

## **Roundtable Discussion (II)**

Dr. Donald Fredrickson  
Professor Evelyn Fox Keller  
Dr. Orville Bentley  
Professor Susan Cozzens  
Professor Eugene Skolnikoff

### **Moderator**

Vice Provost Michael M. Crow

CROW: We have an eclectic group for this panel. We're trying to touch a number of bases. They are connected, to varying degrees, and so we're going to look at health sciences, defense sciences, agricultural sciences, and social sciences in the post-World War II era. And we're also going to look at the effects of Bush-based science policy on a particular group.

To start, we have Don Fredrickson on the far end of the table, who is trained as a physician, has worked on Capitol Hill in his career, has been President of the Institute of Medicine, Director of the NIH, world traveler – including today [laughter], which is why he's going first. Someone is awaiting him in Europe. And he's going to talk on the impact of Bush policies within the health-sciences sector itself. So, Don?

FREDRICKSON: I have a broad mandate, so I'll do it in compressed bites.

The year 1944 was a very pregnant year, as many of you know. It was the year that Avery and his colleagues reported the transferring principle in the small laboratory they had in the hospital of the Rockefeller Institute – itself a remarkable institution from 1910 onward in America.

It in itself was the dawn of molecular biology, and the twilight of philanthropically supported research because, by that time, the benefactions, which were large, to the medical community for many years from the various foundations were draining off in the Depression.

When the Bowman committee met, they said what they thought about the state of American science. And they said that not one of the scientific disciplines could compete with the European universities, except the medical schools – the fact that they did well when it came to getting private support, which was so essential in that time.

The next era was about to end in 1944 because the Committee on Medical Research, which had funded \$20 million's worth of contracts to research for the OSRD, was going out of business. Now, when he wound down the OSRD in 1945, Bush was very much determined to keep medical science within his proposed new foundation. The medical-research

division was to have about \$5 million out of a proposed budget of some \$30 million, and it takes really nothing from Bush's tremendous contributions to conclude – even if it means I'll be stoned – that in the case of medical science, he'd been badly mistaken.

I think Bush's failure to sequester medical research was to prove over the next 50 years to be a blessing, not only for biomedical research but for the nation's universities and non-profit research institutions. The Public Health Service, which was the successor of the Committee on Medical Research, had not been Bush's idea of a proper steward for the research that was to come.

You can see this in reading *Science: The Endless Frontier*, where there's a great deal of text about his idea that any existent federal agency could be trusted to preserve the scientific freedoms that he and his advisors considered absolutely threatened by federal involvement in science. This new mechanism was also opposed by his own Medical Advisory Committee, which had tried to get their own separate organization, and by, really, the great bulk of the many academicians who tried to support the Magnuson version of the proposed new charter on the NSF.

I just wanted to give you a chart of the testimony of the head of Cold Spring Harbor Laboratory, a distinguished molecular geneticist at that time, who said the record showed clearly that placing fundamental research under the control of agencies that anticipate practical applications seriously limits and restrains the freedom of thought essential for basic advances.

Well, how did Public Health Service finally pull off this caper? In 1930, Congress converted an old hygienic laboratory of the Public Health Service to the National Institute of Health. Now, parental hopes were really grandiose, but the rechristening was most inauspicious for the daughter of the great Depression – NIH had no gallery at all. A few leaders of the Public Health Service, notably chief scientist Jimmy Thompson and Surgeon General Perrin, however, were determined to increase the medical research responsibilities and capabilities of the NIH.

In 1937, their efforts resulted in the acquisition of the National Cancer Institute, which went with astonishing unanimity and fervor through Congress when it was introduced. I think that some of the volcano heading up for the future of biomedical research was quite visible then, but it was not heeded by Vannevar Bush, who was an engineer, nor even his more conservative Medical Advisory Committee.

Well, the new Institute of Cancer was given the authority to give grants – a very small program, which ended during the war because it had no money. But the NIH learned a lot at that period about how to have peer review, how to do it by dual review with their committees, and they also learned those delicate and important differences between contracts, grants and aid, and grants. But an even more important and timely dividend for

the Cancer Institute authorities came in that same year, the summer of 1944, when Perrin and Thompson were assisting assiduously Congressman Alfred Bullwinkle of the House Commerce Committee to reorder the chaotic public-health statutes.

A bill emerged that extended the features of the Cancer Act, including its granting authority to all of NIH. And embedded in Section 301 of that very important Public Law 78410, which you have to memorize as director of NIH [laughter], were the most sweeping authorities for support in conducting research ever extended to any government agency.

Now the next year, NSF legislation was beginning to stall for the reasons we've heard today. The CMR was faced with expiration of all of its short-term research contracts, and at the last moment, NIH director Eugene Dyer was given a kind of cryptic message from Bush, a fraction of the unexpired grants.

The next year, 30 sections were already put in place. And within a year, an estimated half of the medical research in the United States was being funded through NIH. It's very interesting that the NIH decided to go right to project-research grants and fellowships as the major source of funding. And it continues today to be still the primary mechanism.

And it's interesting because the Bowman committee and the Medical Advisory Committee that served Bush both recommended large-scale, block grants to institute universities, which once in, would be kind of perpetual in endurance as long as the university had a committee to allocate the funds locally and kept the books for the auditor.

Now, why did the NIH do this? Well, I think they did it because Vannevar Bush had a penetrating effect on them, and so did the arguments that were loudly voiced in opposition to their being a potential steward. They were determined not to fail as a steward to this new program, as their detractors had all predicted in '45, and they chose proven methods, which certainly were known to be acceptable to Bush. They imitated the old OSRD model: study sections, peer review, an investigator-initiated project as the mechanism.

Now by 1950, the NSF opened with an authorization of \$5 million. The NIH appropriation in the same year was \$50 million, and Congress had already added four more categorical institutes. Nearing completion on the NIH campus was a clinical center to be the largest hospital in the world dedicated solely to research. And the initial funding for this had been obtained by NIH just a year after the money for the first grants.

