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Assessing the Value of Research in the Chemical Sciences

Chemical Sciences Roundtable, Board on Chemical Sciences and Technology, National Research Council

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Assessing the Value of Research in the Chemical Sciences
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Assessing the Value of Research in the Chemical Sciences

Report of a Workshop

Chemical Sciences Roundtable
Board on Chemical Sciences and Technology
Commission on Physical Sciences, Mathematics, and Applications
National Research Council

NATIONAL ACADEMY PRESS
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Preface

The Chemical Sciences Roundtable (CSR) was established in 1997 by the National Research Council (NRC). It provides a science-oriented, apolitical forum for leaders in the chemical sciences to discuss chemically related issues affecting government, industry, and universities. Organized by the NRC's Board on Chemical Sciences and Technology, the Roundtable acts to strengthen the chemical sciences by fostering communication among the persons and organizations—spanning industry, government, universities, and professional organizations—that are engaged in chemically related activities. The principal way in which the CSR does this is to organize workshops that address problems and issues in the chemical enterprise that require national attention.

At its first meeting in February 1997, the CSR identified the topic of assessing the value of research as an issue of increasing importance to all sectors of the chemical sciences. In a world with many needs and limited resources, it is important to find mechanisms to assess the value of various endeavors so that resources can be focused on those activities expected to yield the maximum benefit to humankind and society. These such endeavors include scientific research, long protected by its linkage to national security. But the very nature of scientific inquiry—its inherent complexity and interconnections, long lead times from discovery to demonstration, and focus on the unknown—poses formidable obstacles to developing a set of criteria for predetermining the value of research. To provide a forum for exploring this topic, an organizing committee was formed, and a workshop was planned for September 1997. The resulting workshop, "Assessing the Value of Research in the Chemical Sciences," brought together research managers from government, industry, and academia to review and discuss the mechanisms that have been proposed or used to assess the value of chemical research. The papers in this volume are the authors' own versions of their presentations; the discussion comments were taken directly from a transcript of the workshop. The workshop did not attempt to establish the value of chemical research for the general public, but focused instead on the assessment procedures that have been or will be established within the various organizations that carry out or fund research activities. The expectation for the workshop was not that a single set of assessment techniques would emerge that would be appropriate for all sectors of the chemical research enterprise. Rather, the intent was to allow leaders in each of the areas

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to share approaches and ideas that will help to identify new and useful ways of assessing the value and potential impact of the research activities for which they are responsible. We believe that the workshop was successful in meeting this goal.

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Acknowledgment of Reviewers

This report has been reviewed by individuals chosen for their diverse perspectives and technical expertise, in accordance with procedures approved by the National Research Council's (NRC's) Report Review Committee. The purpose of this independent review is to provide candid and critical comments that will assist the authors and the NRC in making the published report as sound as possible and to ensure that the report meets institutional standards for objectivity, evidence, and responsiveness to the study charge. The contents of the review comments and draft manuscript remain confidential to protect the integrity of the deliberative process. We wish to thank the following individuals for their participation in the review of this report:

Carol Creutz, Brookhaven National Laboratory,
Jack Halpern, University of Chicago,
Joseph M. Jasinksi, IBM Research Center, and
Peter J. Stang, University of Utah.

Although the individuals listed above have provided many constructive comments and suggestions, responsibility for the final content of this report rests solely with the authoring group and the NRC.

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Summary

The first workshop of the recently established Chemical Sciences Roundtable (CSR), "Assessing the Value of Research in the Chemical Sciences," was held in Washington, D.C., in September 1997. The topic of discussion was an issue of long-standing importance that has taken on even greater significance since the Government Performance and Results Act (GPRA) was enacted in 1993. This volume presents the results of that workshop.

As expected, the speakers at the workshop did not present a single set of assessment techniques that would apply across the governmental, academic, and industrial sectors. Instead, they shared their individual approaches and ideas in the hope that other participants in the workshop might identify useful concepts for assessing the value and future impact of the research activities in their own sectors.

HISTORICAL OVERVIEW

In the first session of the workshop, David A. Hounshell (Carnegie Mellon University) and Don E. Kash (George Mason University) established the context of the workshop by providing an overview of the problem of predetermining the value of research. Hounshell's opening presentation, "Measuring the Return on Investment in R&D: Voices from the Past, Visions of the Future," provided a historical account of the attempts within E.I. du Pont de Nemours & Company to establish a set of guidelines for assessing the value of its research investments. He noted that the problem of evaluating research has been with research managers and corporate executives as long as research has been recognized as a separate activity (even extending to medieval times, as noted subsequently by Trueman Parish). In the early decades of this century, DuPont managers intensely debated this issue. As hardheaded businessmen and the inventors of such financial tools as ROI (return on investment), they eventually concluded that it was simply not possible to develop an approach that was suitable for all types of research. Long-term research to understand in detail the mechanism of a particular chemical process required a different approach than did short-term research focused on important yet incremental improvements of a specific product. For the first type of research, fundamental research, they concluded that the most important metrics were the following:

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- Does the project have high scientific merit?
- Does the principal investigator have a record of accomplishments?
- Is the proposed work in a scientific area relevant to DuPont?

Hounshell found that the technical competence and business perspective of the DuPont manager making the funding decision were also crucial. This individual bore tremendous responsibility, for it was his or her job to decide which research projects opened up new scientific and technological opportunities on which the future of the company would depend. If he or she did not understand the essence of the scientific or technical issues being addressed in the proposed projects, poor scientific and technical investments would be made. On the other hand, if he or she did not understand where the business was going in the next several years, poor business investments would be made. The same arguments can be made in other sectors, e.g., for government agencies—the technical competence and mission vision of the program managers are critical to the success of the agencies' research programs.

Kash's presentation, "The Sources of Commercial Technological Innovation," emphasized that is important to approach the establishment of performance measures and metrics realistically. As Kash noted, the connection between research (especially basic research) and the public good is a crucial but general one. For any given commercial product, research may not be the most important link in the chain that leads from discovery to product, especially for complex products resulting from the integration of sophisticated technologies. Nonetheless, Kash noted that in industry research is widely recognized as providing the future of the business, for without new discoveries, fundamentally new products and processes are simply not possible. Thus, in setting performance measures and metrics for research, the role of research must be kept in perspective and no more benefits of it claimed than can be delivered.

ASSESSING THE VALUE OF RESEARCH IN THE CHEMICAL INDUSTRY

Joseph M. Jasinski (IBM Research) presented the "Accomplishments" approach pioneered at IBM for assessing the value of exploratory research. A noteworthy aspect of this approach is the long-term perspective it provides. The process does not focus on just the previous year's accomplishments, but rather reevaluates the impact of discoveries made in earlier years. From the point of view of research, this long-term view is critical, because past discoveries are the basis for today's products. The institutionalization of the Accomplishments process served IBM Research well as the company was dramatically downsized in the early 1990s, for it had at hand the documentation needed to validate the importance of research both for IBM currently and in its future. This fact is now acknowledged in the definition of the role of IBM Research in the corporation: "Vital to IBM's Future Success." Through the Accomplishments process, IBM management and the research scientists that they employ have also gained a better understanding of the impact of IBM Research on IBM corporate needs as well as on the scientific and technical community.

James W. Mitchell (Lucent Technologies) described the evaluation of research in the context of improving the effectiveness of research and (in the case of industry) enhancing value for the corporation. He noted that "valued research will have been assessed by some type of method to measure its effectiveness and productivity"—a truism that is implicitly, although not always explicitly, recognized. At Lucent the most frequently applied method for measuring the effectiveness and productivity of research is to compile a matrix of outputs (patents, inventions, intellectual property, etc.) on which a value can be placed. However, this approach has several limitations when applied to "breakthrough" or longer-range research. To address this issue one must assess the effectiveness of the organization in managing its research portfolio, because the portfolio will exhibit, by necessity, a balance between

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breakthrough research and shorter-range research. Finally, Mitchell noted that scientists at Lucent have found research self-appraisals to be useful, providing the research scientists with a better understanding of the "path to value."

Trueman D. Parish (Eastman Chemical Company) discussed the approach to evaluating research that was developed by the Industrial Research Institute, a multi-industry organization that has invested considerable effort in defining an appropriate set of performance measures and metrics for research. This approach has been labeled the "Technology Value Pyramid" (TVP). The TVP provides a valuable model for developing a set of performance measures and metrics for research efforts that have a clear set of deliverables using well-defined technical approaches (as are found in industry and many government agencies). Although the presentations by Jasinski (IBM) and Mitchell (Lucent) did not specifically refer to the TVP, many of the concepts that they discussed could be tied to ideas presented by Parish. Parish stressed that performance metrics must be "credible," "relevant," and "reasonably simple" if they are to be of use. It is also critical that they capture the essence of the enterprise, for history has shown that the mere existence of performance measures will alter the activities being undertaken. If the measures do not truly represent the values of the organization, the measurement process can undermine rather than strengthen the organization.

THE LINKAGE OF PUBLIC RESEARCH AND PATENT APPLICATIONS

Francis Narin (CHI Research Inc.) discussed the linkage between research and specific public benefits as revealed through the scientific underpinnings of patents. He presented data illustrating the linkages between patents and publicly funded research and concluded that a large fraction of the scientific papers cited on the first page of a U.S. industrial patent originated with publicly funded science. This linkage is more important in some industries than in others. For example, biotechnology patents are more science driven than are automobile manufacturing patents, and have a strong nationalistic component, as illustrated by the heavy dependence of German-invented patents on German research, and of Japanese-invented patents on Japanese research.

ASSESSING THE VALUE OF RESEARCH IN THE ACADEMIC SECTOR

K. Barbara Schowen (University of Kansas) discussed the importance of research in undergraduate education, particularly in the context of the National Science Foundation's (NSF's) Research Experiences for Undergraduates (REU) program. On the basis of her experience during the 10 years of the REU program's existence, she argued that research internships are as important a part of the undergraduate chemistry curriculum as are lecture and laboratory courses. As noted in a report by a group of NSF-REU chemistry site directors at a workshop held in Washington, D.C., in 1990, "Chemistry is a dynamic experimental science for which research is an inherent component. Such a discipline requires 'learning by doing,' an inquiry approach, and an apprenticeship experience. A student's education in chemistry is incomplete without research experience."

Jules B. LaPidus (Council of Graduate Schools) argued that scholarship is critical to the educational function of the university. The importance of research at the graduate level is usually taken as a given, but research is done in many places and clearly is not the defining characteristic of doctoral education. LaPidus argued that research is an integral part of graduate education because of the habits of mind (in other words, the process of scholarship) acquired by graduate students as they seek answers to questions that do not yet have answers. These are not the tidy questions posed in textbooks for which the answers are already known, but the messy questions that the new Ph.D. recipient will encounter in the real world.

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It is the knowledge of "how to read and listen critically, define and analyze problems, determine what the important questions are, decide what research needs to be done and how to do it, understand what the results mean, and learn from the entire experience" that forms the "irreducible core of graduate education."

Richard K. Koehn (University of Utah) discussed the interaction between universities and industry as well as the ambiguities that arise therefrom. He noted that for research universities, the process required to develop an appropriate set of performance measures and metrics may help resolve the conflicting set of measures and metrics being used (often implicitly) today. However, there is no universal metric that can be applied to the many missions of the research universities, which are expected to train students, educate students, bring in research grants, and create jobs. He noted that there is confusion over the first two goals (which are not the same) and pointed out that conflicts are inherent in this set of expectations. If the faculty do more research, they have less time to educate students. If they focus on educating students, they will not be able to help create jobs, because jobs are a spin-off of research. If one of the goals of the university is economic development, how can its success or failure be measured? These questions were posed by Koehn; much more thought will be required to resolve them.

In the end, Koehn argued that it is best to keep the intent of performance measures and metrics in mind. They are for the use of research managers—not for corporate executives and government officials to decide who the winners and losers are. Their purpose is to help research managers understand the impact and relevance of the research portfolios for which they are responsible—to provide information on where they are succeeding and where they are failing, in order to celebrate the former and correct the latter.

ASSESSING THE VALUE OF RESEARCH IN THE GOVERNMENT SECTOR

Patricia M. Dehmer (U.S. Department of Energy) described efforts at the Department of Energy's Office of Basic Energy Sciences (BES) to assess the value of its research portfolio, to determine the tools and metrics by which that value can be quantified, and to assess the results of scientific research by using these tools and metrics. She noted that performance measurement and assessment have always existed in BES, but that GPRA, as well as other laws and executive orders, has given new impetus to these efforts. BES will evaluate performance in four areas: scientific excellence; relevance to the nation's energy future; stewardship, both of scientific user facilities and of scientists, disciplines, and institutions; and program management. Evaluations in these four areas ultimately determine the BES research portfolio, guide its funding choices, and provide a measure of the socioeconomic value of the program. BES measures performance in several ways: peer review; indicators or metrics (that is, things that can be counted); customer evaluation and stakeholder input; and other assessments (which might include cost-benefit studies, case studies, historical retrospectives, and annual program highlights). However, it is recognized that the relevance of each of these measures varies from area to area.

Judith S. Sunley (National Science Foundation) discussed NSF's approach to responding to GPRA requirements. NSF has established its goals by determining what types of outcomes from its programs advance the progress of science and engineering. For research, the most relevant are discoveries at and across the frontier of science and engineering, connections between discoveries and their use in service to society, and a diverse, globally oriented work force of scientists and engineers. Because the timing of outcomes from NSF's activities is unpredictable and the annual change in the research outputs is not an accurate indicator of progress toward outcome goals, NSF has developed performance goals against which progress can be assessed on a continuing basis. A variety of mechanisms will be used to assess NSF's performance, but the process will rely heavily on the use of expert external panels.

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Finally, Mary Groesch (National Institutes of Health) described the approach that NIH is developing to respond to GPRA requirements. NIH is considering two broad program outcomes for its research programs: to increase understanding of normal and abnormal biological functions and behavior, and to improve the prevention, diagnosis, and treatment of diseases and disabilities. A combination of qualitative and quantitative goals and indicators will be the most meaningful for gauging performance. For example, narrative descriptions of research accomplishments will be used to place specific incremental advancements into a larger context. They will describe what was previously known and unknown, the nature of the accomplishment, its contribution to understanding and improving human health, its significance for advancing science, next steps, and, when possible, the economic impact of the advance. Quantitative goals and indicators will be employed wherever feasible and appropriate, for example, in assessing progress in sequencing the human genome. Program management is also an important component of NIH's research programs. Activities that could be assessed include grants administration and peer review, communication of results, technology transfer, and management and administration.

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Measuring the Return on Investment in R&D: Voices from the Past, Visions of the Future

David A. Hounshell
Carnegie Mellon University

INTRODUCTION

In the century-long history of industrial research, three problems confronting large, diversified manufacturers have remained essentially intractable, or, more precisely, perennial. I say, "essentially intractable," because they have not been solved; I say, "or, more precisely, perennial," because managers of industrial research have over the years thought they had solved these problems once and for all, only to see the same problems reemerge a few years later with equal or even greater prominence. All three problems revolve around resources. The first problem is how best to deploy research and development resources: in a central organization removed from the business unit, in a decentralized organization closely affiliated with the business, or through some combination of the two approaches. Over the years firms have moved from predominantly centralized research organizations to predominantly decentralized organizations and then back again, only to swing back yet. Each organizational form has its costs and its benefits, and the combination of the two creates new organizational complexities with their own costs and benefits. Movements from one form to another have been largely cyclical, and firms have tended to move in herds as they are wont to do in so many domains of business.

The second problem is a close cousin to the first: how to allocate resources between the short term and the long term. This is not a simple optimization problem because, unlike very short term R&D investments, the benefits of long-term research are highly uncertain. The longer the time horizon, the greater the uncertainties. Yet if firms invest strictly on a short-term basis, they risk being ruined by what the economist Joseph Schumpeter called the "perennial gale of creative destruction"[1] —technological obsolescence, market displacement, and the like.

Finally (and flowing directly out of the second problem), measuring the returns on investment in R&D has proven intractable. If people tell you they have an accurate and infallible way to measure ROI in R&D at the firm level, the industry level, or the national level, take it with a grain of salt. It is likely

[1]Joseph Schumpeter, *Capitalism, Socialism, and Democracy* (New York: Harper & Row, revised 3rd edition, 1950; Harper Colophon Books, 1975, p. 84).

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that the claimant has an agenda that conflicts with the goal of truly assessing the costs and benefits of R&D. I say this because research—especially long-term research—is highly uncertain, and also because the benefits of research are highly complex, going well beyond the abilities of most models to comprehend them except in a crude probabilistic sense.

As a historian, I cannot—and I will not—say that history "proves" my argument. But I can definitely say that in the past, some very smart people have wrestled with the issue of measuring the return on investment in R&D and have frankly admitted that it is an intractable problem. Moreover, they concluded that any scheme they might develop was so flawed as to be dangerous if used alone for decision making; consequently, they relied on other criteria.

This paper centers principally on the case of the DuPont Company, which until relatively recently has consistently been one of the nation's leaders in industrial research. In 2002 the company will celebrate the bicentennial of its founding and the centennial of the founding of its first major industrial research laboratory. DuPont was among a handful of what I have elsewhere termed "the R&D pioneers" in the United States.[2] DuPont's scientific and technological achievements are many, and some have even become legendary.[3] DuPont also holds a special place in the annals of business history, for as Alfred D. Chandler, the dean of American business history, has shown, DuPont pioneered in 1921 the organizational innovation of the multidivisional governance structure for a diversified multiproduct firm—the classic M-form organization.[4] Even though both structural developments and fashion have served in recent years to undermine the extent and stability of the diversified, multidivisional firm—especially the vertically integrated firm—the M-form remains a fundamental organizational framework in business today.

RESEARCH IN THE DUPONT COMPANY

Even before DuPont arrived at the M-form of organization, its executives had been pioneers in another area of business management: the formulation of a method to calculate return on investment and the development of decision rules or norms based on ROI calculations. The DuPont innovation of ROI calculations represents one of the most significant turning points in the history of modern accounting and management. As Chandler emphasizes, it allowed for the first time the integration of financial accounting, capital accounting, and cost accounting.[5] I suppose one might be tempted to leap to the conclusion that this development marks the triumph of the bean counters, but that would be a dangerous conclusion to reach in the case of DuPont, at least for much of its history. The development of the ROI calculation was the work of F. Donaldson Brown, an executive in DuPont's Treasurer's Office. Brown became treasurer of DuPont during World War I and in 1921 vice president for finance at General Motors, a company then headed toward bankruptcy but rescued by DuPont and an alliance of DuPont family members and executives, who together gained controlling interest of the automaker.

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[2]David A. Hounshell, "The Evolution of Industrial Research in the United States," in Richard S. Rosenbloom and William Spencer, eds., *Engines of Innovation: U.S. Industrial Research at the End of an Era* (Boston: Harvard Business School Press, 1996).

[3]For a review of some of these achievements, see David A. Hounshell and John Kenly Smith, Jr., *Science and Corporate Strategy: DuPont R&D, 1902-1980* (New York: Cambridge University Press, 1988).

[4]Alfred D. Chandler, Jr., *Strategy and Structure: Chapters in the History of the American Enterprise* (Cambridge, Mass.: MIT Press, 1962), Chapter 2, pp. 52-113. See also Alfred D. Chandler, Jr., *The Visible Hand: The Managerial Revolution in American Business* (Cambridge, Mass.: Belknap Press, 1977), pp. 438-483.

[5]Chandler, *The Visible Hand*, pp. 445-448.

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Donaldson Brown's ROI formula eventually became a landmark in textbooks in control, accounting, and finance.[6] In 1993 I interviewed a man who had been an assistant professor of accounting at the Harvard Business School in the early 1940s and later president of a Fortune 10 firm in the early 1960s. Without prompting he recited the formula for me and proceeded to give me a short history of Donaldson Brown's development of the formula and its application in corporate accounting and decision making. His command of this information was as fresh and as complete as if he himself had just developed the ROI formulation rather than having taught it more than five decades earlier. (The interviewee, by the way, was Robert S. McNamara.[7])

I call attention to Brown's ROI formula not only because of its importance but because it was merely one analytical method developed at DuPont to guide its executives in making decisions about the allocation of assets. Brown's work was done in the context of DuPont's bold program of diversification, which over little more than a decade moved the company from being predominantly an explosives manufacturer to becoming a diversified chemical giant. The company's executives needed objective methods to guide their resource allocation decisions. Would DuPont realize a greater return by investing in this business rather than that one? Should executives fund the expansion of this plant rather than that one? Brown's methods helped guide these executives, and it also allowed them to measure the performance of existing DuPont businesses.[8]

In acquiring companies, DuPont's principal method of diversification, executives also needed methods for evaluating the worth of potential acquisitions. How much should DuPont pay for a company that made a commodity product in plants that were on average 10 years old versus a company marketing a branded product in a plant that was 20 years old? What value should the company assign to trade secrets compared with products and processes protected by patents? These are but some of the questions faced by Walter S. Carpenter, a brilliant young executive who carried out most of the evaluations of potential acquisitions. Like Donaldson Brown—and indeed like almost all DuPont executives—Carpenter had attended engineering school.[9] He approached problems methodically and analytically. Despite the high degree of uncertainty in such evaluations—cooked books, inaccurate plant appraisals, and other drawbacks—Carpenter derived a simple way to assign worth, which guided executives in carrying out one of the most successful chapters in diversification in American business history.[10] Despite his youth, Carpenter replaced Donaldson Brown as treasurer of DuPont in 1919 and continued to develop analytical methods for cost analysis and asset appraisal and to refine Brown's ROI formula. (As a member of

[6]As Chandler points out, Brown's methods were still in place and widely emulated in 1950 when the American Management Association issued its case on DuPont's accounting and control methods (American Management Association, *How the DuPont Organization Appraises Its Performance*, no. 94 (New York: AMA, 1950)).

[7]Robert S. McNamara, interview by David A. Hounshell, September 7, 1994. Before he became secretary of defense in the Kennedy Administration in 1961, he served as the president of the Ford Motor Company, where he had built a reputation of being a wizard in accounting and control methods and procedures. For more information on McNamara and his career, see Deborah Shapley, *Promise and Power: The Life and Times of Robert McNamara* (Boston: Little, Brown & Co., 1993).

[8]As Alfred P. Sloan wrote of Brown in his classic, best-selling autobiography (initially published in 1964), *My Years with General Motors* (New York: Doubleday, 1990), "When the du Pont Executive Committee met with the du Pont general managers, Mr. Brown displayed charts on the efficiency of divisional performance, a technique of presentation which he initiated" (p. 118).

[9]Brown was trained as an electrical engineer at both Virginia Polytechnic Institute and Cornell University. Carpenter studied mechanical engineering at Cornell but left the university in 1909, a few months before his scheduled graduation, to take a job with DuPont.

[10]Carpenter articulated much of his and DuPont's strategy in an article, "Development—The Strategy of Industry," *Annals of the American Academy of Political and Social Science* 85 (September):197-201, 1919.

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General Motors's Board of Directors, Carpenter later assisted GM's Brown and Alfred Sloan in solving some seemingly intractable problems in executive compensation and pensions.[11])

Both Brown and Carpenter were nurtured by Pierre S. du Pont, the man who can be credited most for the transformation of DuPont into a highly profitable modern corporation. Trained in engineering at MIT, Pierre was simply a brilliant businessman. His career was distinguished by one principal mode of operation: gather the best information possible—both current information and historical data; assemble it into comprehensible charts, graphs, and tables; and make decisions based on reasoned analysis of the data. Only those who have spent time reviewing Pierre's business records can fully appreciate the extent to which Pierre's brilliance was the product of rigorous information gathering and reasoned analysis of data, rather than flashes of insight or daring decision making.[12]

I have focused on Pierre du Pont, Walter Carpenter, and Donaldson Brown to make a point. These men believed in quantitative data, cost analysis, the benefits of ROI calculation in decision making, and management of the business through tight accounting and financial controls. Yet they never assumed that the firm's research could be managed by the numbers. That is, they never thought for a moment that the firm's investments in research could be evaluated by the same means it used in evaluating whether a new plant should be built, an existing plant expanded, or another company bought. Let us examine more closely what they thought about the management of R&D and how they actually proposed to evaluate its returns on investment.

For a brief period in the first decade of this century, Pierre du Pont found himself with the responsibility for the direct oversight of the Experimental Station, which from its creation in 1903 was responsible for research related to all the company's products and for monitoring and evaluating technologies developed outside the company. Pierre was well aware of a conflict within DuPont's executive ranks about how best to organize industrial R&D. One of his fellow executives, Hamilton Barksdale, had been responsible for creating DuPont's first industrial research laboratory in 1902, the Eastern Laboratory. Unlike the Experimental Station, the Eastern Laboratory was focused on one line of research: high explosives. It worked on both product and process research, and in both of these areas it brought substantial, quick returns on modest investment.

As early as 1904, Barksdale had begun to campaign against the broad, general mission of the Experimental Station. He argued that DuPont would get the greatest return on its investment in R&D if it organized research for its other principal products—smokeless powder and black powder—in the focused manner of the Eastern Laboratory. Pierre du Pont shared the views of his business mentor, Arthur J. Moxham, the head of DuPont's Development Department, who believed that the Experimental Station had a much broader mandate and that ultimately it would bring major returns to the company. DuPont's Executive Committee argued strenuously over which approach was best, and it ultimately stalemated, leaving DuPont with two approaches to industrial research, one centrally managed and focused on corporate-wide research and the other a narrower, business-unit-focused laboratory with a

[11]Carpenter's business contributions and life as a business executive are brilliantly discussed in Charles W. Cheape, *Strictly Business: Walter Carpenter at DuPont and General Motors* (Baltimore: Johns Hopkins University Press, 1995). As Cheape points out, Brown vehemently (and ironically) opposed the election of Carpenter to succeed him as treasurer of DuPont, largely because of his youth, despite the praise heaped upon Carpenter by DuPont's president, who said that "Carpenter's experience in the study of financial statements of companies which we have investigated, as well as in studies of the investments in branches of our own company . . . eminently qualify him for the position" (p. 51).

[12]See Alfred D. Chandler and Stephen S. Salsbury, *Pierre du Pont and the Making of the Modern Corporation* (New York: Harper & Row, 1971) for a rigorous treatment of Pierre's life and work.

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clear mandate and relatively short term objectives.[13] During the brief period of Pierre du Pont's oversight of the Experimental Station—1907 and 1908—Pierre instructed the director of the Experimental Station to think broadly and for the long term.

> In our Experimental [Station] Laboratory we should at all times endeavor to have in force some investigations in which the reward of success would be very great, but which may have a correspondingly great cost of development, calling for an extended research of possibly several years, and the employment of a considerable force. I outline this policy for two reasons; first, that it will tend to build up a line of well trained men whose continuous employment will be certain. Second, and more important, the value of the Laboratory will eventually be much greater on this account.[14]

Pierre du Pont thus made a remarkably clear statement that the returns on research investment cannot be measured solely by their direct effects—a new product or a new process, an improved product or an improved process—but also must be evaluated on their secondary and tertiary effects, which in many instances transcend the primary effects.

In 1911, in a major reorganization of the top management of the company, which came about entirely independently of debates about research management, Hamilton Barksdale became the general manager of the company, or what in today's parlance would be the chief operating officer. Barksdale's philosophy about the management of research has already been noted; he believed that it should be closely aligned with the operating or manufacturing units of the company and must be measured by short-term criteria. Even before taking the reins of the company, Barksdale moved to make his point about the different performances of the Experimental Station and his near-and-dear Eastern Laboratory. He convinced the Executive Committee to request each research organization—the Eastern Laboratory and the Experimental Station—to prepare a retrospective three-year evaluation of what it had contributed to the company's profits.[15] Charles L. Reese, the founding director of the Eastern Laboratory, had an easy time of it. He simply took four projects at Eastern that had resulted in new products and processes; reported on the sales, earnings, and savings stemming from these innovations; and compared these figures with the total costs of the Eastern Laboratory's operations over the same period. He was able to show that for each dollar the High Explosives Operating Department spent on R&D, the lab had returned roughly three dollars to the company. As Reese crowed, "In consideration of the fact that only four of the many subjects worked upon at the Eastern Laboratory are included in the estimate of saving, it is safe to say that the Eastern Laboratory has justified its existence."[16]

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[13]This battle within the Executive Committee is discussed in Hounshell and Smith, *Science and Corporate Strategy*, pp. 26-29. Early in the Experimental Station's history, Pierre's business mentor, A.J. Moxham, and the founding director of the Station, Pierre's cousin, Francis I. du Pont, believed that the work at the Station could be easily accounted for and, as they said, "be put entirely upon its merits as a business department" (quoted from the minutes of the DuPont Company's Executive Committee meeting of December 17, 1903). They anticipated that the Station's research could be valued much in the same way as the company bought patents, and that once a project was sufficiently developed by the laboratory, it could be sold to an operating department or be sold freely on the open market. But such optimism for accounting for the return on research proved very short-lived, especially when executives realized that much of the Station's work could not be readily sold on the open market, for reasons that economists such as Kenneth Arrow would later explore. On these initial attempts to account for research, see Hounshell and Smith, *Science and Corporate Strategy*, pp. 33-34.

