Design Area Six:
The Organization, Management, and Funding of Federal Science and Technology

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NELSON: The arena under discussion certainly is an important and contentious one. As we all know, the system of governmental research finance in the United States turned out to be a highly decentralized one, with a number of different government agencies looking after their own needs and their own research missions associated with those needs.

Beginning as early as the 1950s, a number of commentators have suggested that that system was extraordinarily messy. And that the possibilities of coordination of various mechanisms were limited and inadequate. Several times over the course of postwar history, proposals have been put forth for the establishment of a broad-gauged department of science and technology or something along those lines. And always, counter-arguments have held the day.

That's the background for what our design presenters are going to be dealing with this early afternoon. David Hart will start. David is now an assistant professor of public policy at the Kennedy School at Harvard. He is just coming out with a book. And I think a tiny bit of advertising is appropriate, since I had the chance to read an early version of it, and it's terrific. His book is called *Forging the Postwar Consensus: The Governance of Technological Innovation in the United States 1921-1953*. It'll be published by the Princeton University Press this coming year, and it's going to be a terrific book.

HART: The U.S. faces a perennial challenge that is growing more acute – how to deploy its limited resources to best achieve the very large goals that we hold as a people. There are a lot of these goals: military security, environmental quality, insurance against infirmity and poverty, and so on. My view is that like money, science and technology should be seen primarily as means to achieve these ends, rather than as ends in themselves. My concern is the confusion between means and ends.

The debate over science and technology policy has begun to resemble too much the debate over fiscal policy. In fiscal policy, the nation has gotten caught up in rhetoric about deficit reduction. This has become an end in itself, and we no longer talk about the deficit as a means to achieve economic growth and stability.

Science and technology policy, too, has been marred by confusion between means and ends. This problem can be seen most clearly in discussions about a Department of Science, but I don't think the confusion has been confined to that proposal. This confusion of means and ends distracts us from grappling with the more important problem of choosing well among means.

That is really what ought to be engaging our attention. I will return to that later, but first, let me begin by discussing the Department of Science and related ideas.

My argument draws upon a debate among the giants in the history of science policy that was carried out on the pages of *Minerva* about 30 years ago.[1] Michael Polanyi and Alvin Weinberg were some of the participants in this debate. This was a time that budgets were growing by something like 15 percent per year. On that note, we have to marvel at their foresight, to foresee this day and age when we would come to what Derek DeSolla Price called the steady state.

In this debate, Stephen Toulmin proposed what he called the chalk and cheese principle. In a well-structured administration, Toulmin argued, decisions have to be taken among commensurable alternatives, comparing in each case chalk with chalk and cheese with cheese. This principle, Toulmin said, holds in the administration of scientific affairs as forcibly as it does in the rest of public service. His point was not that R&D projects should be compared against one another, but rather that they should be compared against other ways of achieving the goals laid down by political authorities. Although both chalk and cheese are solids that crumble differently, one is for writing, the other for eating. The goal of policy analysis – if I can stretch this metaphor – should be the best writing and eating, not optimizing crumbliness. The latter demonstrates confusion of means and ends.

This confusion of means and ends appears on the contemporary scene in a number of different guises. Take the analysis of total federal R&D spending. Perhaps because so many scientists are recipients of federal funds, I think we have grown into the habit of judging the budget in terms of its year-on-year growth. This mode of assessment appropriately prompts a couple of criticisms. Scientists and engineers are perceived as arrogantly assuming an entitlement that the representatives of the people have not voted, or else they are seen as a classic Washington interest group clutching at the federal purse for no other reason than their own material benefit. In either case, the ends of the spending are not specified.

Another way to analyze federal R&D spending is to add it to private R&D spending and then compare the sum – that is, total national R&D spending as a share of our gross national product or gross domestic product – with that of other nations. When the nation falls behind its competitors on this indicator, the federal government is presumed to have some responsibility to make up the difference. Unlike the first approach, this method typically relates total national R&D to some goal: in the past, military security; more recently, economic growth.

But even though a national goal is specified, I would argue that this approach of taking R&D as a percent of GDP still violates the chalk and cheese principle. If the national goal is economic growth, R&D spending ought to be compared against other policies that might achieve that end, like deficit reduction or demand expansion, rather than comparing it with the fraction of R&D as a percent of GDP spent by other countries. The question is, to maximize economic growth, would the marginal increment of federal spending best be spent on R&D as opposed to other

[1] For a sample of this discussion, see Michael Polanyi, "The Republic of Science," Minerva 1:54-73 (1962); Stephen Toulmin, "The Complexity of Scientific Choice: A Stocktaking," Minerva 2:343-359 (1964); and Alvin M. Weinberg, "Criteria for Scientific Choice II: The Two Cultures," Minerva 3:3-14 (1964).

ways of spending, or not spending it at all? I admit this is a difficult calculation to make, but I think it the way we ought to pose the problem.

The Department of Science concept is equally confused, in my view. The idea of a central institution to manage the nation's science and technology has been traced back to the Constitutional convention by Hunter Dupree. The idea for a Department of Science has been offered up more than a hundred times just since Vannevar Bush, although Bush didn't make exactly the same proposal.

The latest of these proposals was put forward by Representative Robert Walker when he was chair of the House Science Committee. The proposal excluded the bulk of R&D funding, that of the Department of Defense (DOD) and the National Institutes of Health (NIH), but it did include such disparate elements as parts of the Department of Energy (DOE), the Department of Commerce (DOC), the National Aeronautics and Space Administration (NASA), the National Science Foundation (NSF), the Environmental Protection Agency (EPA), and the U.S. Geological Survey (USGS).

Walker argued that the main mission of these entities is the promotion of science for its own sake. Of course, a brief look at their authorizing legislation, with the exception of NSF partially, shows this is not the case. NASA is supposed to explore space, EPA to protect the environment, and so on. It seems to me that unless Congress has accepted science as an end in itself to a much greater extent than it has, Walker's Department of Science would be little more than a holding company and a juicy target for budget cutters.

The travails of the National Endowments of the Arts and Humanities in recent years suggests that the cultural argument – this is the label that Alvin Weinberg applied to the argument of science for its own sake back in 1964 – is no more politically persuasive now than it was in the past, and perhaps less so.

The last example that I offer as the confusion of means and ends in the contemporary debate is *Allocating Federal Funds for Science and Technology*, a report of the National Academy of Sciences that was chaired by Frank Press (Press 1995). The Press report's central recommendations include the establishment of a federal science and technology budget, as well as executive and legislative institutions to manage it. The main goal of this budget is to assure U.S. world leadership in science and technology. To this end, the budget provides a mechanism to trade off R&D projects across agencies. The Press report's vision is in some ways more ambitious than the Department of Science, since its federal science and technology budget includes NIH, as well as about eight billion dollars of DOD.

