratory change.

An additional prerequisite for institutional design is a reduced role for line agency management. I have seen nothing to convince me that the federal laboratory systems' agency affiliation is rationalized in terms of mission or management structure. There is relatively little flexibility even now and not enough decentralization in the federal laboratory system to allow the implementation of an institutional design approach. If we are going to get serious about changing federal laboratories, we have to identify likely agents of change and provide the resources and political will to help federal laboratories fulfill their enormous promise.

CROW: Here is Nick Samios, a physicist. He's been the director of the Brookhaven National Laboratory for about 15 years, prior to that working in a wide range of physics areas – those that are the most complicated, I might add. Nick's going to give his perspective from the seat of a national laboratory director.

SAMIOS: As you noticed, the paradigm changed, and mission went to industry mission and so on, and being lab director through all this is quite a job. And in fact, in my 15 years, I never had a budget at the beginning of any fiscal year. So managing a DOE lab or any federal lab is difficult.

As you've heard, the federal lab complex is rather diverse and under many agencies: Department of Agriculture, Defense, NIH, NSF, and DOE. One distinguishing feature between the DOE labs and the others is that the DOE labs are so-called GOCO labs: government owned but contractor operated.

A comment was made earlier today that it would be nice to separate the government from running the laboratories and that this was wisely done by the Atomic Energy Commission, many years ago in 1946. The other labs are GOGOs, meaning government owned, government operated. In fact, John Deutch, when he was at the Department of Energy, had a review of his GOGO labs about four or five years ago, and the answer was, change them all to GOCOs in order to increase their efficiency and yield of science.

In the discussions here, everyone's familiar with the GOCOs, NIH has been discussed at some length, the NSF has been discussed at some length, so I'm going to concentrate my remarks on the DOE labs – one, because I'm more familiar with them, and secondly, there seems to be really a lack of knowledge on the part of many people of what they are and what they do. Let me take my remaining time to discuss them.

The DOE labs, which are not large in number, break up into three large categories. The first are the defense labs that have been tasked with worrying about national security from the beginning, Los Alamos, (UNINTEL) and Livermore. And now they're going into weapon stockpile stewardship, so their mission is rather straightforward.

Second category is what I would call the single-purpose labs, those that have a single, well-defined mission such as FermiLab, which does high-energy physics, and Stanford Slack and CBATH lab at Virginia, which does nuclear physics.

What they do is really defined by their missions. National security goes out the window? The national security labs have some difficulty. If high energy goes out the window, then these other labs have to do something.

The labs that are more confusing are what I would call the multipurpose labs, which also rose after the Second World War – laboratories such as Oregon, Oak Ridge, Berkeley, Brookhaven, and Pacific Northwest. They are more complex and, as a result of the AEC coming in, these labs did a multitude of things – very little in national security but in the energy domain, in nuclear medicine, and so on.

They evolved for another reason also. One of the main reasons for these labs is the ability to design and construct very large facilities – large facilities that universities and industry acknowledge are too big for them to manage.

So I'd like to make a few comments about this capability of the DOE labs. First, the ER part of DOE, the part that is responsible for research, is in funding comparable to the NSF. Its budget is \$2 billion to \$2.5 billion a year. That's the first secret that people don't understand. In the research area, DOE is as big a player as, if not a bigger player than, NSF.

The second comment I would make is, most of these labs, nearly all of them, operate under a peer review system in the sense that DOE sets up committees to review them with outside gunslingers mainly from universities. The contractors who are responsible for the health labs do that whether they're universities such as University of Chicago, University of California, or consortia such as associated universities and University Research Associated, AUI (Associated Universities, Inc.)and URA (University Research Administration).

Third, universities are starting to realize that the single-investigator mode has seen its better days and that one should have groups of people working together and go for block granting. This occurred at the national labs years ago, and most labs are getting their scientists to be less and less individual investigators and more and more in groups.

Furthermore, to evaluate how good the science and international labs were, there have been two national studies over the last six years, one in materials science and one in nuclear physics, where the research of the group of national labs and universities as funded by DOE and NSF was evaluated – and if anything, the DOE research was deemed slightly better, but you couldn't distinguish. So on all these things, I wanted to point out just as background that the research labs, the DOE labs have their Nobel prizes and so on, on a peer review system.

The other asset of these multipurpose labs is their ability to do multidisciplinary research. Universities know how difficult it is to get two departments together or make one appointment between two departments – but in national labs, setting up new departments, getting rid of old departments is rather straightforward. One has to be a little careful. I personally have probably wiped out at least three departments and created a similar number. Not easy but doable.

