Fermilab Industrial Affiliates Roundtable on Science, Economics and Public Policy

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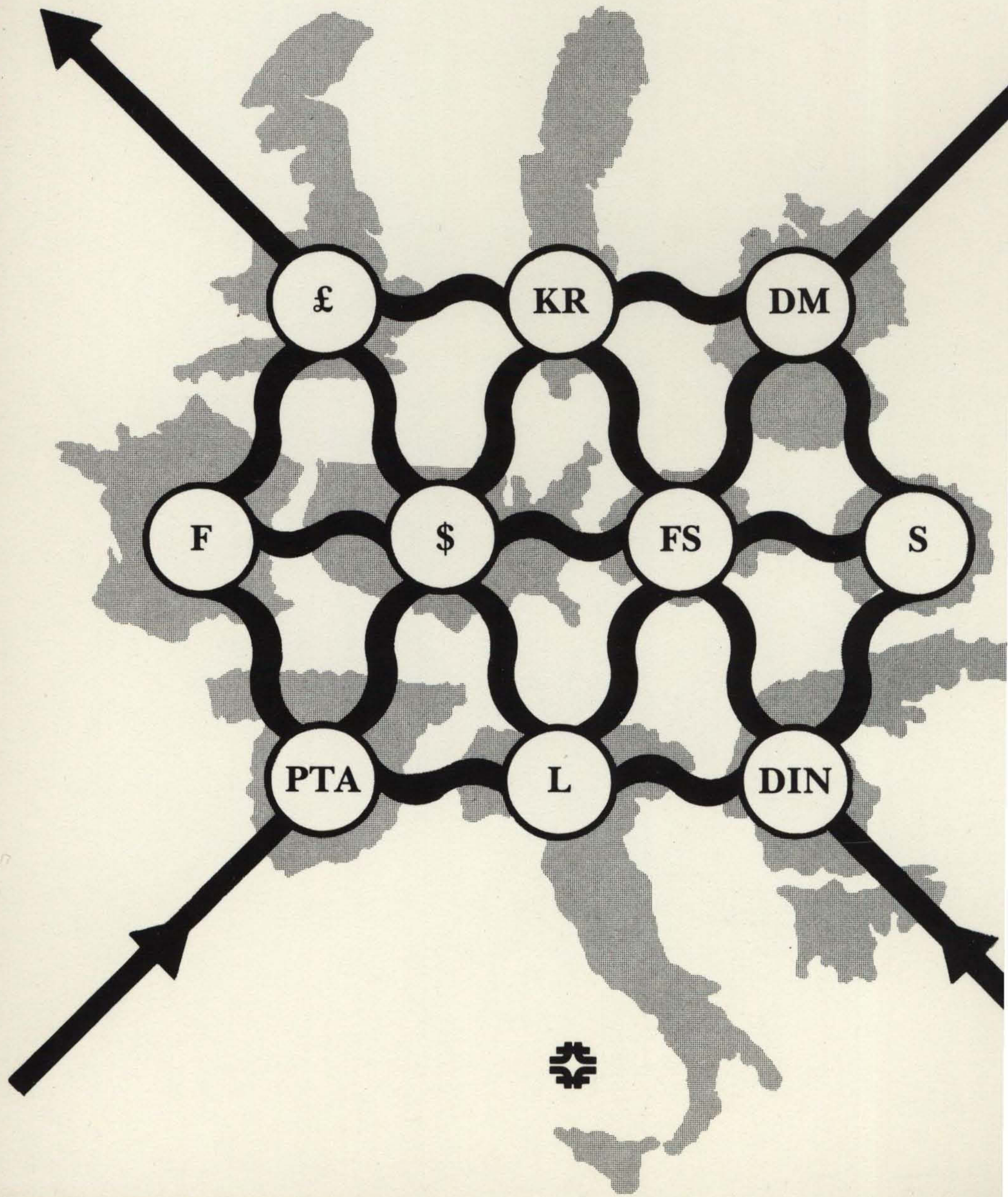

Fermilab Industrial Affiliates
Sixth Annual Meeting, May 29-30, 1986

Roundtable on Science, Economics and Public Policy

Sponsored by Fermilab
and the Fermilab Industrial Affiliates

Fermi National Accelerator Laboratory
Batavia, Illinois

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On the cover:

Quark interaction via gluon exchange based on a metaphor of international currency arbitrage. Quark color is the analogue of the face value of a country's currency; the vector potential is the analogue of the exchange rate between neighboring countries. A gauge transformation corresponds to relabeling the face value of one currency while simultaneously changing the exchange rate with other currencies, thus doing nothing. A physical interaction is the analogue of a real change in the value of a particular currency (for example a national bank failure) which propagates outward through loops of currency exchange and can precipitate a physical effect (e.g., increase in interest rates) in another distant country.

The metaphor was used by Christopher T. Hill of the Fermilab Theory Department to present new developments in particle theory to the Affiliates' meeting. Angela Gonzales prepared the illustration and designed the cover.

(Note: Dick Carrigan and Richard Fenner edited these proceedings; Susan Winchester assisted with preparation of the publication. Principal photography by the Fermilab Photo Unit.)

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Introduction

Leon M. Lederman
Director
Fermi National Accelerator Laboratory

National laboratory directors Leon M. Lederman (second from left) and Alan Schriesheim (right) converse with Affiliates during the Sixth Annual Meeting.

The Fermilab Industrial Affiliates (FIA) organization, the sponsor of this meeting, was established in 1980. The purpose of the FIA is to improve communications between academic and industrial research. Some people question the fact that I use the word "academic" instead of national laboratory. Technically, Fermilab is a national laboratory, but in every sense it really is an off-campus facility for some 70 universities in the United States. There are 56 that are formally organized into a consortium called the Universities Research Association (URA) which has an office in Washington and a board of trustees of some 20 members. URA manages the Laboratory under contract with the United States Department of Energy.

Essentially all the action here at Fermilab is carried out by university scientists. Whatever we do here is so intimately interwoven with the research activities of the universities that we really are a central location where you can see some of the work, at least in our field, that is carried out at most of the major research universities in the United States.

Technology Transfer at Fermilab

Initially, the objective of the FIA was technology transfer, although I didn't know that was the buzz phrase when the FIA was established. In 1981, at our first annual meeting, the approach was simplistic. The session began with a very spirited talk on particles and what we were doing about quarks and leptons at Fermilab. Then we had all of our bright engineers and physicists tell the audience how clever we were and all the things we were inventing. We expected that the participants would go back to their companies and immediately manufacture these nifty items and then sell them. The GNP would take a big jump for which we could claim credit.

Of course, nothing like that happened. We had a wrap-up at the end of the meeting and the best comment we got was, "You should have told us more about quarks, THAT was interesting!" Then we began to have some respect for the complexities of this process called technology transfer, so the theme of the 1982 meeting was just that: What is technology transfer? How do you do it? The keynote speaker at that meeting was Bob Frosch, Vice President for Research at General Motors, who pointed out that it is very difficult to transfer technology from a GM laboratory to a General Motors manufacturing facility. This is a point of continuing study and interest.

In 1983 we picked as a theme "Supercomputer Developments in the Universities," which was kicked off by Ken Wilson, a recent Nobel Prize winner from Cornell. I remember the highlight of that Roundtable was a challenge that Burton Smith, Vice President of R&D for Denelcor, made: "Can somebody guess as to the factor of increase of speed of computers by the year 2000?" The highest factor suggested was 10^{14}. The basic idea that emerged from the discussion was that one of the interesting things that has been happening in the last five years or so is a resurgence of interest on the part of universities and labs like Fermilab in *much* more powerful computing. The needed factor of improvement vastly exceeded what one might expect from the computer industry. The developments in university labs of enormously powerful parallel processors, organized for special purposes, was surely influential in the developing supercomputer architecture.

In 1984, we talked about basic science projects, in particular something called the Superconducting Super Collider (SSC) as an example of some very large pure-research projects which will depend very heavily on industrial participation. I believe those discussions helped to further the industrial understanding of the SSC and helped us scientists to better understand what industry could bring to a mammoth project like that and how industry should be approached.

Last year we thought we would have some fun, and we decided to go way out on a limb of speculation. We had serious people, a Nobel Prize winner and people of that ilk, discussing the practical applications of anti-matter and quarks and muons and other such exotic things. The ground rules were that Roundtable participants were allowed to speculate as long as they didn't violate the basic laws of physics. All of these Roundtable monographs are available from our Office of Research and Technology Applications (ORTA).

This Roundtable covers a less exotic theme: science, economics, and public policy. The subject has largely been stimulated by two factors: my interest, as well as that of others, in trying to measure the value of basic science; and the deepening fiscal crisis in the government and the perception that, in fact, basic research and even applied research in this country are not all that healthy. Several years ago, I reviewed my own perception of the value of basic science in a *Scientific American* article. One of the initiatives I called for was a study of the economic value of basic research. Edward Steinmueller, one of our Roundtable participants, and David Mowery have now proposed to undertake just such a study of high-energy physics. Likewise, Norman Metzger, of the National Academy of Sciences, has recently nurtured a somewhat similar study on the federal role in research and development. The basic question is, "What are some of the issues related to how one supports basic research in this country in the face of the Gramm-Rudman-Hollings environment?" Once you get professional economists into the act however, there is no telling where they will take us.

Research at Fermilab

Now that is the history of the Industrial Affiliates. At Fermilab we have a very simple-minded mission, to do research in particle physics. We are quite modest; we just have three questions we would like to answer: What are the fundamental objects that make up the Universe, what are the fundamental forces, and how does the Universe work? Try as I might, I have not been able to put how far we've gotten, on both sides of a t-shirt. Just to summarize, we believe that we are really in a revolutionary phase. I believe that in the 20th century we can recognize a revolution produced by the relativity theory, and another revolution produced by the quantum theory. We are in the middle of a third revolution that doesn't have a name yet. It has something to do with the basic particles and the basic forces. It also includes the merger of interests, in the last five years, of the particle physicists who use giant accelerators, like the one at Fermilab, to probe into the structure of inner space, and the astronomers who have been using telescopes and space probes looking at outer space. Both of these groups are now finding that their progress is linked to one another. Here we are talking mostly about the cosmologists and astrophysicists who are interested in the early Universe. It turns out that the early Universe was simply an accelerator laboratory with an unconstrained budget. Therefore, astronomers, in order to model the Universe from creation onward, have to know more about what are the fundamental objects and forces. So we now have, at Fermilab, an Astrophysics Group that institutionalizes this new symbiosis.

Fermilab was created as a result of a Ramsey Committee recommendation to the Atomic Energy Commission in 1963. In a typical planning cycle of some 10 years, the first beams arrived at Fermilab in 1972. At that time, it was a 200 billion-volt accelerator. In 1973, again a 10-year cycle, R&D began on superconductivity as appropriate for accelerators here at Fermilab, and in 1983 the TEVATRON was brought into operation. The new accelerator had a funny series of names. It sometimes has been called the Energy Doubler, because it was going to double the energy of Fermilab's accelerator, and sometimes the Energy Saver, because superconducting magnets don't get hot and don't use as much energy. In fact, the Energy Doubler/Saver (whatever it's called) *did* double the energy and *did* save some 30 or 40 megawatts of electrical power. Before construction started on the TEVATRON, the original Fermilab machine had evolved to 400 billion volts in 1974. We started operating the TEVATRON at 800 billion volts. That continued through 1985. We hope to come on in 1986, after a shut-down, at 900 billion volts, and we should ultimately get close to 1000 billion volts. Thus, the new accelerator is called the TEVATRON because "TeV" stands for 1000.

The TEVATRON construction program began in 1979 and should terminate in 1986. If you drove through the site, you saw a lot of civil construction going on. This is the last stage in the TEVATRON construction program.

The first really full-scale application of the 800-GeV machine occurred in 1985 when we extracted 800-GeV protons and fed them to a large number of experiments for the so-called Fixed-Target Program. In October of 1985, we had our first test of a more exotic application, namely head-on collisions of protons and antiprotons, both of them circulating in the Energy Saver magnet ring. We achieved collisions at the world's highest energy: 1.6 trillion electron volts.

Now we are in the early stages of another, hopefully, 10-year planning cycle for a new accelerator called the Superconducting Super Collider, which had its official birth in 1983. That will be a 40-TeV supercollider.