You know, it's interesting that there was a backlash against what the NIH did in '50. In 1955, the secretary of HEW decided to put up a committee, for reasons that are not clear, to see if NIH was a proper steward for the money that it had. A committee was guided by the NSF, which had the authority to do so, and it had as its head the Dean of Yale Medical School, C. N. H. Long. The committee didn't take very long to come back with the report that NIH should be severed, the intramural program could go off by itself, and the

extramural funds should all be put into a new organization and the bulk of the money distributed to the universities in bulk grant.

Well, this report had an undesirable effect from the standpoint of its opponents because the next HEW chairman, Marion Fulsom, put up his own committee. It was headed by another dean of Yale Medical School, Bing Jones, and they diametrically opposed the first study – recommending that the NIH continue with a doubling for the next 10 years of its budget, that it assist the medical schools, and that it address the education and structure needed for better and more research.

Now this event took place just before the long reign of Senator Lister Hill and Congressman John Foray, who were the most remarkable troika, counting James Shannon, the director, that ever existed in NIH funding. The congressmen would bring citizen activists in to hustle up these hearings and to make the demands upon the Congress, with the real result of the doubling every three or four years of the President's budget between the period 1955 and '65.

Note that Bush insisted that the NSF not have an intramural program. I think it was such a multi-categorical group that it would be impossible to do that. The NIH, however, had an intramural program, and it turns out to be, in our view, extraordinarily important for maintaining the institution, the agency of the central core of the world's biomedical research for a long time.

With it setting generic rules, the ethics of clinical investigation, shepherding public acceptance of recombinant DNA research, for a great many reasons including the Selective Service Act – the intramural program and the clinics there actually trained a very large fraction of the next two generations of teachers in medical schools here and abroad. The clinic brought to a final point the conversion of clinical investigation to a really full-time profession from a previous avocation.

You know it's also important, in thinking about the various acts that occurred at that time, that in '54, Bill Carey of the AAAS wrote an executive order for President Eisenhower that ended Vannevar Bush's ideal of the single agency for the funding of fundamental science. It had two aspects. One was the relief of the NSF and its science board from defense research, but it also was an order that the mission agencies, of which NIH most certainly was one, could conduct basic research.

And after that act, NIH more or less remained outside, as did some of the other agencies, any attempt at coordination or oversight by the NSB or the presidential science advisory apparatus. Perhaps this will change for the President's new NSTC, which has for the first time included the NIH director.

I think that this may be the end of another era, because we have talked now a brief bit about the history that occurred during an almost entirely long period of Democratic Congress. I hope the prediction from other members of the panel that NIH should still prosper, or health research should prosper, is true. We'll have to wait and see.

CROW: Thank you. Why don't we go with one or two questions for Don?

ROBINSON: David Robinson with the Carnegie Commission. Do you have any advice for the physics and chemistry communities of how they can follow in the footsteps of the National Institutes of Health, other than hiring James Shannon to be the head of NSF?

FREDRICKSON: Well, I think it may have to be partly just serendipity, because certainly Mary Lasker wasn't recruited by the NIH in 1944 when she got interested in health research, particularly. But she and her followers really had a most remarkable effect on—

UNIDENTIFIED: Perhaps they might find some housewives who are prepared to testify about how concerned they are about the top quark.

FOX KELLER: That's what I was going to ask, what about the citizen activists for physics and chemistry.

FREDRICKSON: Actually, the NIH, by the way, came about because of a chemist who got after Senator Ransfield of Louisiana and kept after him for four years until his act was finally passed. I've forgotten the name of the chemist, but Ransfield went home to Louisiana and was defeated by Huey Long and was not seen again for a couple of years until he came back to recruit with a tin cup for the penniless NIH.

GELOBTER: Michael Gelobter, Columbia University. I think NIH is an interesting case of the tension between the hard or physical and natural sciences and other types of research. There are extreme estimates of how much research and basic sciences have contributed to improvements in health, but it's pretty clear that public health practices and social practices have probably contributed the most to public health improvement over the last hundred or two hundred years.

I think there's a flaw, perhaps, in the model of continued funding for health research, if for some reason the public should become aware of the fact that primary preventive care, for example, would do much more to lower health care costs and improve longevity, etc., in the long run.

How do you see that having been played out historically and into the future in terms of the tension between public health-type research and more social science-type research, into improving basic health and primary care vs. biological models? And the most extreme example recently, perhaps, being the Violence Initiative, in which a great deal of funding is

being targeted towards genetic sources and chemical sources of violence, which may or may not be significant.

FREDRICKSON: Let me say that the NIH had an effect on the Public Health Service that perhaps in the historical sense will not have proved to have been very good. The bench scientists took over Bethesda, and the rest went to Atlanta, the few people who had other missions and other ideas about public health research. And I'm sorry for that separation that existed for the next 20 or 30 years.

It's clear that public pressure, pressure within the community itself and not unrelated to the research community, will have to bring back attention to these pressing problems of the value of interventions, of prevention. But NIH, through the years, had such a concentration on science, such a demand on the quality and the way it did it, that it is not the primary organization to reform the health system. I'm sure of it.

LEDERBERG: Joshua Lederberg, the Rockefeller Institute. The remark about project grant vs. the institutional block grant plus possibly some other alternatives – I think there was great wisdom in the focus on project grants, for the following reason: As soon as you have large aggregates of funding involved, as would be the case if the chunks are to institutions, you can't but get into a lot of political jockeying.

You have interest groups trying to weigh in with the Congress; we've seen this happen over and over again with construction grants. Keeping the individual quantum of distribution at the individual projects was the way of at least minimizing the intrusion of secondary political considerations.

One of the problems of the project grants is that there's such a hypertrophy of demand for specification about just what the discovery is going to be, the anticipation before the event, that's strayed quite a bit from Jim Shannon's original perspective, which is to find the best people and give them, one by one, the freedom to do the best that they can. But I just wanted to delineate what some of the policy principles might be about the structuring of that funding. I would be interested in your comment on it.

FREDRICKSON: I'm certain you're right. Today, the NIH, just as then, puts out about 50% of its resources in research project grants, investigator-initiated. And that's despite the fact that there's been an enormous amount of micro-management of the internal programming of the NIH, so it's still possible to do it, but it's a hard fight.