[14]Pierre S. du Pont to C.M. Barton, August 17, 1908, as quoted in Hounshell and Smith, *Science and Corporate Strategy*, p. 45.

[15]The Executive Committee passed this resolution on December 18, 1910.

[16]Charles L. Reese, "Eastern Laboratory: Its Work and Development," 1911, as quoted in Hounshell and Smith, *Science and Corporate Strategy*, p. 50.

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Reese's counterpart at the Experimental Station—by this time, Pierre's younger brother Irénée du Pont—had a far more difficult time. He could not demonstrate in any rigorous manner any direct returns to the company's investment in general research. The time horizon was too short, he stressed in his report. However, he believed that the Experimental Station had definitely generated many of the secondary and tertiary benefits that Pierre had identified four years earlier. Much of the work of the Station had been devoted to meeting new regulations being imposed on the company by the government.[17] He might have added that the research the Station had done in the area of smokeless powder had kept the company ahead of the U.S. government, its sole customer for the product and a potential competitor. This fact would soon be borne out.

Under pressure from the anti-big business forces that had begun to build in Congress in 1908 (shortly after the Justice Department launched its antitrust case against DuPont), the Army and the Navy had been pressured into finding means within each service to make the nation less dependent on DuPont for both the development and the manufacture of smokeless powder. In 1912 the Justice Department won its case against DuPont for violations of the Sherman Act. The company was forced to accept a consent decree that called for DuPont to divest two-thirds of its manufacturing capacity in explosives, establishing two new competitors, Atlas (which eventually became ICI's U.S. operations) and Hercules. But the military services interceded, defending DuPont as a progressive company whose research in military propellants was critical to the nation's security. In the consent decree of 1913, DuPont was allowed to keep all of its military propellants capacity. World War I broke out the following year, and DuPont could not have been better positioned to make vast profits from the sale of military propellants.[18] In this case, the tertiary effect of the research program at the Experimental Station was simply enormous.

Hamilton Barksdale attempted to bring greater "relevance" to the Experimental Station and to narrow its research areas and shorten its development horizon in 1911 by putting his favorite research director, Charles Reese, in charge of the Station in addition to the Eastern Laboratory. Ironically, over the next 8 years or so, Reese built a large, powerful central research organization that Barksdale would have found highly objectionable. But by 1915 Barksdale was pushed out of the company's general manager position and replaced by Pierre du Pont, who had emerged as the leader of the faction of the du Pont family that gained controlling interest in the DuPont Company following a major schism in the family. Any immediate attempt to evaluate the return on investment in R&D by narrow financial criteria was soon abandoned. The R&D organization played a critical role in the company's diversification efforts in ways that have been discussed elsewhere.[19]

During the next 20 years, as DuPont's research organizations grew, some executives made occasional efforts to bring DuPont's R&D programs under the same ROI regime that the rest of the company operated by. But those efforts failed—or, I should say, cooler heads prevailed. In the late 1920s, DuPont's general managers—the heads of its diversified businesses who today would be called group vice presidents—began to compare notes about how each operating department determined how much to spend on R&D. To the amazement of Charles Stine, the head of DuPont's central research organization, there were essentially as many rules of thumb for allocating research as there were operating

[17]Irénée du Pont's report to the Executive Committee was submitted on December 23, 1910, and is discussed in Hounshell and Smith, *Science and Corporate Strategy*, pp. 49-50.

[18]These developments are discussed in Hounshell and Smith, *Science and Corporate Strategy,* pp. 54-55.

[19]The reorganization of DuPont's research programs and the diversification of the company as a whole are discussed in Hounshell and Smith, *Science and Corporate Strategy*, pp. 56-110.

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departments. One general manager allocated research expenditures on the basis of how many pounds of product his department made each year; another simply spent a certain percentage of his department's sales; yet another tied R&D spending to earnings; still another let instinct be his guide. How these research monies were actually spent within each department also varied widely. Some departments devoted most of their research expenditures to product research, others spent more on process improvement, while still others invested in more basic research.

Under Stine's leadership, both while he served as the head of the company's central research organization and after 1930 when he became the member of DuPont's Executive Committee who was in charge of the company's research portfolio, DuPont moved toward the standardization of R&D accounting across the company. It established five classifications of R&D expenditures (chemical control, improvements to existing processes and products, development of additions to established lines of product, development of new products or processes in entirely new fields, and fundamental research). Once implemented, this classification method allowed executives to see more clearly how the company was spending its R&D dollars, and it also improved the coordination of research across the company.[20]

The implementation in the early 1930s of standard accounting procedures for R&D expenditures soon led to a new round of thinking and debate within the Executive Committee about how much the company should be spending on R&D, now that it knew how and where it was spending R&D money. What was a good benchmark? Was it 3 percent of sales, or should it be 6 percent of earnings? Or should it somehow be tied to new investment in plant and equipment? These were tough questions, especially in the context of the Great Depression, when one-fourth of U.S. workers were unemployed. For a while, the company settled on tying R&D to sales, which it began to do in 1930. But soon members of the Executive Committee sensed that the general managers who reported to them, not on the operations of their respective departments but on their departmental profit-and-loss statements, were cutting back too much on R&D to keep their balance sheets attractive. Led by the brilliantly analytical Walter Carpenter, the Executive Committee changed its policy regarding how much it would spend on research, beginning in 1934—a revision that held until the mid-1960s. Rather than tying R&D expenditures to sales, DuPont would thereafter make decisions on research allocations based on the merits of the research itself—that is, on the research opportunities in the various domains covered by the company. Carpenter stated categorically that DuPont would fund any "well conceived" R&D project that "we are prepared and willing to undertake . . . with perseverance, enthusiasm, and ability."[21]

This policy echoed the earlier policy articulated by Pierre S. du Pont. Research was of such a nature that it could not be managed by the bean counters, who could not possibly capture in their numbers all the returns on investment possible in research. Opportunities in research must be judged by research managers; research budgets would be determined not by formula but by the opportunities for improvement in products and processes, for new products and processes, and for the production of new and useful knowledge. DuPont would fund any and all projects judged to have significant scientific, technical, and commercial merit. And so it was at DuPont for more than three decades. Whereas capital asset allocation had to clear DuPont's hurdle of 15 percent ROI, allocation of R&D monies had simply to meet the new criteria. Executives assumed that as long as a project remained interesting—as long as it

[20]See Hounshell and Smith, *Science and Corporate Strategy*, pp. 310-311, for a discussion of the standardization of research accounting methods across the company's business units.

[21]Walter S. Carpenter, Jr., "Outline of Talk . . . at Chemical Directors's Luncheon," December 7, 1934, as quoted in Hounshell and Smith, *Science and Corporate Strategy*, p. 314. The discussions and decision of the Executive Committee that gave rise to Carpenter's presentation are discussed on pp. 313-314.

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possessed technical merit—the company would support it, even though no direct benefits could be demonstrated or measured. The promise of primary benefits was sufficient, and certainly the secondary and tertiary benefits might even outweigh any of the primary ones. But these could not be measured accurately and would therefore not be measured at all. This policy is all the more remarkable when we recall that it was made during the Great Depression, when uncertainties about the company's future were perhaps at their highest in two decades.

SUMMARY AND CONCLUSIONS

This, then, was the DuPont formula for the allocation of R&D monies. This formula was derived by the same men who were absolutely hard-nosed about setting ROI hurdles when investing in capital equipment and plant and when making major acquisitions. These were the same men who built an enduring organizational structure that has had enormous global impact. These were the same men who not only helped to make DuPont one of the most profitable and successful corporations in U.S. history, but also helped to rebuild General Motors from its near-bankrupt state in 1920 into a phenomenally profitable company over a long period, using the same organizational structure and hard-nosed decision making as at DuPont.

The obvious question is this: Why did these men treat industrial R&D differently from other types of investment decisions and asset allocation steps? Why did they not apply the same rules? Why did they resist measuring the returns to investment in R&D in a formulaic manner? One answer might be that these men were incapable of devising the proper measures of R&D productivity. But I think not. They were brilliant executives who proved their analytical abilities many times over in all aspects of overseeing the largest firm in the U.S. chemical industry as well as the largest firm in automobile manufacturing.

Another answer might be that DuPont was so profitable and had so many assets that its executives did not need to worry about the firm's R&D spending. The historical record will not support this argument, because these executives worried about every penny the company spent. They watched the firm's assets carefully.

Yet another answer might be that the company spent so little on R&D relative to its sales and earnings that its executives did not waste time on measuring returns on R&D. Here, too, the record does not support such an argument, for the same reason as above.

To me at least, the most plausible explanation is simply that these executives understood research. They especially understood the high degree of uncertainty that accompanies long-range research. They understood that the further out on the horizon R&D moved, the greater the uncertainty in terms of specific, predictable results. But they also understood that even though the primary effects of research (that is, new and improved products and processes) might be difficult to predict and to measure, the returns on longer-term R&D were not limited to primary effects. Secondary effects (such as continuity of programs, improved capability for recruiting scientists and research engineers, and increased organizational capabilities) were perhaps more certain but by no means easily or accurately measured. Tertiary effects were also considerable but no less difficult to capture in a model or formula.

Most certainly, DuPont's executives knew that the returns on short-term, more precisely focused R&D investment were both more certain and more easily measured. But when the company moved its research objectives out on the horizon just a little bit, uncertainty grew rapidly. Further out on the horizon, the only certainty was that if the company did not do research, it could not remain competitive.

Some DuPont executives invested in long-term research and development as a matter of faith. Their experience with research supported their faith in research; that is, their belief in research inevitably paid

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off. Still other DuPont executives saw research as a form of gambling. Predicting in advance the exact payoff from any particular investment in R&D could not be done, but over a large number of investments the odds of winning any particular gamble (or investment cycle) could be established, at least roughly. And as long as the payoffs were sufficient over a large number of cycles to pay more than the wagers, and as long as the differential exceeded the returns from other forms of investment, DuPont's executives were willing to continue gambling on R&D.

Both the faith-based decision making and the probabilistic decision-making heuristics worked reasonably well as long as executives could judge the scientific and technical merits of the research they were funding. Only later, when executives lost the ability to evaluate the merits of proposed research or to judge research opportunities—or failed to develop suitable mechanisms to accurately appraise the merits of research—did the company run into serious trouble when evaluating its research programs. It was at this point in the company's history that executives sought supposedly "more sophisticated means" to anticipate the expected returns on investment in R&D. This was largely an act of desperation, for there was no real substitute for informed judgment.

The forces that today are driving both public and private investment in scientific and technical research to be justified by supposedly rigorous or "hard" analytical methods signal, I believe, a similar state of desperation. This desperation is borne out by a loss of faith in the inherent benefits of enlightened research and by the inability of policymakers to judge the merits of both proposed research and research opportunities more broadly, not to mention the capabilities of the researchers themselves. The nation is in for some rough times. Lying with numbers, cooking data or inventing numbers outright, and distorting programs to ensure that some arbitrary investment hurdle or public benefit criterion will be met will surely follow. DuPont went through such a period in its research history.[22]

DISCUSSION

Audience Member: In the current environment, with the discussion of the nation's largest companies cutting back on research and development, where is DuPont in the 1990s, and what is going to happen in the future with regard to R&D at DuPont?

David Hounshell: You will have to ask a DuPont spokesman about what is happening now. I can answer any question about DuPont's research until 1980. I'm a historian.

Audience Member: What about this perception that people have that big companies are cutting back because of foreign competition?

David Hounshell: With the end of the Cold War, a general malaise seems to be setting in with regard to R&D. There is intense debate about public investment in research. There is this question about the forces of globalization: To what degree is global competition driving incentives for investing in R&D down, lowering those incentives? These are very complex issues.

There are a number of non-U.S. based corporations as well as some U.S. corporations that have actually increased their R&D spending since the end of the Cold War. They have tended to move toward more fundamental research in some cases. So I would say that there is not a uniform pattern. We do have

[22]See the discussion of the financial projections models used as part of the New Venture Program of the 1960s at DuPont in Hounshell and Smith, *Science and Corporate Strategy*, pp. 509-540.

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a few notable examples where large, previously very successful central R&D laboratories, in terms of scientific and technical output, have been shut down as companies have been acquired. One example is the acquisition of Gulf in the 1980s by Chevron, where they closed one of the major petroleum research organizations. Another resulted from GE's purchase of RCA and the subsequent closing of RCA laboratories, as well as the impact of the deregulation of the telephone industry and Bell Laboratories. I am happy to report that I hear from people at Lucent that Bell Laboratories has never seen more commitment on the part of its owners toward long-term research.

All in all, I would say there is no uniform pattern. There is certainly a sense that it is not the way it used to be, and we know that is the case.

Robert Lichter, Camille and Henry Dreyfus Foundation: As a historian, could you say a bit more about your views on why the transition in the 1960s occurred? You described its occurrence, but it wasn't clear why the change took place then. What were the factors that contributed at that time?

David Hounshell: The factors go back to a misreading of a major period in DuPont's history that began in 1926 when DuPont moved to establish a new central research organization, a small fundamental research program, which in one month, March of 1930, led to the discovery of the first wholly synthetic rubber and of the first wholly synthetic fiber. The outcome of the first led to the commercialization of neoprene. The outcome of the second led to the discovery in 1930, and then the development and commercialization in 1940, of nylon. Nylon was an enormously successful product. It was the product of a fundamental research program that contributed in a major way to polymer chemistry.

Then, there were the very powerful technologies that came out of World War II. DuPont was involved in the development of one of them, the Manhattan Project and the atomic bomb. It was very clear in a publication in January 1939 that fission, the splitting of the nucleus, could lead to the development of a powerful weapon—the atomic bomb. This was achieved in 1945. This and the other technological leaps forward in World War II (radar, etc.) were very powerful confirmations of the power of basic research. DuPont saw these developments and in the postwar period invested very heavily in the expansion of its fundamental research program.

What the company lost sight of was that it would not have been possible to commercialize neoprene or nylon or to develop an atomic bomb without tremendous technical and organizational capabilities, and that merely investing in research would not be sufficient. For a period of 15 years, the company invested heavily in research, not thinking about the commercial aspects and the organizational aspects of what it was doing, and it began to see diminishing returns on its investment in research. In an act of desperation, in the 1960s it established what came to be known as the New Venture Program, in which it tried essentially to remove some commercial constraints and it wound up basically bankrupting the company. The company had never borrowed money previously, and the efforts that came out of the New Venture Program eventually led to the company having to borrow money for the first time. So, it was a failure to appreciate that research is important, but other factors—technical capabilities, market knowledge, marketing expertise, manufacturing expertise—were critical in the success of its own stellar products, neoprene and nylon.

Thomas Manuel, Air Products & Chemicals Inc.: To contribute to the point of cyclicality you brought up in your presentation on DuPont, I would like to note that they have a tremendous record and history, and that there have been some very interesting articles published recently by Joseph Miller and Perry Norling of DuPont, who describe cycles of productivity of generation and development and exploitation and so on, which have repeated throughout the history of the corporation. I forget the period.

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David Hounshell: Fourteen to 16 years.

Thomas Manuel: Yes, 14 to 16 years. In a sense that is reassuring because, like most sine waves, this suggests that investment in R&D will tend to turn up again. DuPont's history strongly suggests that.

David Hounshell: At least Joe Miller is hoping that is the case. He, of course, has reasons for finding that periodicity.

Vern W. Weekman, Mobil R&D Corporation: Did DuPont use any type of a rolling average over a 5- or 10- or 15-year period of the return on the basic research programs in the Experimental Station? If they did, did this influence their faith in long-term research?

David Hounshell: In the 1920s, some of the executives, who were keen on the return on investment calculations, wanted to do this. There was extensive debate in DuPont's management, which I've only glossed over here, on how to measure these returns. There were DuPont managers who derived models that made use of rolling averages, smoothing functions, and so on. These individuals were very sophisticated with their analytical methods, and they tried to use these techniques to place the ROI from basic research on a firm footing. This was finally brought to a head in 1934, when both Stein and Carpenter concluded that none of these models works. The models simply could not capture all of the possible returns on basic research, particularly the secondary and tertiary returns. Stein and Carpenter decreed that DuPont was not going to use any of the proposed ROI methods for basic research.

There was another factor behind this decision. As the DuPont executives gained increased experience with their ROI measures, they realized that, although these measures led to better informed decision making, their decisions were not based solely on the ROI calculations. There were many times when they made investment decisions where the ex ante ROI calculations were 8 and 10 percent rather than 15 percent. So the ROI calculations were merely advisory. They were no substitute whatsoever for judgment. That is what was critical. The company's executives, Carpenter in particular, believed that as long as the top executives were knowledgeable and talented enough to know both the scientific and THE commercial merits of an enterprise, they would make the right decisions.

James Fry, University of Toledo: In the state of Ohio, the Science and Technology Council, which advises the governor, is making a serious attempt to demonstrate that science and technology development have an impact on society and economic development. Is there any message from the studies you've done that would suggest there is a right way to approach the public on these matters, as opposed to an incorrect way?

David Hounshell: Yes, I think there is a better way. One can look at the West, the history of the West from the 18th century forward, to see that those nations that have been committed to enlightenment—that is, to the advancement of human understanding about the world and about humans themselves through systematic research—have done better on average than those societies and nations that have not done so. I take it as a matter of enlightened faith. I realize that one cannot base all policy on faith, but I would submit to you again that any set of calculations that try to predict the return on investment for research, especially fundamental research, is subject to daunting problems. As soon as the measures go into effect, people will start "cooking" numbers. The result will not be better ROIs, simply better numbers.

We need to look more intensely at our own history in Western society, recognize the enormous

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benefits that we have gained through scientific and technical change, and identify the sources of that change. Much of the change has come about through research and development. One cannot understand the history of the 20th century without understanding the development of industrial research and development laboratories, and the heavy commitment by corporations to scientific and technical change. If we look at the 1993 science indicators, we find that corporations spent 68 percent of the nation's R&D dollars. They employ the bulk of scientists and engineers in the United States. These numbers are very consistent with other trends in the 20th century. We reaped tremendous benefits from these investments. Unfortunately, I cannot demonstrate it rigorously, quantitatively.

Andrew Kaldor, Exxon Research and Development Corp.: My understanding is that somewhere in the 1900s, the accounting principles for R&D changed. Early in the century, R&D was considered to be part of the capital investment made by the company, but later it was considered to be expense.

David Hounshell: That is correct.

Andrew Kaldor: I wonder whether this is a fundamental problem facing the R&D enterprise in industry. In industry, we trade in options. It's a perfectly acceptable legitimate business principle, yet I don't see the options concept, and how you manage it, applied to research. Did DuPont, or some other company, investigate this approach?

David Hounshell: Pierre du Pont explored the options concept at the outset. As early as 1902, when the Eastern Laboratory was established and the Executive Committee was created essentially to oversee DuPont, Pierre strove to obtain a better accounting for research. One of the things that led to the establishment of Donaldson Brown's ROI formulation was the work that Pierre had done on accounting, which was very different than the accounting procedures used by the railroads. In particular, he worked on capital accounts as opposed to expensing or cost accounting.

Pierre initially thought that you could place R&D funding entirely under a capital account, treating it in the short term as an expense account and then readily converting it to a capital account. He investigated this concept intensely between 1902 and 1904. By 1904, he had abandoned this concept entirely. He just didn't think that it worked. He was not satisfied with an expense method either. That is why he wanted to treat it very differently than the other expenses incurred by DuPont. Certainly, in the overall ROI calculations of the company, it was treated as an expense, except where it had generated identifiable intellectual property that could be assigned value—patents, for example. Patents were handled on the capital account, on the capital side of the ledger rather than on the expense side.

Andrew Kaldor: I wonder if you can confirm another piece of DuPont folklore or history, I don't know which. At a recent meeting in San Francisco, someone was talking about DuPont's retrospective study of their technology. They apparently have a list every year of their top 10 technologies and the bottom 10. They have followed these technologies over a 15-year period to see how they fared. The result was remarkable! As I remember, 8 of the top 10 technologies failed, and roughly the same number of the bottom 10 succeeded. This certainly raises some questions.

David Hounshell: Yes, this is essentially correct. I call your attention to the dissertation that Joan Adams is working on. She is doing a number of case studies on polymer innovations. She's studying several cases from DuPont, but many other cases in the chemical industry as a whole. You might want to talk to her. She is much more aware of what's taking place at DuPont today than I am.

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2

The Sources of Commercial Technological Innovation

Don E. Kash
George Mason University

The debate over the role of research in the innovation of commercial technologies has demonstrated one thing. As a general rule, it is very difficult to identify and prove the contribution of research, and particularly fundamental research, to commercial innovation. Common sense tells us that the role of research vis-à-vis commercial technological innovation varies greatly from one area of technology to another. It also varies greatly over the lifetimes of particular technologies. What seems clear is that research is central to the innovation of some technologies—for example, pharmaceuticals, chemicals, and biotechnologies. It has marginal value in the innovation of other technologies.

In seeking insight into the processes that deliver innovation, I found it useful to divide technologies into two groups: those that are simple and those that are complex. A simple technology can be understood in full detail by an individual expert sufficiently well, so that that individual can communicate all of the details of the process or product across time and distance to other experts. Alternatively, a complex technology is one where that kind of understanding and communication is not possible.

Economic rewards from the innovation of simple technologies commonly flow from the ability to gain legal monopoly protection for the intellectual property involved. This is a pattern one sees manifested in the pharmaceutical and the chemical industries. In addition, simple technologies are commonly derived from research.

Complex technologies manifest a different pattern. Economic benefits flow much more heavily from the ability to carry out repeated incremental innovations. It is difficult and sometimes simply not possible to use patents to protect complex technologies. The route to economic payoff is to incrementally enhance the complex technologies ahead of or in parallel with one's competitors. This is a pattern manifested in technologies ranging from computers and telecommunications technologies to corn planters.

For very large numbers of complex technological innovations, the capacity to carry out systems integration is critical to successful innovation. My colleagues and I like to characterize this innovation process as involving synthesis—that is, complex technologies appear to benefit primarily from the ability to put diverse components and subsystems together and obtain a synergistic result. The technol-

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ogy that comes out of this synthesis ordinarily has either performance or quality advantages or reduced production costs; under the best of circumstances, the innovation produces all three.

A key point with regard to complex technologies is that there is no capacity for understanding them in detail. Certainly there is no such understanding of how the innovation of complex technologies occurs. Complex technologies are largely the result of trial and error. They build substantially on an accumulation of knowledge within an industry or a technological area, and they benefit especially from organizational arrangements with established organizational routines and heuristics that inform and guide the process of incremental innovation.

It is generally true that incremental innovations of complex technologies involve a process of learning by organizational networks. The way the organizations interact in the networks is at least as important as the research, and in many instances a good bit more so.

To set this in context it is useful to use a series of trajectories to illustrate what commonly occurs with regard to complex technologies. Complex technologies tend to be launched either by what are called radical innovations—that is, innovations that are first of a kind—or by what we call trajectory transitions. A trajectory transition occurs when the basic design or the technological platform at the center of a continuing series of incremental innovations changes fundamentally.

In Figure 2.1 are three S-curves defining the trajectory of audio technologies. The bottom left curve indicates the audio technology that started with Edison's cylinder and then moved incrementally to the

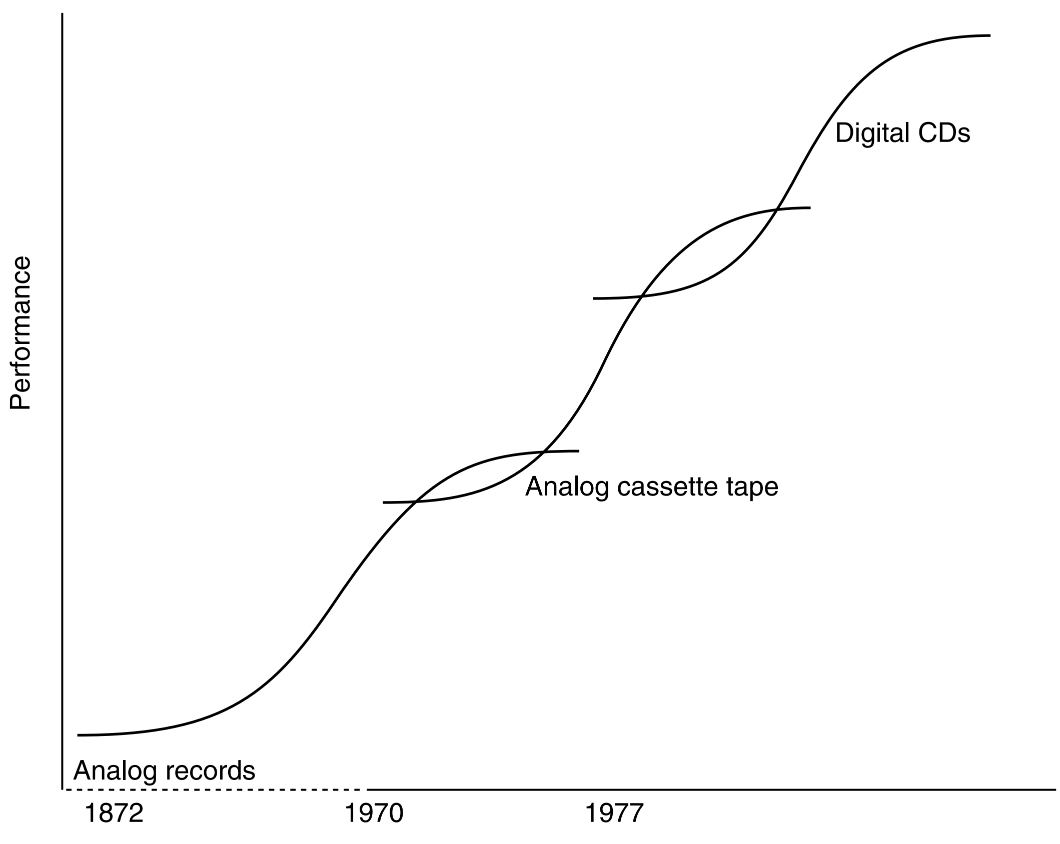

FIGURE 2.1 Trajectory of audio technologies.

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long-playing record discs that we know. The second S-curve resulted from a trajectory transition triggered by the arrival of analog magnetic tapes. The third trajectory is the arrival of compact digital discs.

It is commonly recognized that in the case of complex technologies, being the innovator that produces a new trajectory is not necessarily the same as being the winner in the economic competition. What is quite clear is that if you are first and you continue to incrementally innovate ahead of your competitors, then you do well. But if you are first and you stop being innovative, you don't do well at all.

This distinction between simple and complex technologies is particularly important as we think about the future of commercial innovation. The importance of complex technologies in the economic marketplace is accelerating rapidly. Simple technologies represent a declining percentage of the value of exports. Complex technologies, on the other hand, are becoming more and more important.

There are two trends at work. More and more of our commercial technologies are becoming complex. In addition, those that are already complex are becoming more so. Recall that success in the incremental part of the innovation of complex technologies comes from the ability to carry out synthesis.

Next I will show what happens if we use this simple-versus-complex categorical distinction to look at exports. In Figures 2.2 and 2.3, we took the 30 most valuable goods exported worldwide and the 30 most valuable manufactured goods exported over the last 25 years and classified them by type of product and the type of process used to produce it (Figure 2.2). We classified each of these exports and the process used to produce it as either simple or complex. This has been done by asking those who were experts with regard to these technologies how they would classify each using our rule-of-thumb distinction. What we essentially have are four categories of technologies into which we could classify the 30 most valuable goods exported and the 30 most valuable manufactured goods exported over the last quarter-century. Those categories are simple/simple, simple/complex, complex/simple (that is complex process, simple product), and complex/complex (Figure 2.4). I might note for you in this connection that the 30 most valuable goods exported represent nearly half of the total value of all exports in both 1970 and 1995—specifically, 48 percent in 1970 and 46 percent in 1995.

