Panel Discussion

George E. Pake: In trying to make some kind of progress with respect to economic return on investment in research, I wonder just what kind of economics should be applied to this kind of investment. Steinmueller has taken some very important forward steps, but there are many times we make investments where we don't really concern ourselves too much with the economic output or the return on investment. For example, what is the meaning of return on investment if you wanted to maximize the return on investment in the premium you pay on your life insurance policy? In a certain sense national security has that characteristic. We make this enormous investment in a gargantuan military establishment to buy some kind of protection or security. I really don't know how to evaluate that, but I don't evaluate it in terms of economic return.

Participants in the Roundtable on Science, Economics, and Public Policy are, left to right, Dick Carrigan (Head, Fermilab Office of Research and Technology Applications), David Morrison (Moderator), Norman Metzger, George E. Pake, Alan Schriesheim, and W. Edward Steinmueller.

It seems to me that there are many motivations for making investments. One that impresses me a great deal is the construction and maintenance of a huge scientific infrastructure. Consider the strength the U.S. showed in World War II. In the case of World War II, we had these people with all kinds of talent from many esoteric fields of scientific endeavor who suddenly were mobilized into critical tasks for the military, for the whole war-time effort. There is an enormous value to be placed on having such an infrastructure. Again, I wouldn't know how to determine that value in any quantitative fashion.

Alan Schriesheim: In recent years there has been a rise in an evaluation of research, certainly industrial research, on the DCF basis, that is the discounted cash-flow basis. Of course, when one evaluates research on a discounted cash-flow basis, which places so much value on discounts for dollars that one receives in future years, there is a peculiar burden that is placed on innovative long-term research which pays off in the future. Most industries that I know of have gone to that discounted cash-flow method. I believe that concept has crept into the federal sector as well. It results in an evaluation of research based on a short-term pay-off. The sector that I am most familiar with is the energy sector. In that sector, this concept is doubled in spades. For national defense or national security, the expenditure of research dollars in the whole multi-faceted energy area would seem to be a reasonable place for the federal government to place its money. Of course, at the moment we are faced with a short-term energy glut. Those people who run the kind of slide rules that are based on the return on investment view, see research in the energy arena as not a profitable return on investment.

Pake: At Xerox we have not often used the discounted cash-flow approach. Of course, there are always a few people in the company who like to calculate that sort of thing and who give you a hard time if you are a research manager. An example I can give from Xerox is some applied research we did on laser scanning applied to what we now call electronic printing: laser scanning of the photo receptor. If we hadn't done that research there is one whole segment of our business we wouldn't even have. That is now our fastest growing segment of business. Sure, we can

proceed to calculate, and will continue to calculate probably throughout the future, the pay-off to the company for being in that business. But we really are in that business for many reasons. We also did much early research on the photo receptors which are the imaging engine inside the copier, and also in this case the electronic printer. That research, done over a long period of time, pays off for us every time we take revenue on a copier or a printer. It would be incorrect to attribute all the profits the corporation makes out of electronic printers to the research we did on laser printing since some of it also depends on the research we did earlier on photo receptors for copiers. It is an enormously challenging task of accounting to see how these things pay off, even though we can certainly demonstrate that they pay off in these examples I've given you.

David Morrison: I wonder if we aren't addressing a broad issue here. In the final analysis aren't we really talking about what per cent of the federal budget should go into basic research, and would that percentage be more easily defined and readily justified on the basis of economics?

W. Edward Steinmueller: My view on that is that economics provides some handle or method for quantitatively examining past experience. We have just been focusing prospective choices, such as allocation of budgets within a company between investment on current plant versus research and development. It is my opinion that as a mechanism, discounted cash-flow is an inappropriate method for prospective investment. A way to gain insight into whether your particular research effort is performing up to its capabilities as measured by your peers is to examine one's retrospective investments based on the internal peer review of the company and to compare it to how other companies perform. In a similar fashion, examining retrospectively the performance of a particular area of science with regard to its short-term spin-offs is an opportunity to gain some insight into what to expect in the future from that particular area of investigation.

Carl Rosner (Intermagnetics General Corporation): I would like to address what I perceive to be an omission in this discussion so far. Some very incisive and intelligent remarks have been made here. However, I am struck by the bleak

introduction that Dr. Morrison provided concerning the state of the competitiveness of U.S. industry. There is a dichotomy about where money is spent for applied superconductivity research between the U.S. and the rest of the world. Here it is primarily spent at an academic institution that prides itself, and rightly so, on tremendous accomplishments in the applied research arena. However, I think that the benefits of this research are probably very difficult to find in this country. Elsewhere in the world, at places like KEK in Japan or the HERA project in Germany, they can be found. In those places the industries have been busy. But U.S. companies can't participate in these countries, almost by edict.