SIMON: Yes, I'm Bob Simon with the staff on the Senate Energy Committee. We've talked a lot about project grants, institutional block grants. In the biomedical area, there are also track record grants, for lack of a better term, where the funding is given to a person based on an appreciation of what that person's track record has been, not so closely tied perhaps to a specific proposal for the next one or two or three years.

Would you like to comment on that? – because I think there's been more experimentation with that in the biomedical area, than perhaps in the physical sciences area. And a subdivision of that question is: What do you see as being the likely long-term impact of the entry of the Howard Hughes Medical Institute into the funding picture in biomedical research in this country?

FREDRICKSON: Well, one can never predict for long terms, but I think that the project grant system will still remain very strong for many years, unless there's a drastic change in the funding pattern. NIH has experimented back and forth with program project grants and grants of different sizes, centers for the various institutes, so there have been various experiments in different mechanisms.

But back when Shannon left and the funding of the NIH dwindled again, and the president's budget wasn't augmented, we had to go back to the question of counting grants, and we did so, and got a guarantee from the House that they would fund at least 5,000 new grants. I thought that would only last one year, but that was actually written into statute and endured for nearly eight to 10 more years, until the grants got too big and they lasted too long. The tendency was to increase the term and the amount involved. So it's a constant campaign to keep ahead, one step ahead of the system and of the political pressures upon it.

CROW: Our next panelist is Professor Evelyn Fox Keller from the Science, Technology and Society Program at M.I.T. She was trained as a physicist and has since moved on to the philosophy of science and the history of science, and in the last 10 years has authored five significant books in the issues related to science and society and society and gender.

And we have asked Evelyn to think about something that we're thinking about in the long term of our three conferences that we'll be working on: If Vannevar Bush's *Science: The Endless Frontier* was a manifesto, as people have suggested here today, it set about a series of designed conditions on the way science resources are allocated, the way that science is organized, the way that scientists behave. All that had effects, in different ways, on various segments of our society. And Evelyn is going today to talk about some of those effects as they relate to women.

FOX KELLER: I am going to take this opportunity to use aspects of the history of women in science to illustrate some more general points.

There's been a great deal of discussion about how productive the vision of *Science: the Endless Frontier* has been for scientific research in the 50 years since Vannevar Bush, especially in the physical sciences, but in medical sciences as well. But very little, if anything, has been said about the productivity of that vision for other parts of the university: the social sciences, the humanities. In fact, in the post-war period it is not only

the natural sciences, but also the university as a whole, that grew tremendously in size and importance.

The balance of natural and social sciences shifted and the balance of the sciences and humanities shifted, but all three parts of the university just mushroomed in the post-war periods. And all three parts benefited, however indirectly, from Bush's manifesto and the sharp distinction he drew between basic and applied science. That "manifesto" – that is precisely what it was, given its political content - was aimed at securing the autonomy of the scientific enterprise; but, in fact, it helped to secure the autonomy of the university as a whole.

As we now know, the dichotomy between pure and applied science was, in part, a fiction. It was a useful fiction, perhaps even a necessary one, certainly for the conduct of natural scientific research. Especially after the Manhattan Project, as we saw from the editorials, the temptation to continue along the model established by the Manhattan Project was strong. So it was an extremely important, useful, and necessary fiction.

Bush's manifesto – or the indirect benefits of Bush's manifesto – bore out Descartes's prediction centuries before about the widespread benefits of the support for the basic sciences. We can learn about the fringe benefits for other parts of the university, or the university as a whole. They are important to understand, and important to understand in relation to the ideology of the time.

Not everyone benefited from this agenda. One group that did not, and is a notable exception, is women in science. At least, they did not benefit in the short run. In the 15 years after *Science: the Endless Frontier* was published, the participation of women in science declined drastically, so much so that we tend to forget about the presence of women scientists in the years just before – for example, Ruth Howes has identified 100 women scientists who were involved in the Manhattan Project. But in the 15 years after the Bush report, many factors contributed to this decline. Certainly women were not pushed out of science by any overt or explicit program, nor was it a result of explicit sexism. Rather, it was a combination of many factors. One factor was the widespread belief that women just didn't have it, and since our aim was to produce the best possible science, we should involve only the people who did.

The degree of disenfranchisement of women scientists can be seen most poignantly in a report of the Hamilton Committee, established in 1956 at M.I.T. to consider whether after 100 years, M.I.T. should admit women. The unanimous verdict of that report was no, it should not, that it was not in the interests of women and it not in the interests of M.I.T. It was, as they put it, a misuse of resources.

Now, as we know, that situation has changed, and changed very dramatically. The turnaround began in the early 1960s, in part in response to political pressures from above,

in particular as a response to Sputnik, and the concern about the growing shortage of scientific manpower. So the government, and the National Science Foundation (NSF) in particular, made explicit attempts to recruit women into the natural sciences. The turnaround was a result of pressures from below, more diffuse social pressures, and the rise of the women's movement.

The success story of the increase of women in science in the 1960s, 1970s into the mid-1980s was very dramatic. While the proportion of women doctorates had declined by the late 1950s to roughly half of what it had been in the 1920s, it quadrupled in the decades afterwards. This is a success story. It is a story that illustrates a very productive interaction between the institution of science, the natural sciences, and the political processes. As I said, the pressures were both from above and below.

It also illustrates the problematic nature of the notion of the autonomy of science. The autonomy of science was an extremely useful, and necessary, ideal in the years after World War II. It was necessary to emphasize that ideal, but as an ideal, it was ill-thought out, and it failed to do justice to the equally important ideal of responsibility of science to the society in which it exists.

In some of my work, I have developed the notion of a dynamic autonomy as an appropriate ideal for individual development. Perhaps some notion of dynamic autonomy is also more appropriate to thinking about the contract of science with society.

The question of when, where, and to what extent science should be responsive to political pressures, and to what political pressures, is a fundamentally and irreducibly political question. It cannot be answered on the basis of any scientific, statistical study. Even the story I tell you about women in science is arguable. There are people who would say the increase of women in science has not been good for women or the social fabric because it undermines the family.