It is also more contemptuous of the chalk and cheese principle. The Press report's budget process would deliberately force chalk versus cheese choices while making chalk versus chalk and cheese versus cheese choices harder. For instance, an EPA research program on the diffusion of effluents in ground water would have to compete not only with EPA enforcement spending, as it normally would in the budget process as it is now constituted, but also with hydrologic programs in other agencies such as NSF. The criteria that the Press report endorses for making these kinds of comparisons – that is, between the two research programs – include not only the program's

contributions to the missions of these agencies, such as safer drinking water or knowledge of hydrology, but also the processes and instruments by which these funds are dispersed. As I understand it – the criteria aren't exactly transparent in their application – the budget-makers could cut EPA's research funding in favor of NSF in this area with little regard for the EPA's larger program if they determine that EPA failed to consult adequately with the external scientific community, which is one of their procedural criteria.

This is not the proper way to go about these things. The proper way is to apply the chalk and cheese principle. It begins with the specification of federal missions by the President and Congress, the setting of priorities among them, and the establishment of budgets.

Working within these budgets, the agencies determine the appropriate level of investment in science and technology for achieving their missions compared to other kinds of spending, such as direct services, procurement of more conventional goods, and so on. This is essentially the system that we have. It is a system that has evolved some instruments, like the Federal Coordination Committee on Science and Technology (FCCST) cross-cuts under the Bush Administration, and the National Science and Technology Council (NSTC) working groups under the Clinton Administration that help deal with the duplication that might arise in such a system as well as facilitate interagency programs and deal with international joint ventures, which are becoming more important.

It is not a perfect system, and it has particular failures, most notably the jurisdictions of certain appropriations subcommittees, which can force perverse tradeoffs. Nonetheless, the fundamental design is sound. We ought to continue to work for incremental improvements and select out those experiments like the cross-cuts that adapt the system well to new circumstances, rather than pressing for the kind of wholesale change that Representative Walker or the Press report's proposals would.

In our decentralized system, a major challenge is to get agencies and their political masters to take a long-term view of the mission: when and how it might be achieved, so that, on the margin, R&D spending might be more favored than it is now. In other words, those who believe that science and technology provide powerful means to give the public what it wants must make the case in those terms.

The supporters of biomedical research have done this extremely well, as the NIH budget curve shows. The cold war Defense Department is another example of successful advocacy of mission-oriented R&D, for better or worse.

It isn't always an easy case to make, since the time horizon of most politicians extends only to the next election. It invites the application of dangerously rigid standards of evaluation, even ridicule. Senator Proxmire used to hand out the Golden Fleece award for projects he deemed especially unworthy of federal funding.

Some of our efforts have to be devoted to ensuring appropriate efforts to measure the contribution of science and technology to agency missions. One argument we might make is that these kinds of evaluative measures should be applied to whole programs rather than individual

projects, since we do not know what the outcomes of projects will be in advance. And we might argue that such programs ought to be evaluated in qualitative terms. Perhaps we should also engage the users and beneficiaries of these programs in these evaluations, not merely peer reviewers.

But however difficult these metrics are to devise, and however disadvantaged long-term thinking might be in our political system, these are the terms in which the case ought to be made. We should not exaggerate the difficulty, because, as Senator Domenici has documented, R&D budgets have done pretty well in recent years. Most of the pain is still prospective from the point of view of aggregate R&D spending, though that doesn't always translate down to the individual level of scientists.

What does it take to make this kind of case? It begins with a community with a deep commitment to its cause, that can be mobilized in its support. I think the scientific and technical community has this commitment, although many in it may lack the time for a lobby day in Washington. Scientists, engineers, and science enthusiasts tend to be reasonably wealthy and sophisticated, and they tend to be widely distributed geographically. These are all highly-prized attributes from the point of view of mobilizing a political constituency.

Second, and perhaps most important, the political leadership of the community has got to know how the budget process works, and have a sense of the tactics and timing appropriate to each stage in that process. It must also possess the organizational capacity to carry out these tactics – that is, to turn out its supporters when they are needed.

Finally, the case for mission-oriented science and technology can draw on a deep sense of faith among the public that these investments in science and technology will pay off. I think the fear of an anti-science trend has been greatly exaggerated. In fact, if anything, that audience is too gullible when it wants to believe that something is possible, like the Strategic Defense Initiative.

That's not to say that the nation has been sold a bill of goods by scientists and engineers. Even in the case of biomedical research, according to the NSF, the total public and private spending on biomedical research in 1994 was $33 billion dollars. That is a lot of money, but remember that the total enterprise is a trillion dollars. About 3.3 percent of this doesn't sound bad to me.

Rather, my point is simply to remind the lobbyists, if I can call the scientific and technical community that, to try to keep expectations reasonable. Convey the promise not of spectacular leaps forward but of broadly diffused pay-offs. I do not believe science and technology can solve every problem, no matter how well funded it is.

A second challenge in a decentralized system of mission-oriented R&D is to achieve an adequate balance between dedicated expertise and flexibility. For missions that are deemed very important and long-lasting, there is no substitute for specialized institutions that cultivate unique knowledge and capabilities. It is impossible to imagine the post-WWII rate of progress in weapons technology without the national laboratories. It is equally impossible to imagine the rate of progress in medical technology without the academic medical centers.

Unfortunately, when public priorities change or when the mission is achieved, like winning the Cold War, these institutions become a burden. The benefit of specialization becomes the burden of rigidity. I think the people in these places can be reoriented, and perhaps some of the equipment as well, but I do not think the institutions and culture that they nurture can be. Rather than try to save them, the proper policy is to reduce or close them in accordance with the new level of mission need, and to facilitate the reemployment of those resources elsewhere on other missions.

If the communities where these facilities are located are mobilized as I have described, closing the facilities can be a pretty difficult job, as we have seen in New Mexico. In these instances, I think it is incumbent upon the S&T community to break ranks, rather than to circle the wagons.

The Press report does a good job of this. It calls for reductions, for instance, in the DOE labs. What it does not do is provide enough of a rationale to articulate new missions to which those resources might be better put in the future. This is especially true for transferring resources to the universities, which I take to be one of the main objectives of the Press report. Its main argument for funding academic scientists is that they are flexible. This calls to mind the Bush report's metaphor of a reservoir of knowledge that can be put to use as new needs emerge. But flexibility is not a mission.

The Press report likewise under-emphasizes the role of academic scientists in education. This mission was fully articulated in the Steelman report (Steelman 1947), which has tended to take second seat to the Bush report in our histories. It's a mission that resonates with the nation right now. The proper role of the federal government in education is far from settled, but that is all the more reason for the community to be mobilized and to advocate on this point.

The advocacy of education for its own sake comes dangerously close to what I referred to before as the cultural argument. And while I think that that argument has limited appeal – although it may appeal very much to those of us who are academics and would like our students to become broad-minded human beings for their own good – it should not be abandoned. There actually is a reasonable amount of public support for areas of research that don't necessarily have a mission application, such astronomy and cosmology. But I do not think that we should make too much of the federal role on that cultural argument.

