Global Science: The Universe and Batavia

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The Industrial Affiliates program started about 15 years ago when, early in my tenure as director, I thought that people from industry ought to hear about all the clever things we had done at Fermilab. They would then rush back to their companies and manufacture these things and praise Fermilab. The Department of Energy would be so impressed that they would give us a one percent increase in the next year's budget. That was my idea. At the first meeting of the Industrial Affiliates, we had a good showing—about 20 companies. The people who came were the research directors of large companies like Westinghouse, and little teeny companies, like Podunk Systems, Inc., and so on. To begin the proceedings, I talked a little bit about what Fermilab was doing with its accelerators, and the quarks and the leptons. Then I turned it over to the laboratory, and engineer after engineer and physicist after physicist got up and talked about the incredibly clever technical things they were doing at Fermilab. We had a day and a half of this, with tours and exhibits, and so on. At the wrap-up session, we said, with great smiles, "What do you think, fellows?" They sort of shuffled and looked a little embarrassed, until finally one of them got up and said, "Gee, that stuff you told us about quarks, that was great. The rest of it, well, we didn't understand any of it."

The End of Innocence

That was a profound lesson, so the next year we concentrated on the techniques for technology transfer. I remember that meeting, because Bob Frosch, who was vice-president for research at General Motors, told us how hard it is even to transfer technology from one department of General Motors to another. After that, we got into changing the whole philosophy of Industrial Affiliates from one in which we would show you how clever we are and you guys would turn it into commercial value, to a much more modest goal of communication: Can Fermilab, an off-campus facility for 80 or so universities, engage in a conversation with industrial scientists and managers? We'll tell you our problems, you tell us your problems, and maybe out of this conversation something good will come.

Each year for eight or nine years we struggled for an interesting new theme that would set the mood. I remember one I'm very proud of, called "Out on the Limb of Speculation." We said, "Don't take notes. Turn off the tape recorders. Let's speculate on the most bizarre things that could possibly come out of far, far-out research, and how they might come about." Maybe some of you remember that theme, and the "Proceedings" that came out of it. Some of the things that were way out on the limb of speculation turned out not to be so far out after all. Therefore, I'm pleased to come back to this 13th Annual Industrial Affiliates meeting, with the theme "Beyond the Cold War: The Changing Arena of Science."

I notice I have chosen an absolutely absurd title, "Global Science: The Universe and Batavia." Where is the universe? Where is Batavia? Batavia is not the end of the world, but everyone says you can see it from here. I don't know what I meant when I invented that title—I always do that. But I do want to talk about the changing status of science. I think a large part of the issue has to do with the end of the Cold War, much to the surprise of many of us who are, or were once, enormously innocent. I never dreamed that the work we do at Fermilab had anything to do with the Cold War. In fact, one of the most famous interviews in this business was given by my predecessor, Bob Wilson, who, in testifying before Congress, was asked about the machine he wanted to build at Fermilab: "Dr. Wilson, what is this machine going to do for national defense?" He tried to wiggle out of the question, but the questioner bored in on him, and finally, in desperation, Wilson said, "It's not going to do anything for the defense of the country, but it will make the country more worth defending." That was a cogent, brilliant answer, and it was what all of us thought who work at Fermilab and labs like Fermilab—not quite as good, of course, but like Fermilab. All of us in this business of basic research and particle physics and how the world works have thought we were contributing our bit to society by bringing illumination to the world. We have believed that this is a good thing to do, and that it will enrich our culture. Our drive was largely a cultural drive. Now, much to our surprise, the Cold War is over and people are saying to us, "Well, the war is over. Maybe we're not so interested in science anymore."

Good Questions Then—and Now

Let me bring the current situation into historical context by reviewing the situation over the last 50 years. It was November 1944, even before World War II was over, when Franklin Roosevelt wrote a letter to Vannevar Bush, then chairman of the Office of Science Research and Development. That office supervised all that engineering did to win the war. Roosevelt's letter asked four fundamental questions. First, How can we publicize to the world the vast contributions to science and technology made during the war? Roosevelt pointed out that the fusion of such knowledge should stimulate new enterprises, provide jobs for returning servicemen and defense workers, and contribute to national well-being. (It's amazing how some of these phrases come up over and over again!) The second of his four questions was, What can be done to organize a program for continuing the research spurred by the war? Third, What role should the government play in aiding science research by public and private institutions? Fourth, Can we find an effective program for discovering and developing the scientific talent of American youth to ensure the future of research in this country?