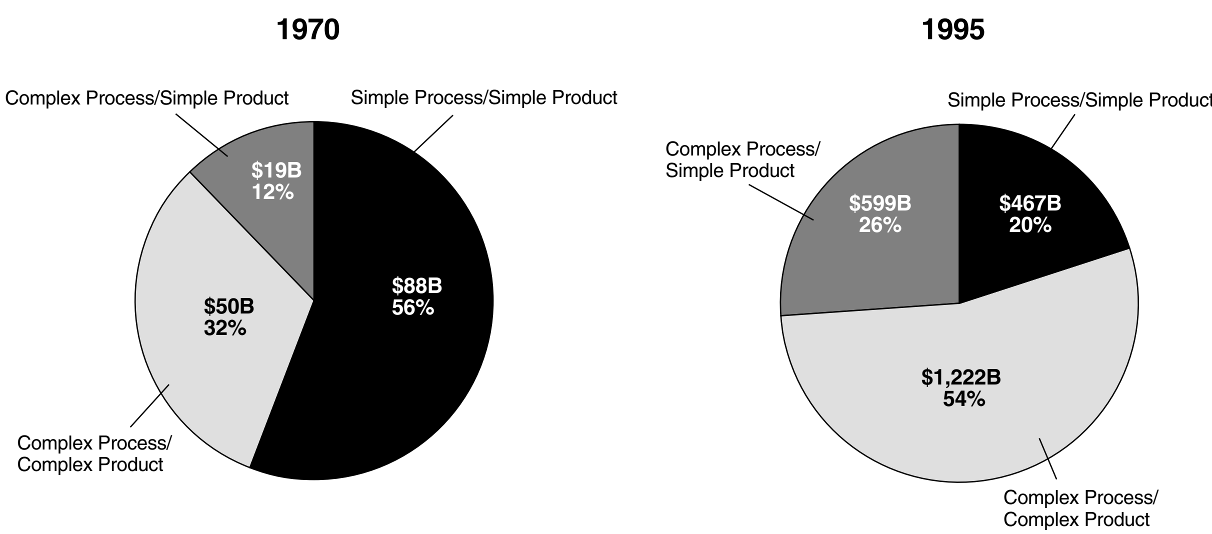

FIGURE 2.2 Thirty most valuable goods exported.

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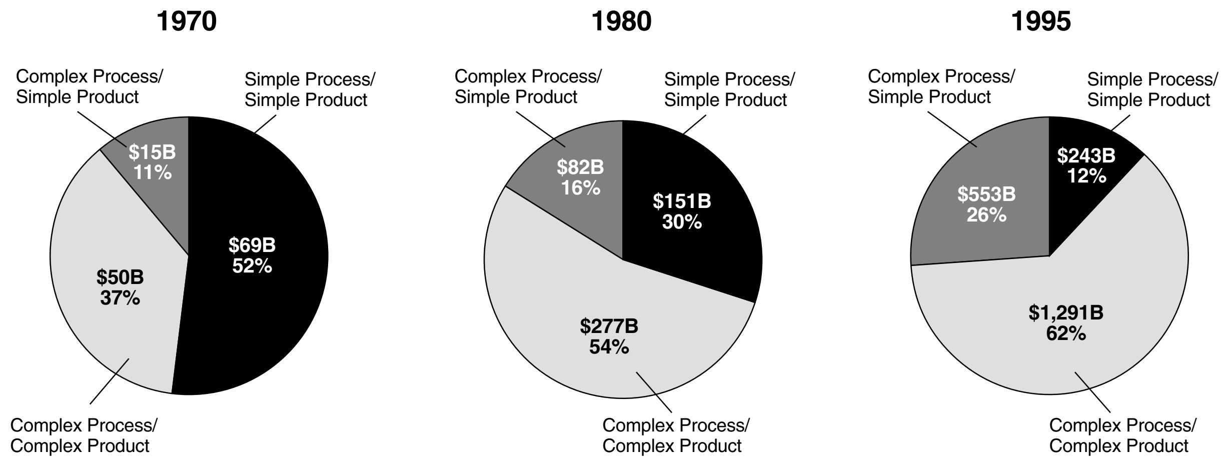

FIGURE 2.3 The 30 top manufactured goods exported.

Simple Process/Simple Product	Simple Process/Complex Product
Complex Process/Simple Product	Complex Process/Complex Product

FIGURE 2.4 Four categories of technology for classification of high-value exported goods.

In 1970 the simple/simple group represented 56 percent of the value of the 30 products and the complex/complex group represented 32 percent. By 1995 the complex/complex category included 54 percent of the total value of the top 30 product exports, while the simple/simple category declined to 20 percent. From this it is clear that those who want to be where the money is in the future want to be in complex/complex technologies. The fact of course is that over the last 25 years simple technologies have become complex technologies.

If non-manufactured exports are taken out of the picture and only manufactured exports are examined, what is being taken out is crude oil, we see a similar pattern. That is, even with regard to manufactured goods in 1970, 52 percent of the top 30 manufactured goods exported were in the simple/simple category (see Figure 2.3). By 1980 that category had dropped to 30 percent, while the complex/complex category had grown from 37 percent to 54 percent. By 1995, the simple/simple group of manufactured goods made up 12 percent of the market, and the complex/complex group represented 62 percent.

Again, if we look to the future, it is the capacity to innovate complex technologies (particularly the capacity to successfully carry out incremental innovations) that is the key to economic success.

The next question is, How is the United States doing if we apply these categories? What we find is peculiar. It is an anomaly and is inconsistent with almost everything else in the literature. If we look at

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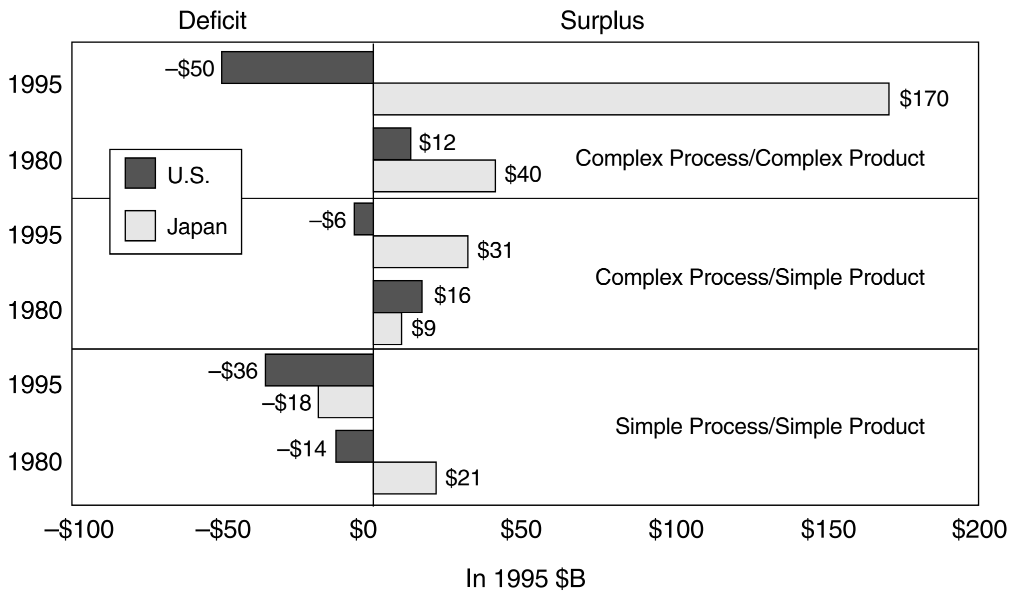

FIGURE 2.5 The U.S. and Japanese trade balances, 1995.

the trade surplus or deficit of the United States using these three categories and we do the same thing for Japan, what we find is that in 1980 the United States had a $12 billion trade surplus in complex/complex technologies. Japan had a $40 billion surplus. If we look at the situation in 1995, what we find is that the United States had a $50 billion deficit and Japan had a $170 billion surplus (Figure 2.5).

It is important to note that this is a period in which the value of the dollar vis-à-vis the yen has decreased by roughly 50 percent. In the 1990s the United States has been in economic expansion and Japan has been in what is termed a recession. In truth, by almost every measure the United States is currently booming, while Japan is in the doldrums. Yet if we break out the top 30 technology product exports for the world and compare the performance of the United States and Japan using balance-of-payments data, this pattern is a particularly striking anomaly (Table 2.1). In truth, no matter how we look at it, the United States does not do very well in this complex/complex category.

Table 2.1 lists the product categories in the U.N. trade data. The largest category is automobiles, in which the United States runs huge trade deficits. This deficit remains very large despite the recovery of the U.S. auto industry. Japan runs a deficit in a single category, aircraft manufacturing.

I conclude by saying that we cannot explain the above findings by any study of relative research and development efforts in the two countries. It has to be explained in other ways. I think very clearly, one of the most important of those categories is clearly the nature of organizational relationships.

DISCUSSION

Richard K. Koehn, University of Utah: I'm a bit worried about what you've just said. Although the conclusions appear to be fine, they depend on this distinction between simple and complex. What got me worrying about this is that you classified drugs and the pharmaceutical industry as simple. Related to this are two major points that you did not mention but which I believe are critical. One is the degree to

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TABLE 2.1 Balance of Payments for Complex/Complex Technologies, United States and Japan, 1995

Product Category	United States	Japan
781 Pass Motor Veh excl. Buses	–49,327,745	31,689,249
776 Transistors, Valves, etc.	–5,245,303	28,586,562
752 Automatic Data Process Equip	–12,345,386	6,733,480
784 Motor Veh Parts, Acc NES	2,660,637	18,201,864
764 Telecom Equip Pts, Acc NES	–805,052	12,398,541
759 Office ADP Mach, Pts, Acc NES	–6,185,914	11,044,216
778 Electrical Machinery NES	–1,190,106	11,443,713
792 Aircraft, etc.	19,225,396	–2,768,035
728 Other mach. for Spcl Indus	2,688,104	9,550,854
713 Internal Combus Piston Engine	–2,603,547	13,119,205
874 Measuring Control. Equip	6,139,574	4,237,599
782 Lorries	–4,874,498	9,615,997
793 Ships & Boats, etc.	1,243,569	10,932,151
744 Mechanical Handling Equip	424,584	4,850,386
Subtotal	–50,195,597	169,627,782

SOURCE: United Nations, *International Trade Statistics Yearbook, Vol. II: Trade by Commodity,* Commodity Matrix Tables (New York: United Nations, 1996).

which an industrial sector is regulated, and the other is the rate of innovation, which you characterize as a trial-and-error process.

The reason you don't need a patent in the computer chip industry is because the product cycle is only 18 months and the company will be out of the market long before the patent is ever issued. So, a trade secret is used to control the information, and economic success is dependent on market penetration. You could do the same thing in the drug industry if the government didn't prevent you from killing people while you were carrying out the trial-and-error studies. But, of course, it does. So this industrial sector depends on patents because patenting drugs is the only way to recoup the $300 million investment in developing the drug. If a company could not obtain that monopoly, there would be no drug innovation, because there would be no economic benefit resulting from development of a drug.

The above seems to go well beyond your classification of simple and complex, yet it seems to be critical to the conclusions that you have drawn.

Don E. Kash: I don't disagree with anything that you have said. A proper classification system is very difficult to develop. What seemed to us to be the case is that there were very different patterns of innovation that occurred as we studied one technology area and then another. What we were looking for was a classification system that did not require us to consider every technology independently, while nonetheless ensuring that we did not discuss all technology as if it were the same. I would be interested in any suggestions as to how you might do it differently.

Richard K. Koehn: First of all, I would do it industrial sector by industrial sector.

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Don E. Kash: Categorizing technologies by industrial sector can be difficult also. One year when we were looking at the data, the fastest growing category was miscellaneous. Why was that? As technologies progress from being mechanically based to electromechanically based to electrooptical-mechanically based, knowing how to classify them becomes exceptionally difficult. So, the people who are collecting data at ports and filling out surveys in companies look for the right place to put them, and there is never an appropriate category. For example, is software a sector?

Richard K. Koehn: Yes.

Don E. Kash: I can tell you that nobody collects data on software, except the software industry. We have very good data on packaged software, but people in the software industry keep telling me that there is all kinds of software embedded in hardware. My problem with your sector-by-sector definition is that the sectors are continually undergoing metamorphosis, and that makes the data very difficult to track.

Richard C. Alkire, University of Illinois at Urbana-Champaign: My question is in the nesting of S-shaped curves in Figure 2.1 on the development of audio technology. You made the point that research is very important at the initiation of a new curve. I wonder if you could consider scientific and engineering research separately, where engineering research determines whether the curve has an S shape or is a constant.

Don E. Kash: First, in terms of asking companies questions about research, research was whatever the people in the company said it was. With this approach research can mean many different things. Second, much of what engineers do, which is terribly important in the innovation process, is not research and is not classified as research. It involves design, experimentation, and problem solving, which are never classified as research.

I think this is a problem. I would expect this group to be very much like I am. Anything that contributes to innovation is research where innovation is, by definition, the first introduction to the market. Many people who are very sensitive about defending the role of research have a tendency to lump this type of engineering into the research category. Research is, if you look over time, one of those wonderfully expandable categories. For example, in a company, it shrinks and expands, in part, based upon tax definitions, but it also shrinks and expands depending on what you're trying to defend.

Paul Anderson, DuPont-Merck Pharmaceuticals: I too was wondering what your definitions were for complex versus simple. I certainly would agree with you that if you look at a product from the pharmaceutical industry, then it's simple. But if you look at the process for getting to it, then it's complex/complex, because it requires at least eight different disciplines working together as a team over a long period of time (10 years is normal), even though very smart people are involved. In some of these areas it may help to look at the product versus the process to get to the product, when you classify the technology.

Don E. Kash: We put the pharmaceutical industry in the complex process, simple product category. We put most of the chemical products in that same category, unless you go back to 1970. In 1970, an isopropyl alcohol plant tended to be run by people who turned valves on and off. Now the same plant is full of computers and sensors and controls and has become a very complex operation.

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Thomas A. Manuel, Air Products and Chemicals Inc.: I think we may be getting into a semantic bog, and I have to apologize for taking us one step further, but I shared the groans about research being everything that led from the idea to the commercial product. Research is usually a close bedfellow of development, which tends to include much of the engineering community. I find it helpful to split them, because we start with science and we wind up with statistics on technologies. I would submit that, as a practical matter, there's something in between the science becoming a technology, which is that exercise we call development. That's certainly true in the chemical industry.

To make one further comment in a different direction: I love S-curves, and I cook enough numbers to get them for performance. But it is important to realize that, if you have the same time axis, the slope of performance improvement is not the same as the slope of perceived value, which in turn means that the product life cycle doesn't have the same slope either. That's why in the chemical industry, our products have very long life cycles. If you look at the 50 top industrial chemicals of 1996, you will find a very great similarity to those of 1966, 1976, or some other time period.

I'm a believer in some sort of sorting by sectors as well; otherwise there are no landmarks.

Don E. Kash: Well, I don't really disagree with you at all on sorting by sectors. What we did in accumulating the numbers was to characterize each first as a sector. Then the effort was to try to collapse those numbers into meaningful units given the changes that are always taking place.

When it comes to making a distinction between research and some other types of activities, I repeat that, for us, research is whatever the people in the industry tell us it is. It is perfectly feasible to have a definition of research—the federal government has a number of very precise definitions of research. People in industry and other areas make great distinctions between types of research: engineering research, scientific research, etc. However, the line between engineering research and design is frequently a very tough line to draw.

Jack Halpern, University of Chicago: The point that Dr. Manuel made about the top products in the chemical industry being substantially the same, or very similar to what they were 50 or more years ago, has an important corollary with respect to the changing role and value of research in a given sector or industry with time. What happens with respect to any given product with time is that it improves, or the processes by which it's made improve, and you reach a point at which further innovation with respect to that product or that process has diminishing returns. When a chemical costs $1.00 a pound, there's lots of room for realizing substantial returns if you can drop that cost to 50 cents. When it gets down to 5 cents a pound, even if you could cut that in half, you're only saving pennies. So the investment in developing further improvements declines.

One consequence that we're seeing in the chemical industry is a shifting to non-chemical fields that are at an earlier stage of their technology. Monsanto is an example of this. Monsanto, which was one of the major chemical companies, went out of the chemical business in recent years as a result of this. Monsanto is now a biotechnology company.

There are products that are virtually impossible to improve upon. A friend of mine in the pigment industry, which is a large industry, points out to me that there are certain pigments (certain colors, that is) that are so good that it's absurd to think of investing in further research leading to improvements of the kind that sustained much of the chemical industry in the last century and that still goes on with respect to some other pigments and products. This is a point that's particularly important for this group.

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Don E. Kash: Let me underscore one tentative conclusion that we have drawn. With regard to many of these complex technologies, the organizational arrangements that make it possible to access and integrate knowledge and expertise in diverse areas are as important as research. The ability to take a 35-millimeter camera and convert it into an electrooptical-mechanical system and enhance it on a continuing basis is obviously dependent on research. But if you look back into the process, it is this capacity to do the systems integration, to put things together, that leads to commercial success. I must confess to you that I've now been doing this for several years, and one of my standard games is to go around and ask people in industry to tell me what systems integration is. The reply is: Systems integration is what we do when we put different things together.

Judith C. Giordan, International Flavors and Fragrances: I would like to make two points. When you take a look at the S-curves, is it just a result of scientific and engineering research? Now more than ever before there's a third component to research, and that is market research. Market research played far less of a role 40 or 50 years ago than it does now. When one debates the definition of research, one has to take this element into account.

The other point was Dr. Manuel's comment about semantics, with which I agree completely. We don't want to get lost in that morass. By asking anyone in the organization to use whatever definition they want for research, as well as asking anyone in the organization the value of said research, I suspect that you can obtain widely different views of research and its value.

Don E. Kash: Well, I can assure you that what your answer is depends on whom you ask. However, I have talked to no company that includes market research in their research budget. That is, when I ask people what the contribution of research is, I've never had anyone identify market research. Of course, for most of the complex technologies that I discussed, market research is very important—it's a major input to the innovation process. But I am sure that it doesn't show up in DuPont's research budget.

Roland Hirsch, U.S. Department of Energy: I would like to go back to an anecdote that I think illustrates the dilemma that is at the heart of the issue we're discussing here. Although a Toyota executive is said to have stated that Toyota really doesn't do research, I would submit that Toyota's Model T would probably look a lot like Ford's original Model T if it weren't for research in all sorts of areas, fundamental and applied, and that in fact Toyota is probably paying as much attention to research now being done in a variety of industries and academic laboratories as it ever has. Otherwise, it won't be around 10 years from now.

Don E. Kash: Well, the first point I should make about that anecdote is to tell you that that comment was made about 10 years ago. Toyota, in fact, spends a lot of money under the category of research now. So there's been a real change in the company as it has prospered and come of age. Toyota believes, as best as I can tell, that research needs to be done today.

But I again repeat my cautionary note: One of the biggest problems that people get into when they try to make this linkage between research and commercial product and process payoff is claiming too much for research. What happens, if you're not very careful, is what happened to DuPont in the 1960s (see Professor Hounshell's paper in this volume). If you're not conscious of the process whereby production, marketing, and all of these other factors are major inputs into the innovation process, you can get into deep trouble. People who have responsibility for research have—regularly and consistently over time—made claims that I simply do not believe can be supported.

However, I must also make the point on which there is general agreement, and that is, I haven't

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found anyone who doesn't think research is critical. In no small part, it's what you bet on to be able to respond to the unknown. But with regard to complex technologies, there are other inputs that are important that are not research inputs. They're primarily organizational.

Michael P. Doyle, Research Corporation: I submit that the problem that you're dealing with may be one that is limited by the data available. The data that you have that could speak to the issue may not, in fact, be a reliable measure of what you're trying to measure.

Let me give you one point in fact. A 1970 study was produced by the Illinois Institute of Technology. This was a study commissioned by the National Science Foundation, and its purpose was to trace the origin of critical technologies. They picked out four technologies, two of which were transistors and birth control pills, and traced the fundamental development of each technology over a period of 100 years or so, compiling a list of the individual discoveries involved. This study clearly shows that research was a critical feature of the development of these technologies—that certain elements could have not been developed were it not for discoveries that were made in that period of time, both in terms of fundamental scientific discovery and in terms of discoveries that brought those materials to the marketplace.

I submit that, were we to do such a study now, we would arrive at similar conclusions. In fact, a few years ago the Board on Chemical Sciences and Technology traced this same path for a set of critical technologies (although in a less direct way) and concluded that R&D, either as fundamental developments or as the basis for bringing those technologies to the marketplace, was fundamental to the process. So I wonder, why is there such difficulty in identifying those activities as being critical? I think it is the limitations in the data available to provide those types of measures rather than the realistic system in which we all work.

Don E. Kash: Well, I start with the assumption that you cannot find data that will convince everyone. One of the reasons that you have great difficulty is because this whole discussion is embedded in ideology. You, of course, know that TRACES was study number two. It was preceded by HINDSIGHT. In fact, some people believe that TRACES was a direct result of HINDSIGHT. HINDSIGHT was an attempt by the Department of Defense to define the role that its fundamental research funding had made to weapon systems. It wasn't very successful. The National Science Foundation, which has some interest in defending fundamental research, got the Illinois Institute of Technology to do TRACES. TRACES found that there was a relationship.

If you operate in this town, is there a message? I suggest to you that that is what's important here. I don't think there is any argument about the importance of research. What we're dealing with is a set of circumstances where people are trying to force the agencies (and very frequently, corporations) to demonstrate a cause-and-effect quantitative linkage between research and commercial products. It seems to me that Professor Hounshell's point is the key one. If you try to do that, you immediately get yourself into trouble, and you start arguing over the process.

There's a more important issue as far as I'm concerned. Innovation involves important inputs beyond research. The most important one of these today, I believe, is organizational flexibility. With regard to complex technologies, I would, for example, eliminate all antitrust regulations, because they're barriers to information flow. I think that simple act would be an important contribution to the innovation process.

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Panel Discussion: Introductory Session

Andrew Kaldor, Exxon Research and Development Corp.: Actually I have a question and a comment. I really like your definition of simple technology because it's workable. One of the problems in the industrial sector is that management systems are not rewarding the complex innovation system participants to the same degree as they do the single expert. That is a cultural problem that the U.S. industry in particular has a problem with.

I wanted to tell you about a study that we have done—but unfortunately it hasn't yet been released so I can only outline it for you—of a dynamic model for major innovations that we did collaboratively with nine other companies. It's a dynamic business model that was constructed to measure the performance of each company and integrate the results into a single model. Out of this study, six drivers emerged, and their impact is phenomenal in terms of the success rate of major industries. Some of them are the following:

- Interspersing business and technology—This includes the marketing aspect. You say that marketing doesn't show up in the R&D budget; I assure you that any economic assessment of a potential technology is in the R&D budget.
- Multiple approaches—This captures the notion that, if you're trying to develop something significantly new, making an early selection of the approach to use is absurd. You really have to explore parallel paths for a while.
- Constancy of purpose—Once you make a commitment, you must stay with it long enough to give it a real chance to succeed. The science base has got to be very strong, and it has to continue to grow.
- Extremely aggressive goals—Goals that you don't know how to achieve. This draws on concepts like integrated multidisciplinary teams, skunk works, and so on.

What is fascinating is the comparison of the performance of the dynamic model to the traditional approach. The dynamic model yields a 25-fold increase in major innovations. Furthermore, in the first 5 years there is often little indication that it is working. There is essentially no output during this period, although I am sure that this number will change/improve as we gain more experience with the model.

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Another interesting feature of the dynamic model is that your ability to make incremental improvements is dramatically improved (as much as 100-fold) compared with investing in short-term research.

My question is: Is anybody else working on dynamic models like this? Do you think it's useful?

Don E. Kash: Yes, there are people working on such models. There is an exponentially increasing community of people who are studying complex systems, and the model you describe is roughly consistent with the themes that are being investigated. I have a 60-page bibliography, if you are interested.

David A. Hounshell: I have been studying the Rand Corporation in Santa Monica as one of the first instances of a think tank, a model for many other nonprofit research organizations that have proliferated since 1945. In 1954, Rand undertook a formal research project on the economics of R&D. By 1956 or 1957, the group, which was led by economist Burton Klein and included other well-known economists such as Richard Nelson and Kenneth Arrow, reached essentially five of your six points, drawing the same conclusions. Since they were focused heavily on military R&D, the only conclusion your group reached that they did not was the importance of the interaction between business and technology. The five other factors were there in their conclusions in 1956-1957.

Eric C. Beckman, University of Pittsburgh: In Professor Hounshell's presentation he noted that DuPont in the 1930s and 1940s had an instinct that research was good. They couldn't put a number on it, but their instinct was that it was important to the company. Somewhere along the line their instincts changed, as represented by the recent spate of downsizing in large chemical R&D organizations, including DuPont. Can you maybe describe how this happened, and will it reverse itself'? If we agree that research is good, then how do we reconcile this statement with the current loss of research jobs? Is it just communication?

David A. Hounshell: In the post-World War II period, DuPont had too many resources, or at least it allocated its resources in the wrong way vis-à-vis research. Taking the path they did, they substantially weakened the connection between research and the business units. By making such a heavy commitment to basic research, the central research unit lost focus and had no support in the business units for any new developments that they might make. The scientists in central research generated many good ideas and did excellent academic-style research, but there was no mechanism for nurturing their discoveries to a commercial product. Because of their experiences in World War II, fueled by the Cold War and the massive government infusion of research funds, they simply overinvested in research.

In 1959 Richard Nelson published what is now the fundamental paper in the economics of innovation, "Simple Economics of Basic Scientific Research." This paper was partially a response to the crisis that had developed with the launching of Sputnik in 1957. Nelson made a statement in that paper that has become an economic truism—among economists, anyway—that firms, because they cannot capture the full benefits of their investment in basic research, will underinvest in research. So the nation overall will systematically underinvest in research.

What he was arguing was very consistent with what the scientific community was saying in response to Sputnik in 1957: namely, that the reason for the missile gap was that the nation had not invested enough in basic research. We had moved away from basic research. Nelson explained in simple economic terms why the nation had systematically underinvested in basic research. The conclusion is obvious: the public sector needs to make up this difference, because there's a different incentive—it's the public good, social welfare.

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If you read one of the footnotes in his paper though, he says that economic theory suggests that there should be overinvestment in basic research but notes that the evidence of Sputnik suggests the contrary. In fact, if you look at economic theory, you can see there's a complexity there—there may be too much investment in basic research and not enough in downstream research.

In short, I am saying there are alternative reasons behind your observation that we once invested heavily in research, and, because of changing circumstances, we're not investing enough now and need to go back to our earlier practices. The jury really is still out on that. There is the possibility that the United States actually invested too much in research. I'm not advocating that position; I'm just noting that it's theoretically possible.

Don E. Kash: There are a couple of points I would like to make. First, if you look at research expenditures by industry, the numbers are increasing rapidly. There may be a downsizing, but that is an organizational construct. It is at least reasonable to speculate that part of the downsizing is associated with moving research into fabrication facilities, production facilities, and so on. In no small part, that is tied up with the fact that, if you make very large investments in long-range research in these complex areas, you repeatedly find instances such as the Japanese commitment to analog high definition TV (HDTV). The whole enterprise can succumb to an invisible enemy, in this case digital electronics. In many of these complex industries, there are powerful reasons for not trying to go too far down the road. That's one of the manifestations of complexity.

Andrew J. Lovinger, National Science Foundation: I'm trying to draw common threads between the talks of our two speakers. One thing that stands out is that industry did not question the value of research when things were going well. For DuPont, for example, this was the 1930s, 1940s, 1950s, when it was developing nylon, Teflon, polyesters, and so on. The golden age of Bell Laboratories was when AT&T was a monopoly in telecommunications.

Now, in Japan, we see that investment in research and development is skyrocketing precisely because Japan has a very favorable balance of trade in terms of complex/complex technologies. Are there examples and precedents when either times were bad or companies were in economic sectors where the outlook was not so rosy, where they did not question the value of R&D? Where they did not try to establish metrics and evaluate the risk they took strictly? Where they took that risk and prevailed and were able to demonstrate to people at the time that it is worth taking that risk, even in adverse circumstances? That would be useful to all of us as we plan in the present environment.

Don E. Kash: I've not made any systematic look at that in the United States. What I can tell you is that in Japan research expenditures until very recently were not affected by how well the companies were doing. I remember one time interviewing at Hitachi when their profits—and I never know what "profits" means in Japan—had been cut in half in one year. I asked the president of Hitachi if they were going to cut R&D and he said, oh no, that's critical to my profits going up in the future.

There is another very consistent pattern. If you want high-level R&D performance that disregards what's going on with the economy, you want to invest your money in companies that are 10 percent owned by a single family, and where that family is involved in the management. These are cases where you get high performance with regard to R&D. We pay little attention, particularly if the companies have people like Pierre du Pont.

