SCIENCE THE ENDLESS FRONTIER: LEARNING FROM THE PAST, DESIGNING FOR THE FUTURE

Highlights from the Conference Series

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Introduction

Since its publication in 1945, Vannevar Bush's report *Science: The Endless Frontier* has come to occupy a biblical status in science policy. On the day it was issued, the report was greeted by front page headlines in the *New York Times*. Since then it has been the subject of innumerable studies, reports, analyses and interpretations, studied as if it were the word of God, invoked to legitimate a wide range of sometimes contradictory science policy models, decisions, and priorities. The Bush Report is most often associated with a linear and unidirectional model of knowledge creation and application, where lone researchers work at the frontiers of science to provide the intellectual grist for societal progress. From this perspective, *Science: The Endless Frontier* has often been interpreted as a pillar of support for the prerogatives of fundamental and unfettered research. Yet Vannevar Bush was an engineer with a keen appreciation for the complexities of the innovation process, and others have seen his report as a clear assertion of the close and necessary links between fundamental investigation and practical application. Despite its Rorschach quality, all would probably agree that the report was intended to be a blueprint for a new era of science—and of government in science—following the transformational experience of World War II and its technological culmination in the detonation of two atomic bombs over Japan.

Of course, every dogma wants revisiting and clarification from time to time. The Catholic Church has grappled with evolving doctrine (not to mention competing Popes) over the centuries, even as the words of the bible have remained more-or-less the same. Likewise has the context for Bush's report—and science policy—in modern society evolved, with the end of the Cold War in particular demanding a careful reconsideration of the meaning, relevance, and implications of *Science: The Endless Frontier*. In response to this changing context, Columbia University organized what might be thought of as a Vatican Council for science policy, an ambitious exploration of the historical, present, and future implications of Bush's seminal work at the time of its 50th anniversary.

Three conferences were held, on December 9, 1994, June 9, 1995, and September 21-22, 1996. Fifty-three leading scholars, practitioners, and observers of science policy made formal presentations addressing an extraordinarily broad range of issues—testimony to the impact and influence of science on modern society, and of the Bush Report on science. This booklet contains highlights from those three conferences—a selection of presentations aimed at illustrating both the breadth and depth of Bush's work, and the challenges facing science policy fifty years after he completed his report.

One concrete outgrowth of the conferences was the creation of a new organization, the Center for Science, Policy, and Outcomes (CSPO). In compiling these presentations, CSPO seeks to make more widely available a resource that contributes to and advances Bush's legacy, while also shedding light on CSPO's own mission of enhancing the capacity of science to achieve desired societal outcomes.

Even in the several years between the three conferences and the compilation of this booklet, the context for science policy has continued to change. What may seem, from the perspective of the year 2000, like an irrational despondency in several of the presentations, has given way to the irrational exuberance of the dot-com world. Budget deficits of the mid-1990s have been replaced by budget surpluses, economic expansion has persisted at historically unprecedented rates, and the texture of society has tangibly evolved under the influence of transformational innovations in information technologies and molecular genetics. So soon after the conferences were held, these presentations, which shed so much light on the Bush legacy, seem themselves to capture a moment in history. In doing so, they vividly illustrate the need to design science policies that are themselves flexible and adaptive—policies that allow the world to continue to benefit from science, even as science continues to change the world.

The full transcript of all sixty-two presentations made at the three conferences is available on the CSPO web site: www.cspo.org.

Part I

Understanding the Bush Legacy – *Jonathan Cole*

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

> 'Newton's career also illustrates an important observation by Merton on the significance of the formation of a scientific community; almost certainly Newton would not have written his *Principia* had there not been a discussion by the London virtuosi of the Royal Society of the possible force responsible for the observed Keplerian motion in the planets. It was as a result of this discussion (by Hooke, Wren, and Halley) that Halley went to Cambridge to see Newton and to explore this topic with him. The subsequent encouragement of Newton by Halley and the approbation of the Royal Society were significant factors in pushing Newton to complete his researches and write them up for publication under the Royal Society's imprint. It is doubtful that without the Royal Society there would ever have been a *Principia*.'