These were four good questions in 1944, and they are four good questions in 1994. Bush's response was the famous document, "Science: The Endless Frontier." It wasn't the last report—there were follow-up reports such as the Steelman Report in 1946—but essentially Bush's response to Roosevelt's questions was a blueprint for America's fantastic post-war investment in research and education. It led to the creation of the National Science Foundation and to a civilian-dominated Atomic Energy Commission, which later became known by the curious initials "DOE." If you look at the Vannevar Bush report, you can find weaknesses in the proposals and even failures in the applications of some of Bush's ideas. Nevertheless, the program catapulted the U.S. into the world leadership of science and technology that we enjoyed in the 1950s and 1960s and 1970s, and in fact still enjoy. We still have a robust scientific environment, although there are danger signs that we need to discuss when we talk about the changing arena of science.

America dominated almost all fields of research. In the beginning, it was easy, because our competitors were countries recovering from the damage of the war. But even after Europe and Asia recovered from the effects of the war, American science and technology stayed ahead. It takes a long time to rebuild an infrastructure, and American industry profited from the opportunity to be economically competitive—more than competitive. What's amazing is that before the war, in the 1930s, the U.S. was a kind of scientific backwater. Americans who wanted to get a good Ph.D. went to Europe. Today they go to Harvard or University of Chicago. Some of the great leaders of American science got their Ph.D.s in Europe. To go in 25 short years from the status of backwater to world leadership was a tremendous feat. Of course there was a lot of luck involved, including the tremendous emigration of Europeans before, during, and after World War II. These emigrant scientists found receptive hospitality in universities and in industry and could promptly take up their work. There was also the respect that science and technology earned from the successes of World War II. Our country had a stable, vigorous economy and—perhaps the key—a conviction, a national conviction, that trust in the future would pay off. Investment in new ideas and new products and new explorations would yield great returns. These days, I think we sorely need such a belief in the future.

The returns were enormous. American science and technology made tremendous accomplishments. Feared diseases have been eradicated. We have reached the moon and launched the computer age and spread the green revolution and given birth to biotechnology. Our market shares were in the 90-percent range for all kinds of products: machine tools, high-quality metals, consumer electronics, airplanes, computers and so on. All kinds of new science and technology emerged from the war and after the war: semiconductors, integrated circuits, microwave telecommunications, high-strength alloys, antibiotics, medical diagnostic tools such as CATS and PETS and MRI, x-ray lithography, superconductivity. A long list of technologies that came out of the great post-war period are still supporting our economy today.

It Takes More than Science

Where did all of this research take place? There are three places where science research is done: by the government, largely in national laboratories; by industry, in industrial laboratories; and in universities. I think the success of the years from 1950 to 1980 came from a healthy balance of competition and collaboration among all these elements, sort of managed—or maybe not managed—by the U.S. government. That is how it worked. Nothing has happened to cast doubt on the potential for vast advances in our understanding of nature in many fields today. I have to admit that physics, which had an explosive growth from 1900 to roughly 1950, has probably been edged out by biology, which had its growth spurt in the second half of the century. It is not that physical science has decayed but that biology has exploded, with the incorporation of molecular structure, the discovery of DNA, gene splicing, genetic engineering and all of those things. We have also seen the information explosion, the blinding speed of communications, the continuing logarithmic growth in the speed of computers. In one of our Industrial Affiliates annual meetings, we concentrated on computers. We took a poll among the computer experts: By the year 2000, we asked, by what factor will computers be faster than they are in 1983? The pessimist said

10^4, and the optimist, (it was Ken Wilson, Nobel Laureate in Physics who has been devoting a lot of time to computers) said 10^{14}. I don't know how it will turn out. We have eight years to see what happens, but I think we've already reached about 10^8.