David A. Hounshell: At the coffee break, one of the things I talked about that I had not mentioned this morning is a very important trend that is in part driving shorter-term outlook in industry vis-à-vis R&D, although again not consistently. This is the drive for better performance on the part of institutional

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investors, such as my own TIAA-CREF retirement fund as well as many other large pension finds. These funds have contributed to much more emphasis on quarterly earnings and performance reports. To some extent, this has undermined longer-term commitments. How you rectify the situation, I don't know.

Robert L. Lichter, Camille and Henry Dreyfus Foundation: I come at this whole question from maybe a somewhat different perspective, because, as we all do, we approach these questions from our own experience and backgrounds. For me, the question that was brought up earlier that was alluded to as being strictly semantic—what we mean by research—is really at the heart of the issue. It's more than just semantic. We're talking about a type of behavior. We've used the expression "research," and we've used the expression "R&D" as if they're synonymous. As we know, they're not. When we talk about 68 percent of the expenditures for R&D coming from industry, most of that is in the "D," not so much in the "R." But, we still haven't discussed what "R" is.

Dr. Kash said that research was whatever people who were doing it told them it was. I'm reminded of my mother who had two sons—one was an engineer, one was a scientist (presumably me). When she was asked what they did, she said they did research because they were scientists and engineers. So the definitions can become quite vague, yet the distinctions can be very real. I work with colleges and universities and, as we all know, students (both graduate and undergraduate students) are involved in research, "doing" research. But when you get at what the undergraduates are doing—well, they may be having a research "experience," but is that really research? I think that we really need to talk about what we mean when we refer to research.

I'm going to be provocative and give my definition of research. Research is any activity that produces new knowledge that is subject to critical review by experts and peers in the disciplines. Both components are important. To me that's very simple and straightforward, and that's separate from what the drivers are for research (i.e., why one does the research). Professor Kash commented that one of the main outcomes for universities is the production of people who are well trained and educated. I share that view. We're going to hear about research in academic institutions and the assessment of and the metrics for it in the session tomorrow.

We talk about basic and applied research. Some of you may know or have known Donald Stokes at Princeton University, whose book, called *Pasteur's Quadrant* (which has yet to appear), challenges the linear model of research-development-commercial product and arrives at a much more complex two-dimensional model. The question of the drivers for research is something that also needs to be talked about but kept separate from the concept of research and how one assesses its value in the context of this workshop.

That brings me to my last comment, which is that both talks have suggested that there's no point in pursuing the exercise, at least in my mind, of developing metrics for the assessment of research. I have trouble accepting that. I'm willing to be convinced that my interpretation of the talks is totally off base. But nonetheless, I felt it might be useful to put these out on the floor and see where the discussion goes.

David A. Hounshell: Your definition of research is one that I fully accept and use quite regularly, that is, the production of new knowledge. You further add the idea of peer review. One could have market tests rather than a peer-review process as part of your definition.

Clearly, I think Professor Kash is right. His experience is that, in many respects, evaluating research is very much like evaluating beauty—it's really in the eyes of the beholder. You could adopt a rigid definition of research (namely, the production of new knowledge). But how much further will that get you in terms of implementation, in terms of evaluating investment in research within a firm or across an industry or within a nation? I'm not at all convinced that it would help.

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Also, I think there has always been a great deal of tension, at least in policy making circles, which is borne out in part in the 1960s with the HINDSIGHT and the TRACES study about fundamental (basic) versus applied research. Professor Stokes has mounted the richest challenge to the linear model to date and I think his new model has moved away from a lot of the ideological positions that people have staked out. Professor Kash is absolutely right, these relationships always have had strong ideological components. I don't see a way around that either.

The final point is that I was not saying that the exercise is not worthwhile. I'm saying that the danger with GPRA in particular is that when you implement monitoring systems, either ex ante or ex post, you have to recognize that you have instituted a very dangerous activity. You have to recognize human behavior. You are going to get people "cooking" the numbers to present them in the most favorable light.

Robert A. Lichter: With respect to the Government Performance and Results Act (GPRA), I fully agree. I'm trying to look at this from a broader perspective. GPRA is just one particular constraint.

David A. Hounshell: My point is: It's symptomatic, even within the private sector.

Don E. Kash: I would agree with that wholeheartedly, and I would go a step further and say the enterprise that you're involved in not only is useful, it's absolutely necessary, but it is not necessary because it's going to demonstrate any good way to quantitatively measure this relationship. It is essential, because the political system requires us to go through this about on the same cycle as DuPont's 14- to 16-year cycle. That is real. The key point is that in going through what is a politically, socially, and perhaps financially mandated requirement, don't buy into something where the numbers get "cooked" and then come back and kill you.

Andrew Kaldor: Professor Kash, you lumped the world of R&D into your four categories. I guess my take-away message is that complex/complex is growing, and the Japanese are doing it better than we are. But in terms of our mission today, I'm not sure how we can handle this in terms of getting a measure for the effectiveness of research. It seems like a more productive approach would be to work backwards from a well-known or valuable product or development of some kind and trace the innovative process through the development stages back to the research phases and then back to "eureka." Gathering a database like this might help us better understand this process and come up with an effective measure.

Don E. Kash: We've done that with seven cases. One of them is a chemical case where the innovation clearly came out of the laboratory, the central laboratory. Now, we also have done it for a blade on a high pressure turbine on a jet engine. Here the ball game gets very mixed. It's a complex world out there—it goes back to the water wheel—and at least some of the engineers you talk to refer to the "black art" in casting.

One of the things that's really been terribly important is that much of the "black art" has been converted from tacit knowledge into explicit knowledge because of a whole new technology: computer-aided design. It is absolutely fascinating when you get these old engineers who know how to do things without understanding why, and put them at the front end of the process, inputting their knowledge into the computer. My point in this connection is that a lot of technology seems to take place without any understanding. It is surely not overwhelmingly based on explicit scientific research. In fact, an awful lot of scientific research is explaining what technology has done in advance, and so it has been for at least 400 years.

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3

Assessing the Value of Research at IBM

Joseph M. Jasinski
IBM Research
Thomas J. Watson Research Center

INTRODUCTION

Almost all scientists, I assert, believe that knowledge is a good thing, and I also believe that the public at large agrees with this statement. Most people, especially scientists, would also agree that the goal of scientific research is to produce new knowledge through discovery. The fly in the ointment is, of course, that gaining knowledge through scientific research costs money. It requires an investment in the future, and necessarily in the unknown. Whether the supplier of this investment money is a foundation, a corporation, or the American people through their government, the investor naturally wants and often demands to know that the investment is paying off—thus, the need for assessment of research.

In this paper I describe how IBM Research attempts to perform such an assessment of its own results against its mission and responsibilities to IBM. Since assessment is meaningless taken out of context, I begin with a brief overview of IBM and IBM Research and the relationship between IBM and its research organization. Needless to say, IBM is not a part of the chemical industry, although there are many chemicals and chemical processes involved in the production of computer chips, displays, and disk drives. The assessment process is, however, fairly general, and the key ideas should be transferable regardless of the specific nature of the research work.

International Business Machines Corporation is a large, global, vertically integrated information technology (IT) company, with 1996 revenues of $75.9 billion. The corporation can be viewed as consisting of four functional blocks: corporate headquarters, manufacturing and development businesses, marketing and service operations, and research. The manufacturing and development businesses include servers, personal computers, software, semiconductor chips, and magnetic storage. Marketing and services includes marketing and sales function for all product lines, as well as industry-specific technology solutions and services such as operating computer systems for large corporate customers.

IBM Research is a largely autonomous function that reports fairly directly to the CEO's office and is closely aligned with and coupled to (but not tied organizationally to) all other parts of the corporation, as well as IBM customers and the worldwide scientific and technical community. The technical work within IBM Research is segmented into four major parts: silicon technology; magnetic storage; com-

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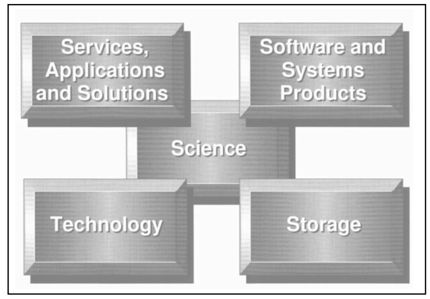

FIGURE 3.1 Segmentation of technical activities within IBM Research.

puter systems and software; and services, applications, and solutions (see Figure 3.1). The first three areas are self-explanatory. The fourth is a segment of IBM Research that works closely with IBM customers to develop integrated solutions (hardware and software) for major industry segments such as health care, finance, manufacturing, utilities, and so on. Its goal is to bring IBM Research into direct contact with IBM's leading-edge corporate customers—in other words, to provide Research with an entry into the demands of the marketplace.

The fifth segment of technical work in IBM Research is science. As illustrated in Figure 3.1, our science programs touch all four "aligned" segments in a major way. We also conduct long-term exploratory work that is not directly aligned with a current product or development plan. This diverse mixture of work including basic science must and can be assessed. The assessment process, particularly assessment of the longer-term work, is the main topic of this paper.

IBM RESEARCH

IBM Research is just over 50 years old, tracing its roots to the Watson Computing Laboratory at Columbia University, which started in 1945. Box 3.1 lists some of the major contributions of IBM Research to science and technology. IBM scientists have garnered three U.S. National Medals of Science and six National Medals of Technology, shared three physics Nobel Prizes, and received

BOX 3.1
Major Contributions by IBM Research to Science and Technology

One-Device Memory Cell
Reduced Instruction Set Computing
High-Temperature Superconductivity
Magnetic Disk Storage
Thin Film Magnetic and Magnetoresistive Recording Heads
Relational Database
Token Ring
FORTRAN
Scanning Tunneling Microscope
Fractals
Speech Recognition
Scalable Parallel Systems

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numerous other forms of recognition in the external scientific and technical community, including memberships in the U.S. National Academies of Sciences and Engineering.

IBM Research currently consists of approximately 2,800 employees worldwide, or about 1 percent of IBM's total global work force. Research currently operates facilities at seven locations around the globe: the Thomas J. Watson Research Center in Yorktown Heights, N.Y.; the Almaden Research Center in San Jose, Calif; the Zurich Research Laboratory in Ruschlikon, Switzerland; the Tokyo Research Laboratory in Yamato, Japan; the Haifa Research Laboratory in Haifa, Israel; the China Research Laboratory in Beijing, China; and the Austin Research Laboratory in Austin, Texas. The China and Austin laboratories have opened within the past few years, and IBM has announced plans to open an eighth location, the India Solutions Research Center in Delhi, India.

In the early 1990s, IBM Corporation experienced a sudden, dramatic change in its business environment. In one year it went from a highly profitable enterprise to one in financial and strategic disarray, largely owing to major changes in the information technology industry. In 1992, IBM reevaluated everything about itself, including what its key business should be, what its long-term strategy should be, and whether or not a central research organization was still important to its future. This necessarily caused IBM Research to evaluate and change itself. This evaluation led to a definition of the Research role as "vital to IBM's future success," and resulted in the organization as it exists, healthily, today.

As its high-level mission, IBM Research seeks to transform basic scientific and engineering knowledge in fields such as chemistry, physics, mathematics, electrical engineering, and computer science into new products and technologies that affect IBM's existing businesses or lead to new business directions for IBM (Figure 3.2). The values and assets we create in the process include technical

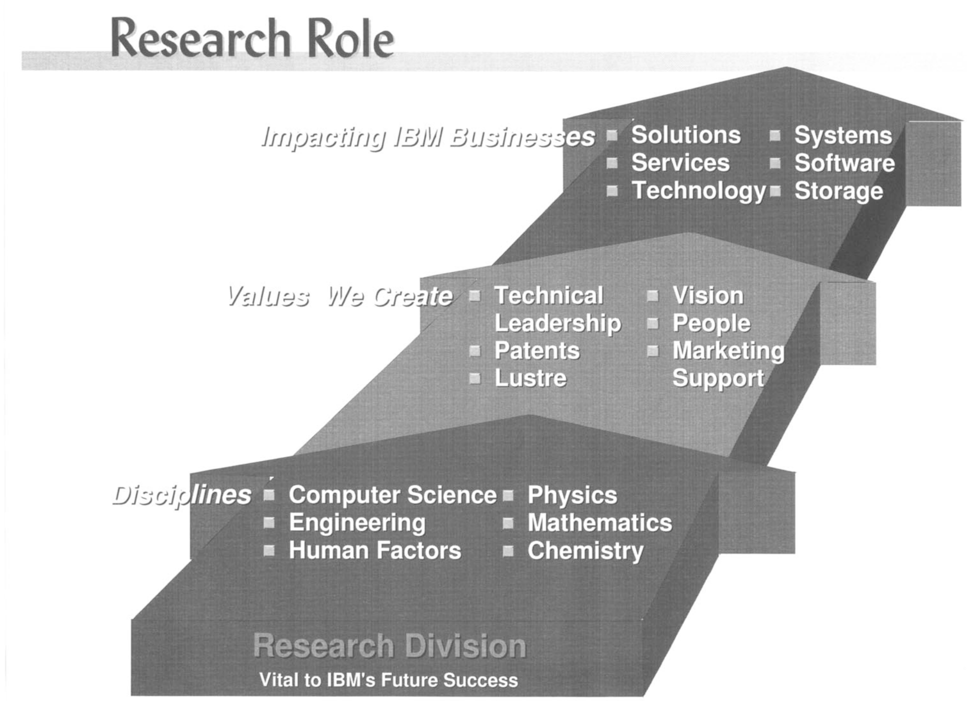

FIGURE 3.2 IBM Research seeks to transform IBM by advancing knowledge in critical scientific and engineering disciplines and using that knowledge to have a positive impact on IBM businesses.

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leadership in selected fields and in IBM's products and patents; a vision of the future in information technology; people who are technical leaders and in many cases future executive leaders for IBM; support for our marketing efforts by direct technical interaction with leading-edge customers; and luster—the substantiated perception by the industry, and the world at large, that IBM really does have the best people, the best ideas, and therefore the best IT products.

THE RESEARCH DIVISION BUSINESS PROCESS

A schematic of the annual business process cycle for IBM Research is shown in Figure 3.3. There are four major elements. These are the technology outlook, or "Ten Year Outlook"; the environment, vision, and strategy; the technical plan; and the end-of-year assessment. The process begins in the January-February time frame, when senior management and key technical staff take a look at the Information Technology (IT) industry and try to envision where it is headed and what IT will look like over the next 10 years. This outlook drives the strategy process for each Research segment. The strategic goals and opportunities we identify, combined with our vision of the industry, are the main elements used to develop our financial request for the next year. The budget is then negotiated with corporate headquarters. These three factors—outlook, strategy, and budget—then lead to a technical plan of work for the following year. The technical plan closes in the fall. It defines our intended work for the following year and sets a number of the parameters against which we will assess our performance that year. The cycle closes with the assessment phase of the *current* year's work in December, against goals set in the *current* technical plan, which was created in the *previous* business plan cycle.

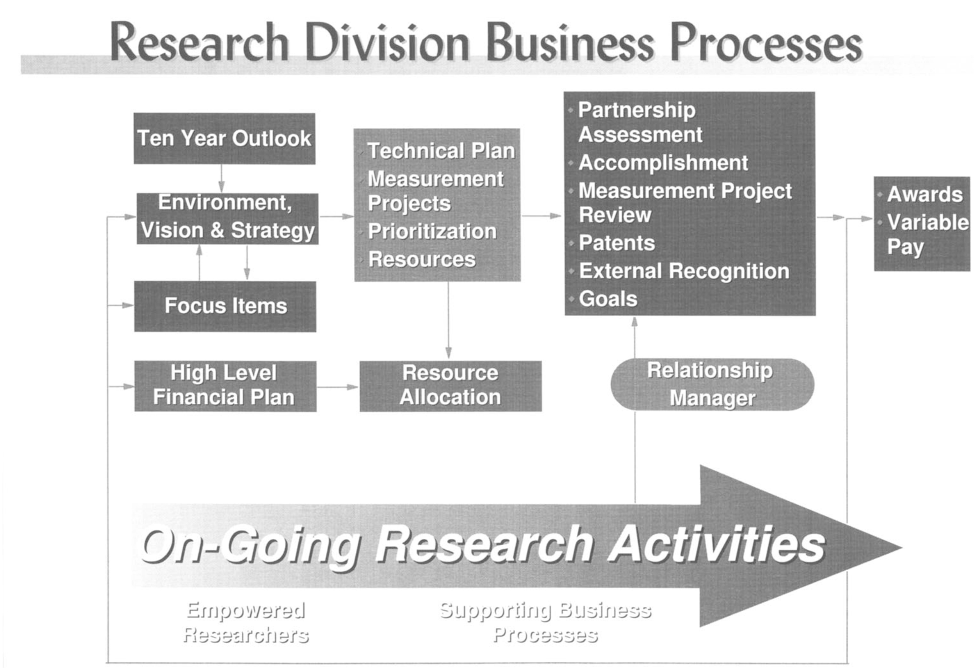

FIGURE 3.3 Outline of the annual business cycle for IBM Research.

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The assessment consists of a report on seven distinct items that constitute Research's "contract" with the IBM Corporation. The preface to the report states:

> This report on the IBM Research Division results is focused on values created for and having impact on IBM. Success and impact are measured by success in delivering technology to IBM's customers, except for long-term science, where the measurement is major contributions to knowledge in a specific area.

The following assessment items are described more fully below:

- Measurement projects,
- Accomplishments,
- Partnership assessments,
- Intellectual property,
- External recognition,
- Financial goals, and
- Division goals.

The weighted outcomes of these items determine the overall assessment of Research's performance for the year. The only *immediate* impact of this result is that it is a factor in determining the annual bonus for all Research employees. Obviously, there is a long-term feedback between the assessment process and the rest of the cycle, but it does not, for example, determine the next year's budget in a direct way.

Of the seven assessment items, three are easily measured and easily described. These are intellectual property, external recognition, and financial goals. We count the number of patents filed by Research in the year and make a semiquantitative assessment of their potential value. The result is then compared with a target set by the IBM Intellectual Property and Licensing organization. We tabulate external scientific and technical awards and honors received by our staff, and we determine whether or not we met our budget. The division goals item is a set of high-level goals that are essentially a confidential performance plan for the executive management of the Research Division and are assessed by the CEO's office. Partnership assessments are "customer feedback surveys" from the other parts of IBM with which we work. The remaining two items, measurement projects and accomplishments, are designed to assess the results of our technical work in a very broad way.

Measurement projects are defined during the technical plan cycle and are typically projects that have a well-defined goal, a "deliverable," and a "customer" to receive and evaluate the deliverable. The deliverable could be an advanced chip design, a new process for fabricating devices, a piece of software, or a prototype solution, such as an advanced voice recognition system. The customer might be another part of IBM, an external customer, or (in the case of work done with government contract funding) the funding agency. The intent of the measurement project item is to assess our ability to deliver what we promise to our business partners.

Finally, and perhaps of most relevance to this proceeding, we assess our exploratory work through the item referred to as accomplishments. Accomplishments are used to assess our entire portfolio of technical work, including, and perhaps of most relevance to these proceedings, our exploratory and basic science work. Accomplishments represent a modest subset (on an annual basis) of IBM Research's exploratory technology and basic science work. Candidate accomplishments are proposed by line management and evaluated and "graded" according to a set of well-defined criteria. There are three classes of accomplishments: accomplishment, outstanding accomplishment, and extraordinary accomplishment (Table 3.1). The criteria for each grade are given below. There are two sets of criteria for each level: one that applies to work intended to have an impact on the scientific and technical community, and one that

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TABLE 3.1 Criteria for IBM Research's Three Classes of Accomplishments

Science and Technology	IBM Impact
Accomplishment	
Significant development in a field • Advances the state of the art • Externally recognized impact • Invited talks/papers at important conferences • Results in significant work in field • Publications in refereed journals • Patent/patent application	Recognized significant impact on IBM product or pre-product program • Significant impact on business • Technology transfer • Recognized significant impact on IBM customer • Leading-edge technology solutions • Significant IBM business benefit Patent/patent application Publications (internal or external including press or media)
Outstanding Accomplishment	
Fundamental development Starts important new field Important publication in first-rank journals and conferences Many citations Valuable patent(s) Many invited papers/talks including most prestigious institutions and conferences External awards from significant societies/organizations	Recognized major impact on IBM business or major product (recognized in IBM and in the industry) • Depth • Breadth Fundamental new development and/or change in direction Transfer of key technology or solution Valuable strategic patent Possible corporate award class
Extraordinary Accomplishment[a]	
Worldwide recognized revolutionary long-term impact on science or technology Recognized by major external awards (Nobel, Franklin, Buckley, Turing, Field) Engenders large efforts worldwide in new area	Major innovation of overriding importance to the success of one or more major offerings Outstanding long-term impact; industry standard Fundamental patent(s)

[a]Clearly beyond outstanding. Examples: High-temperature superconductors, scanning tunneling microscope, RISC architecture, 1-transistor DRAM cell, relational database, atomic force microscope, and magnetoresistive head technology.

applies to work intended to have direct IBM impact. These criteria can be orthogonal, although the very best accomplishments—the ones that are truly extraordinary—will frequently meet the criteria in both categories.

An extraordinary accomplishment is a rare event. Very few, if any, accomplishments will achieve the extraordinary (E) rating when first presented as an accomplishment. It is expected that the E rating will only be achieved after sufficient time has elapsed to allow demonstration of the outstanding long-term impact and importance of the accomplishment; this will generally be after a number of years.

An important feature of the accomplishment assessment is the long-term tracking of accomplishments and the option to revisit and upgrade accomplishments in later years as they become more

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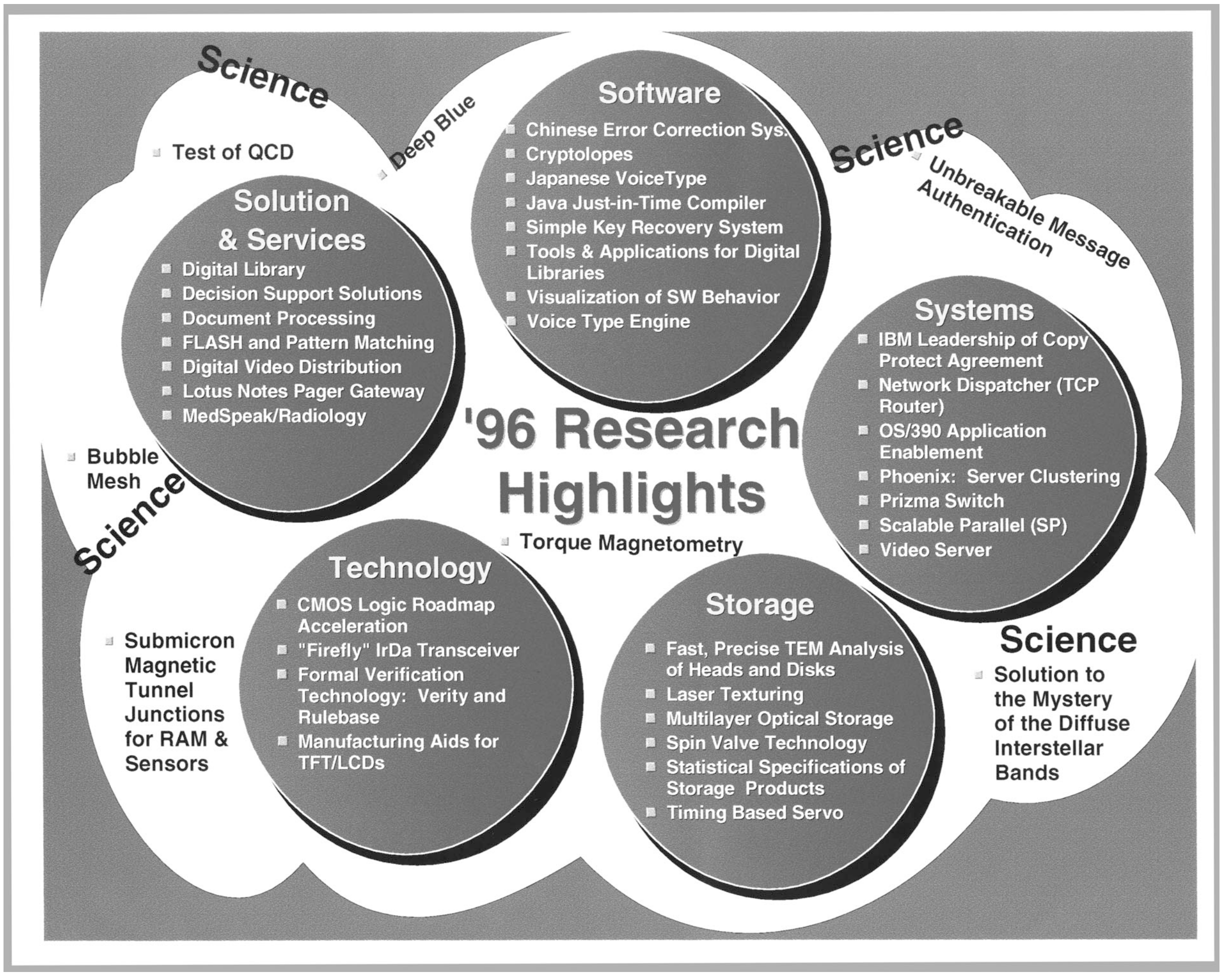

FIGURE 3.4 Sampling of IBM Research accomplishments in 1996.

significant. A sampling of IBM Research accomplishments for 1996, the most recent year available at this time, is given in Figure 3.4. A comprehensive description of each is not possible here, but collectively they span a wide range of topics, including basic, numerically intensive computational tests of quantum chromodynamics; prototype products or significant enhancements to IBM product lines; and Deep Blue, the chess-playing supercomputer that in 1996 beat World Chess Champion Gary Kasparov in a single game (but lost the match) and in 1997 won a six-game match against Kasparov.

CONCLUDING REMARKS

I have given a brief description of how IBM Research, a large, complex research organization with multiple goals and responsibilities—ranging from having a direct impact on IBM's bottom line to long-term basic science—assesses its work. My intention was to point out that meaningful assessment of a wide range of technical research is possible and must be flexible enough to allow for unanticipated achievements, and that conducting such an assessment is a serious task in itself. As an organization, thus

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far we find that such assessment has been a worthwhile endeavor as a method both to explain our value to IBM and IBM's shareholders and to understand more clearly ourselves the value we create.

DISCUSSION

Charles Zukoski, University of Illinois at Urbana-Champaign: The "Accomplishment" levels that you described were clearly for major breakthroughs. You have a large research organization, and everyone's work does not necessarily fit into one of the three categories that you described. Could you explain how you value the other, smaller accomplishments?

Joseph M. Jasinski: I will answer that in several ways. Some of the work you describe is covered in what I call measurement projects, which I did not discuss in detail. That's an equal portion of the report to the chairman, and it covers activities that are much more short term than those described by accomplishments. Over time, we do expect most of the projects to which we devote significant resources to make it to the "Accomplishment" level. If you are not headed on that trajectory, you are probably not in a very comfortable position, if you are not involved in a measurement project or some other type of activity.

Thom H. Dunning, Jr., Pacific Northwest National Laboratory: Could you explain the process that you use to decide on whether projects have met the criteria for an "Accomplishment" or an "Outstanding Accomplishment"? The requirements for an "Extraordinary Accomplishment" are fairly clear, but the process that you use to assign projects to the other levels is not.

Joseph M. Jasinski: Once a year we ask our managers to submit their proposals for the projects they think should be "Accomplishments" in their groups. These proposals are collected and matched against the criteria. In some sense, we rank-order them in a not terribly quantitative way, and we select a certain number that we think make the cutoff. At this time we also decide whether we think they are an "Accomplishment," "Outstanding," or "Extraordinary." This information is then passed on to the appropriate vice president, who submits his or her list of proposed accomplishments to the director of research and his staff. If necessary, they investigate or challenge the criteria by which proposed projects were graded, whether they are in fact accomplishments, whether they're outstanding, or whether they're extraordinary. We occasionally solicit outside input from customers. If the proposed project is a purely scientific activity, we may actually ask for outside evaluation to verify that the result is as important as we think it is. Finally, the director of research officially assigns the ratings.

Francis A. Via, Akzo-Nobel Chemicals, Inc.: I really enjoyed your presentation, which identified the outstanding scientific accomplishments of IBM, including voice recognition and superconductivity. Within the chemical industry, many of us are constantly confronted with issues associated with whether or not we can capture the value of our exploratory research.

In your industry, we have recently seen an abundance of competitive products in each new area of technology development, even in voice recognition. Of the wonderful technical accomplishments that you have listed, have these contributed significantly to the bottom line, or have you found that capturing the value of technology is more difficult than it has been in previous years?

Joseph M. Jasinski: That is a very complicated question. Speech recognition is one of the oldest projects in IBM research. It started long before it was practical. Our position is that IBM will be the

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dominant player in speech recognition. Whether this is true or not is not for me to say. There are clearly other products on the market, and the market has not yet sorted out the winners and losers.