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

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

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

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

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

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

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

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

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

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

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

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

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

Understanding the Bush Report – *Harvey Brooks*

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

First, from page 19:

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

And then,

> Industry is generally inhibited by preconceived goals, by its own clearly defined standards, and by the constant pressure of commercial necessity. Satisfactory progress in basic science seldom occurs under conditions prevailing in the normal industrial laboratory. There are some notable exceptions, it is true, but even in such cases it is rarely possible to match the universities in respect to the freedom which is so important to scientific discovery.

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Completing the Bush Model: Pasteur's Quadrant – *Donald Stokes*

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

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

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

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

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

Bush recruited a small army of gifted colleagues for the scientific tasks of the war, with full backing of the strongest president of the 20th century. The Office of Scientific Research and Development (OSRD), as Hunter Dupree has noted, became as close to a General Ministry of Research as this country has ever had. And the flow of resources for scientific purposes – including basic nuclear science research that produced the weapons that decisively altered the course of the Pacific War – showed the scientific community, as it showed the nation, what might be done.

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

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

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

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

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

A great deal of the vision of the nature of basic science and its relationship to technological innovation is contained in two aphorisms in the Bush report, both worthy of Francis Bacon. Each was cast in the form of a statement about basic research – a term that was given currency by the Bush report.

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

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

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

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

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

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

The reception of *Science: The Endless Frontier* was full of irony: the organizational plan was defeated, while the ideological view prevailed. In the five-year gap between the publication of that report in 1945 and the creation of the National Science Foundation (NSF) in 1950, the authority of the NSF, which Bush had wanted to keep whole, was shattered by the policy process.

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

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

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

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

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

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

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

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

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

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

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

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

The creators of modern analytical demography have always regarded population change not only as a process that challenged understanding on a fundamental level, but as a problem with immense human consequences. Both the molecular and non-molecular ends of modern biology are profoundly influenced by scientific and applied objectives at once. And the earth sciences have always been influenced by natural disaster and economic gain. Indeed, every one of the basic scientific disciplines has its modern form, in part, as the result of use-inspired basic research. We should no longer allow the post-war vision to conceal the importance of this fact

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

Research is inspired by:

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Chalk or Cheese?: Ends and Means in Science Policy – *Donald Kennedy*

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.

[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).

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 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.

Show Me the Money: Budgeting in a Complex R&D System – *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.

Part II

Two Cheers for Democracy: Science and Technology Politics – *Bill 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?”

Universities: Costs and Benefits on the Academic Frontier – *Donald 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.

Federal Laboratories: Understanding the 10,000 – *Barry 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.

Technological Change: Costs and Benefits on the Academic Frontier – *Donald Kennedy*

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Health: The Devil of a Problem – *Nathan 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.[3]

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.

[3] 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.

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. 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 20th 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.

Health Care: Coping with Consolidation – *Kenneth 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.

International Cooperation: What's in it for Us? – *Eugene 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.

Social Sciences: Shunned at the Frontier – *Susan Cozzens*

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

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

Part III

Toward a National R&D Policy – *Peter Eisenberger*

In this article, I begin by reviewing the thoughtful efforts of others to suggest a new framework to replace the Bush paradigm. After that, I 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.

Beyond the Endless Frontier – *Michael Crow*

In this article, 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.

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Biographical Sketches

B

Barry Bozeman

Barry Bozeman is Director, School of Public Policy and Professor of Public Policy, Georgia Institute of Technology. Previously, he was Director of the Center for Technology and Information Policy at Syracuse University's Maxwell School of Public Affairs. Bozeman's research concentrations include R&D Policy and Organization Theory. His books on R&D Policy include *Investments in Technology: Corporate Strategy and Public Policy* (with Albert Link) and *Strategic Management of Industrial R&D* (with Albert Link and Michael Crow).

For the past thirteen years he has been co-director of the National Comparative Research and Development Project, a multinational project involving more than 30 researchers in four countries focusing on the structure and policy environment of R&D laboratories. Currently, Bozeman is directing a three-year project using the "R&D Value Mapping" evaluation approach to examine the impacts of government-funded basic research on innovation and technology commercialization.