The whole arena of internationalization for both research and industry is really the crux of the matter, and we have not yet come to grips with it. It seems to me that the possibility of using the resources of the U.S.-based national laboratories to improve the competitiveness of the nation has really not yet been examined in a practical sense because of the impossibility of trying to do that in an international academic environment. That is what we really have to examine and come to grips with if we are going to improve the competitiveness of industry in this country. How does one, on the one hand, maintain academic freedom, openness of what you are doing, and on the other hand improve the competitiveness of U.S. industry. That is where we need some statesmanlike and visionary approaches and decisions that are rooted in factual experience. In spite of the important developments in all the laboratories across the U.S. over the last 15 years, we are still struggling. We are being frustrated by the success of the foreign enterprises that literally have totally free access to the results of the Fermilabs and the Argonnes and the Brookhavens; as a result we in the U.S. can't compete anymore internationally.

Norman Metzger: While I can't answer the question directly, I can give some examples of where they are trying to deal with this issue. An example is the Microelectronic Center in North Carolina. It is a joint initiative of industry, state, and universities in that area. They have no Japanese companies as members, even though there are Japanese companies in that area. While Japanese companies have applied, the U.S. companies have made it clear that if the Japanese joined they

would leave. On quite another plane, we have had the creation, within the last three or four years, of four centers in the U.S. for doing magnetic recording research. This is another industry which we are now almost totally out of, except perhaps at the high end. Those centers have been funded almost exclusively by industry. Whether that is a healthy response or whether that is the response we are looking for is another issue. That is one way that problem is being dealt with.

Morrison: Alan, you must face this question on a day-to-day basis: the issue between academic freedom and access to results vs. the need to try to respond to the broad federal mission of economic competitiveness.

Schriesheim: From an industrial standpoint, information that is available to everyone is available to no one. For this reason it is important to follow the passage of various technology transfer bills, such as the Bayh-Dole bill and others, through Congress. These bills are really aimed at providing the national laboratories and other federal laboratories with the ability to grant exclusive licenses, and also to enable a company to embark on a joint venture or a limited partnership with a national laboratory. Assuming that all this legislation becomes part of the federal regulations, and that is certainly the intent of the current administration, it will then be possible for a company, as it is today, to transfer technology into the private sector via these mechanisms. This is true for most of the national laboratories, with some caveats on the defense labs and certain aspects of nuclear power. To a large extent much of this legislation is already in place. Certainly we are taking advantage of it at Argonne.

With this legislation it is possible for a private company to come into a national laboratory and get the lab to give them a patent waiver so they have an exclusive license. Typically, this is granted by the laboratory's contracting organization. In Argonne's case, this is the University of Chicago, while at Fermilab it is the Universities Research Association. Now I believe that this mechanism is the critical linchpin in the utilization of these large national resources from an international competitive standpoint. I don't know what else is required; there is already an entrepreneurial spirit on the part of private industry. I can give chapter and

verse of entreprenuerial venture firms who already have plucked things out of different national labs: Oak Ridge for an alloy development, Los Alamos with a cell sorter. Lots of examples exist.

Leon Lederman (Fermilab): I thought that Rosner's question was really directed more towards basic research. His suggestion seems to be that we try to restrict the flow of basic research information. However, for basic research in this country the whole idea is to publish as fast as we can.

Steinmueller: I think this is an important international policy issue. If we compare ourselves to our allies, we find that Japan, for example, spends a much lower proportion of its GNP on basic research than we do. In trade negotiations we should be concerned with the contributions of our trading partners to the stock of scientific knowledge. In addition, we should examine the commercial appropriability of various technologies. In basic research it is clear that active efforts by many countries contribute to the stock of scientific knowledge. For example, if there were no cooperation in the dissemination of basic physics research results between CERN and the U.S., the rate of progress in HEP would be reduced.

Morrison: Steinmueller has raised an interesting point. In talking about the Japanese under-investing in basic research, it is my understanding that that posture is changing and the Japanese have established quite a few basic research institutes over the last several years. The shoe may be on the other foot now, so that we may be seeking the Japanese technology through the open literature rather than what they were doing a decade or two ago.