I believe it has been extremely productive both for women and for science. Most obviously, it has brought into the practice of science a whole new reservoir of talent. Some people say it has enabled the introduction of different perspectives. I am skeptical about this argument, but I do believe that the increasing number of women in science has contributed very critically to undermining what had been a very costly ideology for the conduct of science, the ideological equation between thinking scientifically and thinking like a man.

Not everyone will agree with me about the productivity of that responsiveness to political pressure. My point here is that if neither the Manhattan Project nor some ideally autonomous insulated institution is an appropriate model for science, just where we are to come down between those two extremes is not something that we can answer independent of context, independent of time.

After World War II, the fiction of an autonomous science was necessary because the dangers to the conduct and autonomy of science were so great. In the late 1940s, Bush sought to secure for the conduct of science freedom from convention, prejudice, and commercial incentive. He saw the university as the best location for the conduct of research, the location that would best guarantee these freedoms. He also helped to guarantee that the university was the proper locus for these freedoms.

By the 1970s and 1980s, the situation of science in the universities had become very different. As an institution, it had become vastly stronger. Some people would say it had become too insulated. Certain limitations became evident that were consequences of too great an insulation of the scientific community. The scientific community had become exclusive. There was no conscious prejudice in keeping women out of science, but certainly there was a great deal of unconscious prejudice that has since been exposed by historians.

Today there are other issues. One question that has arisen is “Where are the scientists of the next generation going to come from?” There is obvious exclusivity still operative in the scientific community. And we need to think carefully about how these exclusivities come about. There are appeals for science to take more responsibility for thinking about the underclasses of the world, and I agree. All those extensions of the mindset of the scientific world need to be practiced.

At the same time, however, there is a way in which I would have to agree with many who would say we have gone too far. In the 1990s, there is a new danger to the autonomy, not only of science, but of the university as a whole, and this danger, I suggest, requires us to rethink and regroup.

The danger I refer to comes not from the left, as some people have argued, but from the right. It is promulgated by a different set of fictions, not the fiction about the autonomy of science or the sharp separation between basic and applied research, but by fictions that are as useful to those who would wish to undermine the autonomy of the university today as Bush's fiction was to him in underwriting that autonomy.

One of these fictions is that science has become a hotbed of fraud and deceit, and therefore requires surveillance by independent committees, perhaps Congressional committees, to oversee the proper conduct of science, and perhaps even direct management from the public sphere. This concern with the inability of the scientific community to manage itself coincides with the decline in funding for basic research, and surely it serves as fodder for the arguments in favor of more direct management of science.

On the other side of the campus, in the humanities and social sciences, another fiction has been promulgated – that is that the university has been taken over by a band of subversives,

feminists, multiculturalists, post-modernists, and that the humanities and social sciences need more vigilant control. Many people expect, in parallel with the decline in funding for basic research, that the chances for survival of the National Endowment for the Arts and the National Endowment for the Humanities in the next few years are quite minimal.

Some people have even blamed this band of subversives for the decline of funding for the basic research. But given how difficult the literature of high theory in the humanities and social sciences is, not to mention the literature of the natural sciences, I can't imagine that Congress has been so influenced. Still, both of these are useful fictions. Not that they aren't based in fact, there were frauds and misconducts that were available to identify and inflate public anxiety, just as there are scholars in the university writing irresponsible about the sciences, coming from the humanities, who are not adequately informed. The use of examples of abuse to discredit the whole enterprise – on the one hand, the natural sciences; on the other, scientific study or even the university as a whole – is the problem I want to call your attention to.

As for the solution for these abuses: I believe those in the natural sciences can manage their own affairs. They are as concerned as anyone with the practice of fraud, deceit, and misconduct. And I believe the university as a whole can manage its own affairs, that the time-honored practice of dialogue and mutual respect and good old-fashioned academic freedom is exactly what we need.

If people are writing irresponsibly about the sciences, it is the responsibility of the scientists to educate them. My fear today is not the undue power of the natural sciences that worried many people in the 1970s and 1980s, including myself. Indeed, today the power of the sciences does not seem to be so great. Far more, I fear the power of a political structure to roll back all of the positive gains bequeathed to us from the Bush legacy, not only for high-energy physics but for the university as a whole.

CROW: Are there a few comments or questions before the last three panelists?

L. HORNIG: I'm Lilli Hornig, Wellesley College. I'd like to elaborate a little bit on some of the early part of the history that Professor Keller just described, and that has to do with the immediate post-war era and the, let's say, relative disenfranchising of women in higher education and in the sciences, both, during that period.

Earlier today, Harvey Brooks alluded to the effects of the G.I. Bill, and it's not often recognized that bill had an enormous impact on the education of women, in the sense that it made unavailable many of the places to which they had previously been admitted. The major state universities, for example, the University of Michigan at Ann Arbor, literally arbitrarily reduced their admissions of women by 30% in order to be able to accommodate the entering GIs. And the after-effects of this policy, or by-products, surely unintended, were to create a climate in higher education that envisioned it as a predominantly male

enterprise. This had not been the case before the war, and I think that's not widely appreciated.

In 1940, women were approximately 42% of graduating classes. This dropped down to well below 30% within the next five years. And the effect was not due to declining enrollment of women; they were simply pushed into other institutions, predominantly what had been the normal schools, which were now rechristened state colleges. But they did not in the course of that rechristening add large laboratory facilities or a great deal of science faculty, nor did they have engineering schools. In other words, the institutions to which women were more or less freely admitted did not present possibilities for pursuing science in any way we now regard as meaningful.

And I think this is an excellent illustration in the context of the policy discussions during this meeting, of the unintended effects of what are otherwise wonderful policies. We need to, as Professor Keller emphasized, really try to include all of the possibly affected groups when we devise some of these policies. [applause]

FOX KELLER: Thank you for the comment.

ETZKOWITZ: Henry Etzkowitz, State University of New York at Purchase. My question is inspired by my former colleague at Purchase, Evelyn.