The potential for economic health, for enhancing human capabilities, for solving societal problems is mind-numbing today, if we survey our science. But as we know, or as we should know, it takes more than science and clever engineering to address the problems of society. What does it take? It takes determination, it takes vision, it takes will, it takes courage, it takes leadership—all are commodities that tend not to exist in surplus quantities. Let me look at the dark side of what's going on in the three organizations I mentioned above, where we still do all of our research. First, let's look at industrial research. I remember being fascinated by the willingness of companies like RCA and Westinghouse and Bell Labs and IBM and General Motors, General Electric, Xerox and so on to help us scientists when we needed some complicated gadget. The industrial laboratories provided the wherewithal for progress in American industry. Today, if you look for these labs, you will find that either they are not there anymore, they have been sold to the Japanese, or they have downsized to the level where they spend all their time trying to please some short-term CEO in a hurry, who does not have much patience with research, which, after all, sometimes takes months and months to get a result. More often, of course, it takes five or ten years. I think there is general agreement that something has happened to industry's interest in the investment in research. At a recent AAAS symposium, everyone agreed that, as a generalization, industry is no longer interested in long-term investment. On the other hand, the companies that are still interested in research do very well. It pays! I remember a vice-president of research telling me, "I proved to the company that I pay back 20 percent a year on every dollar invested in my laboratory. They said, 'That's very interesting. We're cutting you 20 percent next year, because we want to declare larger dividends, and the CEO needs a raise.'" I don't know all the reasons, but clearly loss of interest in the long-term future is widespread.

Research universities, where 70 percent of basic research is done, are in many ways in equally bad trouble. The universities' financial state is precarious. Even Harvard is having financial problems. The universities can't raise tuition anymore. I don't care how rich you are, if you are paying for three kids in college, you are a pauper. The state universities have been asked to contribute to meet the state budget deficits. Whether they've been asked or not, that's what they're doing. Overhead charges are moving in the wrong direction. Increased costs of meeting regulatory requirements, security, and harassment by congressional watchdogs add to their problems. As if that weren't enough, now those poor universities have to keep professors on after the age of God knows what, because there's no retirement anymore! What you have is hiring freezes, phasing out of departments, and all kinds of austerity measures, which does not cheer up the scientists in the universities who have to do research. They are depressed, and when the professors are depressed, their graduate students are depressed, the assistant professor is depressed, the undergraduates are depressed, the custodians are depressed. Everybody's depressed. Except the Dean.

National labs, the third place—look at Fermilab. I've heard that things around here aren't all that good. They have fantastic facilities, but the operating budgets for using these facilities are tragically limited; and the problem of keeping up to date and using the kinds of facilities that Fermilab and Argonne and other laboratories make available is growing harder. Some laboratories, such as Los Alamos and the other weapons laboratories, have to redefine themselves or they might find that they're not there anymore. Thus, the three places where we do research are all under enormous stress that comes, either directly or indirectly, from the changing arena of science.

Dark clouds hang over the failed pre-college educational system. We are still a nation at risk, ten years after the appearance of the report by that name. It convinced any reasonable person who read it that our educational structure has collapsed and that we're not educating children to join the science and technology workforce or even to be citizens with opinions on the life-and-death issues in which science and technology play a role. In our graduate schools over 50 percent of the graduate student body are foreigners. If not for foreigners, we would be in terrible trouble. As a nation—although it's wonderful to have foreigners come here and go to our graduate schools, and some of them will stay—as a nation, our long-term survival as a leader in science and technology has to depend on Americans going into science.

Uneasy Alliance

While I'm cheering you up with these dark clouds, let me tell you about another cloud, an important one, although perhaps a subtle one. I refer to a general disquiet among policy makers in this country over the stewardship of science. Part of this simply comes from our enemies, the cynical, dangerous, self-aggrandizement of bullying politicians. We could condemn them and worry about them, but we've seen them come and go before. I want to set these guys aside and worry instead about the more serious challenges that are increasingly heard in Washington, in the media, and even in polite intellectual circles in the universities. I recently learned that this cloud has a name: "post-modernism." Politicians, perhaps frustrated by the continuing social malaise of decaying inner cities, environmental problems, high crime, persistent poverty, have turned to science and said, "What have you guys done about all this lately?" I can only quote the words of perhaps the best friend science has in Congress, Congressman George Brown, who has been a proponent of science and is himself a trained scientist: "The uneasy alliance between scientists and politicians is beginning to come unglued. Budgetary stress, economic stagnation are forcing political tradeoffs and sacrifices that affect a broad range of federal programs." Congressman Brown goes on to argue that we must test the hypotheses—this blows my mind—that link economic and social benefits to research. In an expression of what I believe is frustration, he has noted that in spite of 50 years of strong federal support of science, we still have poverty, we still have pollution, we still have crime: and he figuratively asks, "Why haven't you guys fixed these things?" He expects that as the knowledge of the world increases, the problems of society should go down, and that's not happening.