Metzger: Hasn't biotechnology basic research already become proprietary to some extent in some universities?

Pake: I think that there are campuses on which some faculty members allege that it is happening or worry about it, but I don't know of a case that has been documented. The universities I know of that are involved in some of these joint research ventures, with major subventions from major corporations, try very hard to keep the same freedoms they have always had. There can be issues. For example

my corporation is one of the 20 sponsors of the Stanford University Center for Integrated Systems. At the very beginning of setting up that center, there was a lot of discussion about patent rights for things that might be invented inside the joint activity. That was a fairly sticky wicket but we got through it. I would point out parenthetically that the university introduced as many complications into that as anybody.

Tom Kirk (Fermilab): Earlier, an interesting distinction was made between the *process* of basic research and how that impacts the society, versus the *content* of the basic research which may have an impact quite a bit further down the road. One aspect of the process that may be under-appreciated is the flow of trained people from the basic research environment into other applied or industrial environments. In my own experience this may be one of the critical areas in which basic research impacts our society. People coming from basic research bring attitudes, methodologies, and information to new applications where a tremendous impact can be made, an impact that may never be recognized as an accomplishment of basic research in bettering our society.

Pake: I strongly support that view. It is analogous to the point I was making about the value the nation reaped in World War II when people from far flung and esoteric parts of science were mobilized. These people made great contributions through attitudes, through knowledge, through techniques. This happens all the time. In our own industrial organization we have quite a number of people who have left other fields of physics, including even high-energy physics, to come into industrial laboratories. They have done first-rate work. Without wanting to puff up anybody around here, I would say these high-energy physicists are pretty smart.

Tom Jacobius (IITRI): Another related issue might be, instead of looking at the downstream industries (in basic research) and speculating about possible benefits and spin-offs, one should consider the value of analyzing what happens if a basic research effort is *not* funded, i.e., the downstream opportunity cost. For example, many things may then be predicted to *not* happen which are perceived as desirable (and in the national interest), such as students sustaining an interest in

entering a particular field, or specialists in the field transferring to other fields to apply their knowledge. A strong and clear message to decision makers that, if funding is *not* sustained, a field of basic research may dry up (along with U.S. competitive positions of companies which benefit from the resulting knowledge or expertise), may carry more impact than trying to speculate about as-yet-undetermined spin-offs which could, some day, materialize.

Metzger: An example of this is something I mentioned earlier, which is magnetic recording. This basically is a field in which almost no academic research has been done in the U.S. Part of this is because the density of recording has gone up nicely over the years mainly due to precision engineering. We find we are reaching fundamental limits and we have had to start a research program. But, in the meantime, the Japanese have once again taken a substantial part of that industry away from us. One could argue that there have been substantial opportunity costs because there wasn't an active U.S. research program.

Pake: In my introductory remarks, I commented about the fact that we shall certainly be involved in global economic competition. I don't want that to be misinterpreted. At Xerox, we have a Japanese affiliate with whom we work very closely. Their people come through our laboratories frequently and freely, and we go through theirs on the same basis. We own 50% of that company, with Fuji Photo Film holding the other 50%. The point I would make is, we share our basic information with them and even fairly broadly useful applied information. However, in my view, we as a nation have to, and I believe can, compete in the application of that science. In our company, we simply will not accept the notion that we cannot compete. In fact, we must! We do it every day.

Lederman: Infrastructure is important. If you have the infrastructure, and you are going as fast as you can, a new piece of basic research gives you an advantage. A great example is Russian science. It has great mystery. Genetically the Russians are okay. They can play great chess, yet if you evaluate some of their science, it is pretty awful. Los Alamos illustrates several good examples. The Secretary of Energy was being taken through Los Alamos and it was pointed out that three of

the major projects they are working on were Russian ideas. We were developing them because we can go faster. Several years ago another Xerox person at one of our Roundtables admonished us to be wide open, to be completely free. That lets you go as fast as you can and you can go faster than the other guy. That is the way you win.

Pake: I think this question of opportunities foregone was an extension of something that Steinmueller was talking about earlier when he was showing those curves about the revenue returns earlier or later. Perhaps it's the logical limit. Opportunities missed entirely are something too late even to make the curve.