In the 1930s, Carl Compton, as President of M.I.T., went to Washington and tried to persuade the federal government to "put science to work." To help end the Depression by putting science to generate new economic growth for the country. At that time, the scientific community rejected Compton's initiative, fearing that if the federal government invested money in science, put money into the universities, that this would inevitably lead to influence on the research agenda, to control over the direction of science.

Now, after the years of the fiction of autonomy being stripped away, does this finally come to roost? And must we now accept the consequences that if large funds are to be distributed, that eventually the holders of the purse will want a say in how those funds are distributed? I recall just a few years ago when there was a debate in Congress over whether funds should be earmarked, whether universities could apply for special centers, as Columbia did for chemistry, in fact, and successfully won one.

The argument was that, no, they should go through the peer review process. And a congressman said, what is this, what is the peer review process? What do you mean? You mean I can't have a say over public monies that are spent? That's unheard of, money to be outside of the regular political process. And so now that the fictions are stripped away, won't we have to make our justifications for funding for science on the basis of new ground of legitimacy? For the past several decades, it was implicitly on the basis of support for the military. Now that's lessened.

The issues that are now before the nation have to do with economic competitiveness, with social problems that we're only beginning to recognize officially. This may mean perhaps involvement with social scientists, perhaps not. The debate may say that government should not be involved in any of these questions.

I would suggest that we're about to enter upon a debate over the future of sciences, their role in society, and we should be willing to enter into it and engage in lobbying and other political activities that are incumbent upon any group in society that wishes access to the public purse.

FOX KELLER: I don't think there's any doubt that we have a very difficult task before us, that we cannot return to the fictions of the '40s. Not all bargains are equally Faustian, but I do agree that the particular illusions that helped to build the strength of the research enterprise can no longer be maintained. That doesn't mean that we immediately forget any notion of the autonomy or the independence of science. Science does require insulation from direct political process, there's just no question about it.

The notion that it can be completely insulated from social and political pressures is a joke. But that doesn't mean that we don't have to think of ways that guarantee some capacity of the university as a whole and the scientific professions themselves to govern themselves.

CROW: One last comment or question.

CLAUSI: Yes, Al Clausi, the Institute of Food Technologists. I would like to compliment Professor Keller for using the expression "to dispel the fiction of the independence, if you will, of basic science." I think it has been a fiction and I think if we draw a new manifesto, if you will, that's based on that fiction, in the new society, we're not going to succeed.

We should draw a line more on the basis of maintaining the basic-science capability, which is important – it's just as important today as it was back in the days when the original manifesto was laid out by Dr. Bush – the creativity that's connected with that, but build it into a better structure, if you will, a more responsive structure that can deal with the 21st Century and our country in the 21st Century and our science in the 21st Century.

We are leaders in many areas and we want to remain leaders in those areas, but we're not going to do it, in my judgement, by perpetuating a fiction. We're going to do it by talking about what the need is and how we can best accomplish that need. Thank you very much.

FOX KELLER: I second that.

CROW: We'll take the next three panelists as a group, because they're each going to be looking at a sector of science and technology or science itself in the post-Bush

era, and then we can deal with questions and comments from the floor.

Our first panelist in this section is Orville Bentley, former Assistant Secretary for Science and Education under President Bush and President Reagan in the Agriculture Department, Dean Emeritus at the University of Illinois, and a lifelong agricultural researcher trained in biochemistry. We thought it would be very important to have a leading agricultural scientist and a leading agricultural science administrator as a part of this panel. So, Orville.

BENTLEY: Thank you. I haven't heard much speaking about little science and big science here. I represent sort of little science, as far as the federal budget is concerned, even though it is an event that started over a hundred and some years ago.

I want to just track a little bit of the history of the formation of the so-called agricultural research system. It's not a firm system, it's a cooperative, diversified, extensive system of people working together.

It started really with the grant at the time of the Civil War to start public universities teaching agriculture and the mechanical arts and those sciences that related thereto. The system of research, though, really had many origins. Certainly, it was greatly influenced by the science that came from Europe, and the need to have a kind of scientific development system that was more apropos to the needs of a rapidly expanding nation in the post-Civil War era.

Remember that there was a great western movement, there was a movement from people that had been in the Eastern part of the United States, who left because they said the land was farmed out. And they moved to new lands that were available in the Central United States and to the West. The political pressure that came on to create the experiment stations, like many things we talked about, took a long time.

A Yale University teacher in Latin who came to Central Illinois to teach Indians Latin in the 1830s said this is a hopeless task, what we need to do is help people learn how to farm. And he said, let's have something like medicine or something like engineering technology, and we'll apply it to the needs of what he called the working class – the farmers and their offspring. Well, the experiment station really in a sense is kind of a model. It's one that has been maligned over this 120 to 130 years. It's had successes and it's had failures.

By the Hatch Act, the Congress said it shall be the policy of the United States to promote research in agriculture and all related areas. It's only about four or five sentences long, and it's about as well-crafted a statement as you can find by Mr. Hatch, and he apparently wrote three or four other pieces of fine legislation. It created a system of agriculture experiment stations in every state to think about the diversity and the site specificity of agriculture, and it addressed the need to get some system that would be functional at the local level. Almost

all of those experiment stations were attached to the then-developing colleges of agriculture and universities.

It made the magnificent sum available, on a matching basis, to each state, provided they adopted the proper legislative authorities, of \$15,000. That amount of money didn't go up very fast, and it took a while to establish the idea that book-learning had anything to do with agriculture. At one time, it was said that anyone associated with this kind of activity was not allowed to come on farms, that they were dangerous and dogs would attack them if they did.

I want to go now to the concept that in the agricultural sciences – whether done in universities or in cooperation with the U.S. Department of Agriculture, which had been established also during the time of the Civil War – that teams of people working with the private sector could begin to solve important problems.

Someone graciously mentioned the hybrid corn, and I'm going to use that example a little bit myself. Hybrid corn was based upon the application of the Mendelian laws of genetics, and the inbreeding and the fixing of the corn was done in several locations. That created industries and made Pioneer Seed Company a tremendous organization. And it said to farmers and the country as a whole, that here we could take something of a biological nature, and it could be manipulated in a way that increased productivity.