Socially Redeeming Activities

There are a number of other programs at the Laboratory not directly connected with the basic research mission that are quite interesting. One has to do with the work on cancer. We have been probably the largest facility in the world for treating tumors with fast neutrons. That program is more than 10 years old. Now we are involved in a process which could be called "technology transfer," or perhaps spin-off is a better word. This is the construction of a small proton machine. We were asked, in fact, by the medical people involved in neutron therapy to provide protons as a byproduct of our accelerator operation. However, when we looked at the cost of having a facility here, including the treatment rooms and all that, we decided it is more sensible to build a proton machine that can fit into a hospital room. The State of Illinois gave us a grant which encouraged us to do a preliminary study. We have now entered into a collaborative agreement with Loma Linda University Medical Center and we are building a prototype here that will eventually be turned over to them.

An activity that I'm very proud of is something called the Illinois Math-Science Academy. Some of you who live in Illinois may know about this. It will open in September. It took us about three years to bring it to reality; to convince the Governor and the legislature that this is a good idea. This is a school for gifted math-science students. It is very rare, I think, in history that somebody hands you a blank pad of paper and tells you to design a new educational strategy for bright kids, with no constraints. Forget about boards of education, forget about all of the rules and sit down with the best possible advice you can get and design a school for bright kids. That is exactly what is now happening.

The Topic: ECONOMIC DEVELOPMENT

The subject of today's Roundtable is not exactly brand new. It was first broached by Francis Bacon in about the year 1600. Bacon, at that dim period between Galileo and Newton, had grasped the power of science and forecast the great social value of scientific knowledge. Science, Bacon said, is for improving the condition of the human race - not to make humans perfect but to make imperfect humans comfortable. Bacon did understand the cultural drive but stressed the payoff: to endow human life with new inventions and riches. This was early technology transfer; I'll be interested to see if our panel makes it any more clear.

Roundtable Participants

Moderator

David Morrison

IIT Research Institute

Morrison is President of IITRI - the Illinois Institute of Technology Research Institute in Chicago. IITRI has been responsible for formulating a proposal to locate the Superconducting Super Collider in Illinois. Prior to assuming the presidency at IITRI, Morrison was Associate Director of Battelle Memorial Institute. Morrison is a chemist.

Panelists

Norman Metzger

National Academy of Sciences

Metzger is the Deputy Director of the Office of Government and Public Affairs of the National Research Council. Recently, he has also served as a study director for the Committee on Science, Engineering, and Public Policy (COSEPUP). In that capacity he directed a November 1985 COSEPUP workshop on "The Federal Role in Research and Development." Metzger was trained as a chemist. He has published several books including *Men and Molecules* (Crown) and *Energy: The Continuing Crisis* (Harper and Row).

George E. Pake

Xerox Corporation

Pake has recently retired as Group Vice President and head of corporate research activities for the Xerox Corporation. He currently serves on the Board of Overseers for the SSC and is also a member of the Silicon Valley Research Project Advisory Committee for Stanford University and their Center of Economic Policy Research. Before joining Xerox he was Executive Vice Chancellor of Washington University and has served on the President's Science Advisory Committee. Pake is a solid-state physicist and author of several books on electron spin resonance.

Alan Schriesheim

Argonne National Laboratory

Schriesheim is the Director of Argonne National Laboratory. He has spent most of his professional life in calculating return on investments from scientific research - not in the philosophical sense, but in dollars and cents. His 30-year career at Exxon included service as General Manager of the Engineering Technology Department and Director of the Corporate Research Laboratories. He is a past member of the Department of Energy's Energy Research Advisory Board, and has served on a variety of committees for the National Research Council, the National Academy of Sciences, and the American Chemical Society.

W. Edward Steinmueller

Stanford University

Steinmueller is currently Deputy Director and Research Associate at the Center for Economic Policy Research at Stanford University. He was co-founder, with three colleagues, of the High Technology Impact Program, a research project aimed at quantifying the economic effects of technical change in the microelectronics-based industries. Steinmueller and David C. Mowery, of Carnegie-Mellon University, have recently been involved in a study of the economic payoffs from high-energy physics.

Basic Science: Let's Opt for World Leadership!

George E. Pake
Xerox Corporation

Basic Science: Let's Opt for World Leadership!

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Introduction

My physics experience is rather far removed from high-energy physics. By my calculation, the research I did flipping the proton's nuclear magnetic moment in a 1-tesla magnetic field involved a 160-femto micro-TeV process. Nevertheless, I have watched particle physicists in general, and Leon Lederman in particular, for a good many years. In fact, as the saying goes, "Some of my best friends are high-energy physicists."

In what follows I want to focus on the value of basic science. To do this I will first consider the payoff from basic science at a business corporation like Xerox. With that insight I will then move on to discuss the broader questions of the pay-offs from basic science in general and high-energy physics in particular.

Business Payoff from Basic Science at Xerox

To understand why we should opt for world leadership in science, I shall first discuss basic science in a Xerox context. Then I shall touch on high-energy physics and the SSC.

In Xerox, we of the Corporate Research Group have developed a mission statement as follows:

> The mission of Corporate Research is to develop positions of technological privilege that both support and extend the company's business strategies.

We often add another statement to the effect that this mission requires efforts by us in research to bring about effective transfer of research results to the development and engineering organizations, which of course it does.

The phrase "positions of technical privilege" applies not only to products whose function incorporates, or is enabled by, a superior new technology. It can also apply through enabling more effective design or manufacture. Thus, the technical privilege can be manifested either in *product* or in the design and manufacturing *process*.

Within this broad mission, I have a list of the generic objectives that the research manager and his teams of researchers set for themselves. In my view, these objectives are drawn from the items in Table I.

Table I.

Objectives of Research Investments

1. Search for new technological concepts of commercial value.
2. Harden a technological concept.
3. Extend/defend a deployed technology.
4. Develop new research or engineering tools.
5. Improve performance/cost for materials.
6. Inform decisions on purchase or acquisition of technology.
7. Build a base of knowledge or expertise.

In the Xerox Corporate Research program, we have activities aimed at each of these objectives. For purposes of this discussion, I want to focus on basic science, which falls primarily under objective 7, and to discuss how achievement of that objective enables us to pursue more effectively the other six objectives listed, all of which have an applied character.

As a general matter, I don't care much for the categorization of science or research in industry as being either basic or applied, because those two categories have much more to do with the motives of the researcher or his organization than with intrinsic aspects of the science itself.

But, even so, the terms "basic science" and "basic research" are widely used. Let me try to explain what I mean by basic science, when I do use the term. Basic science results usually are either broad principles that govern a series of natural phenomena or they are specific numbers relating to properties of natural materials or phenomena. Among the broad principles I would list, from physics, Newton's laws of motion which govern all macroscopic mechanical phenomena, and Maxwell's equations which govern all electromagnetic phenomena (including radio waves, microwaves and radar, and physical optics). There also are quantum mechanical laws that most frequently manifest themselves in microscopic phenomena in the atomic or sub-atomic domains. Examples of specific numbers are such properties as the charge on the electron, the gravitational constant, the wavelengths of significant spectral lines, and somewhat more esoteric numbers such as electron energy band gaps in particular solids. When scientists in any laboratory (whether university or industrial) obtain results of such broad general scientific interest, they typically report them at international scientific conferences or in professional scientific journals that are published and circulated worldwide.

Industrial research organizations normally place only a small portion of their research program investments in basic science. There are reasons why this proportion is small, but there are also strong reasons for participation in basic science if the corporation has a large enough research budget to be able to afford inclusion of the basic activity.

To understand this, let us first consider why most industrial research is applied rather than basic. The applied research leads directly to products or services and therefore is less remote in time from commercial use, that is, from revenues and eventual profits. Basic knowledge, especially in domains of great business interest, is likely to have ultimate application, but in unpredictable ways and after an unpredictable length of time. In general, all applied research rests upon and draws from the world's accumulated reservoir of basic scientific and technical knowledge.

Thus all of us in industry are dependent upon the output from the world's basic-science research laboratories. These are primarily in the universities, with which we in industry therefore have a critically important symbiosis.

In order for industrial research organizations to be in close contact with new advances in basic science, it is important for the industrial group to be an active participant at the leading edge of world science. Effective technical interchange requires that the industrial organization have its own basic research results in the relevant scientific area to use as the currency of exchange. Participation in world scientific and technical conferences is the most important way for an industrial research organization to place itself in intimate contact with the advances in world science. These advances may offer new or at least improved opportunity for commercial benefits to the corporation, and it is the responsibility of the company's research scientists both (1) to be scientifically knowledgeable enough to comprehend the technical advance itself, and (2) to be alert to any business potential the new scientific knowledge may hold for the company. Participation in these conferences is better than merely following the publicly available scientific literature because there is delay between conference announcements and publication, and the conferences also allow one to ask questions of other researchers and to establish a personal professional relationship with them.

Depending upon the nature of a corporation's business, there are certain areas of basic science which are clearly and obviously of interest. For the telephone company, communication theory is such an area. For Xerox, whose copiers depend upon image-wise photo discharging of electrostatically charged surfaces, the study of electron energy states and mobilities in photoconductors is an area of basic science with which Xerox needs to be conversant if it is to do its reprographics business knowledgeably. I believe that we and our customers are much better off, as we purvey copiers containing photoconductors, if we do so in a state of knowledge and understanding than if we are in a state of ignorance: Stated a little inelegantly, I have maintained that it is better to do our business "informed and smart" than to do it "ignorant and dumb."

I would like to mention some specific examples of past basic science research at Xerox and to discuss corresponding commercial benefits. For the first of these examples, I select the chalcogenide-based photoconductors employed in most Xerox copiers until the recent introduction by Xerox of a new organic photoconductor for some members of our 10 Series of "Marathon copiers."

In the initial disclosures of electrophotography by Chester Carlson, he suggested that, among other materials, a layer of sulfur or a mixture of sulfur and selenium would serve as the photoconducting imaging medium. Early in the researches on xerography at Battelle Memorial Institute, W. E. Bixby found elemental selenium in its vitreous form to be a better photoreceptor. Our first copier products used selenium. In the early 1960s, Xerox established a Fundamental Research Laboratory (forerunner of our present Webster Research Center), and this new laboratory initiated programs on the physics and the materials properties of photoconductors. One project, stimulated by Russian scientific literature of the time, studied binary systems of As-Se, Sb-Se, Sb-S and also ternary systems of As-S-Se. These studies led directly to commercialization of what we in Xerox called Alloy 6, which enabled our 4000 Series and 7000 Series copiers. This was a materials science research project which studied electrical, optical, thermal, thermodynamic and xerographic properties as a function of composition. It was all first-rate basic science, much of which was published in the open literature. But it also enabled us to settle on an optimum composition of these so-called chalcogenide ingredients for a photoconductor and to patent that composition for our commercial uses. This is an excellent example of basic science that also served as applied science. Note the interesting fact of Xerox attention to these materials being stimulated by Russian research reports in the open scientific literature.

Not only did basic research results from the world scientific community stimulate our interest in developing these commercially successful photoreceptors, Xerox scientists also did their part in enriching the world store of basic knowledge. Here I quote from the December 1977 Nobel Lecture of Sir Nevill Mott:

> "...the use of amorphous selenium for office copying by the Xerox company is a multi-billion dollar industry developed, as is so often the case, before anybody had tried to make theories of the processes involved. When the subject became fashionable all over the world, we found of course that Xerox scientists knew a great deal about it, and their recent contributions, particularly on dispersive transport, are of highest importance." - Sir Nevill Mott (1977)

This is the general symbiotic pattern of the progress of science. Stimulated by world science, our researchers devised a commercial application, in support of which we developed more knowledge to add to the world scientific edifice.

In spite of the historical commerical successes of the chalcogenide-glass photoreceptors in Xerox products, these materials are not ideal in all respects for such applications. These glasses can scratch easily. They also do not flex readily, which is a desirable characteristic for machines using belt architectures. And their spectral response is often not ideal. For these reasons, and because it is always desirable to reduce cost, Xerox scientists were continually on the look-out for improved photoreceptors. In connection with this search, I quote from a 1973 memorandum by one of our Research Fellows to the manager of our physics laboratory:

> "At the recent U.S.-Japanese Seminar on Energy and Charge Transfer in Organic Semicondutors, held in Osaka, Japan, I had the opportunity to hear of some of the research efforts on charge transport in polymers being carried out at..."

And he named two of our major U.S. competitors. So here was an example of a world scientific conference being attended by Xerox scientists, by scientists of two major U.S. competitors, and also by Japanese scientists working for companies that were soon to become significant competitors in the world copier market. These scientists were exchanging research results and thereby building the base of fundamental information from which we all draw in our applied commercial work. Thus the same reservoir of basic knowledge is available to be tapped by all competing copier companies, and it is up to each company to conceive and implement useful, reliable product designs as they draw upon this common knowledge base.