I want to offer an argument that I think has broader appeal, and that is to link education with the economic needs of the next century. It may be conventional wisdom that the economy is based increasingly on technology and innovation, and therefore requires an increasingly skilled and creative workforce. However, the nation has not done very much to act on that conventional wisdom.

Adopting this kind of argument has serious implications for science policy, and we ought to recognize that. It means that the expected future demands of the job market, rather than the opportunities perceived by academic researchers for science, ought to be the major criterion for allocating funds. It means that teaching ought to be accorded more emphasis and respect.

We can hope that these things will line up – that is, scientific opportunities and job opportunities and teaching excellence and research excellence – but they may not, and all too often in the past, they haven't.

An argument that connects research funding with education for the sake of economic growth – i.e., an economic management mission of the federal government – creates political opportunities for the science and technology community. The Clinton Administration entered with plans to make an array of investments that included R&D but extended also to infrastructure, education, and other sorts of programs. Much of this was abandoned in the name of deficit reduction, and perhaps appropriately so, if I can refer back to the chalk and cheese principle. But in the long run, I think that macroeconomic management is going to be an inadequate substitute for the provision of public goods that make markets work. These kinds of goods, like research and education, are complimentary.

Science for science's sake can be achieved with R&D funding alone. Science for the economy's sake will not pay off without other investments besides R&D. In this respect, the scientific community might be able to join a coalition with labor and business organizations that believe in making these kind of investments for the sake of the economy. We have to remember that there are going to be enemies made along the way, and the process may divide the community. But nonetheless, I think that it is a plausible rationale.

The chalk and cheese principle is not easily applied in the U.S. Our political system is prone to overlapping jurisdictions and turf wars. I submit, however, that this ideal is a more sensible guide for efficiently carrying out the will of the people than simply maximizing federal R&D spending or ensuring that federal R&D spending is done in accordance with the wishes of the scientific establishment.

And it is carrying out the will of the people and participating in the formation and refinement of that will that ought to be the object of the science and technology policy community.

NELSON: Thank you very much, David. Our next presenter is Michael Crow, who has been for the last several years vice provost as well as professor of science and technology policy here at Columbia University. As all of you know, Michael has from the beginning been a major force in the design of this conference. And it seems to me quite fair that someone who has designed the conference has the chance to make a presentation at it. (laughter)

CROW: I'm going to present recommendations for specific changes in the original design that Vannevar Bush gave us 50 years ago in *Science: The Endless Frontier*. This may put me a long distance out on a limb, and I do it with some trepidation, realizing that those who have done this before who are not eminent Nobelists usually have been butchered shortly afterwards.

My premise is that Vannevar Bush's design is not flawed in any serious way. Rather, it is so seriously outdated that it appears completely flawed. To update Bush's design, I have approached Vannevar Bush as if he were a software engineer who laid out the program for the conduct of science in the United States some 60 years ago. I tried to consider it from the perspective of what the design principles were that Bush put into his software code.

There were seven such principles. One was political autonomy. Bush's design parameters separated the scientific enterprise as much as possible from political processes. In practice, there are varying degrees of separation, but autonomy was one of the design parameters.

A second design principle was self-regulation by scientists. Scientists, like the Marine Corps and major league baseball, and any elite group for that matter, were supposed to discipline themselves, set up mechanisms to control their culture, and so forth.

The third of Bush's design principles was a focus on science for science's sake as well as for problem-solving. This principle has been distorted by many people who think that Bush's principal design parameter was science for science's sake. These critics are wrong. Science was both for fundamental discovery and for specific problem-solving.

Fourth, because of both his background as a professor at MIT and his time as dean as well as president of the Carnegie Institution, all of Bush's design parameters are built around a strong academic model of individual achievement. The focus is on the individual – both the individual discipline and individual scientists.

In his last three design parameters, Bush called for scientists to be accountable for achieving national security from an economic, military, and health perspective. Rather than specific accountability, project by project, discipline by discipline, or field by field, he called for scientists and their outputs to be measured in terms of general accountability. Success was to be determined by national achievement.

Sixth, Bush called for a national science organization. That is, he proposed concentrating basic research in a single area. He didn't call it the National Science Foundation. He and his panel had other names for it, but it was to be a single, major, basic research agency.

Lastly, he called for amazingly small budgets. I'm not sure if this was a political calculation on his part and those that were working with him, but the budgets that he called for were very small.

I've taken each of these seven design parameters and, thinking like a software engineer, I have looked at each of them from the perspective of how it could be improved, enhanced, or in some way made better.

Design parameter #1: political autonomy. It may sound like a strange response to this recommendation, but we should establish an institutional mechanism for forecasting our long-term national science and technology needs. This should be a rigorous, ongoing, continuous process that fills a current void.

One of the reasons that political autonomy isn't working for the scientific community is because nobody in the general population knows where they're going or why they're going there. And if they get there, how or when they got there. That is why we need a process that would generate a science and technology roadmap so that everyone can see where scientists are headed and why, and what that means in terms of implementation.

I am not suggesting that we replace the Office of Technology Assessment, which had its own problems. Rather, Congress should establish a means by which a national science and technology roadmap can be developed. A good example of this process has been carried out the last few years at the National Institute for Future Technologies in Japan, which conducts an exercise to plot the direction of national movement.

Second, the Office of Science and Technology Policy, regardless of the administration in power, must look to this roadmap and either follow it or explain why they're not following it. If done well, mission agencies can and should build their agendas around it. I know this sounds a little bit foreign, but I am looking for something concrete that people can think about.

Design parameter #2: self-regulation by scientists. On this parameter, I have three specific recommendations. One, spend a measurable percentage of all national science assets on educating the public about science and research. We are doing only half of this now. We're trying to educate about science, but we are failing to educate about research.

Second, and this is very controversial, develop a science court for internal discipline and conflict resolution. Bush made no account for this. The numbers of conflicts, questions, and debates, are only going to increase in the years ahead. If we do not develop some type of a mechanism, we will not be able to deal with the political backlash that will occur because we don't have the kind of checks and balances in the system that one would think we ought to have.

My third recommendation is to broaden the criteria for peer review to include the potential for considering broader social profit. Social profit is a poorly-defined term, but suffice it to say that it's an amalgamation of all those things not related to science. If peer review processes on a project and program level do not find a way to begin to include social profit as part of the decision-making process, the notion of self-regulation by scientists will have to be significantly modified at some point. It's under attack right now.

Design parameter #3: science for science's sake as well as for problem-solving. I think one of the barriers that we have to this is incessant fighting, discussing, and arguing over the definition of basic and applied research. The National Science Foundation is a basic research agency. The Environmental Protection Agency is not. We ought to do basic research here and not there. It's the old adage that my work is basic, and so therefore I can't explain it; and you just ought to fund it, because you're too uninformaed to understand it anyway.

We are going to have to define these terms once and for all, and there has been a major attempt to do this in the Press report (*Allocating Federal Funds for Science and Technology* Press, 1995). Second, we need to evaluate projects with regard to their purpose, realizing that the type of research – basic, applied or what-have-you – relates to the function of the mission agencies. I suggest that all government agencies have the possibility of doing basic research, applied research, and technology development in support of their missions. This is something that should be better understood and better organized. That is, we should bring discipline to an undisciplined process. Lastly, consider all projects and program areas as equal, regardless of their scientific focus or technical objectives.