Harvey Brooks

Harvey Brooks is Benjamin F. Peirce Professor of Technology and Public Policy (Emeritus) in the Center for Science and International Affairs of the Kennedy School of Government, Harvard University. He received his Ph.D. in physics from Harvard in 1940, was a staff member of the Harvard Underwater Sound Laboratory during WWII from 1942 to 1946. After the war he was at the General Electric Research Laboratory in Schenectady, where he served also as Associate Laboratory Head of the Knolls Atomic Power Laboratory (KAPL) until 1950, when he returned to Harvard as Gordon McKay Professor of Applied Physics, becoming also Dean of the Division of Engineering and Applied Physics at Harvard from 1957 to 1975, after which he moved to the Kennedy School to become Director of the Science, Technology and Public Policy Program until his retirement in 1986.

He is a member of the National Academy of Sciences, the National Academy of Engineering, the Institute of Medicine, and the American Philosophical Society and was President of the American Academy of Arts & Sciences from 1971 to 1975. He served on the President's Science Advisory Committee from 1957 to 1964, and as a member of the National Science Board from 1962 to 1974. He was the second Chairman of the Committee on Science and Public Policy (COSPUP) of the National Academy of Sciences from 1966 to 1971 and Chairman of the Commission on Sociotechnical Systems of the National Research Council from 1972 to 1976. He co-chaired the Committee on Nuclear and Alternative Energy Systems (CONAES) of the NAS from 1976 to 1979. His research has been underwater acoustics, nuclear engineering, solid state physics, energy policy, and science policy. Recently, he co-chaired with Dr. John Foster the Committee on Technology Policy Options in a Global Economy of the National Academy of Engineering, whose report <u>Mastering a New Role: Shaping Technology Policy for National Economic Performance</u>, was released in March, 1993.

Since his official retirement, he has published about thirty papers and book chapters on science and technology policy, the future of academic research, university-industry relations, and technology and economic performance.

C

Jonathan R. Cole

Jonathan R. Cole, currently Provost Dean of Faculties and Quetelet Professor of Social Science at Columbia University, received a B.A. in American History from Columbia in 1964 and the Ph.D. with Honors in Sociology from Columbia in 1969. He has been teaching at Columbia from 1966 to the present time. He served as Director of the Center for the Social Sciences from 1979-1987, when he became Vice President for Arts and Sciences, a post held until July 1989, when he became Provost. Among many awards and honors, he has received a John Simon Guggenheim Fellowship, has been a Fellow at the Center for Advanced Study on the Behavioral Sciences, and he is a Member of the American Academy of Arts and Sciences. He has published extensively on historical and social aspects of science, has been a leading international contributor to understanding the opportunities, challenges and obstacles facing women in the scientific community, has led a National Academy of Sciences evaluation of the peer review system in science, and has published works recently on health risks and on dilemmas facing American research universities.

Susan E. Cozzens

Susan Cozzens is Professor and Chair of the School of Public Policy at the Georgia Institute of Technology. She is the author of numerous articles in science policy and science and technology studies, and several books, including Social Control and Multiple Discovery in Science: The Opiate Receptor Case (SUNY Press, 1990), and Theories of Science in Society (co-editor with Thomas F. Gieryn; Indiana University Press, 1991). She is past editor of Science, Technology, & Human Values, the journal of the Society for Social Studies of Science.

Dr. Cozzens has served as a consultant to numerous organizations, including the National Research Council, the Office of Science and Technology Policy, the National Science Foundation, and the National Institutes of Health. Before joining Georgia Tech, Dr. Cozzens was on the faculty of Rensselaer Polytechnic Institute. From 1995 through 1997, Dr. Cozzens was Director of the Office of Policy Support at the National Science Foundation. Dr. Cozzens received her Ph.D. from Columbia University in sociology and her bachelor's degree from Michigan State University.

Michael M. Crow

Michael M. Crow serves as the Vice Provost of the University and Professor of Science and Technology policy at Columbia University in the City of New York. Previously he served as Director of the Institute for Physical Research and Technology, and Institute Professor of Technology Management at Iowa State University, he holds the Ph. D. in Public Policy (Science and Technology) from the Maxwell School at Syracuse University.