There's a synergism at these labs in a sense that there are things that one can play with and pull together. One good example is the evolution of light sources, the devices that produce intense beams of light. These came about at national labs because there was accelerator expertise there and expertise in chemistry, biology, and materials sciences. Now, there are comments that maybe we have too many light sources, but it certainly has been one of the great successes of the DOE system.

Even lately, the labs are working together, hard to believe but it's true. In the national security area, that’s been true although there's been competition between Livermore and Los Alamos. In fact, they were created to have some competition, but they have worked together in the national security area and rather successfully in the other areas, for instance,, in accelerators. Several accelerators have been built on the West Coast jointly between Stanford and Berkeley laboratory. Another example is a joint effort going on now to design and build a very large neutron source, a pulse neutron source at Oak Ridge and the laboratories involved are Oak Ridge, Brookhaven, Berkeley, Los Alamos, and Argonne. So we have synergism, and we have cooperation.

The name energy, Department of Energy, is probably a misnomer, it's giving us problems, and maybe it should be changed at some time. The business of the Department of Energy has been national security and science, at the tune of three billion bucks a year, but they also do energy and environmental missions. So they really have four missions. However, they're called energy labs, and so that causes some difficulty. Maybe a better nomer is science and technology national labs.

Although they're under attack, one has to look at their missions and what they should be tasked to do. One of the basic things is certainly long-term, high-risk research that has a general, national impact, which universities and industry can't do or won't do. It's scale. It's very hard for an institution even as great as Columbia, which built Nevis and I did my work there.

These facilities are really used by the community at large, both universities and industry. The number of users from universities in each area is on the order of four to five thousand. And in fact, the facilities are used in a manner that inside groups and outside groups compete on an equal basis, and I think that's terrific. I would comment that the light sources have been a great benefit to industry. If you look at the people who are utilizing them, you will find industrial participation at the tune of 30%. And that translates into hard dollars.

For instance, at the light source at Brookhaven, where we have 2,000 users per year, take a third of that, that's the industrial component – but equally important, industry has pumped in over $100 million in equipment on the floor. So I would say in these discussions of dismantlement, changing whatever we do, it should be done in such a way that we have a seamless transition that preserves these great national assets that we have in our national labs.

I'd like to close now with a story and an observation. The story is one told by Vice President Gore, and I read it in The New York Times, so it's got at least a 25% chance of being true. It seems that he was addressing the UJA (United Jewish Appeal), and he said, "The time came when the United States elected a Jewish President." And so the president got on the phone with his mother, who was in New York, and said, "Ma, you gotta come down, I'm being inaugurated." She said, "No, no, I can't do it." Finally, he said, "I'll send Air Force One to get you, I'll even send the Secretary of State to accompany you." "Fine." So she agrees to go down. And she's on the reviewing stand. He's raising his hand, his mother turns across to the Secretary of State, and says, "You see that man? His brother's a doctor."

I tell that because of the NIH exponential curves of funding, and I wish I had their problem. I think one thing I've learned here is that we've got to work together. As we've all said, we've had the great luxury of not having to go to Washington for the last 30 years to really battle as other fields for our funding. We got it very easily. They used to say, you're a great scientist, we love you. That's changed. What did it? Everyone agrees. Partially the end of the Cold War, but equally important is this agreement by the Democrats and the Republicans that the budget deficit at least on paper has to be closed by 2002. That drives the funding in many ways, especially in discussions with OMB. We are part of the discretionary funding, and that's getting smaller and smaller. So it seems to me all that points to working together to make our case, and we must make our case.

Peter Eisenberger asked, what are we doing about it? I have one positive comment to make on that. At least the physicists are convinced that we must work together. I chair the policy committee on the American Physical Society, and at one of our meetings, we set up a group to work with all other disciplines, chemists, biologists, to try to organize an effort whereby we make our case for all of science, not for our individual parochial disciplines. So I'm very optimistic.

In my interactions with Congress, even the Young Republicans, everyone agrees science is great. But then they have a choice, and you've got to give them the arguments why they've got to give us the funds instead of taking them away from us. So I think we've got a good story to tell, let's work together and do it. Thank you.