One of the ideas that permeates the American university setting is that if you go through a Ph.D. program and you're then hired by another academic institution, that's great. If you get a job in industry, that's good. If you get a job somewhere other than those two places, that's not so good. There is a hierarchy in which basic research is the highest order function and all other functions are somehow lesser. I suggest that we find a mechanism wherein all research, all projects, are equal. This goes back to Bush's design.

Design parameter #4: a strong academic model of individual achievement. This parameter has led to a number of problems: barriers between disciplines, difficulty moving in new directions – a whole range of things.

We should develop new, team-funding mechanisms and expand the recognition mechanisms for team participation. We don't have that in the national labs. We have that in industrial labs, but not in academia. We should work toward the evaluation of scientists by discipline and by group. For example, what is the field of chemistry contributing, and to whom?

There is another consideration that goes beyond individuals and individual departments. These are what I call star groups, groups that have the capacity to make significant achievements. We need to find a mechanism beyond the individual model of trying to disperse resources to a large number of people in equal amounts. We should find a way to provide significant funding to these star groups.

Design parameter #5: general accountability. I think there should be a significant evaluation of agency research programs based on their success or failure to attain particular pre-defined goals or objectives.

If we know why we're moving in a particular direction, people should have some understanding of our logic. They will be able to see how or if an agency's programs contribute to moving toward a defined goal or objective. This may sound a lot like central planning, but it's not. I do not aim to differentiate projects based on an artificial modality. I'm talking about a way to determine, down to each and every individual project, the ability of a project to make progress towards a pre-determined goal or objective beyond merely the scientific goal or objective.

Looking at general accountability, this means that the White House Office of Science and Technology Policy and not the Congress – which would probably do this separately – would have a map. They would establish annual, five-year, and ten-year objectives for national science and technology investment. We don't do that now; we just talk about it. We put together the Council on Science and Technology, which has not been that effective.

We have to drive the process by the precursor step, which is constructing the scientific and technological map by asking where the science might take us. Then, following that mapping activity, decide upon a strategy or plan. Instead, what we do now is spend about 90 percent of our science budget on implementation and ten percent on planning, thinking, strategizing, and so forth.

What does this mean in a research agency? It means that U.S. government research agencies that are funding research projects to industry, academia, or laboratories and that don't have an elaborate mechanism for evaluating the progress of their research programs according to a national strategy and national R&D map are wasting money, since they don't have the means to evaluate whether or not they're making systematic progress.

They certainly can know whether scientists have won the Nobel Prize, but otherwise, progress is difficult to determine. We do not have sufficient or appropriate measurement tools today, but we need them. Developing tools of assessment is going to require some new mechanisms, some new thinking, and some new cooperation between social scientists and others that have the capacity to interact with scientists.

Design parameter #6: a single basic research agency. This is a bad idea because there are basic research questions that are linked to all of the agencies' missions. What you can have is a single basic research agency like the National Science Foundation which has a specific role. This agency is in charge of building the foundation, knowledge, and research tools to support the research activities of the other mission agencies of the government.

What does that mean? At a research agency, it means rethinking budget and planning models to define their roles as producers of foundation knowledge, basic knowledge, or specific solutions to problems. Some agencies are working on specific solutions to problems. One of the agencies might be working on foundation knowledge. Those planning and budgeting processes need to be linked together.

Design parameter #7: limited resources. Bush emphasized both in the words and their undercurrent, and in the class of individuals he had participating in the process to build *Science: the Endless Frontier* as a report, that limited resources should only be allocated to the best science.

It has been argued that one of the reasons to spend resources at as many institutions as possible is to enable a bell curve distribution of scientists, such that somewhere in the middle or on the right side of the curve, someone is going to be very successful. And therefore you need to have as many participants as possible.

I don't think that is a logical argument to sell to the public. Instead, one has to argue for two things: first, concentrate resources in the fields of greatest importance, linked specifically to their individual mission. Second, and perhaps controversially, dramatically increase the size of average grants – more funding for fewer groups – making the competition even more intense, in order to separate groups that have the capacity to compete on a world class basis from those that do not.

Clearly, we have moved beyond the parameters of Vannevar Bush's science policy design. The complexity of interactions in today's arena calls for equal complexity in the design of our policy apparatus, analysis, and planning. I have suggested science and technology roadmaps to address the outdated notion of political autonomy. Pubic education, science courts, and peer review reform will help to modify scientists' self-regulation. Looking more closely at the purpose of

research and developing tools of assessment will increase accountability. We need to increasingly work towards linking scientific research to societal outcomes and Vannevar Bush's design does not facilitate this goal.

NELSON: Thank you very much, Michael. Our first discussion panelist is John Holmfeld, who recently has been senior advisor for science policy at the Washington office of The Dana Alliance for Brain Initiatives. For many years prior to that, John was a professional staff member of the House Committee on Science, Space and Technology. So, John more than just about anybody else I know is in a position to respond to the question: How will proposals like that play out in Washington? (laughter)

HOLMFELD: These two papers do what I had hoped all of them would do at this conference. They're radical. They don't accept the status quo, and they ask us to think about some of the underlying assumptions that our science policy has been based on for these last 50 years. And I welcome that.

I think some of the themes that they deal with are ones on which I find myself thoroughly in agreement. They raise the question of the governance of science and the relationship of science to the political community. They raise this question of autonomy. They raise this question about whether or not the policy should be based on the relationship between means and ends. I think that's something we haven't done enough of. And they raise, both of them, the issue of the extent to which we can measure and quantify the benefits that this massive investment yields for society.

So, I find them very useful. I want to go on from that and make some comments on the rationale that has been the basis for this enormous build-up of the federal participation and the question about whether that should be re-structured, as Mike has suggested.

It is my observation over some years that federal officials and scientists who came to Washington and testified for a Congressional committee have presented a sort of three-pronged rationale for that build-up.

First is the technology rationale. Scientific research will produce a payoff in the form of technology, cures for diseases, new processes, whatever. Second is the education rationale. The funding of scientific research will also contribute to the education of the next generation of scientists. Third is the cultural rationale, the prestige rationale.

I would like to make some comments on the last two and then go into the Bush rationale in some detail. But before I do, I think it's important to indicate the extent to which the environment has changed, certainly in Washington, and I think to a certain extent in society as a whole. Not only as a result of the change in the funding situation, but also in the attitude towards science.

There's a story going around Washington – I have it probably third-hand and I can't verify it exactly – but it's illustrative of that change in attitude. You recall that this summer a move was made in the House Science Committee to reduce the role of the social sciences at the National Science Foundation, to eliminate the separate directorate for the social sciences.