You also mentioned high-temperature superconductivity. We don't make any products that use either low-temperature or high-temperature superconductors. There are obviously small niche markets for such products. In fact, we do little research in high-temperature superconductivity now. When we discovered high-temperature superconductivity, there was a great flurry of interest in it that led to the award of a Nobel Prize in physics to the IBM physicists involved. It also led to a lot of very good work in materials science, very fundamental work aimed at trying to understand the mechanism of high-temperature superconductivity. But that activity peaked and quickly died away. We now have only a small effort trying to resolve the mechanism—not a major effort or investment. I do not expect that we will ever make much of an investment in this area, unless for some reason we decide that high-temperature superconductivity is going to be important to one of our technologies.

As Professor Hounshell reminded us, if I tried to give you a return on investment number for our exploratory research, I would be lying. I will not pretend to give one, because we do not calculate ROIs for exploratory research, as far as I know. There may be someone somewhere in IBM who does try to quantify such investments, but if there is, I am not aware of it. As Professor Hounshell noted, it is very difficult, if not impossible, to do.

The guiding principle behind what we do in exploratory research is common sense and flexibility. We want to have impact, but we define impact broadly. We have a research division—I was somewhat facetious in the introduction, telling you research is what I do—but we believe in having a research organization that is differentiated from product development organizations. If we cannot differentiate ourselves and show that the mission has value in and of itself, then we would not be around for very much longer, at least not in the form we are in today.

As I noted in my talk, the earthquake hit IBM in 1992. Research seems to have come through that period relatively intact. In fact, in the last couple of years, we have been growing in terms of numbers in the Research Division. We have opened two new laboratories in areas that we view as emerging markets for information technology—India and the People's Republic of China. That shows you that we do believe research is part of IBM's business at all levels, not just in technology development but in marketing and everything else.

Andrew J. Lovinger, National Science Foundation: I have two related questions. The three categories of "Accomplishments" that you discussed are very broad. Do you use any finer scale and are people ranked against each other? How and when do you decide to terminate research projects, and what mechanism do you use?

Joseph M. Jasinski: I spoke about assessing the performance of the Research Division as a whole to IBM Corporation. I alluded to a small part of your question in my talk when I stated that a fraction of all nonexecutive employees' variable pay depends on the outcome of that assessment. But I did not address the question of how we manage our scientists.

We use a system very similar to Bell Laboratories for managing our professional staff, usually Ph.D.s, that we call RSMs, or research staff members (at Bell they are called members of the technical staff). At the end of the year, we rank them based on their individual and team accomplishments. We assign a number to every RSM on a scale of 1 to 100. An individual employee's compensation is based largely on that number and the change in that number over time.

Now your other question: How do we terminate projects? This is an extremely difficult thing to do. When you talk to academic researchers and you ask them why they have worked in the same area for 10

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years or 15 years (or even their entire career), it is either because it is really good work and really exciting, or they appear to be uncomfortable because they know that you know and they know that everybody knows that they are doing fine work, but it is not leading edge. In that case, they say something like, "Well, that's where my funding is," or "that's where my reputation is established," or "that's the only area I can get funded to work in," or a similar explanation. It is completely different for us; we don't have those constraints.

We necessarily create a potentially difficult situation because we hire highly motivated, intelligent, driven people, usually very contrary to the mainstream. These, of course, are just the people who are going to discover things. The problem comes when they've been doing their work for a while, and somewhere someone decides that this is not the best work for them to be doing. Telling them this is awkward, uncomfortable—you name the adjective. But, in the end, we tell them that we just don't think the project they are working on is the best thing that they could be doing for IBM. They don't necessarily agree with us, and there may be some contention about the decision. Given their input, we think about the issue very long and hard and eventually either the project is terminated, or the individual prevails.

The process described above also applies to very large projects, projects that have gone well beyond what I call the research stage. For example, when I first joined IBM 15 years ago, we were developing Josephson junction technology for superconducting computers. The project involved 50 to 100 people. When it was decided that Josephson junction technology was never going to be a viable technology, the project was simply terminated. It is much easier to terminate a large project of that sort—the costs are high, the hoped-for return dwindling. You simply place the staff into other projects or try to get them interested in other scientific and/or technical problems.

It is much more difficult when only one or two research staff are involved in a project. It is not costing a lot of money, and even if you really don't believe it's the best thing for them to be doing for the corporation, or necessarily even for their own career, it is difficult to pull the plug when they disagree. At that point it becomes a matter of management judgment and employee persistence.

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Evaluating Materials Chemistry Research

James W. Mitchell
Lucent Technologies

INTRODUCTION

Research, even when academically targeted for the most esoteric knowledge-based regime, was once widely considered an unquestionably valuable enterprise. This era of basic research, relatively unrestrained by cost, was driven previously by defense-based needs. However, basic research within government agencies, industrial corporations, and to some extent within universities has been under intense scrutiny to assess the impact and payoff. Today, more research efforts are being channeled into pursuits for meeting industrial, national, and societal needs. With this current emphasis on tangible returns on investments in research, viable methods are being sought for evaluating and maximizing the impact of research programs. For the chemical science and technology areas, many approaches for evaluations exist. All are diversely dependent on the management perspectives within the institutions in which the research is conducted. Even within the same institution, flexible variations will occur within different organizations. For example, the methods used within the Materials, Reliability, and Ecology Research Laboratory of Lucent Technologies have a strong legacy in the corporate culture of Bell Laboratories, but detailed practices are different from those of other organizations within Bell Laboratories in which chemistry-related research is done.

The Materials Laboratory has responsibility for a substantial portion of the chemistry and chemical engineering research and all of the materials chemistry research associated with ceramics and metallurgy. For this organization, chemical research may be examined on three levels. At the corporate level, the issue for management is determining the value of the chemical R&D enterprise. On the organizational level, a primary interest lies in formulating methods for optimizing the value and impact of projects within the chemical R&D program. Analysis and prioritization of the portfolio of projects are needed to maximize the value and impact of the work. For individual scientists, the most valuable process is one that assists the experimental researcher in determining which one of many research ideas to pursue. Evaluation of projects, once initiated, is somewhat more straightforward and strongly parallels the assessments used in prioritization. Although the best way to evaluate chemical research at each

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level is seldom unequivocal, it is certain that valued chemical research will have been scrutinized to ascertain its specific merit and impact.

CORPORATE EVALUATION OF CHEMICAL RESEARCH

From the corporate point of view, the chemical research organization is valued when it fulfills strategic corporate purposes. These may include creating breakthroughs, developing rapid innovations, broadening technological capabilities, driving new business, and supporting or expanding existing businesses.

Institutionally, the chemical organization may also be valued in direct relationship to the perceived effectiveness of its managers. Evidence of effective research management is sought in three regimes. Execution of advanced R&D that generates technology and products for the next generation of business needs is paramountly important. The formation of partnerships with business units either to create prototypes of new products and technology platforms or to jointly develop low-cost manufacturing technology objectives is an imperative as well. In administrative and policy areas, the implementation of processes that really work and the development of technology roadmaps are other expectations. Effective managers are also adept in people interaction dynamics or people skills, a talent that is difficult to describe but easy to discern when it is present (or absent).

There is considerable debate about the efficacy of metrics for determining research effectiveness. However, it is almost universally true that valued research will have been assessed by some type of method to measure its effectiveness and productivity. The most frequently applied metrics for gauging research effectiveness include output evaluations. This involves compiling a matrix of outputs (patent awards, inventions, intellectual property, percentage of revenues traceable to research, and so on) and assessing the value of the matrix of outputs. This quantitative, short-term approach to assessing research has several inherent limitations with respect to gauging the breakthrough and longer-range innovation potential of the research organization. To accomplish this assessment more effectively, the structure and fitness of the research organization are scrutinized and examinations made to determine whether the chemical R&D organization is doing the right things. Are there joint projects with business units? Are research strategies aligned with corporate and business strategies? Are innovations as well as inventions targeted? Are breakthroughs and sustaining products, processes, and technologies under development? By examining these and other questions, the corporation determines whether research programs are correctly targeted. This approach, when coupled with an outcome matrix, provides a more global assessment of research effectiveness.

Research Evaluations at the Organizational Level

The assessment of the value of research has a number of contingencies at the organizational level. In some cases, the genesis of the research imparts its own value. Thus, the method of evaluation may depend on how the research was started. For example, was it inspired by a strategic customer need? Did the market organization identify a window of opportunity? Did a management directive from a business unit or research organization fuel the R&D program? Sometimes, serendipity accompanying an ongoing investigation leads to a focus on new objectives. In addition to a dependence on its origin, the value of research is altered as the project moves through various phases. As the economic potential of the project increases in the direction shown in Figure 4.1, the risk decreases, and the value judgments used to assess the research change as it moves through each phase.

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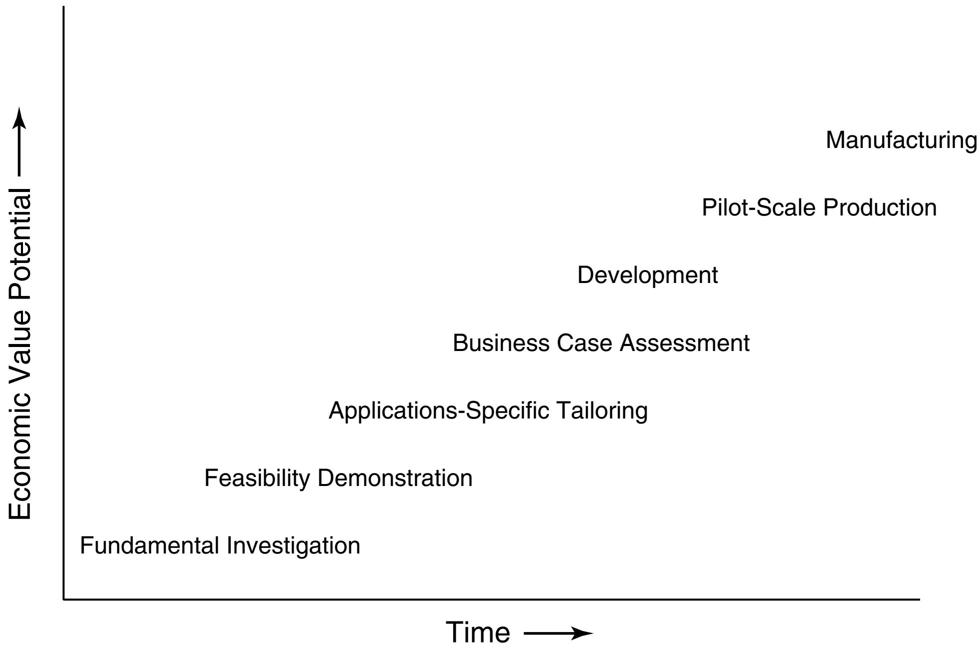

FIGURE 4.1 Phase transitions of research projects.

Prioritization of Chemical Research

Clearly, the most interesting and challenging opportunities for evaluating research are associated with the analysis and prioritization of the portfolio of projects maintained by the chemical research organization. Within an R&D organization, there are always more pressures to engage in activities than there are resources to use. The business units tend to continuously concentrate on projects that will generate revenue within the next year or two. Even though there are development organizations within the business units, research manpower is continuously sought for short-term tactical projects. There must therefore be creditable methods for deciding what projects to undertake. An effective prioritization process must accomplish the following objectives:

- Determine the value that the materials chemistry research organization delivers to the corporation;
- Apply to both joint business unit derived projects and research initiated projects;
- Establish specific criteria for judgment;
- Guide deployment of resources; and
- Optimize the value that the materials chemistry research organization delivers to the corporation.

The process should allow work to be scrutinized and determine why it is being executed. The prioritization analysis should assess this balance between long-term and near-term R&D projects to ensure that the long-term health of the company is balanced by the priority pressures of short-term tactical R&D.

The process must include suitable criteria for making the judgments mentioned above. Applying these criteria increases the value to the corporation by pinpointing low-priority projects for termination.

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More resources and the best people can then be associated with the highest-priority and most complex projects while investments are minimized elsewhere.

A team within the materials chemistry organization was assigned the task of designing a project prioritization process that could be applied to develop a portfolio that would deliver the maximum value to the corporation. The group had four levels of management (directors, department heads, supervisors, and research scientists and engineers). The ultimate process designed by the team needed to apply equally as well to projects initiated by the research organization and to those done jointly with the business units. Application of the prioritization process should create a portfolio of research projects balanced with respect to the long versus short term, high risk versus low risk, high value potential, maturity, and emerging products and technologies.

The portfolio should contain projects executed independently by the research organization. In some cases, the business units may not even be interested in the project because it is at a stage well below demonstrating feasibility or commercial viability. The research organization must be responsible for innovation of long-term new technology directions for the corporation. Other projects, initiated by the business units, are joined by research team members. Still other projects within the portfolio are conceived and executed jointly with business colleagues.

The ground rules used for formulating the research prioritization process include the following:

- Delivers maximum value to Lucent;
- Applies to projects that exceed a set number of people;
- Is widely supported as a tool for achieving greater effectiveness; and
- Does not replace management judgment.

The team decided that the four most important areas to be scrutinized by the process include these:

- Strategic fit: Does the project fit the corporation's and organization's strategy?
- Economic potential (including resource expenses);
- Probability of technical success; and
- Probability of commercial success.

Each criterion is subdivided further, creating a matrix in which each element can be rated for each project. For every entry in the matrix, a team can actually evaluate a portfolio of different research activities. Obviously, there must be background information about each project, and each member of the evaluating team must be familiar with each research project being assessed.

The criteria that emerge (up to four elements for each attribute) provide a framework for attaining qualitative insights, pinpoint areas of disagreement among the evaluating team, and help to resolve disagreements. By simply owning a prioritization process, an organization considerably broadens the scope of criteria used in assessing the importance of various research activities. The application of the prioritization process, while not quantitative, does ensure that a full range of considerations are made during the comparative analysis of projects. The process does not replace managerial judgments. For example, it easily pinpoints excellent or terrible projects. However, the distinction between many good to very good projects is still small, and decisions based on other judgments are usually needed to rank-order a portfolio. Applying a prioritization process does help an organization to decide which projects to enhance, which ones to eliminate or reduce, and which new projects to add.

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Evaluating the Entrepreneurial Scientist

Even when the chemical research organization still depends on the individually motivated scientist as a source of significant innovation, methods are still valuable for helping the bench research scientist to determine what projects are likely to provide the most value. Encouraging (not requiring) the individual scientist to conduct a self-assessment of his or her ideas and projects by applying a more global corporate perspective is useful. Established scientists with excellent track records need little guidance. Their personal radar for project selection far exceeds any prescription. With new employees, however, there is an opportunity to increase the awareness of specific insights that may enhance their selection of projects to optimize value to the organization. For example, a scientist should be able to describe the intended research using language that a nontechnical person can understand. One should be able to determine what is the intended value and impact of the work. Even if the project is at the most fundamental edge, the scientist should know that the intended customer is the scientific knowledge base. If the work is intended to have value, the scientist needs to be able to identify the path to value. The scientist should also make a prediction of the probability for success. For example, if the research investigation were a complete success, who would care about the results? One should also ask how much the research would cost. If the project has applications, how much more would be incurred before the business unit customer could extract value? A final question: How long would it take to conduct the research?

By executing a self-assessment before initiating a research study, the bench researcher may eliminate some of the random-walk uncertainties that could lie ahead. Such an approach is not universally practical but may assist the individual in thinking more thoroughly about an intended project before making the decision to discuss it with management. This self-appraisal helps assess whether an intended research investigation falls within the broad context of having potential value.

CONCLUSION

Ultimately, the objective of evaluating research is to improve the effectiveness of research and (in the case of industry) to enhance value for the corporation. If the research is applied, the value can be more easily determined by using economic assessments. For fundamental research, the practical value may be unknown at the onset. However, value is still present in the form of brand enhancement, equity in reputation, and advertising, through publishing excellent scientific work. Several guidelines are useful for optimizing the value of chemical research:

- Informed research managers should have access to information regarding where contributions could be made. Scientists and engineers need to be continuously provided with this information. Direct communication is necessary to permit individuals to volunteer to work in exciting areas rather than being assigned to projects.
- The organization needs to value high-risk, long-term research. Bell Laboratories today puts even greater emphasis on breakthrough research than ever before. The origin of the project may or may not have involved a business unit. However, when appropriate, these projects are executed jointly so that business units may take advantage of an innovative accomplishment.
- Research self-appraisals are useful and should be encouraged.

Organizations and individual scientists should identify paths to value as early as possible in a research investigation. Frequently, the research scientist working alone does not consider cost and complexity

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benchmarks that have to be passed if a project concept is to be cost-effective and useful. When work is applications oriented, cost and complexity should be considered very early on to the extent possible. Where appropriate, market analysis and business case developments are done at the onset of experimental work. Including business units and manufacturing teams as early as possible during the research project is also highly productive.

In closing this discussion, I decided to present a nuts-and-bolts look at evaluating research. As Donald Kash showed, Lucent Technologies also has large, high-level, breakthrough research projects, and international global research as well. However, rather than cover specific research accomplishments in these areas, I sought to cover practical approaches for evaluating and valuing materials chemistry research.

ACKNOWLEDGMENTS

The contributions of members of the Lucent project prioritization team are greatly appreciated. The author also thanks Mel Cohen for contributions and concepts for self-assessment by bench scientists.

DISCUSSION

David A. Hounshell, Carnegie Mellon University: You reviewed the criteria for prioritization, which included at least four areas: strategic fit, economic potential, and so on. They are scored on a scale, and you said that a number of people do the scoring. How do you avoid "group think" in the prioritization? How do you allow for contrariant thought, particularly with the more junior research staff as opposed to the senior staff?

James W. Mitchell: This process is executed by supervisors and department heads, so we don't involve the actual researcher in this assessment process, but rather a team of managers. To some extent, this draws out a lot of the discussion, because the individuals doing the prioritization are not usually the ones executing the projects. The process is also not necessarily applied to all of the projects in a given research area, like chemistry.

Jack G. Kay, Drexel University: There is one type of research that hasn't yet been addressed, and I wondered how you would justify it or categorize it. The type of research I am wondering about, for example, is the research that led to the realization that chlorofluorocarbons underwent a photolysis process and ended up chewing up the ozone layer. You don't have a product to sell; in fact, you've eventually gotten rid of some products that were previously sold. How do you evaluate that type of research?

James W. Mitchell: I gave you particular details on processes that apply mostly to situations where we could see some path to value. In the case of simply contributing to the scientific knowledge base, then you use other principles. There is a corporate culture at Bell Laboratories that says it is impossible for any manager to look at a given project and make an absolute decision that there is nothing of value that can come from it. So one looks at the upper 10 percent of the research population, and there is a fraction in that population that can do almost anything they want. They are the scientists that you depend on to make the decisions about what basic research ought to be going on. They merely need to present a well thought out idea to a manager, and then that manager's job is to see to it that they get the resources needed to pursue that research.

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The percentage of that type of research that can be pursued is certainly smaller now that we are competitive in every aspect of our business than it was in previous years, when we had a monopoly and only had to worry about how best to spend money. So we depend on the best scientists to make those decisions and to pursue them.

Lawrence H. Dubois, Defense Advanced Research Projects Agency: As a former Bell Labs employee, I can say this is a radical change from the way things used to be done. Have you taken the criteria and the concepts that you talked about here and gone back 5, 10, 15 years and applied them to some of the developments there to see if they really work? That is, would these criteria and concepts have identified in the past those technologies that proved to be important to the corporation in the future?

James W. Mitchell: No, we've not gone back in the past and looked at case studies. When you apply these approaches, you can easily pick out the winning projects and the losing projects. In the middle, there are going to be a number of projects that will be closely rated, and then you have to use managerial judgment to prioritize those.

So let me also make it clear that we do not apply this process across the board in every phase of the research organization. I have applied this approach to certain projects, for example, in one particular area where we had many, many projects and wanted as rational an approach as possible to determine which one of those to deemphasize. But no, we have not gone back in the past and applied the process. That would be an interesting project to undertake.

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The Technology Value Pyramid

Trueman D. Parish
Eastman Chemical Company

INTRODUCTION

The Industrial Research Institute (IRI) is not made up entirely of $30 billion companies, but even has some little $5 billion companies like ours. So what I am going to present has a broad range of applicability. Why do we measure research? In a nutshell, because inquiring CEOs want to know: Are we worth our salt? The bottom line in a profit-making organization (and perhaps this extends over into the public sector as well) is: Are we delivering value to our shareholders, our sponsors, or whoever is paying for us? So I do not think this is just something that a CEO asks; I'm confident it's what Congress is asking, and it is a legitimate question.

So the first question we must ask is: Are we really adding value in excess of what we cost? But before we proceed very far down that path, we have to ask ourselves if we are aligned with our company and business objectives. In a broader context, are we lined up with the direction our political leaders expect the country to go? Where the public wants the country to go? That, of course, requires leadership, which means that our leaders have some idea about where we want to go. I can say that, at a corporate and business level, this is a mixed bag. Surprisingly, some companies do not really know what their strategy is. But in any event, if our technology projects are going to be effective, they had better line up with the corporation direction, or we could end up thinking we are delivering value, but the corporation is unable to exploit our work.

Then there is the question of what our technology is worth. There is a fairly easy way to think about this. Suppose your company or your organization were to be acquired. In the corporate world, what you would acquire would be the physical assets, the list of customers, some marketing rights and brands, and so forth, but at least you would hope that an acquirer would pay more than the sum of these. What the acquirer would also be paying for would be your patents, your other intellectual property, and so forth. But probably most important of all would be the expertise of the people in the company. So one of the questions we must ask ourselves is: What is our technology worth—what is the value of our technology assets?

Finally, those of us who are engineers in particular worry about efficiency and effectiveness. You

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can have great technology assets, you can be lined up with your corporation's strategy, and you can even be delivering value, but are you delivering value in the most effective way possible? What does this mean? It means: Do I have processes in place that lead me through projects quickly and that allow me to use my intellectual property efficiently and effectively? Or do I have barriers between my organizations so that effectiveness is impaired? Is my staff well motivated? All of those are important metric issues. We would really like to measure all of them.

Metrics may not date all the way back to the Stone Age, but when we embarked on this project, we did a bibliographic search—as any good scientist would—and discovered that there were references to metrics that go back at least 400 years. There were R&D metrics 400 years ago, when the princes of Europe were sponsoring R&D. They were asking the question: Are we getting our money's worth? So any of us who are sitting in this meeting today who are feeling picked on can take some comfort in the fact that we have been picked on for a long, long time.

But what's happened to metrics since those days? I would suggest that most of the metrics have ended up in the landfill of history. Why is that? There are three crucial tests that metrics must pass if they are to be supported by our wider customer base. By customer base, I mean the people who pay the bills—customers and corporate sponsors in industry, and citizens and Congress in public institutions.

One of those tests is the question of *relevance*. Is the metric that I am trying to use relevant to my organizational mission, objectives, strategies, and so on? This means that the metrics used vary depending on the type of organization you belong to. Though I realize that many leading companies do indeed think of publication as a critical metric, it is one that has not found much favor in the IRI. Surveys of IRI membership find this metric quite low on their list. The general opinion is that, if it is publishable, it is probably not giving much proprietary advantage. On the other hand, in an academic institution, this could be a highly relevant metric—it is important to make any knowledge gained known. In short, it is critical to align the metrics of value creation with the objectives of the organization.

The second test is *credibility*. My favorite examples are the wonderful metrics (and I believe some of us in this room may have used them in the past) where we said, "Let's do a self-assessment." So we got together with our chief scientists and rated each other on how we were performing on a scale from zero to 10, zero being "dumb as a stick" and 10 being "should have won the Nobel Prize, but the committee was unaware of my work." All the scientists rate each other 9.5 and then present this self-evaluation to the business unit, which says, "Yeah right." This process lacks credibility. There are a number of other metrics that have great potential for gaming, but I think you understand the issue. Credibility is a big issue, especially if you are trying to develop metrics that are meaningful to your customers.

The last test is one that particularly appeals to me, and that is *complexity*. It is important for the metrics to be reasonably simple and easy to calculate. If they are not, we could end up having our whole research laboratory working on metrics rather than on science, and our preference is to have people working on science. I will add, however, that engineers tend to like complexity. We like to have a table of 60 numbers or more, to multiply this matrix, invert the matrix, and the like, but unfortunately this activity can be fairly destructive. Even if the metric is theoretically sound, it should be tolerably easy to calculate and, more important, intuitively easy for our customers to understand.

In sorting through this maze, there are a number of bright lights to guide us. This light comes from a number of sources. Some very good academic research has been conducted in the last 10 or 20 years that has begun to uncover the factors that lead R&D projects to commercial success. Which new products have been successful, which have failed, and what's the reason for each? What are the practices in R&D (particularly in business R&D) that lead to success versus failure? The increased focus that has recently been put on this area is beginning to lead to some metrics that have value.

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DEVELOPING THE TECHNOLOGY VALUE PYRAMID

With this background, I can begin discussing the work at the IRI and the road to the Technology Value Pyramid (TVP). The work started in 1992. Dr. Jasinski has already noted that this was a grim time. It wasn't just at IBM that the earth shook! The start of this project in 1992 was no accident. The Research on Research Committee of the IRI does a survey each year to identify the most important issue facing R&D chief executives, and metrics came out number one in 1992. They needed some kind of metrics to guide their decision making.

Why was 1992 such a traumatic year? It was a time when "right-sizing" (one of the terms used), downsizing, restructuring, and reengineering (to note other terms) turned into a feeding frenzy. CEOs were calling in their heads of manufacturing and asking them what they could do to cut costs. "Well," replied each manufacturing head, "you've got to have me, I make the products, so don't try to get rid of me." The marketing people said, "You surely need us, because we sell the products." The R&D staff said, "Well, somewhere out there in the future, I think we do some good, and in another 10 years what we do now will be important." That argument didn't work. So there was a desperate need to develop some metrics for the CEO and for the board of directors that could establish the value of research.

So IRI began looking at the development of metrics for R&D. As we worked on the metrics effort, a lot of other stakeholders indicated an interest as well. In addition to the CEO, we found that chief financial officers and boards of directors were also interested in our activities. Some of the business unit managers even said that our work might be relevant for them too, as did individual laboratory managers. There was interest even among individual scientists.

As noted above, we began our work with a literature search. We found that the creative abilities of researchers were remarkable: They had developed well over 1,000 metrics for the value of R&D. After applying the tests of relevance, credibility, ease of use, and so on, we whittled this list down to about 50 metrics. This was not easy, because people protested vehemently when we threatened to throw out their favorite metric. Eventually, everybody agreed on the number 50. Now all we had to do was find companies that want to use 50 metrics. But what company would want to be saddled with tracking and evaluating 50 of them? Fortunately, we finally found a way to organize these metrics that made sense (see Figure 5.1). We called this organizational method the Technology Value Pyramid, and we made it available as a computer program from IRI.

At the top of the pyramid is *value creation*. But in order to create value, we need a portfolio of projects. So right below value creation, we have *portfolio creation*. Portfolio creation is exactly what we do with our investment portfolios. Most of us do not invest our entire life savings in a single stock of a speculative company. Some of us might and may even get very rich, but most people will distribute their investments over a wider range, with a prayer that some of them will make money. Some of these investments will be long term for our long-term needs, and some of them will be short term; some will be high risk, and some low risk. So there are a number of parameters to be considered in portfolio creation. The same concept applies to the research portfolios of a corporation. I would suggest that is true for government agencies and academic institutions as well.

Immediately below portfolio creation, we put in *integration with the business*, because the portfolio is not going to make much sense unless it is integrated with the company's business needs. So unless you are oriented in the way the company needs to go, you cannot really build the needed research portfolio.

So now I have a bunch of promising projects that make a good portfolio and are integrated with the business. The next set of questions is: Do I have the right technology assets? Do I have the right R&D

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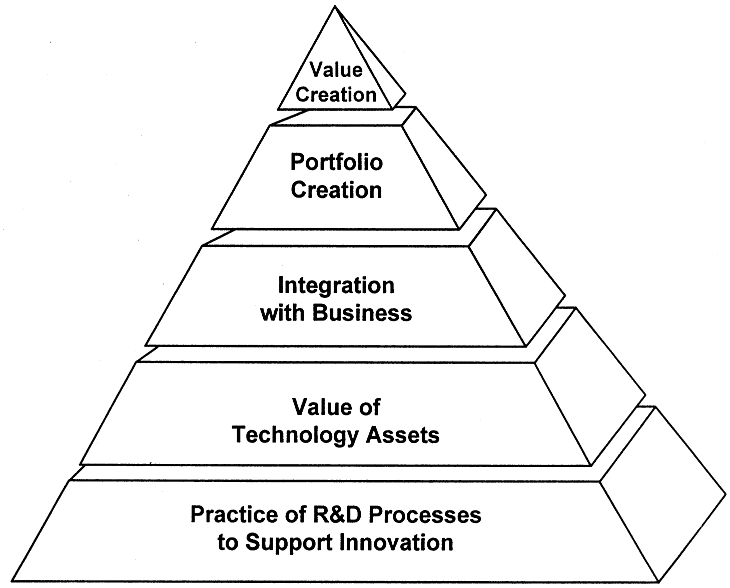

FIGURE 5.1 Technology value pyramid.

equipment? The right R&D expertise? Does this line up with my patent positions? Do I need to acquire some technology assets? So, the next layer is *value of the technology assets*.