CROW: Our last panelist of the conference is Michael Telson. Mike is presently the special assistant to the Deputy Secretary of Energy; he's been doing that the last two years or so. For the 20 years before that, he was a principal advisor and policy maker to a range of groups within the Congress, primarily the Budget Committee on the House side, where he dealt extensively with issues of science, technology, energy, the environment, and so forth from a macro-policy perspective as well as a micro-policy perspective. Prior to his service in the House, and he may even be one of the earliest MIT Ph.D. s who has served on the House staff for a long period of time, Mike traversed his way through that program. So, Mike Telson.

TELSON: I was going to tell a little racy story, given the end of the day and the lateness and our willingness to get out of here, but I can't let Nick Samios get away with telling a Jewish joke and me not doing it, seeing that I am Jewish. So, to reinforce the point of the story of working together, I don't know how many of you heard the story about the yeshiva crew team. They were losing every single race, and so they finally decided they had to go up to Harvard to find out how to really win races.

So Moshe goes up to Harvard and learns and comes back and Aaron says, well, Moshe, so how'd it go?

Moshe says, well, geez, I learned a hell of a lot.

Aaron says, really? Is it going to be helpful?

It's going to be incredible. We're going to start winning.

Really? Why?

Well, in Harvard, they do it exactly the opposite of what we do.

Really?

Yeah, there they have eight guys rowing and only one guy shouting.

So, that is a very good story about this because the cacophony of voices in D.C. when you're seeing everybody shouting is a pretty ugly picture by the way. And we can get into it later. That's not the main point of my story right now.

Let me first talk about some of my observations of the program today as a whole. One of the things I noticed today in the discussion of the Bush report, I found troubling. The lessons of the report don't lie in a line by line analysis of the report. In other words, we're sort of looking at each sentence, each prescription. That's not the point.

The point – and other people have said this – is that the idea was basically right in the historical context in which it occurred. And even though the precise prescriptions that he made did not come to pass, in large part, they affected the development of the postwar scientific establishment, basically because the report came at a time when World War II had taught our parents and our grandparents the power of bringing science to bear on the solution to national problems.

That was really what happened. John Holmfeld said earlier today how groups of physicists worked in labs to create the radar and the bomb, and of course the bomb was what created DOE. That was the beginning of DOE, the Atomic Energy Commission. And of course the Atomic Energy Commission didn't seem to hurt the Atomic Energy Commission's labs even though I would find that a more objectionable name than the Department of Energy. Wouldn't you say?

Roosevelt said it very well in his letter to Bush, which probably Bush wrote for him. I don't know how many of you remember reading the letter request. He said, "New frontiers of the mind are before us, and if they are pioneered with the same vision, boldness and drive with which we have waged this war, we can create a fuller and more fruitful employment and a fuller and more fruitful life." There's no mention of science there. It's more fruitful employment and more fruitful life. To me, that's the basic point.

The group of physicists who worked on the bomb led to the establishment of the AEC, born as a nuclear weapons agency as well as, and few people realize this, a national nuclear physics agency. It was a science agency from the word go. That was not an accident, it was very much in the charter. It could have been called the Department of Physics, but it wasn't quite sexy to call it that at the time.

That's how Brookhaven was born. Nick didn't touch on this because it was a little bit of an embarrassment, the accidental nature of how it was born. I recommend to you Norm Ramsey's comments on the occasion of the 50th Anniversary of Brookhaven, of AUI – really the Associating Universities, Inc., which runs Brookhaven for the Department of Energy. That happened in January of last year, and in that celebration, I read the materials, letters from 1946 leading to the establishment of the agency, and it was basically a deal between Leslie Groves, who had some money left over after the war and who had a hell of a lot of authority, and scientists, physicists mostly. Norm Ramsey, who was at Columbia then, who is still around and who is a Nobel Prize winner at Harvard in physics, who basically said, if you liked what we did for you during the war, let us show you something else – we've got a few more tricks in our bag.

And the kinds of things they referred to, in retrospect, are mind blowing. This is 1946, but let me just name a couple of things they mentioned. This is in a letter to Leslie Groves, who was then head of the Manhattan Project. They identified the application of radioactive tracer methods to the study of physical, chemical, and biological processes, the use of radioactive materials for therapy, and for the production of power. They proposed addressing problems of the biological synthesis of proteins, the photosynthesis of sugars and cellulose, and fixation of nitrogen by bacteria.

These are things that now we take for granted. Then, they were dreams. But these people knew how to express those dreams in ways that would be urgent and real to the policy makers of that time. That's essentially what I think we ought to be doing now.

In fact, just to digress a second, I'm here today because Martha Krebs, who is director of energy research now at the Department of Energy, is scheduled to attend today the 50th anniversary of the founding of the Argonne laboratory. So we're going through a lot of these 50th anniversary situations in the next few months.