In this example, the Research Fellow who attended the 1973 Osaka conference pioneered in Xerox research on electronic charge transport in molecularly-doped polymers, and he was stimulated to invent the structure and improve the performance of a new Xerox organic photoreceptor that was the basis for the one introduced in our model 1075 Marathon copier in 1982.

Thus, the areas of basic science pursued in our Rochester laboratories have related to the materials and physical processes associated with electrostatic imaging on the photoreceptor and to the materials critical for image development. These laboratories also have established a world-leading position in liquid crystal imaging technology. The instruction panels and diagnostic display panels on our 1075 and 1090 copiers were a direct outgrowth of basic work on liquid crystals done by Xerox over the years. The Rochester research laboratories, as well as those in Palo Alto and in Canada, have all participated in basic research critical to our opportunities in amorphous photovoltaics and semiconductors.

Basic work at the Palo Alto laboratories in III-V compounds provided underpinning for the applied research that led to the high-power solid-state lasers. Once these lasers were demonstrated in the research laboratory, the formation of Spectra Diode Laboratories by Xerox and Spectra-Physics became an important joint business venture.

Cognitive and instructional science research in Palo Alto stimulated the formation of the Xerox Artificial Intelligence Systems business unit. This research also led to a design tool now used in Xerox for structuring the human interface to large copiers and duplicators, and to another expert system for designing the paper path through copiers and printers.

As a matter of research policy, we have targeted over the years to have 20% of the Xerox Corporate Research Program be in basic science. This is a somewhat arbitrarily determined target, and I estimate that, as a consequence of the inevitable pressures to do applied research, we have seldom managed to keep the actual percentage above 15%. If it were ever to fall as low as 10%, I believe it would be a genuine cause for concern about the health and vigor of our research organization. I do not know the corresponding percentage for AT&T Bell Laboratories, for IBM, for GE, or for Eastman Kodak research. Also, I believe that these other industrial laboratories have a higher percentage of development activities in their programs than does Xerox Corporate Research. These are the companies which come immediately to mind as those which would have the same compelling reasons to be active in basic research as we in Xerox have.

One of those compelling reasons, which I have not yet mentioned, is the provision of a quality yardstick for our corporate technical programs. By participating at the leading edge of basic science, a corporation's scientists rub shoulders with the leading scientists of the world, through attendance at national and international scientific meetings. This helps them to set quality standards which they reflect and transmit within the entire internal, proprietary research program of the corporation. By participation in world science, our research organization and its members learn to do research in the same way as the best research is done world-wide. And, of course, they also are able to track world science advances, assessing them in the context of Xerox needs and opportunities.

Criteria for Selecting Areas for Research Investments

Any collection of researchers worth its salt will come up with many more ideas for research projects serving the organizational objectives than the organization budget can accommodate. Selecting and rejecting from these opportunities occurs at all levels - by the individual scientist, by his project group ("area" in Xerox parlance), at the section or laboratory level (a unit consisting of several areas), or at the research center level. (In Xerox, each of the three research centers consists of several sections or laboratories.) Selection can occur at my level, as the individual responsible for all of Xerox research, but let me insert here the famous quotation of

C.E.K. Mees who for many years successfully guided the research organization of Eastman Kodak:

> "The best person to decide what research work shall be done is the man who is doing the research, and the next best person is the head of the department, who knows all about the subject and the work; after that you leave the field of the best people and start on increasingly worse groups, the first of these being the research director, who is probably wrong more than half of the time; and then a committee, which is wrong most of the time; and finally, a committee of vice presidents of the company, which is wrong all the time." - C.E.K. Mees, Vice President, Research
>
> Eastman Kodak
>
> (October 22, 1935)

In spite of my subscription to the principles set forth by Dr. Mees many years ago, I don't wander away and ignore the research groups and the scientists - nor did Dr. Mees. There is a very important role in guiding, challenging, continually reassessing the selection process at all levels. And it is surely the responsibility of research management to pose problems - or more likely the problem domains - that are of business importance to the corporation. When it comes to selecting projects, the research management is, I believe, best advised to "tune" the research program by using the budgetary power to adjust emphases. In some cases that may mean shutting something off completely or occasionally giving birth to a whole new project or activity. But these steps should be taken with continual consultation and discussion up and down the entire hierarchy of research management - which, after all, is only three levels deep in each research center in my company.

To my mind, it is this all-important process of selecting the research to work on, and allocating resources to it, that is the essence of what we call research management. It draws upon the combination of technical knowledge, business strategies, research experience, understanding the psychological make-up of research scientists, and, above all, technical taste. The research manager, at any level, brings all of this to bear in a necessarily subjective way. In my own thinking, I have found the questions listed in Table II to be helpful and well worth pondering as I attempt to make these subjective decisions.

Table II.

Criteria for Selection of Research Areas

1. What is the estimated relevance to projected areas of Xerox business interest?
2. What is the ripeness of the field for research exploitation?
   a. Are there good ideas to pursue?
   b. Are there first-rate researchers (in Xerox or elsewhere)?
   c. Is major investment likely to yield advances, or is the technology judged mature and stable?
   d. How many years are likely to be required for useful results?
   e. What do we know, if anything, about prior failures of others to succeed in the proposed research area?
3. Is the target technology or capability better obtained through purchase from vendors, or by acquisition?
   a. Is a research base essential to intelligent purchase or acquisition?
4. Is the magnitude of the necessary investment within Xerox resources?
5. What are the opportunity costs in displacing an existing research program to make room for the new one?
6. Is there adequate prospect or hope that a successful result can be transferred downstream?

National Payoff from High-Energy Physics

Futurists make a big point of the fact that we are entering (or have entered) the information age. There is much discussion of the importance of knowledge-based systems. All of this emphasis - and our growing U.S. proficiency in information science and in understanding cognitive processes - is, I believe, the basis for continuing U.S. technological leadership in the next two or three decades. The focus on ideas and on systems that employ and extend human expertise is made possible by the advances in technology generated through materials research, because, from a broad perspective, materials research has enabled modern solid-state microelectronics. And modern electronics in turn advances almost every field of human endeavor, surely every field of science.

But there are other, more widely extending applications. Solid-state electronic instrumentation is important to every quantitative function in our commercial world. Banking transactions, petroleum, and medical examples merely hint at the dependence of all sciences upon modern electronic instrumentation. Analytical chemistry, biology, earth and planetary science, and high-energy physics are all progressing rapidly to new levels of understanding enabled in large part by modern electronic instrumentation.

There is one of these areas of science that I want to consider next. That is particle physics or high-energy physics. I accepted appointment in 1984 to the Board of Overseers for the projected new particle accelerator, the Superconducting Super Collider - generally known as the SSC - although SCSC or $(SC)^2$ might seem to some to be more appropriate. The community of high-energy physicists has concluded, with a collective consensus and internal discipline seldom to be found within communities of research scientists, that the most important next requirement to advance elementary particle physics is a 20 trillion electron-volt proton-synchrotron, constructed of two adjacent rings in the same large diameter tunnel, a tunnel that will be approximately 100 kilometers in circumference - or about 20 miles in diameter.

Our knowledge of the elementary particles constituting matter has proceeded to the point where high-energy particle physicists require center-of-mass energies of 40 TeV to effect the close interaction of the constituent quarks within colliding protons in order to ferret out the remaining pieces of the puzzles relating to particle species and the strong force.

Our current experience with large proton synchrotrons provides a useful base from which to work in planning to achieve these desired 20-TeV energies. Already functioning since 1983 here at Fermilab is the TEVATRON. (It provides 1 TeV reliably, using 1000 large superconducting magnets. The Main Ring is 6.3 kilometers in circumference, and the machine is upgraded for operation as a proton-antiproton collider, 1 TeV on 1 TeV.)

A 20-mile diameter particle accelerator having about 7000 superconducting magnets in a subterranean tunnel will, of course, cost plenty of money to construct and thenceforth to operate. In order to estimate the costs and schedule, the Department of Energy (DOE) commissioned the Universities Research Association (a consortium of 56 U.S. universities) to conduct a study in the summer of 1984. The initial SSC design study concluded that the total accelerator cost would be relatively independent of the magnet design selected (within 10%) at about $2.8 billion. This estimate does not cover site acquisition cost, nor cost of measurement and detection instrumentation.

The Universities Research Association has established a Board of Overseers to oversee the SSC project. The chairman is Professor Boyce McDaniel of Cornell University, and (as I said) I happen to be a member of that board. The URA and DOE are establishing site selection criteria. A Central Design Group, under Dr. Maury Tigner of Cornell, is in residence at Lawrence Berkeley Laboratory pursuing magnet design and other important issues, under research contract support from the DOE.

Notwithstanding, as of this date the SSC still does not have official DOE approval, much less authorization and funding by the Congress. The SSC represents quite clearly a major decision and potential commitment for U.S. science.

Some of my friends in the scientific community have substantial reservations about the wisdom and desirability of the U.S. going forward with the SSC. There might even be a number of people in this room this evening who hold such skepticism! I think it may be worthwhile for me to describe to you why I accepted appointment to the Board of Overseers, and why I strongly support the proposition that the U.S. should build the SSC.

My own research interests in the couple of decades during which I was an active researcher were condensed-matter physics and chemical physics. I have held responsibility for the entire research program of a business corporation which sustains itself, its customers, and its stockholders by purveying technological applications to the business office. In view of my own vested interests in condensed-matter physics and in commercialization of technology, it may well be asked why I support a taxpayer subvention of over $3 billion to construct the world's largest and most powerful particle accelerator.

Before I offer my rationale, let me concede some points:

Although the SSC is intended to improve our most fundamental understanding of the ultimate composition of matter, I do not contend that the SSC will directly produce substantial advances for my field of condensed-matter physics.

Although there will be substantial technological and engineering fallout from the SSC project - for example in advancing superconducting magnet technology - these are byproducts and quite likely do not in themselves constitute adequate justification for the SSC, although these byproducts are indeed valuable.

For the near-term and long-range future vigor and benefit of the U.S. industrial and even military economy, the most pragmatic posture I can imagine is one that opts by overt choice for the most advanced knowledge and deepest understanding. For the U.S. instead to deliberately choose a me-too or also-ran research-science status is to opt for relative ignorance rather than knowledge. Nothing is more pragmatic than the broadest and deepest knowledge base mankind can attain. And I am just chauvinistic enough to believe that the welfare and economic vigor of the

U.S. is best assured if we lead the world in advancing knowledge and in developing that broad, deep knowledge base. The imperatives of science - and the imperatives even of universities in the broad sustained quest for knowledge - are very nearly congruent with the requirements for industrial technological leadership. Nothing is more pragmatic than technological leadership, which, while it requires many ingredients, rests primarily, I believe, on world-leading science and on our cadre of the world's best, brightest, and most effective research practitioners. For any part of science where we have world leadership, or where it is within our grasp, I would never run the risks entailed in deliberately opting to be an also-ran. I believe that, in some deep sense, it is un-American to choose to be one of the world's scientific also-rans. In respect to world leadership in science, we condemn this nation to protracted miseries if we opt to be second-rate. I believe this deeply in a philosophical sense, but it is also, for me, quite evidently a highly pragmatic point.

Some people may say, "Well, okay. But in particle physics! And to the tune of more than $3 billion?" My answer is "Yes, and even to the tune of more than $3 billion." I know that is a great deal of money. But it is comparable to the cost of a modern aircraft carrier, maybe less if you count its complement of jet planes. I believe that, on military grounds alone, the SSC buys the nation more security than an aircraft carrier does. And who among us here this evening would miss it if we had just one fewer nuclear-powered aircraft carrier? I'm not sure that even the admirals would miss just one fewer carrier. What does the SSC buy in a military sense? First, it keeps us sharp in certain domains of highly skilled craftsmanship and technology. Perhaps most important, it helps us to build and sustain a cadre of very bright scientists accustomed to world-class technical activities aiming at world leadership. Recall the critical roles filled in World War II by our laboratory and theoretical scientists from even the most esoteric of scientific pursuits. The SSC project will help us to build a world-leading, vigorous, human technical capability.

My comparison with an aircraft carrier can be criticized because Congress will not actually make the decision on the SSC by asking itself, "Would we really rather spend the money on a nuclear carrier and a few dozen jet fighters?" Nor will it say, "Let's have the SSC but, to pay for it, we'll simply knock $X billion out of the Defense Department budget." These are valid points.