And finally, underlying it all are the *R&D processes*. They really are the foundation on which all of the above lies. Unless you have the right processes in place, unless you have ways to effectively use your assets, you are still not going to be able to create value effectively.

I will not go through all of the 50 metrics. I will only talk about three of about eight in the value creation layer: the new-sales ratio, the cost savings, and the present value of the pipeline. The new-sales ratio is generally defined as the sale of products introduced into the market during the last "X" years, divided by the total sales of the corporation; IRI typically uses a period of 5 years. I suppose if you were in the computer business, 5 weeks might be more appropriate; but for a lot of the IRI businesses, 5 years is fairly reasonable. Typical values of the new-sales ratios are in the low double digits, so somewhere around 10 percent of sales typically are from new products introduced to the market in the last 5 years.

The question then is: Is this metric credible? Is it relevant? I would argue that it is relevant because corporations hope that their sales are profitable, and in general they won't be in the business if they are not profitable. It is relevant because, obviously, corporations like to grow. They like to see their earnings grow in particular; that helps the stock price grow as well as the value of the CEO's options (and that makes the CEO happy). Is it credible? Well, it also turns out it is. The reason is that the accounting systems of most modern corporations take in the model numbers or serial numbers of their products. Generally, they assign a new number as a new product is introduced. So if your accounting system works well, you can get this information almost automatically.

But what is a "new product"? There tends to be little argument now, but in the past some people would say, "That product isn't really new," or "it is only a little bit new." You can get into semantic arguments, but generally within a company you can resolve the issues and get a consistent definition. As a rule, if the product brings a new benefit that the customer perceives, it is new. There are benchmarks

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available for this metric from IRI. IRI gathers data for this metric from IRI members and reports averages of performance of these companies broken down by Standard Industrial Classification codes.

I am not going to go into as much detail about the cost savings ratios. There are a number of ways they are defined. Generally speaking, this metric measures how many dollars in savings were generated, normalized to manufacturing costs, but the metric is very similar to the new-sales ratio. It is relevant, credible, can usually be generated fairly easily from accounting data, and is benchmarked by IRI.

So, these metrics look terrific, but what's wrong with them? Typically, new products do not achieve substantial sales until they have been on the market for about 5 years. So the first problem is that products introduced 5 years ago are only now starting to affect this metric. If we now factor in the observation that the R&D to develop this product took 3 or 4 (or more) years, then I have developed a terrific measure of how my R&D process was working about 7 or 8 years (or more) ago.

I don't know how you drive in Washington traffic. Sometimes I like to drive just looking in the rearview mirror, because what I see in front of me is pretty terrifying. But that is unwise. Unfortunately, that is what we are doing in this case: driving R&D by looking in the rearview mirror.

I would not completely dismiss this approach, because I think there is some value in maintaining the record. Keeping that picture in front of you is important. I think it's important in dealing with business units, but it clearly does not give much guidance as to how to do things today if you want to accomplish something different tomorrow.

So that leads me to the final metric, the present value of the pipeline. Without going into too much detail or using too many technical terms on this topic, in general what you do is examine the projects that are in the pipeline. In doing this, you should ignore basic research, since the results of this activity are so far in the future that it is very difficult to put any numbers on it. Besides, in industry, basic research is usually a small budget item. But for anything that is to the point where it is costing you lots of money, you will generally have some idea of the size of the market, the probability of success, the kind of earnings that can be expected, and when you expect the product to come to fruition. If you can do that, you can make a present-value calculation.

Notice the variables: All we need is a good estimate of future earnings, when they're going to happen, and what the probability of success is. In other words, we need predictions. Here a quote by Niels Bohr is appropriate: "Predictions are very difficult, especially about the future." (Although the quote is usually attributed to Niels Bohr, I always thought it was from Yogi Berra. It sounded more like him.) This leads us to the metrics dilemma: "What's easy usually isn't important. What's important isn't usually easy." I don't know a way around this. I still think metrics are very important. I am not ready to give up hope, but it is certainly true that the important metrics are difficult to determine. Nevertheless, I would rather have an answer that is off by 50 percent, or even 100 percent, than no answer at all. I would rather have that answer that's off by a factor of two but tells me something to do, rather than one that is very precise and just tells me what I have done.

CONCLUDING REMARKS

Let me conclude. First, I believe you can use the metrics developed by IRI to judge the value of scientific research, at least for research directly connected to business needs. You have to think about R&D as part of the innovation process—a link in that innovation chain. Research cannot take credit for the whole chain. It can't really take credit for the social value of everything good that has happened in the country in the last 100 years either; but without R&D it would not have happened. On the other hand, without an entrepreneurial spirit it would not have happened, and without manufacturing it would not

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have happened. So there are a lot of links in the chain. The R&D link has to be strong enough to hold up its piece of that total value chain.

Second, there are commercial products available that will lead you through the process needed to obtain a reasonable estimate of how likely a project is to be successful or not, both commercially and technically. If you have such a tool, it would certainly help in project selection. A project should either have a big bang or be real cheap. That then leads you back into portfolio management and all of the various dimensions of portfolio management.

If you can do all of this, you can start using the metrics that help improve and estimate R&D productivity. Productivity, by most people's definition, is what you get out for what you put in. It is pretty easy to tell what you put in (just ask your accountant), and some of these metrics help you know what you are getting out. The metrics of effectiveness deal with issues such as stage-gate usage and so forth.

One of our earliest speakers spoke of this whole effort as potentially dangerous, and I share that view. Indeed, quality management, if it tells us nothing else, tells us that you tend to get what you measure. I would argue, then, that you had better be careful in selecting the metrics you use. In fact, that was one of the reasons that, when we built this program, we could not eliminate too many metrics, because we wanted corporations to be able to pick metrics that met their strategic needs. So, pick your metrics with great care, because if you use them, you will drive behaviors that get you what you are measuring. Nevertheless, I think that on balance, metrics are a benefit in terms of helping us all do our job more effectively, and so, the reward is worth the risk.

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Panel Discussion: Industrial Session

Isiah M. Warner, Louisiana State University: I would like to go back to our earlier discussion of Bell Labs. Dr. Mitchell's description of Bell Labs is different from the way it used to be. I remember when I visited Bell Labs one time, there was a young man doing research on dust. I asked him why he was doing research on dust and he said that it was because he was interested in it. Obviously, that is not the way Bell Labs does research now.

I was also thinking about Dr. Jasinski's comments on high-temperature superconductivity. Within a year after that major discovery, the Japanese were doing research on the applications of superconductivity. They were talking about high-speed trains that operated on super magnetic fields as well as many other possible applications of high-temperature superconductivity.

What I'm wondering is, if all of our major research institutions are beginning to move away from open-ended research that has no obvious gain—just the possibility—are we heading for trouble? If Bell Labs can't openly focus on projects that are going to "just possibly open entirely new opportunities," I'm wondering if our research enterprise is in trouble. Could you address this issue? It has not been discussed during the day.

James W. Mitchell: Well, since Professor Warner mentioned Bell Laboratories, I will try to clear up something. I hope I did not give the impression that Bell Laboratories is no longer interested in curiosity-driven research. I think I specifically said that there is a percentage of the population that has the freedom to choose to do whatever they want to pursue if they are going to have the best program conceivable in that particular area. We still do solely curiosity-driven research. Research is still going on in astrophysics; there's a biophysics department. For none of that work can you specifically point to a specific application.

There is not as large a percentage of that kind of work today as there has been in the past, for the reason I indicated in my presentation: competitiveness. If you must now compete for revenues, then you must be sure that you have a program in place that permits you to generate enough revenue so that open-ended research (as well as all other kinds) can continue at a very high level. There is as much emphasis today on extremely complex, almost impossible achievements technologically as there has been in the

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past. But the difference is that someone has taken a specific look at those goals and has communicated with business units to ensure that they can make use of it and can create value from it.

So yes, there is still open-ended research at Bell Labs. No, not everybody can do it, but a percentage of individuals, 5 percent of us, still have the freedom and the focus and the flexibility to pursue hunches, whatever those hunches are. Those who manage those individuals do not attenuate the ability of those scientists to pursue hunches and to pursue interests. I hope I've clarified the situation at Bell Labs.

Thomas A. Manuel, Air Products and Chemicals, Inc.: We're glad that the light is still burning brightly in Murray Hill, but to respond to Professor Warner on a broader perspective, and based on work done in the IRI, the fact is that across industry, the horizons have been drawn in. There's less frontier research being done now than there was some years ago. I think it's foolish for society to rely on industry to do the preponderance of that type of research. It never did and it never will, and in fact, it shouldn't. That is the province of academia and perhaps the government, certainly through funding, and in some mission-oriented cases through the national laboratories.

This concerns people in industry by and large. If you look at the sentiments expressed by the IRI or other groups of industrial people, they overwhelmingly wish academia would stay out of product development and stay out of trying to make money and companies out of inventions on campus, and instead keep on refilling the pool of fundamental knowledge. This is a message that we have to keep discussing. This opens opportunities for partnership, of course, since industry needs the new knowledge and can't do it, and academia can do it but needs funding.

Joseph M. Jasinski: Some of my remarks were also involved in the last question, so let me just clarify one thing. In the case of high-temperature superconductivity, the United States did very much the same thing. Within a year or two of the discovery, a national task force was commissioned. So there was a concerted attempt to make use of this wonderful new discovery.

Are we likely to make similar discoveries in the future? First of all, IBM Research and Bell Laboratories have a long history of being laboratories at the forefront, and we both hope that we still are. We both are certainly trying to be. But, times change. In the case of Bell Labs, there was the divestiture of AT&T, the telephone company. In the case of IBM, there was the financial crisis in 1992 and a big change in our industry, which forced us to change the way we look at things.

Would I like to go back to the "good old days"? I'm not sure I would, now that I've seen what my future looks like. But at the time, I sure was hanging on to everything I had, and so were most of my colleagues. We thought that if we could just hold on hard enough and long enough, we would get back to the good old days. This is, of course, a classic symptom associated with the psychology of change—a very common first reaction to catastrophic change. From the comments I've heard today from academia and the government sector, I believe this reaction is starting to take hold here today.

Janet G. Osteryoung, National Science Foundation: I would like to just repeat something that Dr. Mitchell said during his talk that I thought was profound: that research that is valued has been assessed. I think that is a take-home message, particularly for the people in the academic sector, because that is the sector with the most resistance to measuring the value of research by any means. I think that's a comforting outcome to look forward to, if you go through the agony of trying to do this.

Francis A. Via, Akzo-Nobel Chemicals, Inc.: Two general comments on the concerns of implementing metrics at every level of research activities for both our academic and our industrial colleagues. Our experience and those of leaders in our industry have demonstrated genuine challenges with establishing

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metrics and stage-gate systems for the very early phase of exploratory research activities. For example, 3M is considered a world-class model in innovation technology management and should serve as a valuable guide to many of us. It was recently reported at an IRI Workshop that 3M's experience in managing and setting metrics for exploratory discovery research demonstrated the continuing challenge of this task. In an effort to improve the discovery research process, enhanced management in the form of metrics, controls, and stage-gates was implemented for this first stage of research activities. Over a 3-year period it was recognized that this is not the area in which to apply detailed metrics and management controls. To flourish, this early creative research phase requires flexibility, degrees of freedom, and acceptance of uncertain outcomes. So we want to be careful in applying metrics to allow for degrees of freedom in early stages of research as well as accountability in a total portfolio of research programs.

Another major challenge we find in industrial research in today's globalized economy is slightly different. Over the last 20 years, there has been marked change in the profile and nature of industrial research. The time horizon of our programs and the nature of risk of these programs have in general been reduced. Are we sure we can fully use the fundamental research that is so effectively emanating from our university system as well as we have used this information in the recent past? Does this "gap" serve as an area that deserves additional attention? Should universities seek a higher degree of knowledge integration for research topics? Are there other approaches that should be considered as metrics for this area? The Advanced Technology Program of the National Institute of Standards and Technology has begun to address some of these issues. Nonetheless, it remains a rather modest portion of the total research profile. Are there other less controversial approaches worthy of consideration?

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6

Patents and Publicly Funded Research

Francis Narin
CHI Research Inc.

INTRODUCTION

I have been involved in measuring science and technology for a very long time. I was at the Illinois Institute of Technology (IIT) Research Institute in the late 1960s and was principal investigator in a study called TRACES, with which some of you may be familiar. TRACES was an early attempt to trace events of technological importance back to their origins in science. We carried out studies of magnetic ferrites, the contraceptive pill, the technique of matrix isolation in chemistry, and a few other advances. In all cases, we used experts—individuals who knew the literature on these subjects—to trace back and identify the events that led up to the technology. We traced the evolution of the ideas and technologies over relatively long periods of time. We classified the events as non-mission-oriented research, mission-oriented research, and development and application, and we tried to quantify these various stages. That, in fact, was the real contribution of the original study—the attempt to quantify how and when these different stages of scientific research affected technological development.

Sometime after the TRACES study the National Science Foundation (NSF) became interested in bibliometrics. In 1970 we were awarded an early project in bibliometrics, and then around 1971 or 1972, NSF began producing the *Science Indicators* report, and we worked on that. In fact, CHI Research has basically produced all the literature and patent citation data that has ever been included in *Science Indicators,* from the 1972 report to *Science and Engineering Indicators 2000*, which we have not started working on yet. The next *Science and Engineering Indicators* report is due out in January 1998, and we are just about through with this report. In short, we have been interested in the connection between science and technology for a long time.

CHI Research is involved in three different kinds of bibliometric research and analysis. We do a substantial amount of work in what is in a sense classical bibliometrics, which is examining how scientific papers cite other scientific papers. In these studies we are usually examining how well the United States is doing, or how well a given university is doing—that is, how many papers they produce, how often these papers are cited, and whether they are the key papers in their scientific field. A larger amount of classical bibliometrics is done in Europe than in the United States. A group in the Netherlands

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at Leiden is very active in this area, as is the Science Policy Research Unit at Sussex University in the United Kingdom. Similar work is done in Manchester, England. A number of other groups in Europe also carry out large-scale bibliometrics research. In part this is because many European countries have a centralized responsibility for science (centralized in one agency or group) and within the European Economic Community there are central groups responsible for the function of the scientific establishment. Europeans thus have an administrative motivation to look at science as a whole and to try to develop tools for measuring performance.

We also do a lot of work in technology-oriented areas and in "patents citing patents." Most of this work is done for private clients and is not publicly available. We do a lot of competitive intelligence work and cross-licensing work as well as technology tracing. We have written papers about this work, but generally we do not say much about it.

LINKAGE OF PATENTS TO SCIENTIFIC PAPERS

In this article I discuss the third type of bibliometrics(the linkage of patents to scientific papers. To carry out such studies, we have standardized more than 1 million references to science from the front pages of U.S. and European patents (see Figure 6.1). We put them into a standard "journal, volume,

United States Patent **4,713,814**

[Inventors] Andrusch et al. (Germany) Dec. 15, 1987

[Assignee] IBM (Armonk, NY)

STABILITY TESTING OF SEMICONDUCTOR MEMORIES

References Cited

U.S. PATENT DOCUMENTS

	Firms	Inventors		
3,995,215	11/1976	IBM	Chu et al.	324/158
4,004,222	1/1977	Semi Corp.	Gebhard	324/158
4,418,403	11/1983	Mostek Corp.	O'Toole et al.	365/201
4,430,735	2/1984	Burroughs Corp.	Catiller	371/25
4,502,140	2/1985	Mostek Corp.	Prochsting	371/21
4,503,538	3/1985	Robert Bosch GmbH	Fritz	371/21

Other references cited:
Wiedmann, IEEE Journal of Solid State Circuits,
vol. SC-19, no. 3, pp. 282-290, Jun. 1984

FIGURE 6.1 Front page of a U.S. patent.

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page, year" form so that we can match them to a scientific bibliography. Take a look at the front page of a U.S. patent—and this can be done easily; IBM has a wonderful Web site for U.S. patents (http://patent.womplex.ibm.com)—and look at the form of the scientific references. The nonpatent reference is a tribute to the creativity of the American inventor and the patent attorney. The first word can be anything. It can be a journal; it can be a name; it can be part of a title. It can be virtually anything, and it takes a lot of time and effort to turn these references into something that can actually be matched to a scientific paper. At the moment we are standardizing about 5,000 references a week. This allows us to link patents (technology) to publications (science).

We need to consider a number of the characteristics of the linkage from patents to scientific publications. First, we are discussing the central references—the ones on the front page of the patent, placed there by both the applicant and the examiner, and passed by the examiner. What we find is that patents are citing papers at a rapidly increasing rate. It has increased by 200 percent in just 6 years for a patent system that has grown by just 25 to 30 percent over that same period. Everything else is changing slowly in the patent system, except the way in which it links to science.

The linkage is very subject specific. Patents in biotechnology primarily cite publications in clinical medicine and biomedical research, especially in basic biomedical research; patents in chemistry cite chemistry and chemical engineering publications; patents in computing and communications tend to cite engineering and applied physics papers, especially those published in the journals of the Institute of Electrical and Electronics Engineers. So the linkage is very subject specific, just as citing in a scientific paper is—a chemistry publication largely cites chemistry papers, with a few citing biology or physics papers (i.e., most citations are to a very narrow section of the literature). Citation patterns common in the scientific literature are similar to those in the patent literature.

The linkage between patents and scientific publications is also national. U.S.-invented patents heavily cite U.S.-authored scientific papers. German-invented patents cite German scientific papers, Japanese patents cite Japanese papers, and so on. Patent citations are not homogeneous; they are quite national.

Perhaps the most startling finding is that 73 percent of the science citations on the front pages of U.S. industry patents are to publicly funded science, that is, to scientific publications from universities, government laboratories, government-funded research and development centers, and other public laboratories. From this it is clear that publicly funded science is having a major impact on U.S. technology. Figure 6.2 shows that inventors in every country in the U.S. patent system are increasingly linking their patents to science and that the patents of U.K. and U.S. inventors are linked particularly strongly to science. Part of this is because the United States and the United Kingdom are heavily involved in biotechnology and drug and medicine patents, which are the most science-linked component of the patent system. But even when adjusting for the specific area of technology, a U.K. patent is more likely to cite scientific papers than is a Japanese patent in the same area.

In the U.S. patent system, half of the patents are granted to U.S. inventors and half to foreign inventors. Thus, of the approximately 100,000 patents granted each year, 50,000 or so have U.S. inventors. Of the remaining 50,000 patents, 20,000 have Japanese inventors, and another 20,000 or so have Western European inventors. The other 10,000 are from Canada, Taiwan, Korea, and smaller countries. As an aside, we were examining some patent data the other day and found that both Korea and Taiwan are rapidly approaching the United Kingdom in number of patents granted in the United States—the number of patents issued in these two Asian countries was insignificant 10 years ago. The nationality of the inventor is determined based on the address of the inventor, not on the country in which the company is headquartered. An IBM patent invented in Switzerland, for example, is included in the Swiss patent count, not the U.S. patent count.

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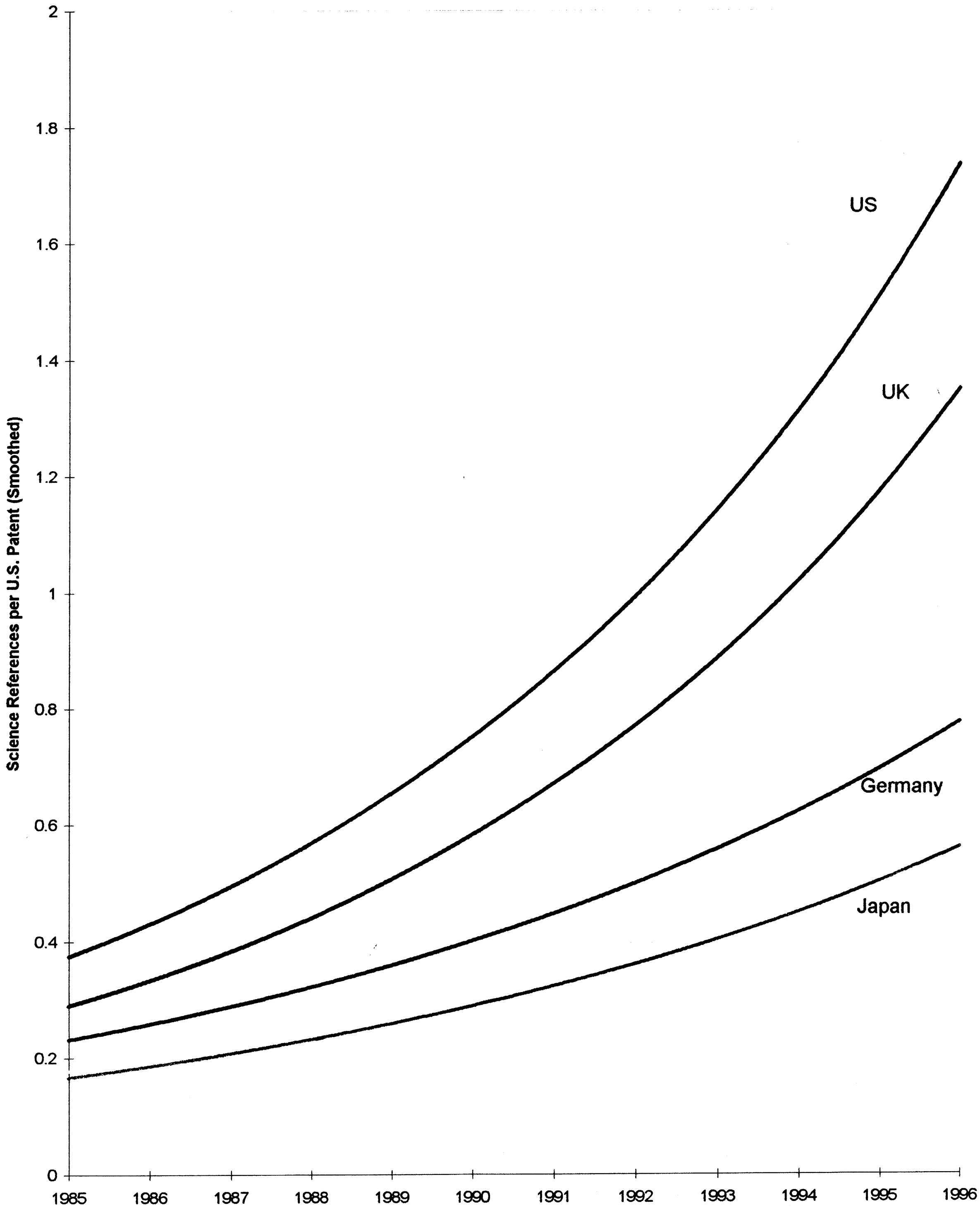

FIGURE 6.2 In the U.S. patent system, the linkage of patents to science is increasing, particularly for the patents of U.S. and U.K. inventors.

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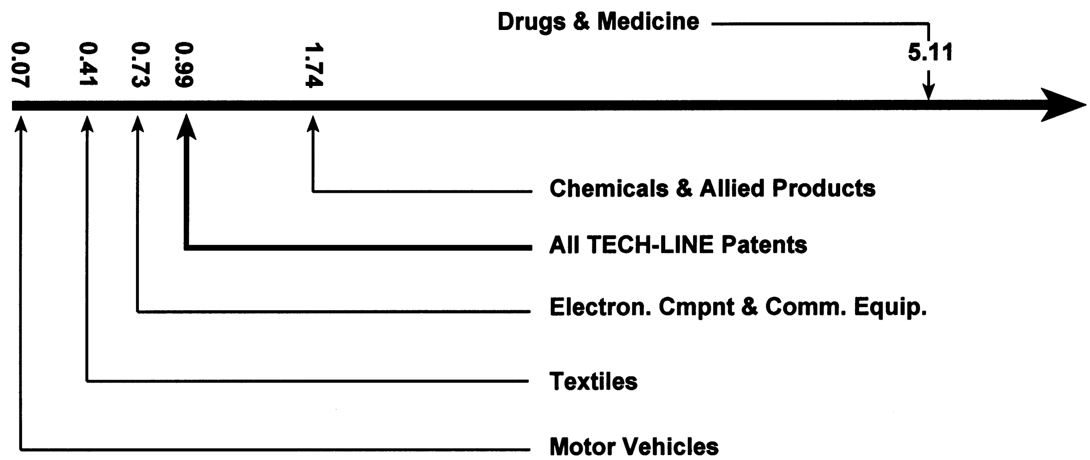

FIGURE 6.3 Average number of references to scientific papers in 1991-1995 patents. The above data quantifies the position of each of these areas on the technology continuum. In this regard, it is interesting to note that human genetics patents have 10 to 15 scientific references per patent.

If one looks broadly at the United States and Japan, the Japanese tend to improve on their earlier technology rapidly, often 1 to 2 years more rapidly than U.S. inventors. Japanese patents thus often cite very recent patents. We call this the technology cycle time, and we interpret this as showing that Japanese inventors are masters at rapid incremental adaptation. As noted above, there is a strong science linkage in the patents of U.S. inventors, more so than in Japan. We interpret this is an indicator that the United States is on the leading edge in technology. One of the strongest points of the U.S. technological system is its ability to take science and incorporate it into technology rapidly: The amount of science that is going into these patents from the public sector is increasing at an incredibly fast rate.

The science linkage varies greatly with the technology, a point illustrated in Figure 6.3. The average U.S. patent has just one science reference. Patents in the automotive area have almost none; patents in chemistry have close to two. In drugs and medicine, the average U.S. patent has 6 science references, and in human genetics technology, the average U.S. patent has somewhere between 15 and 20 science references. (The number of papers cited also depends on the year that the patent was granted. In fact, this rate is increasing so rapidly that I have to be careful how I phrase my comments.) Genentech's patents, for example, are extremely science linked. The average Genentech patent has more than 25 science references. Genentech has three or four patents on TPA [tissue plasminogen activator], its blood-clot-dissolving agent, with more than 400 related science references. So the distribution is highly skewed, with lots of science linkages in biomedicine, a substantial number in chemistry and some of the advanced areas of electronics, and almost none in the mechanical area.