And a number of scientific organizations wrote to each member of the committee, emphasizing the importance of the social sciences and saying that this move should not be pursued – including the AAAS, the representative of all of science in this country. And Congressman Frank J. Sensenbrenner, Jr. (R-WI) wrote back to the AAAS and said, "If the social sciences are so important, why is it that you hardly ever print in your journal a social science article?"

Now, that is mildly amusing at one level, but at another level, I think it is indicative of the attitudes of both parties to that exchange. On the part of the congressman and I think many of his colleagues, it indicates this change in attitude of questioning the statements coming out of the science community. They no longer are willing to simply say, "Oh, Mr. Nobel Laureate, Oh, Mr. AAAS, if you say so, it must be true."

And on the part of the scientific community, I think it represents a lag in being prepared to accept that. I think the scientific community has not yet reached the stage where they're much more careful in analyzing what they say and substantiating the kind of claims that are made. So let me go into a discussion of the three-part rationale.

The cultural or prestige rationale I think is real. It is with us. It's been discussed on several occasions. And the only point that I would emphasize in this is that it in no way can justify the level of research. It can justify a level of research funding by the federal government probably on the same order as the humanities endowment or the arts endowment. And we're talking about $200-$300 million a year, nowhere near the $15 billion in basic research.

The first thing I think that's worth saying about the education rationale is that I find myself in the uncomfortable position of having to disagree entirely with Dr. Cole's observation earlier today, that that rationale appeared in the Bush report. I have not found it nor have I found it in the Moe Report.

You recall that Bush commissioned three studies in preparation for writing his own report. And the Moe Report was the science education report. Both that report and what Bush had to say about science education was focused on replacing the generation of scientists that had been lost during World War II when very few Ph.D.s were graduated and on the issue of allowing more undergraduate students to enter college from families that could not afford it.

There is a one-sentence mention of the relationship between research and education in one of the two other consultative reports. But it is significant I think that Bush elected not to lift that out and incorporate it in his own report. The justification of saying research is good because it contributes to education in my estimation emerged only in the post-Sputnik era.

And it became prominent, because what we were trying to do in the post-Sputnik era was to increase not only science, but also scientists, the number of scientists. One of the things that we are facing today is that we don't hear very much about that. There are serious problems with that rationale. There's this intense debate on the oversupply of Ph.D.s in the sciences. And there's beyond that the whole question of the role that scientific research and the whole grant system is playing within the university structure.

Is the grant system and the associated reward system that encourages and provides incentive for working scientists at the university not to do undergraduate teaching? We all recall President Don Kennedy's call to his faculty to redress that balance, and it's proving to be very difficult to deal with.

But we are not hearing about that rationale. And we could probably spend two days in conference discussing that, because it needs careful, detailed analysis. So, let me go onto the Bush rationale itself – the rationale that science should be supported because it leads to technology.

What Bush essentially was saying – and I apologize to those who resent the discussion of historical developments, but I think it will be relevant to my recommendation towards the end – is that the findings of science go into a large reservoir, and technologists come along and open the tap on the bottom of the reservoir and pick out those science findings that they find can be used in the development of technology that they're interested in.

Now the federal government should pour lots of research findings into that reservoir, but with three qualifiers. The first one is that you cannot predict which of those research findings that you develop with federal funds will in fact produce a payoff. Some of them will, and some of them won't. The second qualifier is that for those that do produce a payoff, you cannot predict when. Some, it may come next year; some of them, it may come 30 years from now. And the third qualifier is that research in one field may produce a payoff in a totally different field.

That is a rather sophisticated rationale. And it's interesting that the Congress and the public accepted that and it served as a rationale for many years and still really does. What I would argue is that we came out of World War II with a second rationale, based on the observation of what happened in World War II.

In World War II, we gathered together lots of scientists, mainly physicists in five laboratories, and they produced technology. They produced radar at Harvard. They produced the atomic bomb, as we all know, at various laboratories. And they produced the proximity fuse and a number of medical innovations.

And the support that we saw in the postwar period was based on that model, namely established agencies or parts of agencies which like our war-time agencies would dip into the reservoir and pick out those research findings that would help us solve problems. And I think there are a number of models there. The Atomic Energy Commission was to do that. They were to develop civilian nuclear power. They were to develop nuclear medicine and a number of other things and perhaps do a small amount of basic research.

The big example in this field is the NIH. The postwar period is full of examples of additional institutes being established at NIH: National Eye Institute, National Dental Institute (now National Institute of Dental and Craniofacial Research), et cetera, which the Congress expected would similarly do research that would lead to solutions of these diseases or the prevention of them.

And that is still very much with us. We still see initiatives in the Congress based on the notion that you would establish groups or agencies, especially in the Defense Department, which would take the findings of basic research and translate them into technological application.

What has happened in the postwar period is that a number of these agencies have gradually – for reasons that are complex, I suppose – increased substantially their support for basic research. And I'd like to give a couple of examples of where we are today in the NIH because there has almost been a flip-flop there.

The NIH does a certain amount of clinical research, but the leadership of NIH and of many of the institutes are committed to molecular biology. Last fall, a House subcommittee held hearings on Parkinson's disease. The chairman was interested in Parkinson's disease, and he called a number of witnesses to discuss it.

The director of the Neurology Institute towards the end of the hearing was asked, "How much money do you really spend on Parkinson's?" And he said, "We don't do research on Parkinson's. We do basic biological research on how the brain and the spinal cord work. And we expect that some of those findings will, in fact, produce a solution to Parkinson's. We don't know when."

Another example is what happened when Christopher Reeve appeared before the convention in Chicago, just a few weeks ago. You recall, Mr. Reeve pleaded for support of spinal cord research. A few days after that, the NIH director felt called upon to issue a long statement, three or four pages, in which he said, we should all realize that we don't do research on spinal cord. We do basic biological research. Some of the research may be labeled Parkinson's, but it'll probably pay off in the field of spinal cord. And some of our research that's labeled spinal cord will probably pay off in Alzheimer's, etcetera, etcetera.

So, we have a situation where the expectation of the political system that research will be in fact targeted is being responded to by the scientific community and the federal administrators, largely in this case made up of scientists following the Bush model. And I think in the long run questions are going to be asked about that. That is where this question of quantifying the output will come in, and I think increasingly the younger members of Congress will ask questions about that.

You recall very recently, in the last Congress, the same kind of question was applied to the National Science Foundation by Senator Barbara A. Mikulski (D-MD) when she chaired the NSF Appropriations Subcommittee, and she directed NSF to do (quote) "strategic research." She was not satisfied that $3 billion worth of Bush-type rationale research – that is, totally untargeted – was really the thing to do. She has actually said that a large fraction of NSF funding should be targeted. And so I think that's what we're up against.

In some agencies, that transition has taken place in a formal way. I can remember in the late '70s being on loan from the science committee staff to the agriculture committee staff at the initiative of Congressman Raymond H. Thornton, Jr. (D-AR), who served on both of those committees, to write the section of the 1977 Farm Bill that established the competitive grant system at the

Department of Agriculture. But in most of the other agencies, it has been a transition that has not been formalized.