The author of three texts in science and technology policy he has most recently published policy on issues related to the design of R&D systems, technology development and science policy in; Science and Public Policy, Technovation, Revue d' Economie Francais and the Journal of Technology Transfer.

E
Peter Eisenberger

Peter Eisenberger joined Columbia University on September 1, 1996, as Vice-Provost of the Earth Institute and Director of Lamont-Doherty Earth Observatory. Eisenberger holds a Ph.D. in Applied Physics from Harvard University. 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. Early in his career, he was Department Head at AT&T Bell Laboratories. He Brings to Lamont-Doherty and the Earth Institute a unique and vital set of skills and experiences.

Columbia has identified the development of the Earth Institute as one of the University's most important strategic initiatives. The activities of the Earth Institute will be key to positioning Columbia as an international university, ideally situated to provide strategies for managing our planet as we move into the next century. The Earth Institute will bring together researchers and educators from throughout the University -- as well as from other institutions outside of Columbia -- to work on complex planetary problems. Lamont-Doherty Earth Observatory and Biosphere 2 Center Inc. will be at the center of the new institute.

Eisenberger understands that the solutions to many planetary problems can be found only at the interfaces of many different disciplines and knows how to do this perhaps better than anyone else. At Exxon in the early 1980's, Eisenberger recognized the increasing importance of cross-disciplinary work and brought together interdisciplinary teams to address complex issues facing the oil industry.

Eisenberger has recently served on the Technology Council of the Advisory Board to Congressman Zimmer of the Space, Science and Technology Committee of the U.S. House of Representatives; the NRC Panel on Bimolecular Materials; the Advisory Committee for the Mathematical and Physical Science Division of NSF (as chair); the Board of the Invention Factory Science Museum; and the Board of Trustees for New Jersey's Inventors Hall of Fame.

G
Bill Green

Bill Green served in the U.S. House of Representatives for eight terms from February 14, 1978, until January 3, 1993. 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 also Served on the Foreign Operations Appropriations Subcommittee during 1991-92.

Green co-chaired the National Commission on Severely Distressed Public Housing during 1991-92. Prior to his election to Congress he had been an attorney in private practice in New York City. He served in the New York State Assembly from 1965 to 1968 and as Regional Administrator of the U.S. Department of Housing and Urban Development covering New Jersey, New York, Puerto Rico, and the Virgin Islands from 1970 to 1977.

Since Bill Green was defeated for reelection to the U.S. Congress following the 1992 redistricting, he has been active in the public and private sectors in housing, science policy, and political reform.

H

David Hart

David Hart is an assistant professor of public policy at the Kennedy School of Government, Harvard University, where he teaches courses on science, technology, innovation, and public policy, and on electoral and advocacy politics. His book, Forging the Postwar Consensus: The Governance of Technological Innovation in the United States, 1921-1953, will be published by Princeton University Press next year. Hart is co-organizer of the Boston-area Workshop on American Political Development, which will be holding its second annual conference at the Kennedy School on September 28, on the subject of "The Politics of Economic Inequality in the Twentieth Century." He also serves as a member of the Task Force on Genetic Testing, Privacy, and Public Policy of the Whitehead Institute for Biomedical Research. He holds a Ph.D. in Political Science from MIT.

K

Donald Kennedy

Donald Kennedy is the Bing Professor of Environmental Science and President emeritus at Stanford University. He received AB and Ph.D. degrees in biology from Harvard. His research interests were originally in animal behavior and neurobiology - in particular, the mechanisms by which animals generate and control patterned motor output. His research group explored the relationship between central "commands" and sensory feedback in the control of locomotion, escape, and other behaviors in invertebrates. Among the issues considered were: How environmental variables that could not be "anticipated" by the animal's genetic endowment could be compensated in fixed behavioral patterns and whether certain circuit arrangements for a given class of motor output were favored in different evolutionary outcomes.

In 1977 Dr. Kennedy took a 2 1/2 year leave to serve as Commissioner of the U.S. Food and Drug Administration. This followed an increasing academic interest in regulatory policy regarding health and the environment, which included the chairmanship of a National Academy of Sciences study on alternatives to pesticide use and membership on the World Food and Nutrition Study. Following his return to Stanford in 1979, Dr. Kennedy served for a year as Provost and for twelve 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. During that time Kennedy continued to work on health and environmental policy issues, as a member of the Board of Directors of the Health Effects Institute (a non-profit organization devoted to mobile source emissions), Clean Sites, Inc. (a similar organization devoted to toxic waste cleanup), and the California Nature Conservancy.