Figure 6.4 illustrates the point I made about subject specificity. The figure shows data on clinical medicine and biomedical patents, plotting the number of references to clinical medicine and biomedical, chemistry, physics, and engineering research journals for the United States, the United Kingdom, Japan, and Germany. In every case, the great majority of the science cited in the patents comes from publications in clinical medicine and biomedical research journals, with some from chemistry journals. For chemical patents we would find the same thing, except that most of the citations would be in chemistry journals, with references to biomedical as well as physics journals. For computers and communications, almost all of the cited papers are in the engineering and physics literature: applied physics, solid-state

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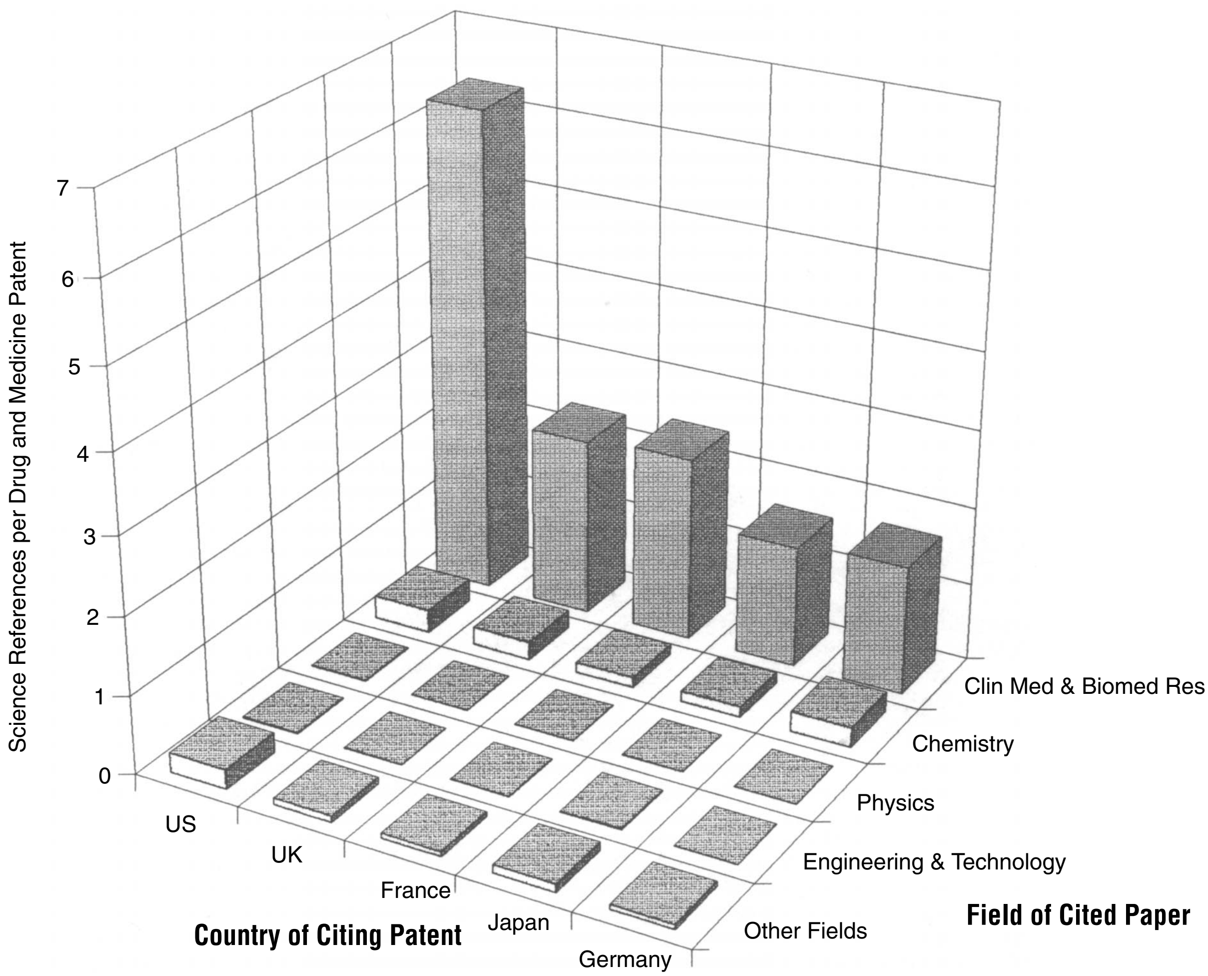

FIGURE 6.4 In the sample of clinical medicine and biomedical patents shown, most of the science cited was published in clinical medicine and biomedical research journals.

physics, IEEE, and other engineering journals. As noted above, there is the same kind of subject linkage between patents and the underlying science that is found in the scientific literature itself.

Figure 6.5 shows is that there is a strong national component to the citing of scientific publications in patents. The bars are the percent of references from each country's patents to its own scientific papers, divided by the percent of papers they have. For example, German scientists have about 8 percent of the papers in the *Science Citation Index*. Roughly 16 percent of the science references in German-invented patents go to German papers. The ratio of 16 percent to 8 percent gives rise to the Germany-Germany bar, which is roughly of height 2. If every country's patents used the world scientific literature homogeneously and there were no significant national component, then every bar would be at 1. This is clearly not the case. U.S.-invented patents heavily use U.S. science, although decidedly less so than Japanese-invented patents use Japanese scientific papers.

I want to point out that, if the same analysis were done on the scientific literature to look at how scientific papers cite other scientific papers, the resulting figure would look similar to Figure 6.5. A

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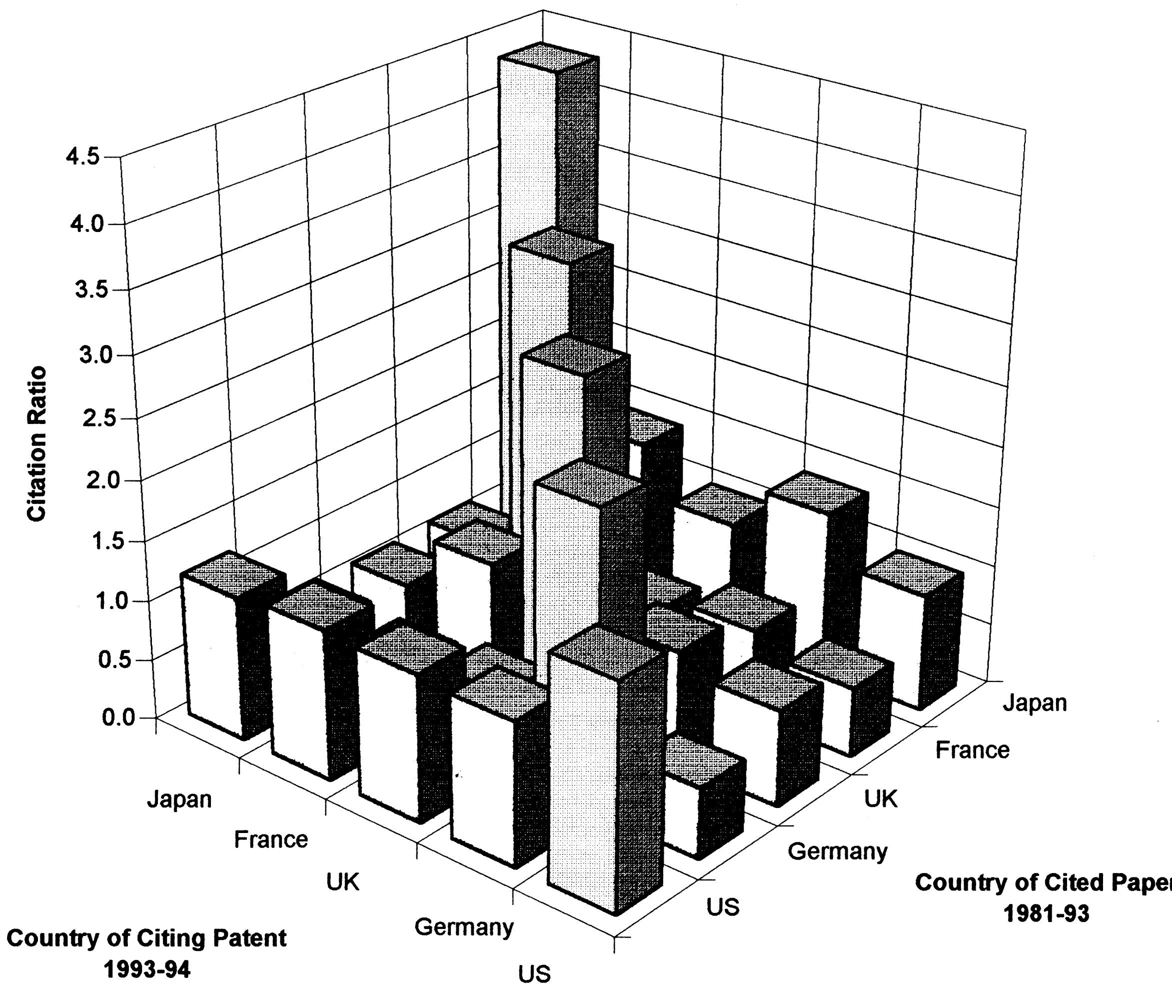

FIGURE 6.5 Citing of scientific publications in patents in 1993-1994 showed a strong nationalistic trend.

German scientific paper is two to three times more likely to cite an earlier German paper than expected, based on the volume of German scientific papers in that area. That is, a German scientist will cite his or her own earlier papers, along with the papers of colleagues in the German universities. It is also likely that the area that a scientist is working in is strong in German science rather than weak in German science. All of these effects give rise to a strong nationalistic trend in the citations.

Looking at patents in the U.S. patent system, the pattern is exactly the same for German-invented patents. German-invented patents are two or three times as likely to cite an earlier German-invented patent as would be expected by adjusting for the size of the German technological literature. So there is also a strong national component in the patent citations. In general, the cited papers are written in English. This is a set of citations matched to the *Science Citation Index,* and about 91 percent of the papers in the *SCI* are in English, so the difference is not primarily because of a language problem. I am sure that some of the bias is attributable to the language barrier, but this must be only a small part of the effect, as most of the scientific literature cited is in English.

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By the way, in our analysis of the patent citations, we did not take out the references to the inventor's own publications. If we took those out, it would change the results a little, but not a lot. For university patents, inventors often cite their own papers. In industry, we find some self-referencing, and we occasionally find it in biomedicine, but it is not found in most other areas. If we corrected for self-references, it would not change the fundamental statistics of the relationship.

Figure 6.6 covers the fundamental finding that we made that has recently hit the popular press. It shows the increase in patent-science linkage in three ways. First, there is an increase in the number of papers that are cited, which is illustrated on the left side of the figure. The number of papers cited has increased from 11,000 to 30,000. The central bars show that the number of citations of those papers has increased even more rapidly, because some papers are cited in 3, 4, 5, or sometimes 10 different patents—that is, the number of citations is higher than the number of papers. Citations have increased even more rapidly than the increase in the number of papers cited.

The last part of Figure 6.6 shows the number of support sources acknowledged on the cited papers. We have gone to the library and looked up the sources of support (NIH, NSF, DOE, and so on) for nearly 50,000 papers. The number of such acknowledgments is increasing even more rapidly, which says, of course, that there is an ever larger number of acknowledgments in each paper cited. The number of papers that acknowledge two, three, or even four different sources of support is increasing rapidly, possibly because much more collaborative research is going on. Every measure of collaboration that we have ever looked at, from how often papers are co-authored to how often they acknowledge different agencies for support to how often patents cite the papers, is increasing at a steady rate. This is really remarkable. We are talking about the difference between 1987-1988 and 1993-1994 patents—just 6 years! And we find that all of these markers have increased substantially—in a patent system that has increased in size by approximately 30 percent over this period.

One of the points I make above is that a large fraction of the scientific references in patents is in biomedicine, and probably 60 percent of all the publications cited in patents are biomedical papers. However, the biomedical literature is extremely large; there are large numbers of papers in clinical medicine and biomedical research. So what we did in Figure 6.7 was adjust for the total number of published papers. This allows us to see how often, in a normalized sense, the different kinds of science are cited. What we found is that biomedical research, the "basic" field in biomedicine, has more citations per paper than does clinical medicine. Interestingly enough, in biomedicine the citation is preferentially to the basic papers—a patent is much more likely to cite a paper in the *Journal of Biological Chemistry* than in the *Journal of the American Medical Association*. This implies that it is basic science that is driving biotechnology, not clinical applications. Note also that there is quite a lot of patent citing to the other sciences, which have far fewer papers than biomedicine. For chemistry and engineering and technology, the papers are not that much less cited, on a per paper basis, than biomedical papers. The number of papers in these fields is much, much smaller than in the biomedical field, but the papers are being heavily used, on a per paper basis, just as is the literature in biomedicine.

I want to make a point about the institutions whose papers are cited in patents, as well as the support sources acknowledged in these papers. These publications come from the most prestigious mainstream universities and companies; see Table 6.1. In chemistry, MIT, the University of Texas at Austin, Harvard, DuPont, Berkeley, Bell Labs, and IBM are the most heavily cited institutions. In general, the papers cited in patents come from basic journals. In biomedicine, the research is primarily supported by NIH. In chemistry the National Science Foundation supports far more cited papers than any other agency (see Figure 6.8), followed by NIH (the National Institute of General Medical Sciences [NIGMS] and the National Cancer Institute [NCI]) and DOE.

Now, we have not adjusted for research budgets at the different institutions. It would be easier to

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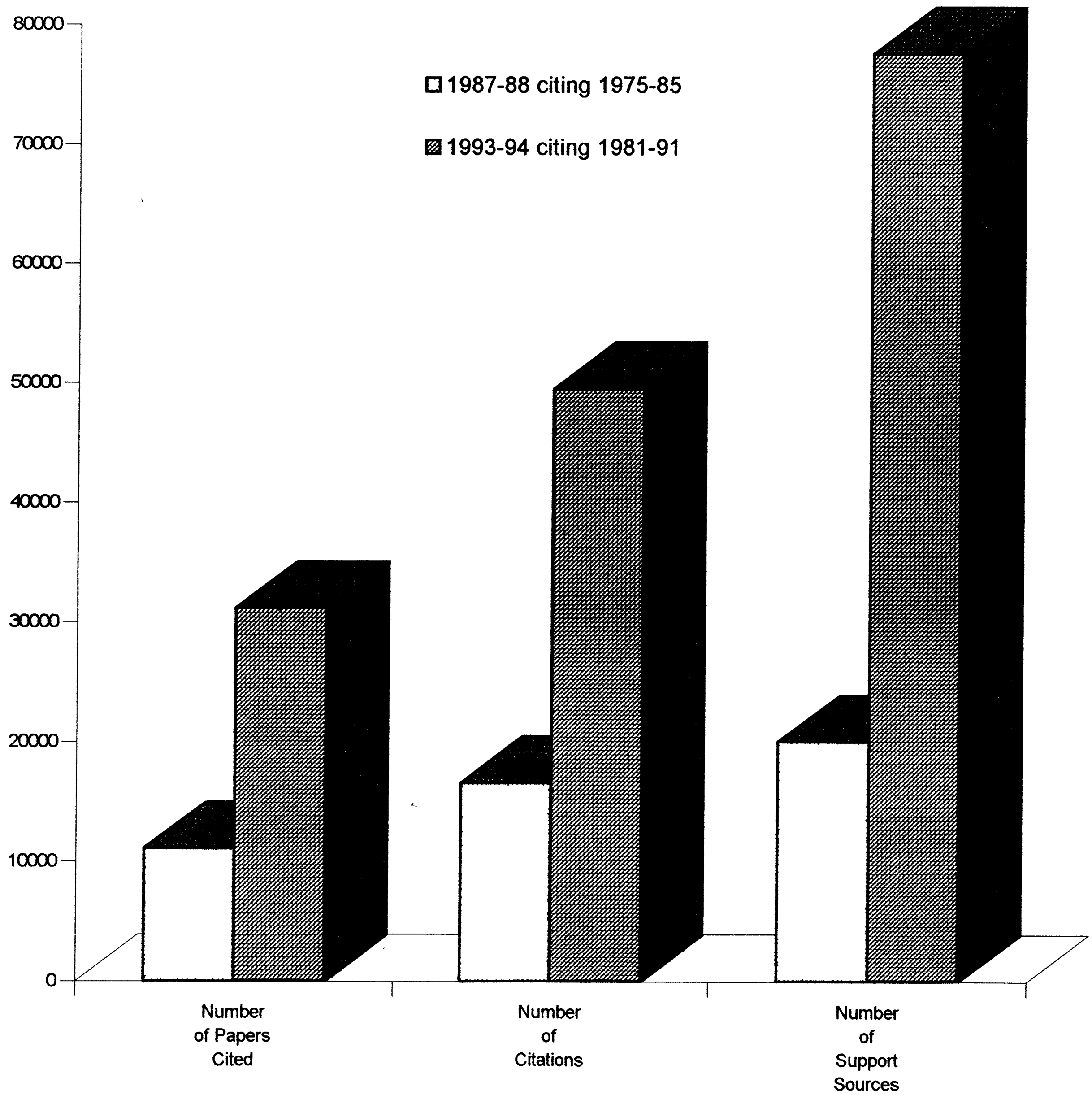

FIGURE 6.6 Three indications of the increase in the linkage of patents to science.

normalize by the number of papers published, but we have not done that either. I suspect that when normalized by the number of papers published, the number of industrial papers cited would increase sharply, because industry cites its own papers, as well as university papers. I do not know what the impact would be if we normalized by research budgets. The general rule is that the number of university papers published correlates with the budget, with a correlation coefficient of approximately 0.7. We

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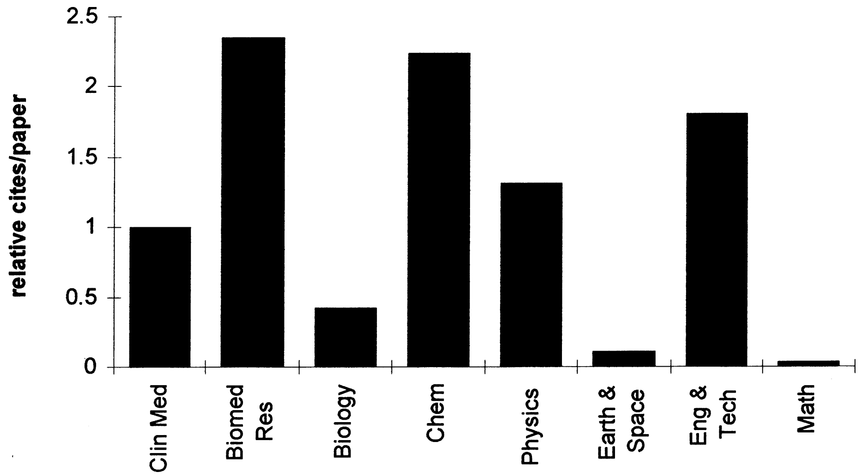

FIGURE 6.7 In the sample shown, biomedical research had more citations per paper than did clinical medicine.

TABLE 6.1 Institutions That Originated U.S. Scientific Papers from 1981 to 1991 That Were Cited Most Frequently in U.S. Patents, 1993 and 1994

Biomedical		Chemistry		Physics	
Harvard University	2,506	Massachusetts Institute of Technology	171	AT&T Bell Laboratories	854
National Cancer Institute	1,279	University of Texas at Austin	171	IBM Corp.	566
Veterans Administration	1,033	Harvard University	160	Stanford University	300
University of California, San Francisco	930	DuPont Co.	142	Bellcore	174
Stanford University	920	University of California at Berkeley	139	United States Naval Research Laboratory	167
University of Washington	845	AT&T Bell Laboratories	130	Lincoln Laboratory	150
Massachusetts Institute of Technology	756	IBM Corp.	122	Massachuetts Institute of Technology	133
Scripps Clinic and Research Foundation	690	Merck & Co., Inc.	102	University of Illinois at Urbana-Champaign	120
University of California at Los Angeles	642	Cornell University	96	University of California at Santa Barbara	110
Massachusetts General Hospital	625	Texas A&M University	95	Cornell University	106

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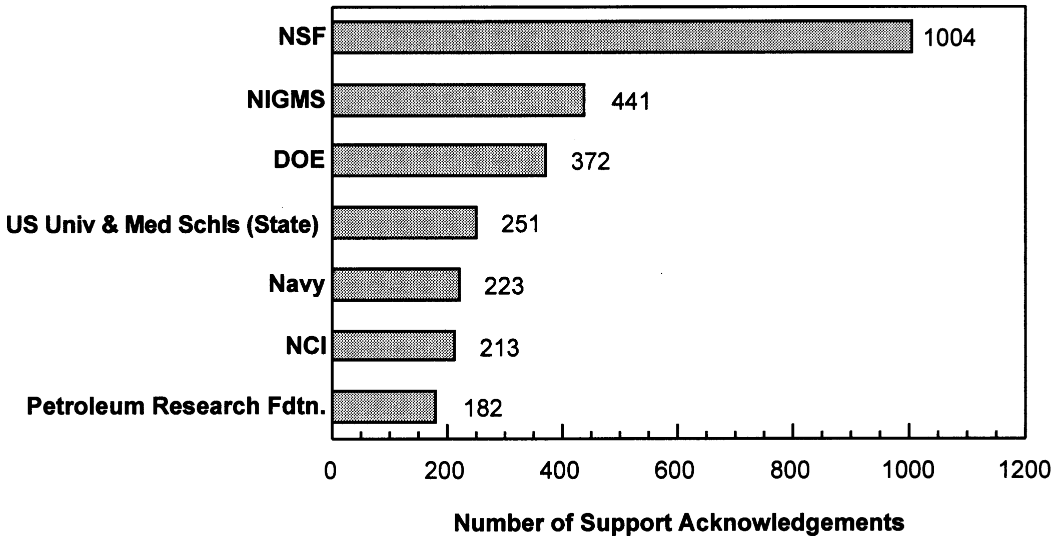

FIGURE 6.8 Funding organizations acknowledged in the chemistry papers cited in 1993-1994 patents. Note that these are the numbers of support acknowledgments given in the cited papers, which is not the same as the number of funded papers, since intramural papers often do not list a funding source. Thus, the number of funded papers might be undercounted.

would like to look at these issues in more detail but have not had time to do it. We hope to do this in the future.

SOURCES OF SCIENCE FOR U.S. PATENTS

I would like to touch on one last point, namely, Where is the science coming from for U.S. industrial patents? To answer this question, we first removed the government patents (for example, NIH's patents), the patents issued to universities, and foreign-invented patents from the list. The result of this analysis is shown on Figure 6.9, the top part of which is for biomedicine. What we found is that about 10 percent of the science base of U.S. biomedicine comes from the U.S. drug industry (private). Fifty-five percent comes from U.S. publicly funded science—that is, from universities, medical schools, government laboratories, federally funded research and development centers, and other public science sources. The other 35 percent is foreign, and the distribution of foreign citations is such that roughly 15 percent of that is industry. Most of the foreign citations refer to public science also. In fact, if U.S. and foreign private companies are taken together, 15 percent of the science cited in U.S. industry drug and medicine patents comes from the private sector, and all of the rest comes from the public sector.

To a large degree the same thing is true for chemical patents. The one area that is quite different is computers and communications. Here a large fraction of the cited papers were written by research scientists from industry. This occurs because Bell Labs, IBM, and other major U.S. companies (and overseas, Fujitsu, Hitachi, and other companies) publish many papers, and those papers are heavily cited in patents. There is therefore a fairly large private-sector contribution in computers and communications (34 percent). However, when we put it all together, since most of the citations are in biomedicine, what we find—and this is the bottom line of this whole discussion—is that roughly 73 percent of all the

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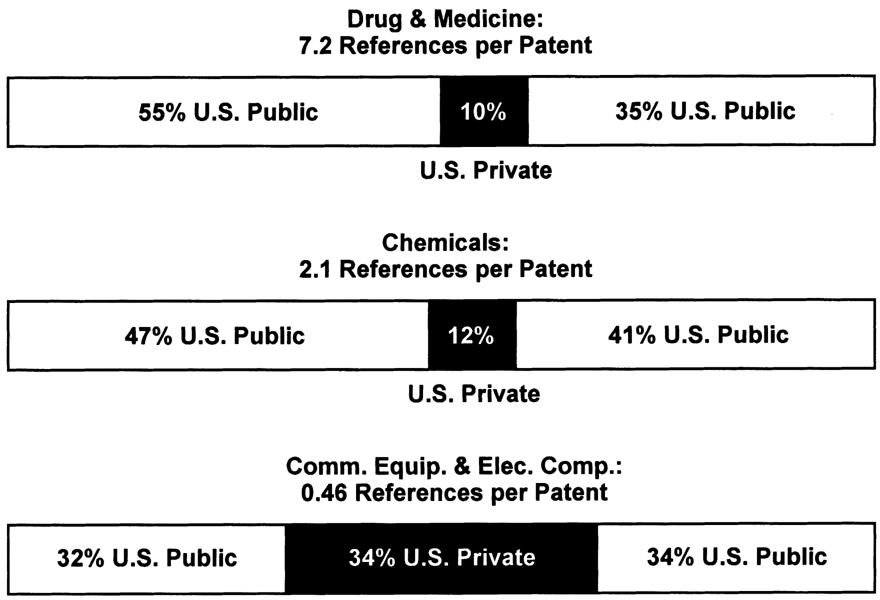

FIGURE 6.9 Sources of science base for patents in drugs and medicine (top), chemicals (center), and communications equipment and electrical components (bottom) for 1993-1994 patents.

science papers cited on the front page of U.S. industry patents had their origins in publicly funded science—in universities, federal laboratories, federally funded laboratories, hospitals, and research institutes.

DISCUSSION

Audience Member: Do you have any direct evidence that high citation count is related to commercial importance?

Francis Narin: The answer is no. We have one particular client, a large industrial client we have been working with for almost 10 years supporting their cross-licensing. The client calls us at least once a day, and we have people there with whom we work on a regular basis. We have repeatedly asked them if they would tell us which of their patents are most important commercially and which ones are not. They simply will not identify the important ones.

However, there is one interesting study that was done by Professor Michael Sherer at Harvard and just submitted for publication. He studied German patents that were applied for in 1977 and had been renewed for 18 years (full term). He went to the companies and got them to say what the economic value of the patents was, in millions of Deutsche marks. He then looked at the relationship between economic value and whether the patents were cited, both within the German patent system and for the patent equivalents filed in the United States. It was clear that the patents that had large economic value had many more citations than the ones that did not. It is nearly a step function. Most of the patents are not highly cited, but a small number are, and most of those were valuable patents.

Also, there is a very small set of patents that has been adjudicated by the courts as pioneering patents. Those patents are cited five times as often as other patents.

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I have the impression (and that is all it is) that when we look at patents, if the patents are cited three times as often as the average (after 10 or 15 years, the average patent is cited five or six times, depending on what area it is in), that is, 15 to 20 citations, then that patent is likely to be of technological and economic importance. This set is, perhaps, 10 percent of the patents, but I have no hard data. No one has ever been willing to provide the material with which we could do that analysis. However, companies must think patents are important. It costs at least $10,000 to get a patent, and there are 100,000 or more new patents every year.

Audience Member: Does the cost of getting a patent affect the decision to apply?

Francis Narin: We do not get involved in it, but most of our clients have committees and groups that do that, and it is a very difficult decision. You are talking about lots of ideas that come up, and it is going to cost at least $10,000 to obtain and maintain a patent. The clients clearly have to be selective. I think they select on the basis of whether the patent will protect their position, but I do not know of any studies or hard data on that. It is a tough question, and one thing you do notice is that when companies are prosperous, they tend to obtain more patents (they have more patent attorneys and will get more patents). When things get tough, they cut back. Those are interesting artifacts in the system.

Audience Member: Aren't you exposing your technology by getting a patent?

Francis Narin: That is true, but it also provides protection. Remember, a patent is a bargain between yourself and the government. The government protects your rights to the invention.

Audience Member: But you are making it public.

Francis Narin: The reason the government grants the patent is so that it will be public, so other people can improve on it. That is the whole idea of a patent. What you do see is, in a company that has a strong patent position, there are clusters of interlinked patents. When companies have a weak patent policy, we say that they have a "chicken pox" patent strategy. There are two or three patents here, one over there, and so on, and they do not connect to one another. But when you have a company with interlinked patents, and Alza, a company with highly specialized technology in drug delivery, is one of our classic examples, all their patents are incredibly interconnected. They have built a patent structure that would make it very, very difficult for somebody else to penetrate into that technology.

Audience Member: Is the difference between U.S.- and foreign-invented patents due to differences in the patent systems?

Francis Narin: Yes, that is another aspect of this issue that I didn't talk about at all. For patents filed in the U.S. system, you expect that the parameters will be the same. However, if the patent originates in Germany, almost always there is a German priority patent, and that certainly influences the way it is written. I think that, in fact, there is a genuinely strong connection between university and government research and industry research in the United States, and that the connection is much stronger (especially historically) than the linkage between, for example, the universities and industry in most areas in the United Kingdom, not necessarily in biotechnology but in most other areas of the United Kingdom. There is a classic paper of perhaps 30 years ago when somebody looked at the British industrial chemical journals and British university chemistry and found that there were very few citations from the

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industrial chemical journals to academic chemistry—a complete disconnection between the two communities. I don't think you would have found that in the United States even then.

Audience Member: Are there differences between big and small companies?

Francis Narin: Yes, but I have not looked in detail at big versus small companies to see which ones are more science linked, except that in the biotech area the small companies are. The companies that have in the past had this "chicken pox" patent pattern generally have not been companies that had a very strong technological base.

Audience Member: Do different groups of companies benefit from public science differently?

Francis Narin: We haven't made that cross-link. I do know that in the biotech area the smaller companies are much more science linked than the big ones, and you can understand why. A big company has lots of old technology that it has to protect; a company like Merck will have lots of process patents that are not at the leading edge of biotechnology, whereas at a biotech start-up, everything is based on leading-edge research. The interesting question is whether those differentiations will give some way of predicting whether a company is going to do well.

Audience Member: Do patents relate to company success?

Francis Narin: We are just beginning to explore that. We are starting to look at IPOs, initial public offerings. One interesting aspect is whether companies that have been successful and had successful IPOs in biotech are the ones linked into public science. I think that they will be, but I don't yet have any hard data. Right now I am trying to get a project going to take a look at that.

Audience Member: Wasn't science developed by industry long before it was supported by public agencies?

Francis Narin: I think the term "science" was used differently in those days. If you read the history of the last century, they were really talking about technology and not science. For example, during the industrial revolution in the United Kingdom, they used the term "science" to describe what we would call "technology."