Let me point out an interesting development in this field. There have been a few examples quite recently where compromises have been arrived at between the targeted research approach and the Bush untargeted approach. The prominent one and the one we've all read about is AIDS research. When AIDS achieved a certain visibility in the '80s, the AIDS community, following the pattern of the postwar world, argued strenuously that the NIH should have a new institute on AIDS research. That's the way you dealt with this kind of thing.

And the leadership of NIH and many in the scientific community argued strenuously against that. We don't want another institute. We have had enough institutes established in our organization to do targeted research. And the compromise was to establish in the office of the director an office of AIDS research, which would be funded but which would provide its funding to each one of the individual institutes within NIH, which would in turn give out the grants.

Now, that compromise is still being fought over. Within the last few months, the House Appropriations Committee has declined to fund the Office of AIDS Research. They say it's a wonderful office. They should coordinate things, but they should not be funded. We'll give the money directly to the institutes. The Senate Appropriations Committee provides funding separately for the Office of AIDS Research. And so in the Congress, that debate is still going on.

Well, let me conclude with another anecdote. And it comes from the memoirs of Vannevar Bush. In 1944, the proximity fuse was a success. American forces were on the ground in Europe, and the military became concerned that if a proximity fuse was fired and was a dud and landed on enemy territory, they might copy it and begin to shoot down our airplanes.

So the joint chiefs issued an order saying the proximity fuse would only be used over open water where duds could not be recovered. Well, Bush's experts told him that it would take at least two years, if the enemy found a dud, to copy it and get it into mass production. So Bush went to see Admiral King, an admiral of the fleet and a member of the Joint Chiefs.

And King, who was sort of a crusty character, opened the meeting and said, "I have agreed to meet with you, but this is a military question. And it must be decided on a military basis to which you can hardly contribute."

And Bush, who was a sort of a crusty New England Yankee, himself came back and said, "It is a combined military and technical question. And on the latter, you are a babe in arms and not entitled to an opinion." (laughter)

And Bush concluded, "It was a good start. And the discussion went on from there and went well." (laughter)

The lesson I would draw from that little story is that we should still take Bush's advice, but that the roles today have been reversed. Today, it is the public and the political community coming to

science, and it is science that too often says, this is a scientific issue that only scientists should be entitled to decide.

I think the time has come to pay serious attention to the fact that this is a joint decision, that we should be willing to pay more attention and be more open-minded, including about some of the very controversial issues of geographic distribution, ear-marking, and all these other issues where the scientific community has been very rigid and unwilling to compromise.

My final remark is a comparison between the scientific community and the military. You all probably recall the enormous prestige the military was held in during the immediate postwar years. The GIs came back from the war and were given the GI Bill. Dwight Eisenhower was made President, and the military was held in the highest possible esteem.

And then over the years, contractual problems, the divisiveness of the Vietnam War, et cetera, et cetera, a long series of things happened – the military is still held in high regard, but people have in the back of their minds reservations about it. They're always asking questions: What's going on here? I am concerned that the scientific community will similarly go through a long series of missteps of that same kind and begin to decline in the perception of the public.

And to conclude, there is another reservoir that is still with us in addition to the Bush reservoir of research funding, and that is the enormous reservoir of public good will towards science. And we should be very much aware that we should not do things to reduce that. Thank you. (applause)

NELSON: Thank you, John. Our second discussant is Dr. Alice Huang who is a biologist by training and now is dean of sciences at New York University.

HUANG: Let me begin by thanking the organizers for getting all of us together to focus our attention on the important issues facing science in the United States today.

Now, of course, after Michael's talk, I'm ready to move in for the kill, but instead I'd rather join him. I'm going to make some proposals and give you some opinions, hang them all out there, and see whether they can be shot down or not.

I am going to take very much the roll of a discussant, from the point of view of someone who is in the trenches, who doesn't administer science on a daily basis. And I have focused on four points. One, I would like to put an end to the discussion of a central Department of Science.

I would like to suggest some mid-course corrections that scientists themselves can make. I would like to issue a warning about our loss of autonomy and how we can get it back. And I would like to give you a chance to vote, all of you, so you can participate on what we can use as a justification to support science.

I believe that, since the end of the Cold War, we as a group have been very often rudderless, without the strong captains that we had looked to in the past. People like Bush, Morgan, and Shannon. We have not always spoken with one voice, and we have become quite defensive.

The idea of organizing science funding under one Department of Science is to me less in vogue today, although it continues to be brought up. The driving force for that suggestion was due to the frustration of not having a Presidential science advisor for some time under Reagan and the hope that science would have a national voice of a cabinet member. We have all seen the central planning and the failure that has occurred in the old Soviet Union. And we are now seeing problems of central planning in both The People's Republic of China and The Republic of China.

I don't think that is the route we ought to go. Michael's suggestion of a science analysis office for the Congress is an important one, and I support that, to help guide decision-making processes and forecasts. Better yet, for our excess number of Ph.D.s, we should broaden their training, so that we can prepare all of them for public service as congressmen. (laughter)

Also, defining scientific missions and drawing better boundaries between federal agencies, getting rid of redundant support and organizing under one monolithic Department of Science is not a good idea. Boundaries as we have heard are not easily drawn in science these days, as we become more dependent on interfaces between scientific disciplines to solve our many problems.

We are a big enough nation and we are a rich enough nation that some overlap is probably to the good. I am reminded of a young scientist, an assistant professor, who was refused funding by both the NIH and NSF, to later get funding from the American Cancer Society. His work subsequently led to the important isolation of oncogenes and to the understanding of their regulatory mechanisms, without which the basis of any cures for cancer would be impossible.

Therefore, this kind of plurality serves science well, because no one of us is so infallible in predicting what will or will not be successful science. I cannot accept the elitist statement of funding only the best science. The corollary to that, if we really think about it, is to forget about all the others, creating much more of a have versus a have-not society. We have seen too many institutions get rich and bloated and arrogant, producing less and less value.

Flexibility in funding choices without neglecting to pull up the institutions at the bottom of the heap occasionally will help us to maintain competition among research universities and institutions. In addition, by concentrating research in only a few top institutions, how can we guarantee that the finest of our human resources will always find their way to these institutions? We all know about the Mississippian or Montanan who if they had not had access to a good research environment in their home states would never have made it on to the national scene.

Again, I would argue that diversity in the kinds of science supported, even at the expense of some duplication, is not to be avoided entirely. To deserve all this, we do have some self-examination to do. Here I echo and expand on what Dorothy Zinberg said yesterday. I run the risk of hanging out some dirty linen, but I think that these issues are fixable. So let's discuss them.

Support of science is not an entitlement. Let me expand. We need to continue to explain what we have accomplished. We also need to avoid the practice of the self-destructive, Chicken Little behavior each year crying out that science is in a crisis. We have also created schisms among

ourselves, between big and little science, between pure and applied science, all of which reflects a creeping parochialism.