Yields of corn or maize corn in many of the states doubled to tripled, even in the last 40 or 50 years. This year, we have a 10-billion-bushel corn crop – it's going to be hard to get it sold whether in export or at home. There was an equal headline in the late 1930s that the United States, for the first time in its history, had produced four billion bushels of corn. And the number of acres under cultivation is not a great deal different than it was at that time.

When we had, for example, wheat rust that had devastated the wheat crop in the mid-United States, on the Great Plains especially, we found out you could control that by understanding rust and how it was generated and how it was distributed. The cotton boll-weevil devastated the cotton industry, the grasshopper hoards were legendary in the West – all were examples of how basic and applied sciences could be used to solve problems that affected literally a major proportion of the agricultural area, in many geographic parts of the United States, and for that matter, around the world.

Well, those kinds of things began to greatly legitimize the idea that the federal government should, through these grants to the states, work with these kinds of problems, whether it's with soil, water, rural living, crop production, livestock health, and all of these types of things.

Now, it's not just an agricultural question, it's a total science question, as far as the Congress is concerned. There've been argument about whether there should be more peer review grants or block grants, as would be the case of a grant to a state. The real success has been that states have appropriated a great deal more than the Congress has ever put into the agricultural research section and that the system was flexible enough to accommodate new developments.

Now, I don't know how we're going to go in the future, but it seems to me that there are some fundamental things that made the system work as well as it did – what made it possible to say today that the entire agricultural productivity of the United States is science-based or drawn upon technology, whether it's in mechanization, biological sciences, the application of molecular biology, physiology, biochemistry.

The multidisciplinary approach that has been used in agriculture is absolutely essential to looking at almost any problem, especially those dealing with water quality, air quality, the environmental impact of agriculture on soil erosion, on pollution, whatever it might be – they require a well-developed, multidisciplinary approach.

And the best way, it seems to me, that this can be done is in the university setting. The provision of both the competitive grants program that's well established and that's functioning very well through the Department of Agriculture or from the existing grants and those funds provided by the state will have to be structured to maintain the strong disciplinary orientation to solving problems.

It's also important to understand the political base. There was always an overarching question: Are we sure that we can maintain a stable, affordable, nutritious food supply or the supply of forest products for the future?

When we consider the population issue and the numbers that people are predicting for our world population and some of the problems of food production and its distribution – this is as highly relevant today as it was when they were talking about the creation of the Hatch Act or the experiment stations in the agricultural research in the 1880s.

The stability and continuity of the system and the ability to work with industry are all important – after all, much of the application of agricultural research and technology has been the initiative and the willingness of the private sector to attack these problems. This is the case, certainly, in the chemical industry, where huge organizations devote a great deal of their research, both basic and applied, in the development of chemicals that can be used to further agricultural production.

Then there's the question: How can the research system deal with the impact of the application of technology to the farm group? Mention was made that only 3% of the work force is involved in agricultural production, whereas a few decades ago that would be up to

50%. It's changed our rural communities. Sixteen percent of our population is in jobs that have to do with food and its preparation, delivery to consumers. We have to be more sensitive to consumer preference, dietary changes, changes in lifestyle, and so on.

Well, I think the system has the capacity if it is funded, even as modestly as it is today, to be flexible, to be able to identify and address some of the problems and to share the scientific and technical capacity to improve the well-being of our people.

Thank you.

CROW: We have asked Susan Cozzens, who is a Professor of Science and Technology Studies at RPI up in Rensselaer, New York, to take on a task that we've bounced around a little bit today: What did the Bush manifesto mean for the social sciences and what has been the impact in the post-Bush era for the social sciences? And so, Susan, that is a heavy load, but I know you are ready to take that on in the ten minutes that we have given you.

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 knowledge 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 21<sup>st</sup> 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.

CROW: The last and perhaps historically most complex and gargantuan task is attempting to understand military science and technology before Bush and military science and technology after Bush. In searching the land far and wide, we find a man trained as an engineer and as a social scientist: Gene Skolnikoff is Professor of Political Science at Massachusetts Institute of Technology and has, as you will note in the bios, been involved in a number of aspects related to defense, science, and technology. For the final ticks of our clock, I will turn it over to Gene.

SKOLNIKOFF: Thank you very much. I can't think of a more inappropriate introduction to what I'm supposed to cover. [laughter] I also can't imagine a worse position on the

program: I'm the only thing that stands between you and the drinks. Just as Harvey Sapolsky from M.I.T. – there may be a pattern here – stood between you and lunch. Moreover, I'm covering a subject that has been mentioned repeatedly during the day. So I'm going to try to be brief if I can.

Let me make one quick comment on the last points about social scientists. As I recall, in the early days of the President's Science Advisory Committee – their repeated discussions about how to include the social sciences in the work of the White House Science Office and whether a social scientist should be on the President's Science Advisory Committee – there were certainly none during the period that I worked in that office. And if I'm not mistaken, the first one who may have been appointed a member of the committee was Herbert Simon.

And in the report of the meeting of the first successor to PSAT, the PCAST, the President's Council of Advisors in Science and Technology – at the very first meeting, there's a little phrase, apparently in one of the reports when discussing global warming, that the social sciences were apparently unable to make any contribution to the study of that subject, which Jack Gibbons agreed to. So I think we still have a problem of how adequately to represent the social sciences in policymaking.

Let me just mention one personal note. When I started teaching at M.I.T. in the mid-60's, Vannevar Bush was there, and I had him a couple of times meet with my class on Science and Public Policy, to talk about some of the key developments of the report that we're here to talk about and what happened afterwards.

He resolutely refused to do that at all. All he wanted to talk about was, with great relish, how he dealt with the politicians in the Congress and how he managed, treating them with considerable, in those discussions, disdain, how he always managed to come out on top whatever the Congressional attitudes were, and how he manipulated them and how he had them dealing out of his hand.

Though this was a certain amount of showmanship – all undergraduates just loved this – at the same time, I think this did in fact reflect some of his attitudes towards what the politicians were like and reflected his continuing view that we've heard about repeatedly today, that somehow science had to be protected from interference as much as possible, had to be buffered from interference with the partisan political process.