Let me turn now specifically to the Department of Energy. Nick touched on some of these things, so I may be repeating some of them. The Department of Energy has a wonderful system of laboratories. When I was on the Hill, I did NSF, NASA, the overall science budget, federal R&D budget preparation, sort of looking over how much we spent each year and divvied up among the different agencies and DOE. And I was always impressed by the different characters of research, the different styles among the agencies. Very different.

Nick mentioned some of the laboratories, I think we face three kinds of problems right now. One is the general budget-cutting climate, which we will deal with over time.

Second is a move to a closure commission, submitting labs, not only DOE labs but other labs as well, to a base closure commission model of legislation. Basically, you get a group of people together to form a commission, and these people go away, deliberate for six months or a year, and come back with a list of labs that will be closed. And if the Congress, on an up or down vote, agrees to it, they're closed.

It turns out that one of the bills in Congress actually says that if the Congress does not vote to keep them open, they would all close. So these are the kinds of deals that are going on these days that have to be watched.

Third, the dismantlement thing. I don't want to take too much time on that, but I can give you chapter and verse why dismantlement would be not helpful to the long-term survival of the system that we have right now at DOE.

The other thing that surprised me about the labs is that, as Nick said, very few people know about the labs outside of the labs. You see, in Congress, you get a very distorted perspective, because the congressmen from the labs know it, the senators from the lab know it, and you know it because they know it. But you hear from everybody else that it's like a free good. I call it the tragedy of the commons being applied to laboratories. Everybody takes them for granted, and because they're everybody's property, they're nobody's property.

So you ask researchers all over the country about Brookhaven, using some of the facilities there, and they'll say, oh, is that a DOE lab? Or, gee, we thought Brookhaven's funding came from Columbia. It's an incredible thing to see where people think the money comes from. They think it grows on trees. They don't realize that there's a system that feeds the laboratories, that basically brings the money, and that they have a stake, even though they don't know it, in making sure those common user facilities – where their students might be employed, where the faculty may have joint research projects – those places have their own funding problems. It's surprising to me how little understanding there is of that.

Just to touch on a couple of other things, the DOE system Nick mentioned – $6.4 billion in '97 is what we think we'll be spending in R&D, $600 million of that to universities. The basic research component, or if you will, the university support component is more than a quarter of the size of NSF, just to universities – that's also not well understood. It's not understood that the DOE and the labs support 40% of federal support of physical sciences in the United States. No other agency, including DOD, NASA, NSF, you name it, Commerce, provides more than half of that amount. There's good reason – we spend about a billion on just high-energy and nuclear physics, $1 billion on just those two fields.

We are very busily engaged in a system of revealing the labs of the Department of Energy to make sure that they're as efficiently run and there's as little overhead as possible. It is not a perfect system, but we have a very detailed, very torturous process underway, called the Strategic Laboratory Missions Plan. We have published some of the materials, opening up what the labs do to everybody, which I think is unprecedented. That will make it clear what they do, how the outside community can work with the labs, how they can compete with the labs, all these aspects.

What I would say to bring us back to the beginning is, I would not blow them up. I would mend them, don't end them, to quote another member of ours. In any event, I think I'll end there and look forward to your questions. Thanks.

QUESTION: Just a speculative question about policy for large-scale facilities such as light sources. Under similar cost pressures in other parts of the world, there's been a move to introduce user fees for these facilities. Would you comment on the feasibility of this, its political implications, if possible, ramifications for the research communities of going in that direction?

MALE VOICE: This is an idea that has surfaced at least twice before in the United States, once when John Deutch was head of ER and even once before that. We ought to keep the books of why we didn't do it.

From an operational point of view, it's a disaster. Every large facility has a leverage factor. In other words, if you ask, how much money does it take just to open up the door and what is the fraction that it takes to do the science? – the factor is between four-to-one and eight-to-one, which means that if you reduce the budget between 10% and 20%, you will do zero science with that facility. And you can't operate that way.

The people who run these facilities are highly skilled. It takes two to three years to train a person to run these facilities, so you don't have the luxury when you reduce their budget of firing 20% of your people to take care of it. That's why the leverage factor is so large. So you must know that number at the beginning of the year. If you have user fees, how do you do that operationally?