We have not always used our funding most effectively. To cite an example, the initial funding in AIDS research. Too much money came too soon. Industrial liaisons with their huge financial rewards have eroded some of the collegiality and openness inherent to our university campuses. All these we need to address and change for the better.

Most importantly, we should be aware that science is dramatically changing how we live. Legitimate fears and ethical issues brought about by these changes must be addressed by us. We have become addicted to federal support, forgetting that there was once a time before World War II when federal support accounted for a very small percentage of science dollars. The rest came from a mixture of state, private philanthropy, foundations, and industry.

As we see the growth of foundations like Howard Hughes and more recently the Burrough's Wellcome Fund and the Ford Foundation, it may be that these institutions will permit some decrease in federal support. Our dependency on the federal dollar has blinded us to creeping political influences on how we do science. We need to watch carefully the intrusions of the political process and the distribution of even our peer-reviewed support from NIH and NSF.

Already, congressional hegemony over some of the federal agencies has resulted in funding of science by geography and not merit. I welcome the suggestion of a science court for settling scientific disputes, leaving out the use of congressional hearings for this purpose. Lost of autonomy by scientists will not only degrade the science that we do, but will poorly serve the nation, not improve the human condition, and definitely waste tax dollars.

Even at the loss of some funding, we should be clear that the scientific autonomy is worth preserving. Do not misunderstand me. Autonomy does not mean total freedom. Whether it be tax dollars, foundation dollars, or any other dollars, we will always have to be accountable and be able to justify what we do and why we want to do it.

That brings me to the final big issue. Without the justification of national defense, we have been searching for other means of justifying what we do. Several suggestions have been made in the course of these two days, and let me reiterate them. One, science contributes to the economy, national competitiveness in the global market. Two, science contributes to human welfare and a sustainable environment. Three, the process of science, the doing of science is important to the educational training of our young, whether they enter science or not. Four, knowledge for knowledge's sake, which translates into an understanding of our physical world, the space around it, and our place in it, providing us with the power to dispel myths and better control our environment.

We have a very experienced and distinguished audience here, so I invite you to think about these issues and raise your hands for those arguments you think we should use. All right? Do you remember all of them. I'll just do it quickly. Economy. Human welfare and environment. Education. Knowledge. Or all of the above. (laughter)

Economy. How many of you think that is a useful and good argument for us to use? Okay. Human welfare and the environment? About equal. Education. You may vote more than once. Education. Knowledge? All of the above. (laughter) All right. Let's use whatever we can and use them all.

When the first of these meetings was started here at Columbia, science did seem in considerable disarray, under siege from many directions. Within the last two years, new leaders like Harold Varmus have begun to have an effect. New allies in Congress and the executive branch have been found. Judged by the output of science – more papers of high quality, better scientists who are trained – we are doing pretty well, while technology as many mentioned is a growing successful industry.

We promised in the '50s that the studies of bacteria-phase genetics would lead to an understanding of cancer and old age. And that is happening. The "Decade of the Brain" is already showing tremendous progress in how we discriminate and process incoming signals, leading to retention and memory. We have just turned the corner on HIV research and have the first realistic hope that a vaccine or cure is possible. I hasten to add that the last two examples, the brain and HIV, are part of current research ongoing at NYU.

Rather than bemoaning the fate of science in the United States, I would suggest somewhat audaciously for this meeting that we all just celebrate science. (applause) By whatever means we can, we should communicate that success as well as that excitement. That is why I support outreach programs, new technology in teaching and learning. I'm working with Leon Lederman to get his primetime series on scientists and how we do science on to the TV.

So, on that positive note, I hope you know that we all have a great deal of work to do. We cannot rest on our laurels. And I will end there. Thank you. (applause)

NELSON: Thank you very much, Alice. The issues that have been posed by our presenters and by our panelists are important and fascinating and controversial. Why don't we take about up to 20 minutes for discussion of these issues, and then break for lunch.

FOWLER: I'm Alan Fowler. I'm a former employee of IBM. I'd like to challenge Professor Crow's ideas about what I regard as a bureaucratization and a rigidifying of scientific administration. It seems to me that the last speaker very ably made the point that diversity in funding is very important in our system, that a centralized way of doing this sort of thing is bad. I would also specifically attack his idea that you can lay out a road map for science.

Now, I worked for 35 or so years for IBM. Every year, we would have a one-year plan, a five-year plan, a 12-year plan. Now in general, those plans were very good, when the development of science was evolutionary. They were total failures when there was a paradigm shift as there was in the 1980s. IBM totally failed to recognize the possibilities that existed that smaller companies were able to take advantage of. And it seems to me that there's the same danger in laying out these plans of research on a national level. Thank you.

CROW: Let me try to deal with the second question first, which is the comment on the issue of what I called the road map as opposed to the plan.

I'm not suggesting a planning process of the same kind of contextual complexity that someone like the IBM corporation might develop. I'm talking about a different kind of process. I ask you to look at the Institute for Future Technology's five-year report, which comes out of a complex assessment of all of the principle scientists in Japan.

This is not a bureaucratic road map of, you know, this is what we have to do. It's an analysis of where science might go within context of national issues or national problems. And it's a very different kind of process than any traditional planning process that you might be familiar with in either government or industry.

And it's something that helps to guide thinking as to what are the problems out there that we might address. What are the findings that science might move us toward in the future? And it's basically something that helps to orient or to give a compass bearing to the masses, who in a sense have no compass bearing, have no way to orient themselves, have no way to understand where science might be taking us overall. That's the second question.

The first question as to bureaucratization. I couldn't agree more that increased bureaucratization would be as negative as anything that we might undertake.

MALE VOICE: If you're commenting as to R&D evaluation as being the means by which bureaucratization would be implemented, one certainly has to develop more of a means to evaluate what we're doing, so as to understand where we are on that path. One can perhaps do that with less bureaucracy.

FLAMM: Kenn Flamm, Brookings. Just a couple of comments I wanted to make. First, I wanted to offer a neo-revisionist critique of the proto-revisionist hypothesis advanced by Hart in the crypto-revisionist view of the Bush report from Crow et al. In particular, I think both of you missed the point, although you have very different sort of interpretations in some respect of the Bush report.

I just wanted to offer another interpretation. David, in particular, makes the point that there's this issue of confusing the means and the end. And that has afflicted U.S. science policy and discussion of U.S. science policy. And I think you can make a real argument that that in fact was one of the objectives of the Bush report, confusion of the means and the ends.

In fact, the logic of the Bush report in some sense is that the means serve unforeseen ends. And therefore, the means become the end. And that in essence is what all the endless frontier stuff was about. It is that the process, the means, becomes the end.

Now, Dr. Crow is quite right in pointing out that Bush also speaks of the need for work on applied problems as well. But a significant portion of the Bush report, it seems to me, is advancing the idea that the means in and of itself has become a goal. To support this particular means, because it serves unforeseen, unpredictable, and therefore undiscussable ends. I think

that's another interpretation of the Bush report, which kind of goes at odds with your fundamental chalk and cheese discussion.