I should also mention, by the way, that landmark is about to disappear at M.I.T., the building that housed the radiation laboratory, what's left of it, will actually go next year. That's 50 years after the end of the war, we finally are getting rid of that old, wonderful, in fact, wooden building. Many of you, I'm sure, have seen it. But it's going to be torn down, not because it still isn't good but because it's got asbestos.

As I said before, we've been talking about the military role repeatedly, and every point that I might have wanted to make, one by one, has been carefully made by somebody else. Let me just make a few comments about the general situation – pre-war, during the war, and post-war – that are relevant.

In the pre-war situation, all R&D essentially was assumed to be carried out by industry in the course of procurement. Some exceptions to that: there were some in-house R&D laboratories, the Naval Research Laboratory was started after the first World War, I believe. But by and large, what R&D was carried out in the military area was assumed to be done in industry and to be paid for through the prices of the products. There were no separate R&D contracts with the private sector.

And as Nathan Rosenberg said, the agricultural research just prior to the war was larger than the military research in absolute amounts. During the war, of course, as we know, Bush turned to the private sector. I think Lew Branscomb called it "privatizing science" in a way, but he certainly had no qualms about moving rapidly in OSRD and DRC to turn to the private sector, especially the universities, for the conduct of R&D related to military purposes.

He established several important ideas at that time, one of them being the full-cost contracts, which pay the full indirect costs. Of course, we see that issue coming back, a serious problem for the universities if the "Contract With America" is carried out as is forecast. The continuing increase in overhead rates, I think, will not continue to be politically viable. Bush also established the principle of parallel paths to development of weapons systems rather than simply one contract to achieve one purpose.

As we know, he worked very closely with the military, with a good partnership, but I think that partnership very much reflected his own diplomatic and political abilities, rather than any real conviction that the military could be trusted with research. I think he had basically no confidence in their ability to understand science or to see new weapons possibilities. And that, I think, was one of motivations for his call after the war to have a division of NSF that would be devoted to defense science to supplement what the military services would actually do.

By the way, Sapolsky and Zachary and David Hart all pointed out the very political context in which the Bush report was made and received, and that, in fact, there was a political necessity for him to have a report. The implication is we shouldn't necessarily take too seriously some of the details. That it was a political context and a political move that had to be made – I think the judgment of that is correct, but I don't think the conclusion is correct. That is, the fact that you had to have a political demarche of some kind doesn't negate the importance of what was actually said, what influence it may have had, and what developed subsequently.

After the war, there was continuation of what had been started during the war, that is, direct military support for R&D in industry and universities and in-house. The whole development was certainly much larger than Bush could ever have anticipated. They talked about a steady state of the Research Foundation being \$100 million, and of course, we know what the total support grew to be.

Many contracts were developed with the private sector, both industry and universities. Concepts like spin-off were assumed – I don't know if the word was used at that time. The assumption was clear that military R&D would eventually pay off in economic terms. It wasn't necessary to spell it all out, except in the very general philosophical way that Bush did. At times, there was a lot of direct support from the federal government for development of particular technologies, for example, in computers. At times, there were guarantees, contract purchases if technology was developed to meet military requirements.

I happened to be in a position to receive the first videotape recorder. In fact, it was the second, because AMPEX violated their contract and sold the first to CBS to make a big publicity splash. But in any case, I was a specialist third class in the army, and because I had once been at M.I.T., I received the VCR – it wasn't called that then – and did the initial tests on it. There was no question that everybody felt at the time that this machine would have enormous commercial potential if you could only reduce it in size and volume. AMPEX, the one that produced it, tried very hard, failed, and the rest is history about the Japanese.

During this whole after-war period came development of the national laboratories. The in-house laboratories in several agencies, not just defense, we often forget, now have R&D funding that's roughly twice what goes to the universities, something like \$22 billion in the current budget.

Today, many of these issues are still with us, still dominated by politics, though we insist on trying to talk about them in purely rational terms. Some of the issues are different – the concern about commercialization, it's obviously very different.

The goal of insulating science from politics is a constant issue, that's not a new issue. But what none of the people involved understood, including Bush, was that the scale of support for R&D was going to grow to such an extent that you could no longer insulate it in the same way from politics, and more to the point, you don't want to.

Somebody made the point earlier that when you start having substantial budgets, you've got to have a champion. I think that's true of R&D and universities in general, that if you insulate it too much, you won't have anybody to defend it in the political process, and you really do need and want to have a champion in the process.

I remember Alan Waterman in the NSF essentially never saw the president. He had almost nothing to do with the political process, either in the administration or the Congress, and I think that had a lot to do with the scale of the budgets that NSF had until after Sputnik and after the White House Science Office was created.

It seems to me interesting that after all the war-time experience, there is really not very much attention to defense science in the Bush report. There's one piece of it, one committee and the talk about the NSF division, but I don't think there's really very much a discussion about it, which is a bit surprising.

I think we've paid a price for the war-time focus on setting up large projects with big objectives – Manhattan Project, the radiation laboratory, and then later the Apollo Program – with a general view in the society that all you have to do is put people together, throw resources at them, and you can solve any problem.

I would argue that the SDI Program, which is coming back today, still represents a real ignorance in sort of rational terms, ignorance about the limits of science and technology – but perhaps more important, it represents the political overlay that will continue to govern what actually happens.

Obviously, since the mid-1980s, there's been increased focus on competitiveness and the end of the Cold War, with defense developing new roles. ARPA has now a formal economic responsibility, not just defense or even dual-use, but it is supposed to contribute to the economy. And national laboratories are seeking industrial partners at the center of whole new programs.

Last month, we had this political earthquake, but we have to remember that the concern about this started much earlier. It's interesting that no one, I don't think, has mentioned the word "social contract" today, about the demise of the social contract between the government and the scientific community, but it's an issue that we all recognize predates the election, but which is going to get more serious because of that.

One issue I'm concerned about is this concern about where the money goes and how much there is, the danger of competition between the national laboratories and universities for what look like the same dollars. I don't think that's very prevalent today, I'm concerned it could be tomorrow.