The foregoing criticism brings to mind another objection I have heard from some scientists about the SSC. They say it will, over the years, place too big a lien on the annual budget for science and research. There is only a very limited sense in which that comment has any validity; it will surely place an annual lien on the DOE budget for research. But in the larger sense the argument is wrong, because there is no U.S. science budget considered and enacted as a whole. There are the respective agency budgets, many of which have an identifiable science component. After the fact, analysts can look over the total federal budget and aggregate these components into the total number of dollars expended for science. Congress, however, does not arrive at the constituent elements of the so-called science budget by using this aggregating procedure.

Still another criticism I hear about the SSC expenditure runs as follows: $3 billion for construction and $X hundred million a year for operation could better be spent on cancer research or, perhaps, on AIDS research. This kind of argument is, I feel, somewhat specious. There are at least hypothetical ways in which the criticism could have validity. If the further advance of cancer research depended upon one major, identifiable $3 billion next step, then we could argue the relative merits of world leadership in particle physics, of advancing cancer research, or of the technological benefits from this one major leap forward in materials research. But most of these other fields of research are not of such a nature that a single $3 billion project determines their future advance.

In my experience with budgets, whether as a physics department chairman, a university executive vice chancellor, an industrial research center director, or as a vice president for corporate research, the issue is never whether a given activity could use a lot more budget beneficially. Instead, each proposed activity must meet certain tests of relative importance to the task or domain in question, and tests of whether the people proposing it have the requisite skills and a credible track record. From my perspective, the particle physicists of the U.S. pass these tests with flying colors. We need to support them in their efforts to provide the U.S. with world leadership in this most fundamental area of modern physics.

I urge support for the bold U.S. practitioners of our most fundamental branch of physics as they take the steps to lead the world in this high scientific calling.

Roundtable on Science, Economics, and Public Policy

Opening Remarks

Introduction

David Morrison
IITRI

David Morrison (right)

The subject of this Roundtable is "Science, Economics, and Public Policy." On the one hand, that is about as timely a topic as one can raise at a meeting like this, or indeed any meeting in which scientists, engineers, or more broadly, technologists, are gathered. On the other hand, with the desire of Congress to try to cut the federal deficit, and the not-too-subtle hand of Gramm-Rudman hanging over expenditures, all of the government agencies and all of the interest groups are seeking means to justify their fair share of the budget for R&D, and then some. More power to them.

As you might imagine, the trade-offs between things like national security and national economic competitiveness, as well as social welfare and education, are very difficult to make. Interest groups of scientists and science advocates, both for large science and small science, are saying we fit in this milieu somewhere. Obviously there is very little doubt that science, technology, national security, and the well being of society are all tied together. The real question is how and what is the relationship among these factors, and is it meaningful to really try to define the kinds of relationships that are involved?

I won't belabor the point to this audience, but research and development expenditures in this country are enormous. Forecasted expenditures for 1986 are about $117 billion with about $58 billion of that coming from industry, $55 billion from the federal government, and the remaining $4 billion from not-for-profit sources, colleges, universities, and the like. Within this distribution of funds, roughly 12%, or $14 billion, will probably end up in basic research; around 22%, or $26 billion, in applied research; and the overwhelming majority, about 66%, or $77 billion dollars, in development.

As I look at it, these investments that we are planning to make in 1986 and all of those that have preceded them, for as far back as when our federal government got involved in research, give us as a nation a tremendous resource of technology. It is one of the last of a vanishing resource base that we have in the United States. You just have to look around to see that we've lost our ability to economically extract minerals and compete with other countries throughout the world that are doing this. We are out of the basic materials, and the processing business. Our consumer electronics industry is, for all intents and purposes, gone. Manufacturing is fighting for its very survival. We are in danger of losing our edge in microelectronics and pharmaceuticals, and our leading position in computers is being challenged. This is not a very good story to tell.

Our present and future, as far as I'm concerned, rests upon our ability on the one hand to expand through science this existing warehouse of technology, and on the other to more effectively use the warehouse of technology and knowledge we already possess The linkages between science and economics, economics and policy, policy and science are critical to the process of using that warehouse to increase our competiveness in the world-wide marketplace.

I think it is easy to see where science and where economics enter this picture. But where does policy enter the scene? The simple answer is "everywhere." I mentioned the federal budgeting process but the same issues are faced by industry. Whether you are government or industry and have money to spend, you have to set some sort of priorities. Those priorities are not necessarily set on great quantitative rules; they seem to be set primarily on judgements. Judgements come from people and people are part of policy. That is where policy enters the picture.

Policy enters from another direction. Picking up from a recent *Wall Street Journal*, I see that "U.S. and Japan have agreed on a framework for settling a politically troublesome series of unfair trade cases against Japanese semiconductor manufacturers." I think that says that the whole subject is global, international, and something that we could spend a lot of time explaining.

I'm counting on the members of our Roundtable to tie this all together so we can understand at least what the issues are.

There is an interesting commonality among the panel gathered for this Roundtable. We are sitting in the seat of high-energy physics; a science that has raised critical issues having to do with the relationship of science, economics, and public policy. The makeup of the panel may be particularly appropriate. Three of us have backgrounds in chemistry and a fourth is a materials scientist. Perhaps this says that the chemists are the most unbiased people in the world and that's why we are here.

Norman Metzger

National Academy of Sciences

Norman Metzger (center)

Last November the National Academy of Sciences (NAS), the National Academy of Engineering, and the Institute of Medicine sponsored a workshop* on the federal role in research and development. In part that workshop dealt with our capacity to measure economic return on federal R&D investments. I'm going to review the conclusions of that workshop, as well as a second document, "Research Funding as an Investment; Can We Measure the Return?," that has just been published by the Congressional Office of Technology Assessment (OTA).

Basically I'm going to focus on what these reports say about our capacity to measure economic returns. I should warn you that I am reviewing other people's materials. I'm not an economist and in no sense an authority in the field. However, having orchestrated the workshop, having read the papers, and recently having

*The workshop report has now been published and is available while supplies last. Write to: Norman Metzger, National Academy of Sciences, 2101 Constitution Ave. N.W., Washington, D.C. 20418.

to review the OTA report, I keep being reminded of that old story about a person coming upon a drunk on his hands and knees obviously searching for something under a streetlight. He asks, "What are you looking for?" "My wallet," the drunk replies. "Is this where you lost it?" he queries. The inebriated gentleman answers: "No, but the light is better here." Looking at some of these methods for economic evaluation, I think we may be looking for the money in the wrong place for the right reasons.

There are four methods for judging the consequences or results of investments in R&D. There are economic methods, which I will discuss briefly. There are science indicators, which use various measures, such as personnel statistics, comparative funding levels, etc., to assess the vitality of a research enterprise. There is bibliometrics, which is a method which has gained some force recently, which uses scientific publications to evaluate the research output. Finally there is peer review. This is easily the dominant method in government and in industry for judging basic research investments and results.

Peer review can be handled in various ways: it can be formal or informal, it can be purely technical, or it can be blended with other perspectives from engineering, marketing, development, and the like. In any case, peer review is the dominant method, especially at the basic research level. In areas closer to applied research and development other methods begin to pop up.

The OTA report points out that economists using the economic method have been able to show quite healthy returns on private R&D investments. According to the OTA (some people dispute this) they have not been able to show comparable returns, and have at times been unable to show *any* returns, on federal R&D expenditures excepting some applied research programs in agriculture, aeronautics, and energy. Why? What are the difficulties?

A principal one, as you all know, is that federal investments in basic research are rarely ever made for economic purposes. They are made principally to serve the public purposes that we understand but can't easily measure. That difficulty saps the reliability of the various available investment models. The reason is that these models embody an assumption that a hypothetical decision-maker can estimate the dollar value benefits of potential investments and gauge the probability of achieving those benefits. As this audience knows, neither of these assumptions is applicable to government-funded research except in special cases. Basic research by its nature creates new, often unexpected understanding which cannot be valued economically. It cannot be evaluated in the usual investment sense, because it is not appropriable. It is available to everyone, and by that token, of value to all, but to no one in particular.

An example: In the early '70s, physicists began to talk about new kinds of semiconductor structures. At the same time, surface science was having a rejuvenation. Concurrently, high-vacuum methods were improving, in part driven by the needs of the high-energy physicists. As a result, people began to create epitaxial techniques for growing thin-layer crystal structures. The dreams of the physicists in the early '70s were realized in actual structures. Fascinating structures were created including quantum well lasers, integration of optical with electronic components monolithically on a single chip, etc. These are very interesting developments, but how would one have valued the work that was going on in the '70s? How would one have estimated what the results and the economic consequences were going to be?

Another difficulty is that the relationship between research and innovation is rarely linear. Most economic models depend on an input and an output, and a production function that links the two. If you put in a production function you are making an assumption about the way the world works. You are making an assumption about how the research system works, and how innovation happens. But, in fact, what I said earlier about the relationship between basic research and innovation is hardly linear. It is long term, indirect, unpredictable.

Innovation also depends on a lot of things other than research. Interest rates, anti-trust policy, patent policy, the quality of the work force, what other countries are doing, on and on. If you still want to attempt to do an economic evaluation, then you have several difficulties ahead of you. Dealing with inputs, you have to decide where does R&D end and a commercial program begin? For example, is the cost of the Manhattan Project part of the R&D leading up to nuclear medicine? What R&D is relevant to a commerical product? How quickly does the R&D start to depreciate? Does one measure gross or net R&D stock? How are the numerous contributors to innovation valued? What weight is assigned to private versus federal R&D? Should one assign different values to federal research done in government laboratories, universities, or companies? Is a government's definition of R&D the one that should be used? Are overhead, training, information dissemination, data collections and the like, which are often included in agency R&D budgets, in fact really R&D expenses?

Those kinds of questions are mirrored when one tries to also think about what the outputs are. For example, how is the value of such non-economic outputs as national security measured? How are intermediate outputs, such as a mathematical theory that may eventually contribute to the development of new products, to be valued? Where do the outputs of a given R&D project end? How are spillovers in an unrelated field identified or evaluated? Is there an acceptable measure of output in service industries? For example, does a count of a physician's work and the number of hospital beds occupied reflect the output of the healthcare industry accurately? Do hours available for work measure the value of health? What does one do with an output such as the maintenance of the scientific enterprise, which involves real costs but can easily be factored into innovation or productivity gains?

The difficulty, again, is that typical federal R&D produces things that have no market value that economists can begin to measure. There is no market price for most health advances or a strong national defense. Overall, then, to quote Peter Reiss of Stanford University, "Current economic measures of returns for federal R&D at most provide crude historical statements about the contributions of that R&D investment." Further, there are no easy shortcuts that can dramatically improve our methodologies. At the NAS workshop, Harvey Brooks of Harvard commented that the simpler the methodology, the less valid the results, and that some studies are little more than propaganda.

Finally, to quote the OTA report, "The metaphor of research funding as an investment, while valid conceptually, does not provide a useful practical guide to improving federal research decision-making or to judging results."

What happens then? Well, we fall back on the anecdotal, on case histories, on retrospective analysis, and the like. By and large they serve reasonably well. I remember Harvey Brooks calling me up last summer when he had been asked to do a paper for the workshop on evaluating the returns on investments in the physical sciences. He said he couldn't find any quantitative data that he trusted or that he thought was reliable. As a result, he fell back on anecdotal case history methodology. Brooks also talked about the advances in superconducting technology, ultra-high vacuum technology, minicomputers - all driven by investments in high-energy physics.

Overall, just to repeat an earlier point, peer review dominates in industry and government. No federal agency has in place a research or evaluation system which appears to have moved substantially beyond the use of informed technical judgement. There are some more recent efforts by the National Institute of Health and other agencies, to try to apply bibliometric techniques to research evaluations. Those are still going on.

Let me add a final caveat which is more personal than anything else. It is based on a comment that Eric Sevareid once made, that the chief cause of problems is solutions. Obviously, the pressures to make an economic case for basic research will remain and probably intensify. I think there are reasonable responses to those questions, and I suspect we will hear some from Steinmueller. But what I think needs to be avoided at all costs is a number. That could be, for example, the per cent contribution to the GNP of high-energy physics, or anything else, or science as a whole. For the reasons I outlined very briefly, any number is at best questionable and open to attack. I would only repeat the caution given by a participant at the NAS workshop, Hendrik Hortzfeld. He was talking about returns on federal investments in space research, but I think his comments apply to other fields. He said no economic study should attempt to put a "bottom ratio" on returns, on space

R&D investment. There is no such number in existence. It lives only in the uncharted world of general equilibrium theory. All such numbers are products of economic models with many limiting assumptions. Even when these assumptions and qualifications have been carefully laid out, the existence of the number is an attractive bait for those who need to justify space R&D. Once a total returns number is used, it quickly finds its way into misuse.