If you think Congressman Brown is bad, you should see his staff. He impaneled something called a Task Force on the Health of Science that carried some of these complaints to an extreme. Some of the suggestions of the task force, which appeared in a government booklet, said, "Congress should exert greater control over the choice of research to be funded. Research should be addressed more immediately to current political, economic and societal pressures. Research programs should be linked explicitly to goals." Implicit in this suggestion is the assumption that it is in fact possible to determine in advance which research is most necessary for a given national goal. How many scientists would concede that assumption? The Task Force goes on: "Legislative mandates should be used to determine how research is evaluated." Inherent excellence of research is not considered a sufficient criterion for judgment because, I guess, excellence doesn't count. It doesn't guarantee policy relevance or potential applications. "Programs that are failing to meet stated goals should be terminated." Stop cancer research, right? Because it hasn't cured cancer—that's the clear implication. The members of the Task Force display an utter ignorance of the history and practice of science. They betray expectations that science can't meet, a misapprehension of its capabilities. They fail to recognize that the motors of policy can't mandate success in science.

Directions in science are ultimately dictated by feasibility. Science is the art of the soluble, of the possible. Our legislators ignore the substantial strategic planning that has guided both fundamental and applied research in the United States over the past 50 years, and the plentiful results that have redounded to the benefit of society. I think we've done very well. Saying that science should be more aimed at national goals is ridiculous. We're already aimed at national goals. Above all, Congressman Brown's task force misplaces the blame.

Science has, for example, long since produced vaccines required to control most childhood infections in the United States, but our society has not found the political will to deploy them. Science has long since sounded the alarm about acid rain and its principal origins in automobile emissions, but society has not found the political will to control the internal combustion engine.

Brown argues that we must test the hypotheses that link economic and social benefits directly to research, hypotheses that I thought were well proven. I think science has become a victim of its own success. We have accomplished so much that far more is expected than we can hope to deliver. Why haven't we cured cancer? Why haven't we cured AIDS? When will there be a remedy for the common cold? When will we be able to produce energy without waste or pollution? When can we convert sand to gold? This is a real problem, and it is having an effect. Complaints come from Congress. There's been an assault on the National Science Foundation, insisting that it focus on perceived national goals. We see increased manipulation of research funding by legislation. An erosion of the infrastructure for fundamental research in the United States is going on, and if we cannot reverse it, the pace of discovery will slow, with widespread consequences for industry, health care, and education.

The supercollider is in trouble. Its troubles are symbolic of the problems of science in general, but the supercollider is a big, big symbol and an expensive one. It raises the question of how much science is needed. When I worked with AAAS, we had to address that question, because people told us, "We have too many scientists—we're doing too much science, we don't need all that science. What good does it do?"

Science Pays Its Way

We know why we want the supercollider. It fulfills a deep cultural need, but the cultural argument is difficult to make. However, I believe we can make a different argument, that science yields paybacks to society in economic terms. That is a good way to talk about it, because science has a tremendous track record. I have a very simplistic notion to present. We have a GNP in this country of $6 trillion and a federal budget of $1.5 trillion. If we neglect the deficit, the ratio is about four to one: that is, one quarter of the value of any commercial activity is paid in taxes, as revenue to the government. So if Fermilab does something that generates an increase in the GNP, then Fermilab can be credited with one quarter of the increase, as revenue to the Treasury from taxes that wouldn't be collected without that particular commercial activity. So if I can find a billion dollars in annual U.S. commercial activity generated as a result of Fermilab science or technology, it means that Fermilab's $200 million annual budget is free to the American taxpayer; it has paid for itself. All I have to do is find a billion dollars in the GNP in order to pay back the Treasury, and more. We have to do this carefully. If I take simply accelerators alone, accelerators generate an annual commercial activity of about $5 billion a year. There are thousands of accelerators in place, curing cancer, treating plastics, doing diagnostic radiography, performing ion implantation. These accelerators were invented by scientists in their desire to learn about atoms and molecules and quarks and so on. If we add to the total the products of these accelerators—people who are living instead of dead, plastics that are irradiated, metals altered by ion implantation—the sum of commercial activity has been estimated to be an industry of about $25 billion a year. Fermilab can't take credit for the whole $25 billion; but, with the other labs, we do have some share of that.