I am also concerned about whether the openness question of research in the context of international competitiveness will become a problem. It started to be a few years ago, and I think at the moment, it's quiescent – but if the trade balances continue to be negative or to deteriorate, I think there are going to be very serious questions raised once again about whether we can afford to have an open research process to which everybody can get access. And that's something that should be of great concern, I think, to all of us.

Finally, it doesn't seem to me that the basis of our support for basic research has been a fiction. I think that sometimes we've overblown the hype about the role of support of basic research. I don't think it's fair to characterize it as a fiction. I do in fact believe we are capable of destroying this national resource that we have in the universities and the national laboratories and in basic research.

I'm obviously in favor of extensive discussion, and I think the symposium is presumably one aspect of that. But I would question that such a discussion should start by throwing into doubt the sources of strength that we really do have and that are the envy of the world. I think there are important questions we have to face about the changing political situation, but I think it would be a bad mistake to throw into doubt what we do have and what has been so very productive for us.

CROW: Thank you. Are there any comments or questions?

BECKLER: I'm David Beckler, I'm a consultant to the Carnegie Commission. I'd like to fill in a few points of the early history. Gene Skolnikoff mentioned the social scientists on the President's Science Advisory Committee, on which I served as Executive Officer from 1953 to 1973. The question on social scientists really was considered in the context of the kind of people who were on the President's Science Advisory Committee. This was an important legacy from the wartime involvement of top scientists of the country, who, after the war, served on the early science advisory committee.

And if you read Dr. Robbie's contribution to Bill Golden's edited book on science advice to the president, you'll find his expression of, shall we say, opposition to involvement of social scientists on the Science Advisory Committee. I think it was due to the concern that one physicist can be reasonably representative of physics, but it's not clear how many social scientists would be required to be representative of social scientists.

I can take some responsibility for urging early on that Herb Simon be the first member from social sciences on the committee, because he had credentials. He was a member of the National Academy of Sciences, and he had credentials in both the social science community and the natural science community. He was followed by James Coleman from Chicago, and then Pat Moynihan [Sen. Daniel Patrick Moynihan] also served on the President's Science Advisory Committee.

I can mention several outcomes. One was a report on educational innovation that led to the establishment of the National Institute for Education and the K-12 model school system proposal that found its way into New York City and the District of Columbia. The report on early childhood education – the first three years of life and the importance of giving attention to that in the context of a more than custodial child care system – that was part of the Johnson initiative.

There was the response to President Nixon's request for an assessment of Alvin Toffler's *Future Shock*— he wanted to know whether this was real and what he should do about it, if anything. There was an interesting report that we did for the Secret Service after the Kennedy assassination, which was to see whether there was a way in which we could advise the Secret Service on identifying potential presidential assassins. There are just some of the possibilities.

And finally, I would just mention that there was this legacy from the Bush era to the next generation of science policy because of the people who were leaders in that era, transferring through the Science Advisory Committee to the Office of Defense Mobilization, and then through Sputnik to the Presidential Science Advisory Committee, which has led to the present committee. So we in effect have our roots in the Bush era transformed into subsequent administrations. Thank you.

WYLEY: I'm Bill Wyley from Battelle. And I am frightened by the fact that we are talking about doing science for the need of the country without involving the social scientists. I know we live by the dictum that nature was created along the lines of the disciplinary departments within the university. But if we only look at the failure of nuclear energy, the failure to get those kinds of technologies into place, we can see very clearly that it is going to take more of an interdisciplinary effort rather than a mono-disciplinary effort that we have exposed in the past.

My question to the entire panel is: What are we going to do in the university to prepare for serving the nation's needs, along the lines that I've just discussed?

SKOLNIKOFF: We're speechless.

COZZENS: Do you want us to answer or do you want to take more questions?

CROW: We'll take one more question and then people can respond.

ZACHARY: Greg Zachary. Got a specific question for Susan. One of the things that Bush feared and predicted – his animus towards the social sciences was transparent – he and his camp said that the private foundations would end up bailing out the social sciences. And as the years went by and more and more of the foundation money did go towards them, he was somewhat disturbed by that.

As a board member of the Carnegie, he had particularly vociferous debates with John Gardner over this. It was his perception that private foundations had bailed out the social sciences. Did they really or were they not compensated for his animus and really kind of stunted?

COZZENS: Shall I go with that one, since that's fairly specific? Well, actually Alpert also had to make a very careful argument when he started social science programs at NSF, that NSF would have a different role than the foundations. And part of his argument was that the foundations were supporting more action-oriented research, and that was one of the reasons to do the hard science core at NSF.

Of course, history, politics, and dollars have overtaken both Alpert and Bush in this regard, and the federal effort is now so much larger than anything that foundations do, that it's now a joke to talk about the foundations bailing out any area of science, even a number as modestly supported as the social sciences.

ETZKOWITZ: My point before was that we no longer need fictions in order to justify basic research. Surely Ed Mansfield's research has shown that there is direct benefit to industry and productivity from basic research.

The Association of University Technology Managers report shows \$7 billion worth of products coming out of basic research, and this is without a consideration of movement to a more strategic form of research. And Vannevar Bush's own career exemplifies how working on industrial problems could lead to basic research, which is what he did with his own students in the university.

My specific question, though, is to Susan. You commented upon how social science followed a disciplinary model in NSF, really following the model of the physical sciences, and everything's always been justified on contribution to theory and method without making a focus on substance.

Now, as you know from the recent European Science Policy Meeting, the European community, the European union, will have a different focus in their social science programs, with specific direct attention to major problems, including 50 million ECUs to science and technology policy.

Do you feel that NSF in our country should take a similar stance and move more toward interdisciplinary programs, even as modest moves in this direction are made in science and engineering?

COZZENS: I can't speak against the proposal that you're making, but I was actually arguing that we need a great deal of fundamental social science work to understand the dynamics of the systems that science and technology interact with, and many other factors interact in.

So while I would certainly like the interdisciplinary work of the sort you're talking about, there's also a lot a room for it to support a much broader range of fundamental work in social sciences.

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CROW: Thank you.