Steinmueller: Yes, I think so. There is a problem here. The prospects of a particular technology at any particular time are uncertain. You don't know whether you are just about to hit diminishing returns. It may turn out, God forbid, that the high-energy physics program as currently constituted may be reaching some limits with regard to its contributions in superconducting technology. This could happen, for example, because the SSC is being designed with a conservative engineering slant to save money in order to justify it economically before Congress. The contributions of basic research to technology can be cyclical. At the same time, if past experience is any guide, the many technological innovations that are necessary in order to conduct the research on detectors and other research methods involved with the SSC should yield benefits in the future. So, there is a problem with the prospective nature of what you suggest, but I think the "past-as-a-guide" approach to the future is an appropriate way to start looking at some of those foregone costs of not supporting basic research.

Robert Meserve (New England Electric): I would like to get the panel's comments on how they think the funding for the SSC should be approached. Should it be approached on a purely economic level, or should it be approached as the greatest scientific project of all time? Just how do you go about selling this idea to Congress? I have a particular problem: Warren Rudman is my senator.

Morrison: That is an excellent question. The scientists have one point of view, the economists perhaps another, and the policy makers a third. Who is going to make a decision?

Steinmueller: As an economist it seems to me that the physicists have made a case that we cannot advance fundamental physical knowledge without the SSC. Within their peer community, I think every effort should be made to gather the critical information necessary to know whether there is wide-spread dissent from that point of view. After that, it seems to me a natural step that if we want to learn more about the physical universe, a proposed facility like the SSC is the natural way to proceed. This is in contrast to saying it's going to be justified on some cost basis which may not ever be captured or may be captured 150 years in the future.

Schriesheim: I don't know if you are talking about a tactical issue, the tactics of doing that. I quite agree the United States has made an investment in high-energy physics over a period of years. I don't know what that investment is, but some large sum has been invested in the high-energy physics community. The nation has done that as a matter of policy, for whatever reason. Now the leaders of that community have come forth and said that for this community to survive in the future in a healthy fashion, this is what needs to be done. From my own viewpoint, that argument is a lot more saleable than an argument that is based on a projected financial return on results that would come from the SSC. Beyond that, it is a matter of tactics. Lederman is in a much better position than I am to comment on the tactics.

Pake: It seems to me that the United States must opt for world leadership in fundamental science. This is not necessarily a tactical scheme for getting Congressional support. A country of our wealth has a project of the scale of the SSC essentially within our grasp. To deliberately decide to be second or third rather than compete for the lead is just inconceivable to me. Unfortunately, there does seem to be more willingness to conceive of that possibility in the nation these days than I've seen earlier in my lifetime. I don't know whether you can sell

congressmen using that argument, because they have other things that they want to spend money on.

William Dyess (Major Tool and Machine Co.): Don't you first have to sell the SSC to the administration?

Pake: That is quite correct. There are efforts underway to do that. I won't predict their outcome. I don't even know the timescale for some of the key steps.

Metzger: As Dr. Lederman commented in a *Science* article this week, it is often difficult to find enemies. I may be wrong, but I don't think I have heard anyone say that the SSC is not of value and that the goal is not of value. Rather the opposition focuses on various subset questions: Should we internationalize it, what is the effect on small science, are we taking money away from one to give to the other, can we afford it in a time of Gramm-Rudman, etc., etc.? That kind of response shapes tactics. I'm not sure it shapes strategies. I really have heard no one say that the SSC is not valuable, it is not something that we shouldn't do.

Morrison: I think I can summarize these comments by saying the decision should be made on a policy basis whether it is the administration or whether it is Congress. That perhaps gets back to the first question I tried to get the panel to answer: What percentage of the federal budget should go for basic research? Then we can argue where those crumbs fall, once we get it. Isn't that a policy issue? How does basic research compare to something closely related to research like education, or something less closely related like transportation or flight safety or some of these other issues that Congress has to deal with?

Lederman: You certainly don't seriously mean that somebody should consult some enormous computer and come out with a percentage. That is not going to work. The question really is, how much is required incrementally on an infrastructure which exists and which is moderately good. Of course the increments must bear up under close examination. I remember, once upon a time, when any good scientist could get his project funded. It didn't break the country. In fact, I

believe the country is still benefiting from those golden years between Sputnik and Vietnam.

Pake: There was even an earlier era in the nation's history (actually the *only* time in the nation's history) when essentially any young male who wanted a higher education could achieve it. That was the period of the G.I. bill after World War II. It is my contention that the U.S. technological and economic advances in the three decades following World War II were essentially spurred by that massive national investment in higher education. We say we are the land of opportunity, but the fact is that opportunities for higher education are extremely limited in this country. There are today many young people who have high potential for science or technology but have little educational opportunity at any level to develop or demonstrate their talents. This limits U.S. R&D competitiveness in the global economy, both in basic and applied research.