His present research program, conducted partially through the institute for International Studies, consists of interdisciplinary studies on the development of policies regarding such trans-boundary environmental problems as: major land-use changes; economically-driven alterations in agricultural practice; global climate change; and the development of regulatory policies. He co-directs the Environmental Studies Program in the

Institute for International Studies, and oversaw the introduction of the environmental policy quarter at Stanford's center in Washington, DC in 1993.

Dr. Kennedy is a member of the National Academy of Sciences, the American Academy of Arts and Sciences, and the American Philosophical Society. He holds honorary doctorates from several colleges and universities. He served on the National Commission for Public Service and the Carnegie Commission on Science, Technology and Government.

R

David Z. Robinson

David Z. Robinson was born in Westmount, Quebec in 1927. He attended Harvard University, receiving an AB *magna cum laude* in Chemistry and Physics in 1946, an A.M. in Chemistry in 1947, and Ph.D. in Chemical Physics in 1950.

In 1949, he joined the company now called Baird, Inc. as a research physicist and was appointed Assistant Director of Research in 1951. While at Baird, he was involved both in developing commercial optical and electronic instruments, and in research on infrared detection devices for the Defense Department.

In early 1961, he joined the White House as a staff scientist in the Office of the President's Science Adviser. In that office, he was involved with many communications issues, including communications satellite policy, command and control, and the hot line between U.S. and the Soviet Union. He also dealt with basic science policy and budgets.

In 1967, he became Vice President for Academic Affair of New York University, which at that time had two undergraduate campuses, 16 schools and 35,000 full and part-time students.

In 1970, Dr. Robinson was appointed Vice President of Carnegie Corporation of New York, an educational foundation. He became Executive Vice President and Treasurer in 1986. In addition to his administrative duties he has worked closely with Carnegie programs in higher education, public broadcasting, college retirement, avoiding nuclear war, and science education.

In February of 1988, Carnegie Corporation established the Carnegie Commission on Science, Technology, and Government, and he became Executive Director. The Commission is assessing the mechanisms by which the federal government and the states incorporate scientific and technological knowledge in decision making, and recommending changes to make them more effective. He was continued his program activities at Carnegie Corporation where he serves as senior counselor to the President.

Dr. Robinson has been an advisor to the President's Science Advisory Committee, the National Academy of Sciences, and the National Science Foundation. He has been a member of the Naval Research Advisory Committee, the New York State Energy Research and Development Authority, Governor Cuomo's Advisory Committee on Education, and the Education and the Science and Law Committee of the Bar Association of the City of New York. He has also been a member of the Boards of the City University of New York the Dalton School, Amideast, the South Africa Education Program, and the Investors Responsibility Research Corporation. He is currently a trustee of the North Carolina School of Science and Mathematics, Prep for Prep, the Institute for School of the Future, the Citizen's Union Foundation, and the Santa Fe Institute.

He is a Fellow of the Optical Society of America, and a member of the Council on Foreign Relations, the American Association for the Advancement of Science, the New York Academy of Sciences, the Harvard of New York, and the Century Association.

Nathan Rosenberg

Nathan Rosenberg is the Fairleigh S. Dickinson Jr., Professor of Public Policy in the Department of Economics at Stanford University. He was educated at Rutgers University, University of Wisconsin and Oxford University. He has taught at the University of Pennsylvania, Purdue University, Harvard University, the University of Wisconsin, The London School of Economics, and Cambridge University.

Professor Rosenberg's primary research activities have been in the economics of technological change. His publications have addressed both the questions of the determinants and the consequences of technological change. His research has examined the diversity of the forces generating technological change across industrial boundary lines, as well as the mutual influences between scientific research and technological innovation. Professor Rosenberg's books include *The American System of Manufactures, Perspectives on Technology, Inside the Black Box, Technology and the Pursuit of Economic Growth* (with David Mowery), *How the West Grew Rich* (with L.E. Birdzell, Jr.), *Exploring the Black Box*, and, most recently, *The Emergence of Economic Ideas*.