Finally, the use of an economic model for R&D success may skew decision-making so that we may find that policy makers will pay too much attention to the simple economic measure at the expense of the more important but less quantifiable criteria for federal R&D.

W. Edward Steinmueller
Stanford University

W. Edward Steinmueller (center)

Norman Metzger has made a comprehensive challenge to the role of economists and the possibility of quantifying the benefits from high-energy physics. I want to begin on a note of agreement before I examine our disagreements.

Like Metzger, I also believe that it is incorrect to try to come to a single measure of the final benefit of high-energy physics, or any other basic research program, to the gross national product. As a nation, however, we are re-evaluating the role of our federal government and its expenditures. This re-evaluation is not the sort that occurs with each new administration, but instead is one that asks what is the role of federal expenditures, federal taxation, and federal debt in order to plan the future economic life of our nation.

The U.S. scientific community has been blessed by 40 years of public confidence and generosity. In good times and bad, the federal government has been the major source of funding for research aimed at exploring and enlarging the frontiers of scientific knowledge. This support from public funds is a direct consequence of the belief that more scientific knowledge eventually makes the world a better place in which to live. And a primary measure of making the world a better place in which to live is increasing economic prosperity.

Recently, the social consensus about the value of science to the economy has been strained. Thus, I think that it is apt at this time that we begin to more closely examine the connections between basic research and technology, between scientific knowledge and technological application of that knowledge, and, further, what impact those improvements and technological capability have on our capacity to do new scientific research.

In the hope of resolving some of these problems, some of my colleagues at the Stanford Center for Economic Policy Research have founded a program called the High Technology Impact Program. The research goal of this program is to provide better measures of the contribution of technological change to economic growth and prosperity. We have focused on the under-appreciated role that technological improvements in upstream industries such as electronic components, scientific instruments, and computers have had in downstream industries and on consumers. In addition, each of us has been concerned for some time with the inadequacy of economic understanding of technological innovation. Part of the reason that there are inadequacies in the current economic understandings of these problems is that there has been insufficient attention on the part of the economics profession to the role of science and technology in innovation when considering questions of science and technology policy. In particular, economics has to date provided little insight into issues such as the role of basic research on long-term economic growth, the trade-offs involved in research contracting, the interactions between basic and applied research, the organizational and incentive issues of how research can most efficiently be organized, and methods to accelerate technology transfer, either from or to federal research facilities.

Early in our research effort we determined that the greatest contributions to be made currently would come from very specific studies of particular industries and technologies. While better general models are needed, they need to grow out of a strong fact base and the sort of detailed case studies and anecdotal base that Metzger had mentioned.

Recently we were asked what economists might be able to say about the economic impacts of high-energy physics research. After some thought, we wrote a proposal outlining some of the avenues for research. I would like to share a brief overview of that proposal with you today.

Our first finding was that economists in the U.S. have never systematically examined high-energy particle physics, which I'll refer to as "HEP." This is indicative of a problem in my profession: too much theory untested by detailed knowledge. Our second finding was that this was going to be a difficult undertaking. The fundamental problem was to separately consider the outcomes of scientific research, and the process by which these outcomes or results are achieved. Our intuition and our working hypothesis is that the *process* of HEP research generates important economic impacts more rapidly than the fundamentally new, and often revolutionary, insights that are the content of HEP research *results*. We were delighted to find that several European researchers had reached similar conclusions. In an economic study of CERN suppliers, major economic benefits were identified. The source of these benefits were technologies developed or improved upon in order to *conduct* HEP research.

We also found that Leon Lederman had provided Congress with a specific list of commercial applications of HEP research outcomes. So, our initial research goals include enumerating technologies that have been affected by the HEP research process or its outcomes. This research goal can be attained with the assistance of the industries represented here today. Such research is both an anecdotal process and the beginnings of data-gathering. If we were to stop with a collection of anecdotes and examples, we would only be providing a larger store of such examples. This is a useful task, but one which fails to quantify the benefits. Quantification of benefits is necessarily an exploratory effort, one which does not result in a single number. Through a series of examples, quantification provides concrete evidence to show where high-energy physics research has had an economic impact on society. The process of quantification can then serve as a guide for further research into the linkages between basic research and its economic connection to growth and prosperity.

In order to begin the process of quantifying these benefits, we considered two methodologies. The first is illustrated in Fig. 1. In this illustration there is a theoretical demand curve that I'll discuss a little more in a moment. This figure illustrates the principle of consumer surplus which is a well-accepted measure of economic benefit.

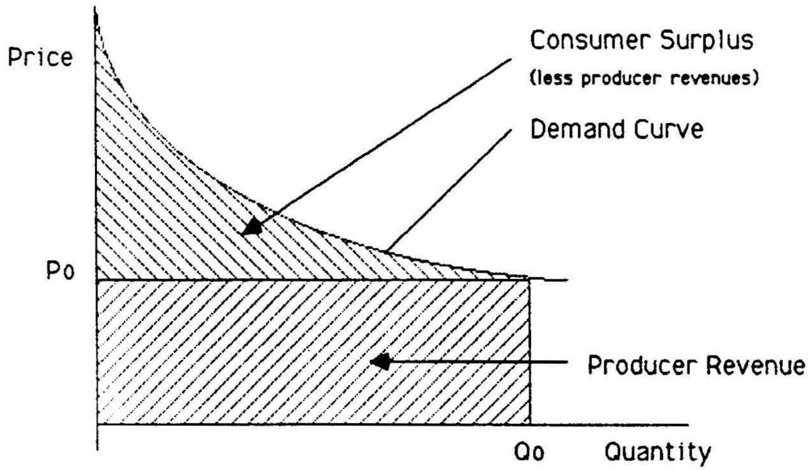


Figure 1

Consumer surplus is simply the difference between what consumers would be willing to pay for a given quantity of a good and what they actually have to pay. In this case, the price that consumers are paying in this market is represented by the number P0. The area above P0 and beneath the demand curve, shaded by diagonal lines falling to the right, is the consumer surplus. Consumer surplus is a measure of what purchasers would have been willing to pay for a product The amount they actually pay is the area of the rectangle beneath P0 and extending to Q0. Measurement of consumer surplus is one method for quantifying the benefits of a specific product. It assumes that we can understand the demand for a product and the value that purchasers place on price-performance improvements. To count this value as a benefit of HEP, we must settle some of the attribution problems that Metzger mentioned, including the question of what the allocation between federal and private research expenditures should be in attributing this consumer surplus. Suppose that, in the process of conducting HEP research, it is necessary to improve a commercially available technology. The "spillover" from HEP research is then a reduction in the price of this technology when it is sold to commercial purchasers. The benefit of this price reduction is measured by the consumer surplus generated by the price reduction. The above discussion and accompanying figure examine the case of a product that would not exist without HEP research.

The same methodology can also be used for products which were simply improved by the existence of HEP research effort. These are products where the price and performance of the product were improved over what they might have been without the benefits of research. I want to stress that this is a technique that requires a great deal of care and specificity in its application and that it is going to be necessary to look at technologies in which HEP research has had a role at a very detailed and specific level. Therefore, it is not going to have any prospect of establishing a general, single number for the contribution of HEP research to the economy. On the other hand, it is a principal technique for coming to some very concrete conclusions and providing some empirical content to the anecdotal information. If we only had anecdotal information without any reference to its quantitative significance, we would always be asked, "Well, is this significant?" This is my primary discomfort with the view presented by Norman Metzger.

Consumer surplus measurement is one of the two techniques that our preliminary research has indicated would be useful. The second is illustrated in Fig. 2 [page 35], and is a somewhat simpler technique that I have applied to the measurement of benefits from the satellite research that was done by the federal government. In many people's opinion this research accelerated the rate of application of telecommunication satellites.

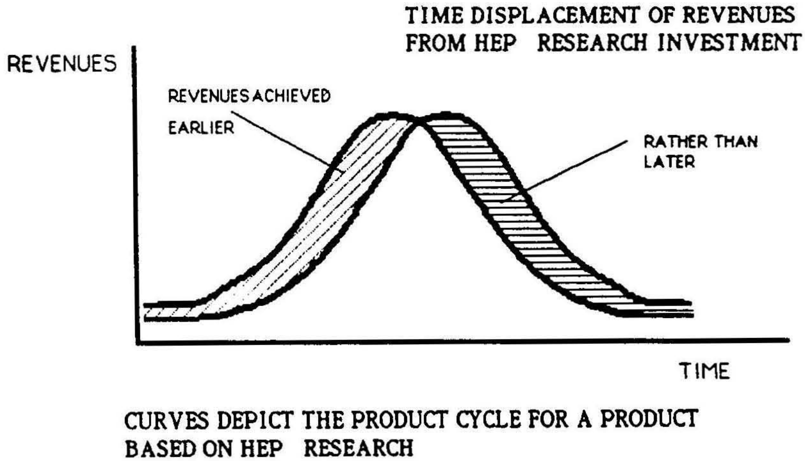


Figure 2

This figure depicts two product cycles. They are both for the same product and the only difference is that the existence of HEP research, or any other federal research program, has accelerated, or brought forward in time, a revenue stream that would have been delayed without federal support of research. The contribution of public research expenditure is measured by the difference of present values of the two revenue streams. The one that occurs earlier, of course, has a higher present value. Such methods can be used to approximate the benefits from high-energy physics research in a specific area. Again, in terms of putting together a specific case study where the level is quantified, this technique can also be useful.

I want to stress that these two quantification methods can only work if we are able to gather and critically evaluate information from industry. I hope that we can cooperate with industry on this research which I believe can go a long way toward helping rebuild a consensus about the value of HEP research.

The last issue I want to briefly mention is the issue of research organization. HEP research and the Universities Research Association are important and unique models for the federal support of basic research. Our proposed research intends to examine some of the benefits, and perhaps limitations, of this form of organization including its role in technology transfer both from and to national research efforts. On balance, the Universities Research Association is an extremely interesting model of cooperation between federal laboratories, university departments, and contractors. We hope to learn more about this effort from both administrators and contractors.

Alan Schriesheim
Argonne National Laboratory

Alan Schriesheim (standing)

Before commenting on all the subjects that have been raised, I should probably admit some of my biases. I want to note that Argonne is a multi-program laboratory that does both basic and applied research. It specializes in bridging the gap between the two areas. In my experience it is difficult to have one without the other. In the current climate it is also certainly difficult to justify the one without the other.

I have spent the bulk of my career justifying both basic and applied research. At various times I've done so according to the Financial Standards Accounting Board, the exponentially expanding regulations of the federal government, the Kabuki theater of congressional hearings, and the bean counters' black magic of several internal auditing systems.

I confess that I have found most efforts to quantify the investment and return-on-research yields no better than 50% fact and 50% perception. If anyone is expecting advocacy of a more sophisticated financial quantification of research costs and benefits, they are not going to get it from me.

There is a presumption that it's easier to quantify investment, and return on investment, for applied research than on basic research. It may be true in industry, at least some of the time. But the comparison is difficult because industry engages in practically no basic research, and I'm willing to argue that point with industrial researchers. What some corporations call basic research is really long-term effort which is nevertheless ultimately focused on some end-use application. Because a chip company seeks to learn everything it can about silicon or gallium arsenide, it hardly qualifies as independent seeking of knowledge for the sake of knowledge.

On the other hand, I'm really not convinced that in federally funded science we have any more accurate fix on the investment and pay-off for applied research than we do for basic research. Part of this has been pointed out before. After all, the general public good is the justification for much federal R&D. Who can quantify the benefits of being protected against armed attack when compared with the investment in defense? How do you put a dollar figure on the benefits of acid rain research? Or to take an example that I'm very familiar with: What is it worth to the nation or to the world to have us develop a meltdown-proof nuclear reactor? On the third of April, Argonne deliberately cut off the cooling flow in an experimental breeder reactor in Idaho while it was operating at full power. The unit shut itself down without human or mechanical intervention. At the time we would have had one estimate of the potential benefits. In fact, we sent a press release out that was picked up only by the Idaho newspapers. They don't have a wide circulation outside of Idaho Falls. Three weeks after that dramatic test, the Chernobyl accident would probably double or triple the estimated benefits of this development. You can't pick up a newspaper, whether it is the *Wall Street Journal* or the *New York Times* or *Time* magazine, without having some comment on that.