Schriesheim: I really would not like to have the argument be one of "How much basic research?" This could, in effect, force the nation into some kind of a figure, or even a range of figures for research. I would argue that it would be more interesting to discuss the support of the infrastructure. What is the infrastructure for basic research? What kind of infrastructure is needed? Hopefully this would turn out to be the academic infrastructure, the national laboratory infrastructure. In fact, for the job I now have, it's damn difficult to look to anybody who feels they have a responsibility for the institution itself. Getting a discussion going on infrastructure could be useful.

Morrison: I think I wholeheartedly agree with you, Alan. I don't know whether I'd like to come out with a specific number or not, but what I'm trying to avoid is the other end of the spectrum with 555 people sitting in the Congress, each having their own specific project in mind. Whoever lobbies the longest or shouts the loudest is the one who gets funded, whether it's needed or whether it's desirable. That is the chaos that results from the unbounded end of the spectrum.

Pake: I agree that I wouldn't just want to start with some early-on revealed notion as to how much basic research there should be. We face this question in my company as to how much basic research we should do. We do a certain amount deliberately; I've discussed that elsewhere. The point I want to make here is that we first ask ourselves how much R&D should we do, how much should we invest in R&D. That is really a dollar question. It is usually viewed as a fraction of our total revenues. Then of that R&D, how much should be basic research? There are also the university joint research ventures we have to worry about funding. I think you almost have to come at it that way. You have to view the basic research in the perspective of the whole R&D enterprise.

Steinmueller: There is even a more detailed trade-off that occurs when you need to consider what are the immediate things that we can address in basic research now. What are our current capabilities and how might we be able to add to them? There are similar questions that can be asked with regard to applied research and development. This is necessarily a political process, be it in a corporation or be it in the public domain. Out of this comes some notion of whether we are behind or whether we are in a period of surplus. Then on a year-to-year basis or on a decade-to-decade basis, we can come to assessments that correspond to the knowledge that, for example, our federal highway system is decaying or that our school system is decaying. Similarly, we can come to the conclusion that our scientific infrastructure is decaying and needs to be shored up and identify areas where we can begin to get some sort of forward motion.

Dyess: This is an opinion, but I believe that since we are staring at deficits for as far in the future as we can see, we are not going to be terribly successful at selling to the administration, this one or the next one, nor to the Congress, a general concept that a certain percentage of our GNP should be spent on basic research. I believe the only way to get money from the government now is to go to them with a specific proposal and say that this specific proposal is good and needed, and we need this much money. That's the only way you're going to do it.

Larry Spires (Fermilab): Are there ways in which private industry can assist laboratories like Fermilab in funding so as to reap direct benefits and exploit some of the products that are available from Fermilab? The reason for asking this particular question is that you can see a great deal of industrial support for universities. They have set up major laboratories in these educational institutions. Since Fermilab is operated by a university consortium, it seems to reflect the same kind of focus. You are still talking about academia.

Lederman: The mind boggles. I find that we are very fortunate in having so many spokesmen from industry to support us. I think we are doing remarkably well under the circumstances just because the Pakes, the Branscombs, and so many others serve on committees, panels, making good speeches. The more of this, the better. I don't know what is going to happen on the SSC. I happen not to know my '87 budget. Unfortunately, there is a process going on in Congress now which is often called a "Doomsday Machine" in Washington. Nobody ever thought Gramm-Rudman would go off. They lit a long fuse and it's now become very short. That is the scary thing.

I think a lot of the hope of economics now is partly a perception process. One convinces Congress by convincing people, the public. Our problem is one of continuous communication with the public to let them know things are happening that are not helping the situation. The secondary school situation that the *Nation at Risk* papers on education identified was very effective by dramatically stating danger to our educational system. As the Packard-Bromley report indicates, our scientific infrastructure is very fragile at the universities and elsewhere. You can see it very dramatically in the fraction of the GNP spent on research. That number is documented as a function of time. During the golden age of R&D it probably was a factor of two higher. The Japanese effort is at least rising. I don't know where they are now, but it is very close to what we are as a fraction of their GNP because of the rising slope of their R&D investment. It's the increment that is important. I believe that science has to have its increments so that a young person coming into science can see the means of his own accomplishments within a short

fraction of his lifetime. Now he sees a crumbling establishment, no equipment at the universities, no prospects for advancement. He goes elsewhere and the scientific infrastructure crumbles. It has been often noted that it's very difficult to erect, very easy to destroy.