Professor Rosenberg has served as chairman of the Stanford Economic Department. He is a member of the Board of Directors of the National Bureau of Economic Research, chairman of the advisory board of the UN Institute for New Technology, and a fellow of the Canadian Institute for Advanced Research. He is an Elected Fellow of the American Academy of Arts and Sciences and the Swedish Royal Academy of Engineering Sciences. He is the recipient of honorary doctoral degrees from the University of Lund and the University of Bologna.

S

Kenneth I. Shine

Kenneth I. Shine, MD, is President of the Institute of the Medicine, National Academy of Science, and Professor of Medicine Emeritus at the University of California, Los Angeles (UCLA) School of Medicine. He is UCLA School of Medicine's immediate past Dean and Provost for Medical Sciences. Currently he is Clinical Professor of Medicine at the Georgetown University School of Medicine.

A cardiologist and physiologist, Dr. Shine received his AB from Harvard College in 1957 and his MD from Harvard Medical School in 1961. Most of his advanced training was at Massachusetts General Hospital (MGH), where he became Chief Resident in Medicine in 1968. Following his postgraduate training at MGH, he held an appointment as Assistant Professor of Medicine at Harvard Medical School. He moved in 1971 to the UCLA School of Medicine and became Director of the Coronary Care Unit, Chief of the Cardiology Division, and subsequently, Chair of the Department of Medicine. As Dean at UCLA, Dr. Shine stimulated major initiatives in ambulatory education, community service for medical student and faculty, mathematics and science education in the public schools, and the construction of new research facilities funded entirely by the private sector.

Dr. Shine is a member of many honorific and academic societies, including Phi Beta Kappa and Omega Alpha; Fellow of the American Academy of Arts and Sciences, the American College of Cardiology, and the American College of Physicians; and was elected to the Institute of Medicine in 1988. He served as Chairman of the Council of Deans of the Association of American Medical Colleges from 1991-1992, and was President of the American Heart Association from 1985-1986.

Dr. Shine's research interests include metabolic events in the heart muscle, the relation of behavior to heart disease, and emergency medicine. He participated in efforts to prove the value of cardiopulmonary resuscitation following a heart attack, and in establishing the 911 emergency telephone number in the multi-jurisdictional Los Angeles area. Dr. Shine is the author of numerous articles and scientific papers in the area of heart physiology and clinical research.

Eugene B. Skolnikoff

Eugene B. Skolnikoff, Professor of Political Science, has focused his research and teaching interests in the field of science and public policy, especially the interaction of science and technology with international affairs. This interest has covered a wide range of international subjects, including recent studies in global climate change. A major new book is entitled The Elusive Transformation: Science, Technology, and the Evolution of International Politics. Among his other articles and books are Science, Technology and American Foreign Policy, and The International Imperatives of Technology. Professor Skolnikoff was Director of the Center for International Studies from 1972 to 1987 and has held posts in the White House Science Office in several administrations.

Donald Stokes

Donald Stokes was a faculty member at the Woodrow Wilson School for Public and International Affairs in the field of political science. He earned his Ph.D. from Yale in 1958 and was a professor at the University of Michigan, Ann Arbor from 1958-74 where he was also the program director for the Institute for Social Research. He was the chairman of the Department of Political Science from 1970-1 and was made dean of the graduate school in 1971 lasting until 1974 when he left to go to the Woodrow Wilson School. Professor Stokes died in 1997.

Professor Stokes published a number of works including; *Pasteur's Quadrant*, 1997; *The American Voter*, 1960; *Elections and the Political Order*, 1966;and *Political Change in Britain*, 1969.

Professor Stokes had been an associate member of Nullfield College, Oxford, Sr. Fulbright Scholar to Britain, Fellow for the Social Science Research Council, fellow of the Guggenheim Foundation, visiting research fellow at the Royal Institute for International Affairs, fellow for the AAAS, National Academy for Public Administration, Brookings Institute, American Political Science Association, American Association of Public Opinion Research and the Council on Foreign Relations.