My point is that it is pretty easy to trap costs and we should certainly try to do so. But comparing them with benefits in anything like the traditional return-on-investment ratio for financial analysis is going to distort our vision. That is the problem! In my opinion it is not going to sharpen it.

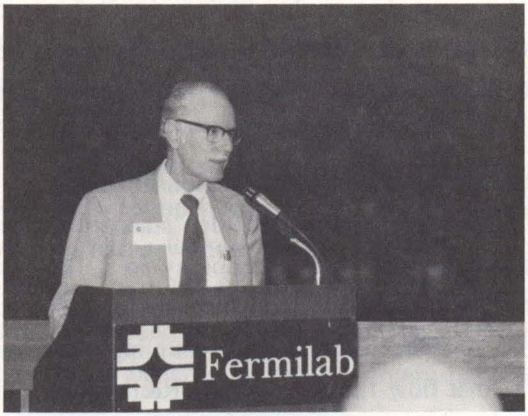
Applied research certainly has one major advantage over basic science. The goal of the work can be described in advance. The results can be shown or demonstrated when the effort succeeds. It doesn't make the results more quantifiable, but it certainly makes them more tangible. The proverbial man-on-the-street and the congressman-on-the-Hill can relate to what is tangible. What they cannot relate to is the basic truth that if we don't pursue basic research, we soon run out of new knowledge to apply.

In the best of all possible worlds I would certainly claim that basic research does not have to be justified, either through elaborate financial analysis or through popular appeal. It only needs to be justified by knowledgeable peers. But we live by a democratic process. We obviously have to tell the layman, who ultimately supplies the buck, what is in it for him, so the best possible justification for basic research is the applied research that grows from it. And to the extent that that can be identified, it is obviously a great thing for those people who are involved in it.

We have a number of initiatives underway at Argonne that speak to this point. I don't really think that I want to take the time to discuss all of them. However, I would like to comment on one to give a flavor for what I consider an important trend of providing research support from federal funding to improve the competitiveness of U.S. industry in world markets. The one that is best known is the steel initiative. It was initiated a few years ago when Jay Keyworth was the President's Science Advisor. Essentially, a question was raised: "Why can't we use the talents and the resources in these major institutions and national laboratories to somehow affect the competitiveness of the nation in a positive fashion?" We examined that particular issue, and the people at Argonne came up with something based on their background in magnets and accelerators. This is a great example of the application of skills that have been developed by the government on the research side, to a nationally important problem.

What does this trend portend for basic research? That depends on how well the basic research community relates and communicates its work to industry and hence, to the public. That is a different issue than quantifying it on a financial basis. This is just a matter of doing what is done in this Roundtable: attempting to define what the fallout is from basic research in some reasonable fashion.

George E. Pake
Xerox Corporation



George E. Pake

I shall begin with an analysis of the sequence of nouns in our topic, coupling my personal experience with each term.

I have, at one period of my life, actually done some *science*; and I have held tenured professorships in physics. Perhaps the greatest contribution I made to the academic world was that I resigned three different professorships thereby opening up opportunities for young people.

As to *economics*, I really know nothing about the subject. Although I have during my life been frequently buffeted about by the repercussions of economic events, I certainly have no scholarly credentials in economics. Perhaps my greatest claim to economic fame was once having the budgets of a major private university in my office; we stayed solvent in spite of selling every university product at a loss!

In the realm of *public policy*, I have been exposed to numerous "science and public policy" issues in my role on the President's Science Advisory Committee from 1965 through 1969, and now on some university boards of trustees and on a significant council of the National Academy of Sciences, called the Government-University-Industry Research Roundtable.

As I attempt to divine what was in mind in selecting the title of this panel discussion I am led to assume that another topic is implied, namely technology. It strikes me that the usual or normal link between science and economics is technology; I believe my experience in industrial research demonstrates that to be the case.

There is not always a hand-in-hand relationship between science and technology. What I have sometimes called the "standard model" has science or scientific understanding upstream of technology in a flow that is idealized to proceed from science to technology to engineering to manufacture to distribution to sales. Sales bring revenues and thereby a coupling to economics. I have seen several examples of that flow first-hand in my R&D responsibilities with Xerox. The model, which I see Mowery and Steinmueller label as the linear model, does have real manifestations, though it is not always followed.

Sometimes, imaginative engineers or technologists slap something together that works, and a useful product is marketed even though there is little understanding of the science underlying the technological events within the product. Several significant inventions have followed this course. Early builders of automobiles knew little of combustion theory, and the Wright brothers knew little aerodynamics. But by the time modern pollution controls via microprocessors characterized automobile engineering, or by the time jet planes began to dominate commercial skies, their critical technologies could be built upon strong and extensive science bases.

It seems to me that the circumstances where technology is likely to get out in front of science occur where necessity has mothered invention. I believe that these kinds of invention are from an era that has largely passed - perhaps one can say that they are from an era we would now characterize as "low tech": the automobile, the Wright flier, the telephone, the cotton gin all fit in that category. When we look at modern "high tech" inventions, they often seem to be close descendants from science: the laser, the transistor or the integrated circuit, recombinant DNA - none of these would have been likely to come from the inventive genius tinkering in his basement or garage. Instead, they spring to life out of the "primordial ooze" of the crackling scientific environment provided in the modern, well-instrumented, highly sophisticated research laboratory.

There are circumstances in these modern research laboratories where invention far precedes necessity. The laser is an example. For several years after its invention, people looked hard for applications of the laser. Indeed, many have been found by now; the laser-Xerographic printer that makes transparencies for speeches or prints thousands of pages per hour benefits greatly. I cannot imagine our Xerox invention of electronic printing without the laser already in existence. It is not conceivable to me that the need for an intense coherent light source for an electronic printer could have provided a powerful enough necessity to induce invention of the laser. Nor would the benefits for retinal surgery have provided sufficient impetus.

What I am driving at here has public policy implications. These sophisticated modern technologies grow out of this primordial ooze of modern research laboratories steeped in basic scientific understanding. Once that basic understanding is deep enough, imaginative scientists see that, here, coherent reinforcement and amplification of radiation can be made to occur, or, there, it has become possible to splice genes. Many of our recent opportunities for economic growth rest on these new capabilities.

The public policy implication is that, the nation requires many first-rate science and research laboratories abounding in the fertile primordial ooze. In order to have a continuing genesis of the new high-tech opportunities for consequent economic growth, the nation also requires a substantial population of bright, well-funded scientists to wallow in this ooze - even to mud-wrestle each other.

A combination of demographic and political events now puts the U.S. at a critically important juncture. As a matter of public policy, our universities and their laboratories have been allowed, through inattention, to stagnate with obsolescent equipment and ever tighter budgets. Scholarship and fellowship support for the few bright ambitious students we have has been curtailed. The primary and secondary school systems that should feed the universities have been allowed to deteriorate - particularly in science education. Ironically, much of this current starvation of education is associated with short term efforts to feed a gargantuan military establishment whose future technological requirements depend critically upon a large supply of trained scientists and technologists and upon a steady flow of new sophisticated technology.

But this supply of technically educated personnel and advanced technology is equally essential for maintaining the U.S. position in the international marketplace - and thus for the economic health of the nation. The warfare for economic survival is a contest in which it is certain we shall have to engage. The current expenditures for the arms race are building an inventory that, hopefully, a skillful foreign policy will avert any need to use. How strange that we are taking almost certain steps toward a longer term economic subservience in order to have a short-term defense against military and political subservience!

I mentioned demographics earlier. We are now missing opportunities to train potential scientists and engineers whom our future economic growth will require - because our public educational system is in disrepair, and we threaten our universities with a similar fate. But the demographic patterns offer very little prospect for making up these lost opportunities during the '90s. In a very real sense, every scientist or engineer we fail to train in the '80s is an irreplaceable loss to our techno-economic engine for future national prosperity. The deterioration of our public educational system, and the incipient erosion of our university system's strength, are critically vital public policy issues.

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."

Technology at Fermilab and How to Access It

Richard A. Carrigan, Jr.
Fermilab

I have heard statements that the role of academic research in innovation is slight. It is about the most blatant piece of nonsense it has been my fortune to stumble upon. Certainly, one might speculate idly whether transistors might have been discovered by people who had not been trained in and had not contributed to wave mechanics or the theory of electrons in solids. It so happened that inventors of transistors were versed in and contributed to the quantum theory of solids.

One might ask whether basic circuits in computers might have been found by people who wanted to build computers. As it happens, they were discovered in the '30s by physicists dealing with the counting of nuclear particles because they were interested in nuclear physics.

One might ask whether there would be nuclear power because people wanted new power sources or whether the urge to have new power would have led to the discovery of the nucleus. Perhaps - only it didn't happen that way, and there were the Curies and Rutherford and Fermi and a few others.

One might ask whether an electronics industry could exist without the previous discovery of electrons by people like Thomson and H.A. Lorentz. Again, it didn't happen that way.

One might ask whether induction coils in motor cars might have been made by enterprises which wanted to make motor transport and whether then they would have stumbled on the laws of induction. But the laws of induction had been found by Faraday many decades before that.

Or whether, in an urge to provide better communication, one might have found electromagnetic waves. They weren't found that way. They were found by Hertz who emphasized the beauty of physics and who based his work on the theoretical considerations of Maxwell. I think there is hardly any example of 20th century innovation which is not indebted in this way to basic scientific thought.

- Henrik Casimir, of N.V. Philips,
at the Symposium on Technology and World Trade (1966)

Technology at Fermilab and How to Access It

Richard A. Carrigan, Jr.
Liason to the Fermilab Industrial Affiliates
and
Head - Office of Research and Technology Applications

Technology at Fermilab - what is it and how can it be used?

To answer these questions it is necessary to know a little more about Fermilab. Fermilab is home to the most powerful accelerator complex in the world. That complex uses superconducting magnets on an industrial scale for the first time. A sophisticated system has been built to store, accelerate, and collide antimatter with ordinary matter. All of this complex is used to study the most fundamental sub-structures of matter, the quarks and leptons we are all made of. The Laboratory is operated by the Universities Research Association, Inc., a consortium of 56 universities, for the United States Department of Energy. More than one hundred universities participate in the research program including many institutions from overseas.

Ninety-five per cent of the work done at Fermilab is basic research directly applied to the single mission of exploring the fundamental nature of matter. On the other hand, the track record of this area of basic research, the study of the roots of nature, has been astounding. On the facing page is the famous quotation due to H.G.B. Casimir, a well-known physicist and Director of the Research Laboratories of N.V. Philips in Holland. Casimir's point is that nearly all of what we now think of as high technology came out of the same line of scientific investigation carried out at Fermilab.

SPIN-OFFS
OF
PARTICLE PHYSICS

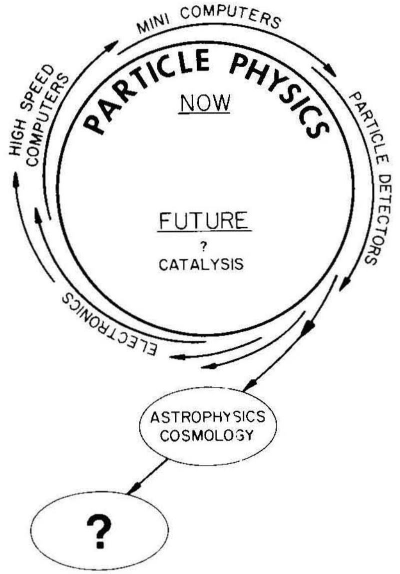


Figure 1

Spin-offs of particle physics

Figure 1 illustrates how applications flow out of a science like particle physics. There are at least three different ways:

One is that the physics itself has been turned directly to some new application. For example, the transistor and the laser came directly out of developments in atomic physics.

Another way science impacts on other fields is through its effect on associated sciences that may then later develop other applications.

Finally, there are the classical spin-offs - technology developed in the process of doing the science. Spin-offs from particle physics have included developments related to computer circuits, high-speed computing, and particle detectors for medicine.

Now for particle physics there is currently no direct application of that science. For the future, there are some possibilities. More on that in a moment. As far as impacts on other sciences, particle physics has had a profound influence on astrophysics and cosmology. In the last decade there has been a growing recognition of the fact that the birth of the Universe, the big bang, was strongly influenced by the laws of particle physics. The field has also had an important impact on accelerator science. In fact, much of the original interest in accelerators came out of investigations related to particle physics.