Schriesheim: Let me give sort of a specific point of view from where I sit. The current administration, certainly, listens very carefully to chief executive officers of major companies. They are very powerful. You ask, what can industry do? Not necessarily for Fermilab, but let's say for the health of the scientific enterprise and the nation. There really is no coordinated effort that I am aware of, of major, powerful, industrial leaders. I don't mean vice presidents of research, not that they are not important. I mean the heads of major corporations who depend on technology, whether they know it or not. Sometimes a number of them don't know it, but they can be educated. I'm sure Pake can talk about that. If these people were to make a representation on the state of the infrastructure in terms of education or something else, that representation would be useful. We are, in essence, another community crying in Washington for money. To get a powerful friend in court, you say: Well, who is the most powerful? Major industrial leaders are indeed powerful. Now, you know they're distracted with such matters as trade policy and tax policy, so they need to be convinced that this is important for the long-term health of the nation.

I'm not talking about lobbying, you understand, for national labs per se, but for science. That is, for the infrastructure of science in the country. I don't think it's useful to get a group of CEO's together just in regard to the national labs. In fact, I don't even see how that could be done.

Pake: One way to do this would be to go through an existing organization, such as the Business Roundtable, which is essentially a collection of CEO's. If one or two of those chaps decided that was a priority agenda item, they could get it going.

Morrison: Actually there is a vehicle something like that closer to home in Illinois. Barbara Chasnoff, the Executive Director of The SSC for Illinois, Inc.,

has joined us here today. The organization was formally established in Illinois several months ago. The SSC for Illinois is a new, not-for-profit corporation that has been formed to serve as the vehicle to try to deal with the interface area between what the state and Fermilab can do, and what the private sector can do. All of the private sector and a number of the state and municipal interests are represented in SSC for Illinois. One of the reasons it was established as a non-profit corporation, was to be able to accept donations from anyone. The private sector can easily donate to that organization. SSC for Illinois was formed to be an action organization. One of its initial projects has been trying to get all the states together in a coalition behind the SSC. The intent was to get a contribution from the states to support an intensive lobbying effort with Congress and the administration. That is proceeding rather slowly. But this other route of looking at a broader charter of investigating the infrastructure may be something else for SSC for Illinois to do.

Barbara Chasnoff (SSC for Illinois, Inc.): If anyone is interested in being involved with SSC for Illinois, please contact me. One of our basic charges is to involve industry in the support for the SSC and thereby ultimately help get it sited in Illinois.

Morrison: This has been a very exciting, stimulating discussion of science, economics, and public policy. I'm sure that there are more questions unanswered than there were answered. A subject like this doesn't lend itself to being reduced to two or three very succinct comments. Nevertheless, let me recap some of the high points:

Metzger summarized what the National Academy did on this matter. My impression is that the National Academy workshop felt that one could not provide a defensible economic measure of returns on federal R&D investments.

Steinmueller has proposed a way to at least get a better handle on high-energy physics policy questions; that is a study that hasn't really begun. Very much needs to be done so that we can get a retrospective look at what the accomplishments

have been from an economic point of view within the high-energy physics community.

Schriesheim touched on some very good issues concerning the justification for basic research. He noted that this justification is largely the applied research that grows out of basic research. He also noted that we need to support the overall infrastructure that is engaged in the whole area of science and technology.

That certainly seemed to rise out of the primordial ooze that Pake was talking about, where one has to get one's hands and fingers and arms and whatever else dirty in the pursuit of science and technology. What Pake really left me with is what I believe is the key to the whole problem here: people! We are very much facing a crisis in this country that all of the speakers touched on: the lack of a solid educational process starting with the primary grades and going through our universities. If we look into the early 1990s we will be some 30-40% below the peak of entering college freshmen, or at least people that are available to enter college at that time. That is a rather significant reduction that arises just out of demographics. If you compound that and look at some number between 30-40% of those eligible in terms of age or factors relating to minority aspirations in large city school systems such as Chicago, you find there is a serious problem. You wonder what the future of this scientific enterprise is unless we start now to do something about it and get these people trained. Perhaps it is an infrastructure problem, perhaps education, or perhaps it's a commitment on the nation's part to get after this problem and say, "We still want to be number one in science and let's put our money where our mouths are."