HART: Why don't I just make a comment on that. Maybe our revisionism isn't too far apart. I didn't mean to represent my views as being those of the Bush report. And when I referred to the reservoir metaphor of the Bush report, I think that is exactly the kind of approach that he had in mind, as you described. I want to disaffiliate my comments with Vannevar Bush's.

FLAMM: That's okay. But then we're coming back to the point that in essence Bush was a chalk and cheese-ist, if that's the right way to put it.

And I also wanted to talk about the Bush report from the perspective of Dr. Crow, because I think you raised a couple of interesting points. It seems to me that if you look at the Bush report and the real innovative proposals in it, there is one key thing: in addition to setting up that the means is the end, there is also this idea of creating this institution insulated from the political process, which is going to fund this means, which is also an end, i.e., science.

And the insulation from the political process is purchased in two ways, one of which you thought was a bit mysterious, but which I don't think is mysterious at all. And that is by offering up a small budget for the thing. The surest way to attract political attention, I think, in Washington today and clearly probably also in Washington circa 1945, although I can't swear to it obviously from personal experience, is to have a significant sum of money out there.

And so that was one form of insulation from the political process, having this little thing with a little budget. No one's going to take that huge an interest. And it can actually get going. And you can actually partition this money off from the legislators on The Hill who are otherwise going to take an intense interest in where that money goes, by making it a small pot of money.

The other means of insulating it from the political process was this whole idea of non-mission specificity. I think the write-up for the next panel describes how, in fact, there's this huge commercial civilian technology agenda that's been sitting out there for some time that really doesn't get talked about in the Bush report.

You could argue that this idea of non-mission specific research was a way of separating what was going to become the National Science Foundation's activities from the entrenched interest – other agendas have already existed in Washington and were bound to come into existence after the war.

So, these two key features – a separate institution insulated from the political process and the small budget without a specific mission – were both devices to keep it away from the meddling of the politicians, if you will, in Washington, it seems to me. That's the neo-revisionist interpretation of the facts you're talking about there. And I offer it on the table for your comment and reaction.

The one other point, and I think it's interesting, is this idea of a national technical road map or a national scientific road map. And it's a particularly interesting idea, because I think in recent

years industry, despite the gentleman from IBM's comments, industry has found this a useful way increasingly to deal with some of the increasingly expensive infrastructure that has to go into some of these technology areas.

But I just wanted to comment to Dr. Crow that I think you've got it located sort of in the wrong place. I think if you're going to have a road map with any kind of lasting power to shape policy in Washington, it can't be a road map that is fundamentally shaped by political institutions, in the sense that those institutions have very short time horizons. Two years at best in the Congress. Four years at best in OSTP. They're always reshuffling, rejuggling.

You can foresee, for example, that we could define national technical goals in each of the separate areas that Professor Huang identifies, where in each of those areas, you'd have industry come together along with academia and perhaps define, without choosing between those different objectives, define the three or four most worthy technical goals that the government should consider funding. Because what actually sways the Congress, what makes Congress stick to the game plan? The bottom line is some kind of political constituency out there in industry.

Perhaps Mr. Holmsfeld would disagree with me on that. But it seems to me that the longevity of any kind of technical plan is going to depend on a significant group with real clout with the Congress, with industry and academia together being the ones who formulate the goals that the political process supports.

DEVONS: Sam Devons, Columbia University. I'm going to comment on something that surprisingly was not said. We live in an age that was certainly not spelled out by Bush or anybody before him, the age of cybernetics and the World Wide Web, which may change the character of the way in which we teach science and do science. In fact, one of our lively economists here, Eli Noam, published some papers that were quite widely read, in which he predicted that the electronic age would make the university superfluous or economically non-viable.

So in a sense, the whole electronic age, which came out of academic research – the university has forged the tool that will make its own destruction, make itself obsolete.

It certainly opens up whole new vistas. For example, you can do space science sitting at your desk at a computer, without going anywhere into space at all. And many do. Much of high-energy physics is computerized. Many of the conferences take place over some sort of a network or web.

These things seem to me a mixture of social and technical factors. But they obviously portend great changes. Whether they will make the university obsolete or change its character, they're bound to have a big influence. And it's just beginning. As a small footnote to this, particularly as I see from our slogan here, "Learning From The Past, Designing From The Future," what I'm referring to is very characteristic of the past.

The electron, it so happens, was discovered just about 100 years ago. In fact, next year is the official centenary. Nobody on any panel could predict what the electron would do to our society.

It was an academic venture, mostly in Germany and England, in Cambridge. Nobody planned it. Nobody foretold it. In fact, for 10, 15 years, it did nothing.

But it has transformed science, our society, our way of doing things. It may even have rung the death knell of academic science in some sense. But I'm surprised that nobody has mentioned the impact of this particular technology.

MALE VOICE: We have three more people in line, and we have three more minutes before we break for lunch.

EISENBERGER: My name's Peter Eisenberger. And I have the gall to ask the shortest question. I've heard us talk about objectives, and I've heard us talk about process. Would anybody like to talk about kinetics? What I mean by that is, assuming we know where we want to go and we have some good ideas about potential process, how do we get these very diffused, unorganized communities to come together and try to do something?

I have a feeling in all these conferences, we've been talking about these things forever. A freight train's coming at us, and we're talking about it. But there's no way to start the kinetics of doing something about it.

MALE VOICE: Twenty seconds. I would locate the key place in your interest groups in Washington. Those people have to become more sophisticated politically, and that's where the leadership has to come from.

MANDULA: I'm Barbara Mandula, and I have a short comment and a short question. These are both from Michael Crow's talk. I thought he talked about changing the NSF to developing tools for other kinds of research. At least a few years ago, there were some fields of academic basic research for which NSF was the only funder – you know, 90 to 100 percent of the researchers, I think one of them was anthropology, were funded by the NSF.

And so if one does decide that the NSF will not fund basic research per se, there are some fields that are going to have to be taken care. They don't naturally fit somewhere else. At one point, I actually tried to document duplicated research. And no matter whom I asked, everyone knew it happened, and nobody could give me a specific example. So, again, when this duplication argument gets made, does anyone have

END OF TAPE
(BREAK)

FUSFELD: Herb Fusfeld, RPI. The previous speaker had referred to the idea of getting industry's input into some of this planning. Let me give you a Bush anecdote. The Industrial Research Institute has most of the big companies as members. Their members do $80 billion worth of research.

The organization tried to get started in the mid-'30s, and it wasn't going anywhere. And about the late to mid-'30s – '36 or '37 – Vannevar Bush was working with, of course, the military and the

top political people in the country, very concerned about what was happening in Europe. And he wanted to be sure industry was ready to help in what he saw was coming. And he heard about this, and he said, if we form an Industrial Research Institute, we can have a mechanism by which industry can participate with government in its R&D planning. And Bush was a force behind the start-up of the Industrial Research Institute.

NELSON: I would like to thank the panel for a fascinating and provocative set of presentations. As always, the time for discussion is too short, but it's lunchtime.