To get an idea of Fermilab's share, I like the magnetic resonance imaging example, although it has sometimes been distorted by unreasonable claims. The MRI industry now generates about $4.5 billion a year. There are 3,000 MRI units in the United States, at a cost of about $2 million each. These MRI units each do 2500 scans per year at $600 dollars a scan, for a total of $4.5 billion. Add to that about a billion dollars extra from the sale of new units, although new sales are beginning to level off. We get a total of four or five billion dollars a year.

Although Fermilab didn't invent MRI, the Laboratory did have a big role in developing the materials that MRI requires for its superconducting magnets. We will give Fermilab 20 percent of the credit for the development of MRI. Twenty percent of five billion is one billion. We have found the billion dollars we need to pay Fermilab's budget. Fermilab is free.

I think similar calculations will prove true of any large scientific activity. I don't mean to say we're going to build the supercollider to pay the Treasury for our operating costs. That doesn't make any sense at all. But it is a different argument from the one NASA uses, the "look-we-invented-non-stick-frying-pans" argument. The difference is that we are doing research to understand how the world works, and the cost of this research is essentially free. Maybe the Treasury even makes a profit on it. That's the point, and I think it will be true of the supercollider. Whatever its operating costs, and its capital investment costs, they will be paid back. But that's not why we do it. We don't do it for the frying pans. We do it because we want to understand how the world works.

The Sense of the Universe

The real payback to society from a project like the supercollider comes from the way science influences human activity. We remind ourselves of the place of the supercollider and its cousins in the history of science. Newton revealed an orderly universe, and was able to understand the motion of planets. Today, NASA programs all of its satellites and its activities with Newton's equations, but the important aspect of Newton's contribution was his demonstration that the universe is an orderly universe, subject to laws that we can write down. Faraday discovered electricity, a notion that in its early days was probably as exotic as quarks, but is now as firmly incorporated into the human environment, human thinking, and human behavior as fire or the invention of tools during a much earlier epoch. Einstein showed us where we are located in the universe, and unified space and time. His special theory gave us the basis for the construction of accelerators and many other things in modern technology. The atomic explorations of Bohr and Schrodinger and Heisenberg gave us quantum theory, one of the revolutions of the 20th century. It had an enormous impact; one estimate says that quantum mechanics accounts for about 30 percent of the GNP. It's a good number—nobody can disprove it. After that came Enrico Fermi and Ernest Lawrence and the conquest of the nuclear domain. You can count something like six major revolutions that began as abstract studies whose implications for society were concealed in the very distant future. In each phase, some new piece of reality was revealed. Gravitation, electromagnetism, atoms, cosmic history, all gave us glimpses of a universe of incredible beauty and coherence, for the enchantment and the comfort of mankind.

The enterprise of science embodies a great adventure, a quest for understanding of the universe, which may or may not be infinite in all directions, not only above us in the large but also below us in the small. We began this quest very well, by building a science of increasing power that illuminates the living and the inanimate, all that surrounds us in time and space. For this, scientists are admired. (They are still admired, surveys tell us.) But they are also feared, mistrusted, even despised. We scientists may offer hope for the future, but we also bring moral conflict and ambiguous choice. The price seems large, but it pales in comparison to what it would cost to deny the future.

The American essayist Annie Dillard wrote, "Who can read what the wind-blown sand writes on the desert rock? I read there that all things live by a generous power and dance to a mighty tune. Or I read that all things are scattered and hurled." Will we live by a generous power and dance to a mighty tune or will we be scattered and hurled? ❖