Materials science is another area where fundamental particles are being used as probes of the properties of solids. Muon spin resonance, discovered in the mid-1950s by Leon Lederman, Fermilab's Director, is now being used as a tool for solid-state material characterization.

In 1985, at the Industrial Affiliates annual meeting, we set up a Roundtable to speculate on the possibilities of applications for particle physics. The publication has taken some time to produce in part because one panelist was wrestling with the question of mining claims for quarks. At present, the best shot for a real application seems to be the possibility of negative muons catalyzing hydrogen fusion, so-called cryo-catalysis. Useful cryo-catalysis is very speculative but not completely ruled out at this point.

Another science at Fermilab also illustrates this flow of applications. This is accelerator science. Accelerator science shows many spin-offs. In the case of Fermilab, applied superconductivity is an example. In addition, accelerators have had an enormous impact both on particle physics and, of course, nuclear physics and material research. Nowadays many materials scientists rely on synchrotron radiation. The devices look very much like particle physics accelerators built 25 years ago. The number of direct applications of accelerators is enormous. The announcement of the 9th Conference on Applications of Accelerators at North Texas State in November of 1986 lists hundreds of talks in 20 different application areas. Just at Fermilab there are two different medical applications. X-ray machines are actually accelerators. The possibilities go on and on and include such

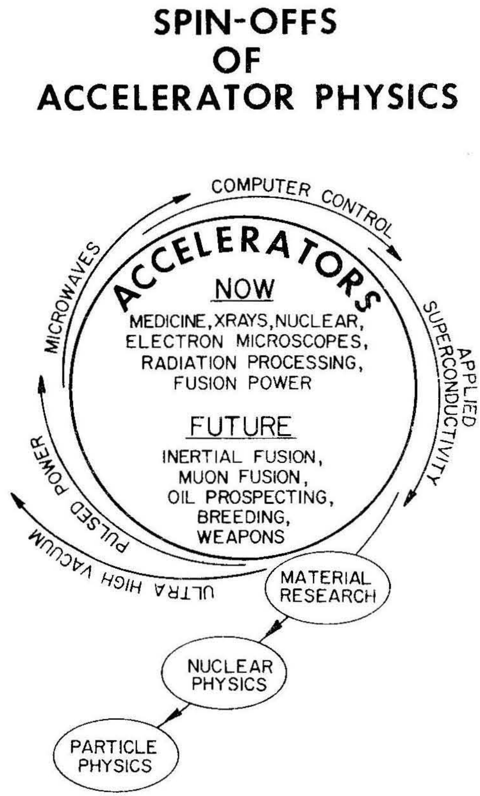


Figure 2

Spin-offs of accelerator physics

interesting topics as pellet fusion. Some of the applications listed as future possibilities in the spin-off diagram (Fig. 2) are now being actively investigated. Several of my colleagues have objected to having weapons on the diagram. However, this possibility has been extensively discussed in the popular press and can hardly be ignored.

What are the technologies that are available at Fermilab? Some of these have already been mentioned. Fermilab is a world center for accelerator science. This expertise goes far beyond producing mere TEVATRONS and colliding-beam facilities. After all, Fermilab has several Cockcroft-Waltons, a powerful Linac, two storage rings, and three circular accelerators. An example of how this technology is being applied is the proton accelerator for medical therapy. This project, sometimes called PAM, is headed by Phil Livdahl, the Deputy Director at Fermilab. TEVATRON technology will be used to build a hospital-scale accelerator with an energy of 250 MeV for Loma Linda University Medical Center in Southern California. Recently, the State of Illinois has funded the establishment of a Fermilab center to foster commercialization of that technology.

Another area already touched on is superconductivity and cryogenics. A good example is the development of the superconducting cable for the TEVATRON. The cable has now been applied to a wide range of applications and more are in the future. Wire based on TEVATRON technology has been used for a wide range of research magnets and is being used in accelerators in Germany and the U.S.S.R. A significant impact of TEVATRON wire development was the scale of the effort. More than 650 miles of niobium-titanium cable was fabricated. Wire was not available on anything like this scale when Fermilab started the TEVATRON. In an important sense, TEVATRON wire established a standard. A wire vendor could adopt a definitive commercial-scale approach without having to specially tailor the wire for each magnet. To make the cable, Fermilab had to cycle the raw material through a long chain of vendors that handled individual steps in fabrication. These included assembling billets of copper interspersed with thousands of niobium-titanium rods and extruding the billets to produce wire with micron-diameter fibers interleaved in a copper matrix. In other steps, the wire was assembled into a multi-strand cable. Fermilab provided a strong stimulation to the wire industry to manufacture the wire. Subsequently, companies such as Intermagnetics General Corporation, New England Electric, AirCo, and Teledyne-Wah Chang developed improved superconducting alloys, wire, and cable. The availability of wire on an industrial scale has led to the modern billion-dollar industry of magnetic resonance imaging systems for medicine.

Obviously, the TEVATRON is an important sandbox for the Superconducting Super Collider, or SSC, the proposed project to build a superconducting accelerator

20 times as large as the TEVATRON. Several years ago at the Affiliates annual meeting, we held a Roundtable on industrial participation in large science projects. What that Roundtable principally addressed was the question of participation in the SSC. That meeting offered the first forum for discussions about the kinds of technology that would be needed for that very large project.

There are also a number of useful developments in the computer field at Fermilab. Many practical and scientific problems have been identified that require

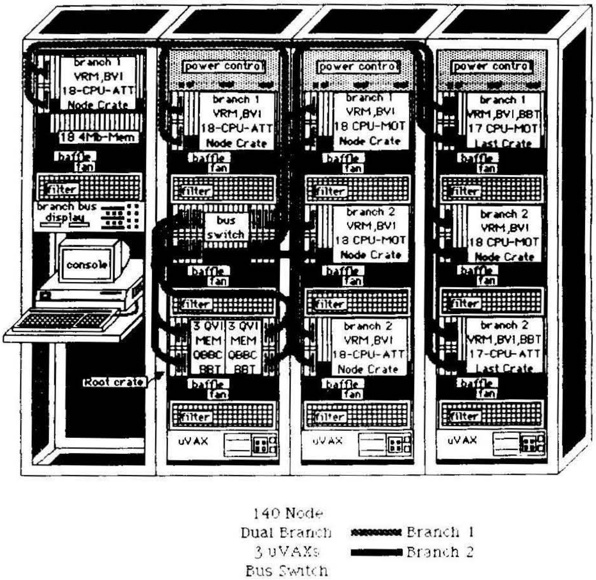


Figure 3

Computer-generated picture of the Advanced Computer Program (ACP)

computers hundreds of times more powerful than existing facilities. The most promising approach is the use of advanced computers with parallel processors. At Fermilab this solution has been attacked by devising a loosely parallel arrangement of hundreds of 32-bit microprocessors. A 140-node version of this device is shown in Fig. 3 above. Software has been developed that can manage these micro-processor farms on a device-independent basis so that new developments and different processors can be incorporated. This type of advanced computer is useful for analyzing thousands of events that must be handled in the same way but are only loosely related. Examples include inventory control and airline reservation systems. For certain problems a $100,000 Fermilab system is nearly as powerful as a CRAY XMP. The Fermilab device was recently awarded a 1986 IR-100 prize. Boards for the Fermilab ACP are being produced by Omnibyte, an Affiliate.

Another area where a great deal of technology has been developed and transferred out of the Laboratory is fast electronics. For many years Fermilab has been an important contributor to the development of the CAMAC modular electronic system. More recently Fermilab has been one of the keystones in the development of the new FASTBUS system. FASTBUS is a standard parallel bus system for high-speed data acquisition and processing. It has been designed for data gathering in the high-energy and nuclear physics research areas. The system is modular so that flexible arrays of different functions (timing, analogue signals, etc.) can be collected. Products developed at Fermilab are now produced by several companies including Kinetic Systems, an Affiliate.

The Fermilab accelerator complex is also a wonderful example of systems of distributed computer control. The control system for the TEVATRON incorporates more than 700 microprocessors. Control for complex refrigerators such as those on the TEVATRON are extremely non-linear. Much of the added complexity in TEVATRON control is due to the cryogenic system. The independent satellite refrigerators must have speed of control, and conversely the ability to control cycles with very long time constants, as well as good quench recovery. A flexible, modular control system has been developed at Fermilab that can handle unmanned remote locations and permit partly autonomous operation of the individual refrigerators. This has required the development of a refrigerator control philosophy as well as a wide variety of special CAMAC units. Much of this system can be applied to any large helium refrigeration system. No such multifunction cryogenic control system existed prior to the TEVATRON.

Figure 4

Halley's comet seen with the Fermilab-Notre Dame Video Data Acquisition System

Yet another area is instrumentation. An interesting example is a video imaging system developed jointly with Notre Dame University. The system was originally designed to record and store the extremely faint and fast light signals from particle tracks in scintillators. A very-high-speed data collection and analysis computer can process this information to average it and remove noise. In essence the system can take and analyze flash video pictures. The system was used recently to take real-time video tapes of Halley's comet (Fig. 4) looking for fluctuations in the comet tail in a period of minutes. This system received a 1986 IR-100 Award.

Now these are generally big, broad areas. Sometimes little individual developments can be fun and quite interesting to people involved in product development.

It turns out that a few of them are actually fairly big. The photograph below shows a technique that was used extensively in the construction of the TEVATRON magnets. I call it "laminated tooling." Basically the idea is to take what used to be magnet laminations, turn them inside out, and use them to actually form the magnet.

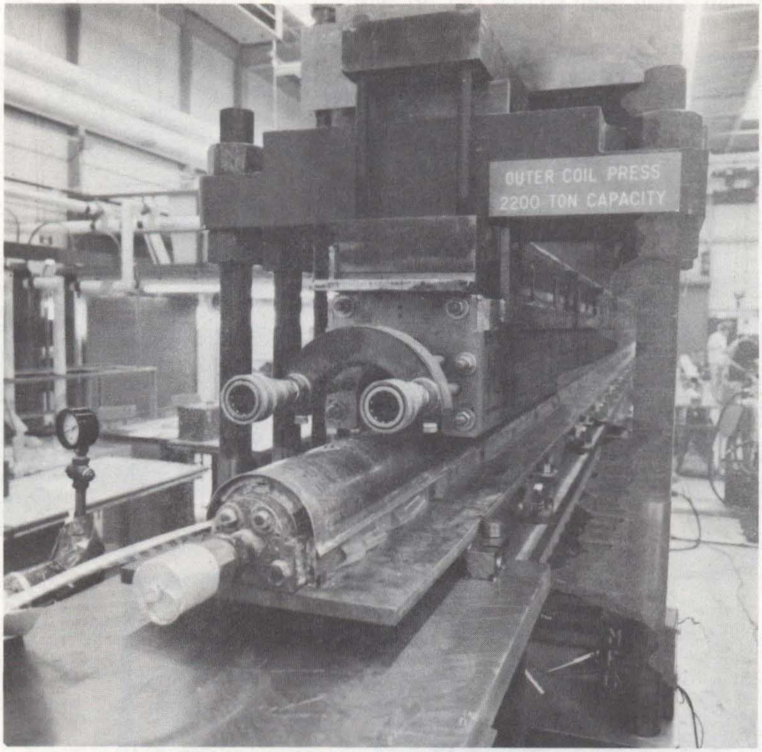


Figure 5

Laminated tooling

One advantage of looking at technology at Fermilab is that it is an open laboratory. I know that many industries would like to take a look at how their competitors are building things. Fermilab offers an opportunity to peek in and see how someone else is doing something, even if you know how to do it better.

Another neat piece of technology is a recent IR-100 winner. This is a wire-position transducer to accurately measure the position of several widely-spaced objects relative to a stretched wire. The idea is relatively old: you take a wire with an AC current on it and run it through some magnetic sensors. Fermilab physicist Hans Jöstlein recognized that modern circuitry made this concept much more feasible than it had been so that sensors with larger openings could be used.

