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 payoffs 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.