I had the pleasure once of running a facility that had funding from four separate pockets: materials science, biology, high energy, and nuclear, and with a leverage factor. At the beginning of the year, one of these disciplines would drop the budget a little bit, and as a result, the budget would have to go up in another discipline, so you had a feedback mechanism that was absolutely out of whack. So that's why I comment that, operationally at least, it doesn't work. There are other reasons why it’s a bad idea, but I'll give it to Mr. Telson.

TELSON: As you'd expect, management's perspective is somewhat different. We are looking for ways to do it, but Nick is right, that it’s not a trivial thing to do. We do have a policy to charge industrial users who are using the facilities for commercial purposes, uses for which they appropriate the benefits. They have to pay, I think, full cost of their use – full cost defined as, they pay the variable cost plus some contribution to depreciation. We had a hearing on the Hill, on August 1, and the transcript of that should be available soon. Deputy Henson Moore under the Bush administration was very interested in the issue, and we are as well, but it's not a trivial thing as you can imagine.

SAMIOS: I’d like to continue on that because Henson Moore was at the lab over three years ago, and he brought it up when he was there, and I told him it was a dumb idea then. The full cost recovery, for instance. Suppose you wanted eight hours on the light source – how much would it cost? At Brookhaven, it would cost you something like $300. You'd say, how can you do that, with it having cost $50 million to build it and operate it and so on? But if you do the arithmetic, and you amortize it over ten years, and you divide by 100 because that's the number of beam lines that are run simultaneously, you could see it's a very cheap thing for industry to come in and do proprietary research at our research institutions.

MALE VOICE: I'd like to comment a little bit on that, surprisingly. Nick doesn't know this, but I did a case study at Brookhaven in 1987, and one of the things that I used as a case study was the light source. One of the first things that I asked the director of the light source was about user fees. While I won't go into this in great detail, I became convinced, one, that user fees were not a good idea for the light source and, two, that the light source is not a good example for the question of whether there should be user fees. So the light source is distinctive in many ways. Perhaps unique.

On the much more broad issue of industry payment for property of research with federal laboratories, our research has shown again and again that it makes a lot of sense and that a very modest amount of industrial financing of government partnerships or federal laboratory partnerships has a big payoff. Apparently, just a modest amount of an investment gets a different degree of attention than what otherwise would be brought to a cooperative research agreement. So I think probably the question of user fees has to be separated out from the light sources, and the question of user fees also needs to be separated out from a question of industrial financing of property or research.

QUESTION: First, I'd like to say that this has been a terrific session. I've enjoyed every one of the presentations. I'd like for each of you to inform us of your ideas on how we can best convince Congress, the media, and the American public to support the national laboratories. Could you each say a few words on that subject?

MALE VOICE: That's not a mission I would undertake personally. I think probably one of the mistakes that we've made is not only overselling the national laboratories but selling the wrong thing. If you look at the policy deliberations about national laboratories – and I know that people at DOE haven't been responsible for this – it's absolutely astonishing what percentage is dominated by technology transfer, industrial partnerships, and so forth, and that's such a small percent of what's going on at DOE labs. So I think I would try to sell something a little bit different than regional economic development. How to go about selling the activities of the national laboratories, I will defer to the people who run them.

MALE VOICE: I'll take a short answer. As I mentioned, one of the great strength of the lab, if it's true, is our user community. It's rather large, and so if we speak for the national labs, it's self-serving. I believe it's up to the user community to say, are these things important and how important to the marketplace are they? I think we've been a little bit negligent about asking for some help, but since times are difficult, I think we've got to turn to the user community to state the usefulness of the labs. And it’s a rather large community, it's rather largely geographically distributed, it’s in all of 40 states, and those people writing their congressmen and senators, I think, would have a very large effect on this issue.

MALE VOICE: Nick is a very good example. He works very well with his communities, in the case of Brookhaven. In certain places, it’s so obvious that this thing is important economically, it’s important politically, industrially, that the sale job is not much. But in other places, they’re having major problems, particularly where the labs are smaller, or where there is a lot of other activity in the state or in the district.

There’s no magic to it. I think every lab has had to discover its own formula for doing it, but the formula basically is, you have to make sure people understand your contributions to the public well-being, both directly and in long term, and you have to tell the public what that means in terms of what the public official representing that area is supposed to do. If you just say great things but it turns out that what the public official does has nothing to do with what he’s being asked to do, that's not good for business over the long term. So it’s a matter of working with the public just like you'd work with any other issue. It’s not just the labs.

MALE VOICE: Well, I want to thank particularly the last set of panelists. It's not easy in a very, very densely packed two-day conference to be the last ones on a Saturday at around five o'clock.