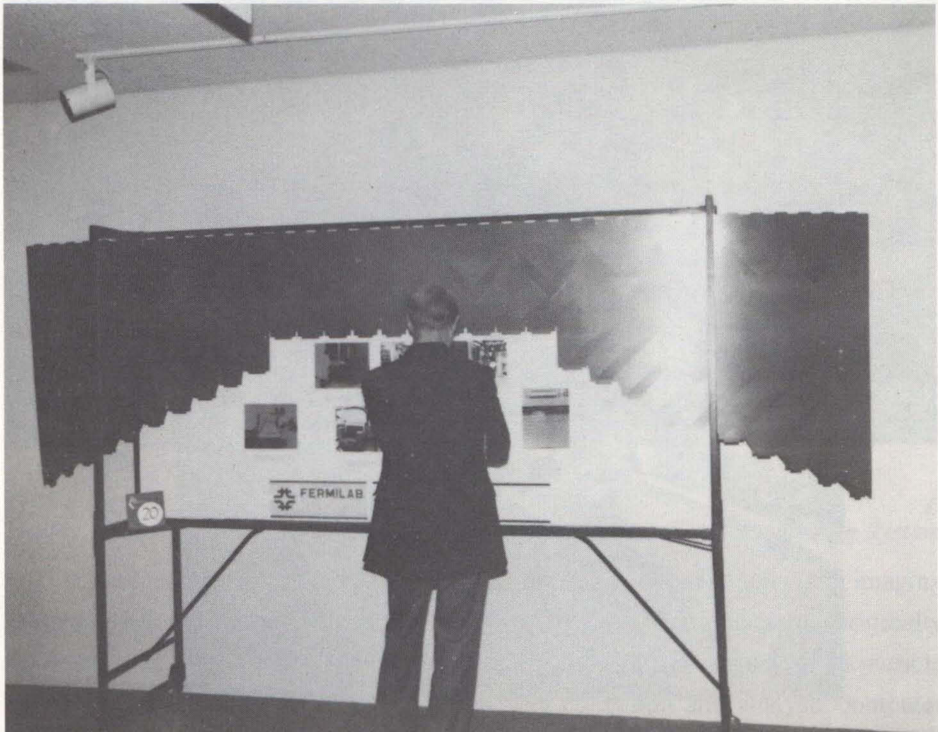


Figure 6

Large printed-circuit board

The photograph above shows a very large printed circuit board produced at Fermilab. This kind of printed circuit board technology is employed for the enormous drift chambers we use for particle detectors. I wonder if this kind of technology could be used inside the dashboards of automobiles or maybe even in modern buildings to eliminate point-to-point wiring.

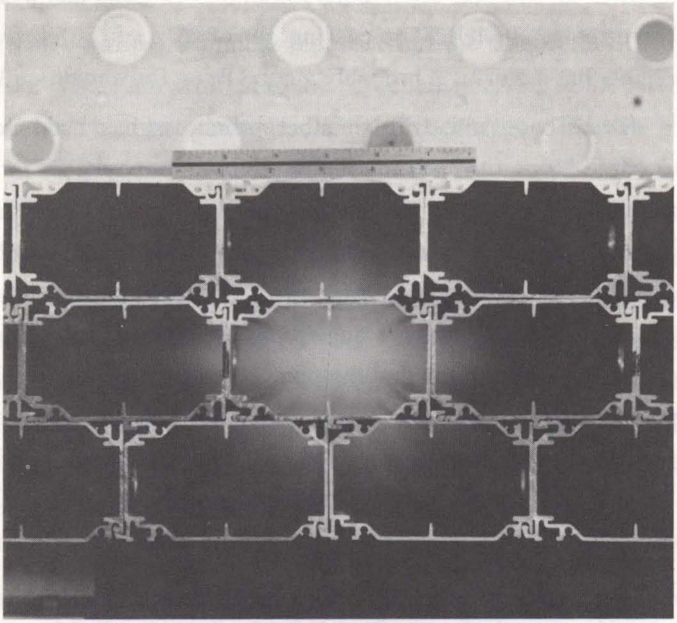


Figure 7
Interlocking extrusions

Another clever idea developed at the Laboratory for drift chamber construction works like Lego blocks for grown-ups. Basically it is a system for interlocking extrusions (Fig. 7). A University of Illinois/Chicago design class was asked to look into the possible applications of this material. They didn't receive much explanation about how it was used at Fermilab. The students developed a whole gamut of ideas, including using the material for making docks.

An example of software is a system developed here called MULTI. MULTI is a so-called event-driven programming system oriented to DIGITAL PDP11 systems. There are an enormous number of computers like this in laboratories scattered around the world. In fact, MULTI has now been transferred to more than

100 universities. Relatively few industrial research laboratories have picked up on it. The wonderful feature of MULTI is that one can do in several weekends of programming what used to take six months for an on-line computer system. Some of our staff here feel that MULTI is passing out of its useful lifetime. My own reaction would be that maturity is probably a good thing for something like this.

Finally, a device based on drift chamber principles has been developed at Fermilab that could be used in positron emission tomography (PET). Remember, this is another kind of CAT-scan-like medical imagery system. Patients swallow a cocktail of positrons, those positrons annihilate into photons, and the PET-scanner looks at those photons coming out. Modern PET-scanners use barium fluoride crystals which can give very narrow pulses. These narrow pulses make coincidences possible. The difficulty is that the phototubes used for these PET-scanners are very expensive so that one might invest $500,000 or so for phototubes for just one scanner. The Fermilab system would use drift chambers to replace those phototubes. These drift chambers are filled with a special gas called "TAMMAE" whose properties were first recognized by David Anderson, a staff member here at Fermilab.

How much technology is there at Fermilab like the individual items I've listed? For the last several years a patent survey has been maintained so that we now have an active invention inventory. That inventory now has more than 400 items in it, and material is being added at a rate of something like 30-40 items a year. We hope in the next year to find better ways to circulate information about this inventory to our Affiliates and the world at large.

How does industry interact with Fermilab? Remember that Fermilab really represents a mix of many, many universities working on a variety of different projects. The best way to establish contact with the Laboratory is to be a Fermilab Industrial Affiliate. In general the purpose of the Affiliate organization is to serve as an effective forum for transferring Fermilab technology to industry. The Affiliate concept gives a window to the Laboratory's technology. If an Affiliate is having trouble making contact with the people they are interested in at Fermilab, they should contact me to see what we can do to facilitate the process. The annual meeting is an important part of our attempt to give Affiliates an opportunity to see the Laboratory and find out what's going on.

The Affiliates receive technical reports from the Laboratory distributed on a monthly basis. These are profiled to the particular interests of the Affiliate, so that the piles shouldn't become too large. We also encourage Affiliates' visits. These have been taking place more and more frequently. Of course, the Laboratory is happy to have anyone visit; however, we find that this is particularly easy for members of the Affiliates.

Another classical way of interacting with the Laboratory occurs when Fermilab buys innovative equipment from vendors or jointly develops equipment with them. The development of the TEVATRON superconducting wire mentioned earlier is a beautiful illustration of this. In this regard, it would be interesting to hear from industry about other ways that we could work together. If you have any ideas for development projects, it might be that we could work together to our mutual advantage.

However, it must be emphasized that Fermilab is not an engineering development center. We can't help someone with their brother-in-law's lottery computer. We don't feel it's appropriate for us to be entering into a wide variety of different technology areas outside of our fundamental mission. For industries in Illinois, the state has now established a number of technology commercialization centers at universities and at Argonne that are more oriented in that direction. Occasionally, we can help someone by directing them to some other institution which handles such matters. In particular, we are happy to try to help Affiliates in this area.

In most discussions of transferring technology, the conversation eventually turns towards questions of licenses, patents, and proprietary relationships.

At Fermilab our general inclination is to publish most of the information developed here. We understand that this could prevent some ideas from coming to market because industry cannot have enough of a foothold to make it worthwhile to undertake the extensive development that is needed for a product. The current Fermilab patent situation is like most government-owned, contractor-operated laboratories in the federal system. In the old days the Department of Energy, our sponsoring organization, undertook all of our patent work. In this arrangement, the federal government owned all the patents.

Several years ago, a new law was passed called "Bayh-Dole." This gave laboratories, like Fermilab, rights to patents developed strictly at the laboratories. This means Universities Research Association, the Fermilab governing organization, would own the patents developed here. Interim implementing regulations have now been published. Based on this law we will begin to try to develop more licenses for material originating from Fermilab technologies.

I hope all of this has shown the advantages of belonging to the Fermilab Industrial Affiliates. If you are not an Affiliate, we hope that your organization will join. We are looking for companies that are generally interested in the technology of the Laboratory. We are not exclusive at all. If you are interested, contact:

Dr. Richard A. Carrigan, Jr.
Head - Office of Research and Technology Applications
or
Dr. Leon M. Lederman
Director

Fermi National Accelerator Laboratory
P.O. Box 500 - M.S. 208
Batavia, IL 60510
(312) 840-3200

Appendices

Appendix A

The Fermilab Industrial Affiliates

AT&T Bell Laboratories
Air Products and Chemicals, Inc.
Ameritech Development Corporation
CBI Services, Inc.
Commonwealth Edison Company
Control Data Corporation
Convair - General Dynamics
Cray Research, Inc.
CVI, Inc.
Digital Equipment Corporation
Digital Pathways, Inc.
Eaton Corporation
General Electric
W.W. Grainger, Inc.
Harza Engineering Company
Hewlett-Packard Company
IBM
State of Illinois
Intermagnetics General Corporation
Kinetic Systems Corporation
Litton Industries, Inc.
Major Tool & Machine, Inc.
NALCO Chemical Company
New England Electric Wire Corporation
Nuclear Data, Inc.
NYCB Real-Time Computing, Inc.
Omnibyte Corporation
Oxford Airco
Plainfield Tool and Engineering, Inc.
Science Applications International Corporation
Signal UOP Research
Standard Oil Company (Indiana)
Sulzer Brothers
Sunbeam Appliance Company
Union Carbide Corporation
Varian Associates, Inc.
Westinghouse Electric Corporation

Appendix B

Agenda of the Fermilab Industrial Affiliates Sixth Annual Meeting: Particle Physics in the Nineties

Fermi National Accelerator Laboratory
Batavia, Illinois
May 29 - 30, 1986

Thursday, May 29

9:00 a.m.	Registration	2nd floor west
10:00 a.m.	Early Welcome and Tour Introduction	Dr. Richard Lundy 1 west conf. rm.
10:30 a.m.	Tours - select one of: 1) Advanced Computer Program 2) SSC Magnets 3) General (Tours will be repeated Friday afternoon)	
12:00 noon	Lunch	
1:00 p.m.	Introduction *1985 has been a banner year for Fermilab with operation of the antiproton collider. This bodes well for the SSC. An interesting technological development is a project on protons for medicine.* *In a different area, Illinois has established a new high school science academy.*	Dr. Leon Lederman 1 west conf. rm.

1:30 p.m.	The Status of the Superconducting Super Collider *The conceptual design has been completed and DOE will seriously consider the proposal.*	Dr. Peter Limon SSC Central Design Group
2:30 p.m.	Proton Accelerator for Medicine *Fermilab is building a prototype medical accelerator for Loma Linda University Medical Center.* *A commercialization center is being established in conjunction with the State of Illinois.*	Mr. Philip Livdahl
3:45 p.m.	Access to Technology at Fermilab *Many technologies are available at Fermilab. The Affiliates organization is a useful way to access them.*	Dr. Richard Carrigan, Jr.
4:15 p.m.	Roundtable: Science, Economics, and Public Policy Moderator: Dr. David Morrison (IITRI) Panelists: Mr. Norman Metzger (NAS) Dr. George E. Pake (Xerox) Dr. Alan Schriesheim (ANL) Mr. W. Edward Steinmueller (Stanford)	1 west conf. rm.
7:00 p.m.	Banquet Speaker: Dr. George E. Pake Group Vice President for Corporate Research, Xerox Corporation	15th flr. north

Friday, May 30

8:30 a.m.	SSC Instrumentation Needs *A survey of some of the technologies that will be needed in the '90s*	Dr. Murdock Gilchriese Cornell
9:15 a.m.	Large Scale Electronic Systems in Particle Physics	Dr. Marvin Johnson
9:45 a.m.	Video Image Intensifier	Dr. Randal Ruchti Notre Dame
11:45 a.m.	New Developments in Particle Theory - Strings and More Dimensions Than You Thought You Saw	Dr. Christopher Hill
12:30 p.m.	The New Illinois Math-Science Academy	Dr. Leon Lederman

Appendix C

Other Volumes in the Fermilab Industrial Affiliates Roundtable Series

1982: *Fermilab Round Table on Technology Transfer and the University - Industry Interface*

1983: *Fermilab Industrial Affiliates Roundtable on Supercomputer Developments in the Universities*

1984: *Fermilab Industrial Affiliates Roundtable on Industrial Participation in Large Science Projects*

1985: *Fermilab Industrial Affiliates Roundtable; Applications of Particle Physics: Out on the Limb of Speculation*

Copies of these books can be obtained by writing to:

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