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7

Research as a Critical Component of the Undergraduate Educational Experience

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INTRODUCTION

I would like to consider the theme of this paper—the undergraduate research experience—in terms of four separate considerations: Does it make a difference? For whom? How do we know? What is the evidence of the last 10 years? I will begin with the answers to the first two questions. Yes, undergraduate research does make a difference—it makes a difference for students, for undergraduate curricula, for chemical science both as an academic discipline and as a profession, and for the nation and society as a whole. But how do we know this? This question brings us to the main focus of this workshop—namely, *assessment*. I see my role in this forum as addressing not assessment of the value of *research* per se, but rather assessment of the value of the (undergraduate) research *experience*. Here, I will attempt to show how the first two questions posed have been answered (or can be answered), and by what means—namely, by a variety of assessment techniques applied during the past 10 years. These include data from the records of undergraduate research—site administrators in academia, survey responses, personal histories, and anecdotes, as well as the general impressions that most people involved with undergraduate education have formed over the years.

However, first of all, you may ask: "Why 10 years?" Suffice it to say that I chose that time frame because the National Science Foundation's REU (Research Experiences for Undergraduates) program got off the ground in 1986 with the first sites in place by 1987—hence this program, which specifically focuses on undergraduate research experiences, has been in operation for 10 years (see Box 7.1 for a short history). Now I do recognize that many undergraduates participate in research without REU funding. I picked REU because it represents a national recognition of the importance of undergraduate research and a commitment to provide research experience for undergraduates. In this sense, it marks a watershed or point of reference—and it started 10 years ago.

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BOX 7.1
History Leading Up to the Research Experiences for Undergraduates Program

A bit of history may put the Research Experiences for Undergraduates (REU) program in perspective. After Sputnik made Americans focus again on what it might take to maintain the technological and scientific leadership enjoyed during and immediately after World War II, many projects were initiated to strengthen the quality of science education. At the same time, the National Science Foundation established a program that would enable undergraduates to enhance their science education in the major. This was the URP or Undergraduate Research Program, which provided funding for students to spend a summer in active research laboratories, often away from the home campus. The locations where students went—usually at research universities—were termed project sites. Typically, 10 students were supported at each project site for 10 weeks. This program was very active throughout the 1960s and 1970s. For example, I remember that during the late 1970s when I administered our project in the University of Kansas chemistry department, we also had projects in the biochemistry department, the chemical engineering department, and the medicinal chemistry department, just on our one campus. It was highly organized at the NSF, with uniform application deadlines and starting and ending dates across the country. It was generally thought that the program was a worthwhile way to introduce young scientists to the kind of practical experience that might allow them to make a rational career decision. Nevertheless, this program was one of the first casualties after the national elections of 1980.

By the mid-1980s there was much alarm at the decrease in the numbers of young people going into science careers. For example, data gathered by the American Chemical Society showed a 25 percent decline in graduation of B.S.-level chemists during the early 1980s, and the National Science Foundation was given the go-ahead from Congress to come up with plans to help counteract this trend. What emerged was a new program, Research Experiences for Undergraduates, or REU. Again, stipends were provided to students so that they could enjoy full-time commitment to research during the summer months at research sites. There were some noticeable differences from the older URP program. Sites were considerably more autonomous and given scope to tailor their programs to suit their calendar and likely participants. Deadline dates varied. Some programs ran for 8 weeks, some for 10. Some programs focused on one area of chemistry only—for example, materials science or nuclear chemistry; some on particular target groups, such as women or Native Americans. In all cases, underrepresented minorities and women were clearly encouraged to apply and often actively recruited.

UNDERGRADUATE RESEARCH DOES MAKE A DIFFERENCE

Does undergraduate research make a difference? To whom does it make a difference? From what point of view? I will look at this question successively from the point of view of the discipline, the profession, the nation, and the student. Let us begin with the discipline of chemistry.

If I had asked, for example, "Does organic chemistry (in the undergraduate curriculum) make a difference, and what is the evidence for that in the last 100 years?," you would, of course, think the question ludicrous and the answer obvious. The answer, I think, is equally obvious with respect to undergraduate research. Chemistry as an undergraduate discipline clearly has two components—one of *content* and one of *method.* It is a body of knowledge (complete with concepts, symbols, and facts)

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traditionally learned through the *classroom lecture*. It is also a collection of techniques (complete with observation, manipulation, data collection, and interpretation) traditionally learned in the *laboratory.* But chemistry is also about *discovery and application*—a dynamic synthesis of content and methodology to find out something, or make something, or do something new. This, of course, is what chemistry is all about, and what we call *research.*

So one way research experience might be said to "make a difference" is that it enhances, completes, or rounds out a science major's education. It helps develop an understanding of the complexity and context of science, not merely the content and methods. It is a way to validate the previous years of learning (analogous to the years of French grammar, vocabulary, dictation, and the reading of literature that can lead to communicating with others in French and writing one's own poem) and of contributing at last, in however small a way, to a body of knowledge that we call chemistry. What we have concluded here, then, is something I really believe should apply to all disciplines—but above all to those in the natural sciences: *Research is a necessary component of a bachelor's degree education.*

This is not a new idea—at least not in chemistry. Many undergraduate chemistry programs require undergraduate research; many others strongly recommend it and help make the opportunity available. This was also the conclusion arrived at by a group of NSF-REU chemistry site directors at a workshop held here in Washington in 1990, namely:

> Chemistry is a dynamic experimental science for which research is an inherent component. Such a discipline requires "learning by doing," an inquiry approach, and an apprenticeship experience. A student's education in chemistry is incomplete without research experience.

Similar statements appear in a report issued by an analogous biological sciences site directors workshop held in 1993:

> Research not only serves as an important adjunct to the traditional classroom, but is absolutely essential in modern, quality, undergraduate science education.

They also are found in the American Chemical Society's Committee on Professional Training guidelines for professional certification, and from the Council of Undergraduate Research.

Conclusion 1. Undergraduate research experience makes a difference in the discipline of chemistry because it belongs in the curriculum, just as a lecture course in organic chemistry or a laboratory course in methods of analysis does.

Of course, the finding of Conclusion 1 is not what the general public, or even many chemists, often mean when contemplating the "does it make a difference?" question. Instead, what they are thinking is: Does exposure to undergraduate research end up producing more and better trained chemists who will contribute to the economic health of the nation and the quality of life for its citizens? This, I am certain, is what Congress had in mind when it reenacted funding for undergraduate research experience in the mid-1980s. Here the questions are: Are we capturing people—that is, are more people majoring in chemistry? And are we retaining them so that they go on to work as chemists, or go on to graduate school, and then opt for research careers?

Let us look at one source of evidence relating to how undergraduate research bears on these issues. This information comes from records maintained for the past 10 years of undergraduate research and REU-site activity in chemistry at the University of Kansas. We have had the good fortune to be funded as a Chemistry NSF-REU site for the past 10 summers, and each year we bring in 10 to 12 students from other colleges who join about an equal number of our own undergraduates for 10 weeks of immersion in intensive and genuine research, including formal presentation of results.

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TABLE 7.1 Postbaccalaureate Activity of Participants in the Chemistry Summer Undergraduate Research Experience at the University of Kansas from 1989 to 1997

Activity	1988-1990	1991-1993	1994-1995	1996-1997
Graduate school (chemistry/biochemistry)	40 (65%)	52 (88%)	31 (70%)	162 (75%)
Medical school	15 (24%)	2 (3%)	8 (18%)	32 (15%)
Industry (B.S. degree)	4 (6%)	1 (3%)	2 (5%)	9 (4%)
Undecided/unknown/other	3 (5%)	4 (7%)	3 (7%)	13 (6%)
Total	62 (100%)	59 (100%)	44 (100%)	216 (100%)

NOTE: Data based on stated plans of college seniors, plus longitudinal tracking of information. National Science Foundation's Research Experiences for Undergraduates (NSF-REU) program plus University of Kansas undergraduates.

Polling these students upon leaving the program and tracking them for subsequent years has generated the information summarized in Table 7.1. It is clear that a very significant number (at least 75 percent of the 216 students who have participated in our research programs since 1988) go on to advanced study in the chemical sciences, and another 4 percent go into chemistry-related jobs. If we compare these students with our majors in general, including those who do not participate in research, the sum of those two numbers is noticeably lower: closer to 50 percent. Similar results are reported at other schools—for example, by Professor John Hogg at Texas A&M: "I estimate that up to 75 percent of students who participate in our NSF-REU program attend graduate school"—and at meetings and conferences.

Conclusion 2. Undergraduate research experience makes a difference by increasing the numbers of individuals who choose careers in chemistry or who go to graduate school in the chemical sciences.

In addition to societal benefits accruing from a larger pool of talented people entering the chemical professions, we should also consider the various indirect benefits to society that might accrue from people educated in curricula involving research experience. For example, are more informed scientists and nonscientist citizens emerging as public school teachers, or as policy makers, communicators, journalists, science advocates and lobbyists, elected officials, critical thinkers, problem solvers, and simply citizens who are science literate? Although they certainly can be obtained, data bearing on these considerations are not as easy to come by. I have no special information in this regard to discuss today, as important as it might be for assessing the value of the undergraduate research experience.

Not unrelated to the concept of indirect benefits to society is the question of the inclusion of all of our citizenry. Does undergraduate research experience have an effect on the numbers of underrepresented people who become productive scientists or who join the ranks of the scientifically literate in this country? In other words, what about the impact of undergraduate research on minorities and women? My own data on the minority question are very limited, so I will give examples only as they relate to women. Our 10-year REU data show a real increase in the percentage of women applicants, such that it is now about 50 percent, and an increase in the percentage of women participants. Since research

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experience has been shown to have a positive influence on later graduate school and career choices (influence that seems not to be particularly gender specific), it follows that more women doing undergraduate research will lead to more women doing science later. Since the numbers are relatively small each year (10 to 12 students), there is a fair amount of scatter. Nevertheless, the trend is clear.

Conclusion 3. The last decade has shown a clear increase in the numbers of women participating in undergraduate research and attending graduate school.

Also, one might ask, has undergraduate research made a difference from the perspective of the student? For example, has it increased his or her understanding and appreciation of the major and of the discipline of chemistry? Has it helped in making career decisions? Has it helped in the decision to go to graduate school? Has it helped define an area of interest for future study or work? And, if a student did go to graduate school, has it made adjustment easier? Did it increase retention?

In 1995, I conducted a survey of current chemistry graduate students with the help of individuals from two other institutions, John Hogg at Texas A&M University and Dale Hawley of Kansas State University. A number of questions relating to those just posed were on the survey. There were 129 respondents, with 110 (85 percent) of those having engaged in research during the undergraduate years. There were 41 respondents from the University of Kansas (about 45 percent of the graduate students), 20 from Kansas State, and 70 from Texas A&M (about 25 percent of the graduate students). Details of the characteristics of those completing the survey forms are given in Box 7.2.

BOX 7.2
Characteristics of Students Completing the Three-School Survey

The students completing the three-school survey (the University of Kansas, Kansas State University, and Texas A&M University) were 61 percent male, 39 percent female, 15 percent international, and about 5 percent members of recognized U.S. ethnic minority groups. All of them had been chemistry majors as undergraduates, and 74 percent had earned B.S. degrees. Slightly more were graduates of 4-year colleges (46 percent) than Ph.D.-granting universities (39 percent). Of these students, 85 percent had engaged in research during their undergraduate years, and 12 percent had done so for 2 years or more.

Of those who had participated in undergraduate research, 95 percent had done so in the field of chemistry, 45 percent had worked in two or more different research groups, and 32 percent had been part of formal summer internship programs. About 23 percent had had their research experience only in the summer months, while almost 50 percent had done it mainly during the academic year at the home institution. More than half (58 percent) reported having received compensation for their work at one time or another. Most of the work was “wet” laboratory chemistry (87 percent) as opposed to computational or theoretical chemistry. Fifty-eight percent of the students had had at least one off-campus experience at a college or university, with an approximately equal distribution among the two types of institutions.

Of the respondents as a whole, 93.5 percent were planning careers in chemical science: 76 percent in research (20 percent academic, 40 percent industrial), and 13 percent in teaching with no significant research component. The great majority (84 percent) were going on for the Ph.D. degree, with 16 percent planning to seek a postdoctoral appointment thereafter. The remaining students were divided between staying for the master’s degree only (5 percent) or transferring out of chemistry altogether (3 percent). (Not all students responded to all questions.)

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Based on data obtained in the survey, where the data refer to those students who had participated in undergraduate research, the following can be observed:

- Less than 2 percent reported an unsatisfactory undergraduate research experience. We have no data as to numbers of students with such negative experience who did not go on to graduate school, but the data indicate that of the 85 percent of students in graduate school in our survey who had prior research experience, at least 97 percent had had a positive exposure.
- For 77 percent of the students, undergraduate research was a contributing factor in the decision to go to graduate school. For 44 percent, it was the major factor.
- Some 60 percent found that research experience helped in deciding which graduate school(s) to apply to.
- An undergraduate research experience was seen as helpful in adjusting to graduate school (56 percent) and in getting started with graduate research (52 percent).
- Almost 90 percent considered the experience useful in helping them decide on a general area of research to pursue in graduate study.
- About two-thirds (67 percent) believed that prior research exposure helped them to "stay the course" and remain in graduate school.

Interestingly, the data showed no noticeable gender distinctions; that is, the responses from males and females were not significantly different. (The only instance where a difference was noticed was in the question about postdoctoral plans.)

Conclusion 4. Undergraduate research experience makes a difference for both men and women chemistry students by favorably disposing them toward graduate school, helping them select a school and area of research, better preparing them for graduate education, and influencing them to remain and complete a graduate degree.

We should also be concerned with questions having to do with other, more intangible benefits to the student resulting from undergraduate research experience. Such intangibles may relate to the establishment of mentor and peer relationships and of networks of advisors, co-workers, and friends. They may equally relate to the personal growth and increased self-confidence developing from an experience of common endeavor, and perhaps of real accomplishment and productivity. They surely should relate to the enhanced personal aspirations and broadened horizons that we believe may come from exposure to scholarly work, professionalism, dedicated scientists and motivated peers, and the joys and trials of discovery.

In this case, all the data we have are fairly anecdotal, coming from conversations or other communication with former students or their research advisors. I will illustrate by describing two individuals. Following his sophomore year at the University of Northern Iowa, the first student came as a 1988 summer REU participant to our department. He chose to work on an organic synthesis problem and, as he put it, "may have gotten a few drops of product." Nevertheless, he got a *lot* out of the program and still maintains close contact with most of the other participants who were with him back in 1988. After selecting a graduate school, because he remembered being captivated by one of our Friday seminar talks given by one of our faculty that described gas-phase mechanistic studies, the student made the important decision to go into gas-phase physical organic chemistry. He has just completed a postdoctoral stint, has developed methods that directly observe transition states, and has just started his academic career as an assistant professor at a major research university. This is an example of the profound influence of undergraduate research on a career path.

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The second student is currently a junior at the University of Kansas and is a chemical engineering major, with an interest in biomedical engineering and a desire to attend medical school. She was a student in my organic chemistry class last year—the very top student out of a class that began with 450 others. She had never done research. Last summer, partly with my assistance, she located an internship working on a biochemistry project at the University of Cincinnati Medical School and returned, glowing with enthusiasm: "I know I want to do research." The director of the program there wrote me the following on July 31, 1997:

> [She] has turned out to be exceptional, intellectually and technically. Considering that she had no background in gene transcription and had never held a pipette, [her summer research advisor] is amazed. On Thursday, she will present the results of her research at a minisymposium.

For both of these people, there can be little doubt as to the *value* of the undergraduate research experience. Further evidence comes from written comments provided by the students who completed the three-school survey described above, just a few of which I will take the time to read now.

> I participated in research at a major pharmaceutical firm, at a different university for a summer, and worked in a lab at my university for 3 years. I consider the experience gained to be the *most* important part of my undergraduate experience.

> Even though I did some research at my small college prior to doing research at a big university, I had no idea what "real" research was like. The research work at the big university influenced more than 95 percent of my decision.

> My undergraduate research helped to integrate me with the chemistry faculty and resulted in 2 full papers which have since appeared in peer-reviewed journals.

(You will note that I have not addressed research output or productivity, although we seem to average about 0.5 papers per student in the program.) Another comment from a student:

> I must stress the importance of the research experience in making students an integral part of the department and converting them from bench warmers in class to productive, thinking scientists.

> A research experience is *critical* to the decision process of selecting a division of study, and helping to decide whether a final goal of academic or industrial [work], teaching, or research is desired.

Conclusion 5. Personal testimony, informal communication, written comments and responses to surveys, and the experience of mentors and advisors all indicate that undergraduate research experience makes a positive difference in the education and the professional and personal development of its participants.

SUMMARY

A variety of assessment methods, some more quantitative than others, can be applied to the question of determining the value of the undergraduate research experience. Results obtained and discussed today indicate that such experience provides an essential component of science education, contributes to more informed career decisions, generates more students choosing careers in chemistry, promotes success in graduate programs and probably future work in chemistry, and results in increased personal confidence and a better appreciation for and understanding of science. Furthermore, it contributes to the nation's research enterprise by playing an important role in producing more, better prepared, and better qualified scientists for the future.

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DISCUSSION

Beverly K. Hartline, Office of Science and Technology Policy: I definitely share the enthusiasm for the value of undergraduate research experiences. But I am very curious to know if there are any data on the performance of these students in courses subsequent to having had that experience as compared with students who have not had the experience (or even to their own previous performance). Is information available on their actual performance in graduate school? In industry, if they take an industrial job? That is, are they really better prepared? Or has their intuition been improved as a result of the undergraduate research experience.

K. Barbara Schowen: Those are very important questions. I personally don't have any data on how students subsequently perform in industry, and so I can't address that point. Do they do better in their returning courses? I also have no hard data here. However, they return from the experience very enthusiastic about their discipline and are certainly going to be paying attention to their courses. But I can't answer the question, as to whether they earn more "A"s than they did before. I think it is possible to obtain these data. In fact, one of the points of this workshop is to determine what metrics should be used in assessing the value of the undergraduate research experience and how to obtain the needed data.

Michael P. Doyle, Research Corporation: Before 1982, the Associated Colleges of the Midwest (which included Carleton, Grinnell, St. Olaf, and Macalester) carried out a similar survey of their undergraduate research participants, and about 650 people responded. Curiously, the same number, about 70 percent, said it was an important determinant of their future career and their success in that career. This survey focused on people who had been out of the program for 5 or more years, and so they had been through school. The data seem to confirm what we have known for a long time.

In your particular case, participation in the Research Experiences for Undergraduates program has a special requirement—namely, that the students have already professed an interest in chemistry, are entering a chemistry program, and are predisposed to go on to graduate school. So, in fact, aren't you just measuring what you expect to find?

K. Barbara Schowen: That is a hard question to answer. The control experiment would be to poll all the people who had not participated in research and find out what percentage of them went to graduate school.

Raymond E. Fornes, North Carolina State University: Many of you are aware that there is a national conference on undergraduate research each year. If you look at the data on students who have participated in that conference (and I think there are on the order of 2,000 papers presented at the national meeting each year), you find that there is a much higher percentage coming from the small colleges—the Carletons, for example—than from the major research universities.

If I understood you correctly, in the REU program, you got five students per year from Kansas and five from other colleges.

K. Barbara Schowen: No, we had 10 to 12 from colleges other than our own. We have about 25 total in the program.

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Raymond E. Fornes: But still, that is on the order of 10 percent of your undergraduate student population in chemistry. So the question is, how do you foster greater participation in research experiences, particularly in the large universities, where such a high percentage of students are in the sciences?

K. Barbara Schowen: That is one thing that I have spent a lot of time doing as an undergraduate advisor in our department. We strongly encourage our students to participate in research, and our department tries to make this happen. Professor Daryle Busch, who is also at the workshop, has a large number of undergraduates working in his group.

From the data that came from the three-school survey, about 45 percent of the students were from large public institutions, and the others were from small 4-year colleges. I believe that the information gathered from the two groups was similar.

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Scholarly Research: Oxymoron, Redundancy, or Necessity?

Jules B. LaPidus
Council of Graduate Schools

INTRODUCTION

In 1991, the Council of Graduate Schools (CGS), an organization of some 420 institutions that grant approximately 98 percent of the doctoral degrees awarded in the United States, published the results of a study entitled *The Role and Nature of the Doctoral Dissertation.*[1] Briefly, the study involved 50 universities, all of which were asked to respond to a survey about dissertations, using whatever campus-wide group the institution usually convened to consider broad questions related to graduate education. In most cases this was a graduate council; in some cases, ad hoc committees were appointed.

The questions were very broad:

- Is there a consensus across disciplines about the distinguishing characteristics of doctoral research? Of the doctoral dissertation? If there is, what is it? If there is not, what are the points of disagreement?
- Are students allowed to use work done in collaboration with others as all or part of the dissertation?
- Can a student's previously published work be included in the dissertation?

Other questions concerned such topics as the time needed to obtain a degree, the role of the advisor, and the final defense.

We also asked that the respondents consider what the ubiquitous adjectives "substantial," "significant," "original," and "independent" really meant in describing doctoral research and dissertations. Someone even revived the old story of the external examiner who claimed to find a dissertation both

[1]Council of Graduate Schools, *The Role and Nature of the Doctoral Dissertation* (Washington, D.C.: Council of Graduate Schools, 1991). Also available online at <www.cgsnet.org>.

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significant and original, but noted that "unfortunately, the part that is significant is not original, and the part that is original is not significant."

I do not intend to describe the study in detail, but several points are directly relevant to the topic of this paper. There was some skepticism at first about trying to define the dissertation in a discipline-free way. Some people doubted that they could have productive discussions about the concept of the dissertation across fields as different, for example, as chemistry and classics. As it turned out, there was agreement across all fields that the dissertations served two purposes: to allow students to demonstrate that they could do whatever people in that field did when they did research, and to produce research that constituted a (significant? original?) contribution to knowledge. These two very different skills are generally considered necessary conditions for the award of the Ph.D. degree in most countries and educational systems.

There was, however, some divergence of opinion (mainly by discipline, although occasionally by institution) about what constituted appropriate Ph.D. research. At some institutions, laboratory research on topics that rank high in national funding priorities was considered most appropriate, since they were thought to represent a kind of "peer" consensus of significance. In some areas, research in theoretical areas was valued more highly than research on practical problems. As stated by one physicist, "Ideally, a dissertation project would be self-contained, would allow individual initiative to flourish, and would address philosophically interesting and non-trivial issues." In practice, many styles of research in physics are not compatible with these "ideas." A graduate dean put it more generically: "Whether the student works alone or on a team, the research project should be an original, theory-driven investigation characterized by rigorous methodology and capable of making a significant contribution to knowledge about the subject under study."

Historically, research has been the characterizing element of doctoral education. Whatever else was done, research was always the centerpiece. Doctoral students had to complete, as in Ireland, "[a] substantial thesis making a significant, original contribution to knowledge."[2] This was the only requirement for the degree, and much the same held true for almost all European and Asian universities. (It is interesting and perhaps revealing to note that in these countries, demonstration of the ability to do original and independent research was referred to as "preparation for an academic career.") Although some or even most students might take courses, the main tasks (as stated, for example, in Japan) "are to submit a doctoral dissertation and pass an oral examination within three years."[3] In some countries, notably China and Japan, it is possible to obtain the degree without being at the university, by submitting evidence of research accomplishments acceptable to the faculty. There are variations of this in other countries, including the United States.

The idea of required courses, or of a graduate program involving both research and coursework, has been associated primarily with the United States and Canada and is gradually becoming the norm in most places. The principal reasons for this are related to the desire to provide students with a broader background so that they will be better prepared for a wider variety of career options, and to ensure some general understanding of their fields, particularly as the number of students increases. As Stuart Blume points out, "The attractiveness of the North American model has derived from the fact that it has seemed able to ensure effective and efficient training of researchers on a much greater scale than has been usual

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[2]CEPES Studies on Higher Education, *The Doctorate in the Europe Region* (Bucharest: UNESCO, 1994).

[3]Burton R. Clark, ed., *The Research Foundations of Graduate Education: Germany, Britain, France, United States, Japan* (Berkeley: University of California Press, 1993).

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in European universities."[4] In other words, graduate education is becoming more programmatic as study demand and employer diversity increase.

In Britain, the discussion has centered on the relationship between research and research training, with strongly held views on the role of each in doctoral education. An excellent summary of some of the arguments on either side appeared as part of a publication on postgraduate research training in Europe.[5] A few of these are worth mentioning here. Those in favor of more training argue that it does the following:

- Provides a more structured transition between undergraduate and graduate school;
- Provides a focus for student-student and student-faculty interaction—lowers isolation;
- Overcomes narrowness;
- Provides a broad range of research skills, not just those associated with dissertation;
- Increases employability.

Those opposed to a formal training component believe the following:

- There is no natural set of techniques that would fit all students in a discipline;
- Courses would be general, and students' needs are specific—a waste of valuable research time for students;
- The imposition of formal, compulsory, taught courses calls into question the whole definition and meaning of a doctorate;
- The imposition of formal training is the result of state-induced restructuring of the university system driven by a narrow conception of national economic need, accompanied by increasing emphasis on the industrial relevance of training.

For the most part, graduate deans and faculty members in the United States and Canada tend to take a broad view of dissertation research projects. This is based firmly on the idea that the doctoral research project is an apprenticeship whose major purpose is to prepare students for careers in advanced scholarship and independent research. It is not at all clear, however, that this goal is universally shared. Different conditions—scientific, economic, political, academic—can markedly alter the concept of doctoral education. Two examples will suffice. As stated in a government policy paper on science produced in England in 1993, "The government is concerned that the traditional Ph.D. does not always match up to the needs of a career outside research in academia or in an industrial research laboratory. A minority of those studying for a Ph.D. in science, mathematics, and engineering can realistically expect a long-term career in university research. The majority will move into other fields."[6] In an interview in 1994, Bruce Alberts, president of the National Academy of Sciences, was asked if we should be producing fewer Ph.D.s. His answer was that "if we're moving toward training with a very narrowly focused Ph.D. that's really designed for people who will be *independent investigators*, then we shouldn't

[4]Stuart Blume, Organisation for Economic Cooperation and Development (OECD), *Problems and Prospects in the 1990s in Research Training: Present & Future* (Paris: OCED, 1995).

[5]Report of the Temporary International Consultative Committee on New Organisational Forms of Graduate Education, *Postgraduate Research Training Today: Emerging Structures for a Changing Europe* (The Hague: Netherlands Ministry of Education and Science, 1991).

[6]Chancellor of the Duchy of Lancaster, *Realizing Our Potential: A Strategy for Science, Engineering and Technology* (London: Her Majesty's Stationery Office, 1993).

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be training so many."[7] He went on to talk about a different kind of graduate education that is much more flexible so that its graduates could become K-12 teachers or journalists or be employed on the business side of technical companies.

These statements represent a real departure from a view of doctoral education as an apprenticeship based primarily on the shared research interests of the faculty to one that holds the career preparation of the students paramount. They reflect a perception that doctoral education is much too narrowly focused on research, and not enough on education. The report *Reshaping the Graduate Education of Scientists and Engineers*[8] arose in part from this kind of concern.

The American research university was defined by Robert Rosenzweig some years ago as a place "whose mores and practices make it clear that enlarging and disseminating knowledge are equally important activities and that each is done better when both are done in the same place by the same people." [9] This once unique and now almost stereotypical kind of university provides a setting for graduate education that involves doing research and learning to do research as parts of the same process. I believe that, in addition, doctoral education must prepare students to understand their work in a broader context than that circumscribed by their dissertation project—that is, at least in the context of their discipline, and preferably as part of science and scholarship in general.

With regard to this issue, the defining moment for me occurred at a meeting of graduate deans in Canada, where we were discussing the CGS dissertation study. One of the deans rose to his feet, identified himself as a chemist, and said that he saw no use in the traditional solo dissertation. This is where the student takes a problem from beginning to end, dealing with the historical background, the literature review, the analysis of various approaches, and so on, and then writes up the entire story. He went on to say that nobody did research that way in the real world, where scientists worked in teams and published their work as it was done, in the form of short papers. He concluded that since the student probably would never again do the comprehensive kind of project usually represented by the classical dissertation, there was no reason why he or she should do it while a graduate student, since he believed that scientists would be likely to have the opportunity to do that kind of research project at least once during their professional career. This elicited a more vigorous but by no means overwhelming reaction from the assembled deans.

This small sample hardly provides conclusive support for either position, but it leads to some useful consideration about doctoral research. The basic question is, What is the purpose of research as a part of doctoral education, and how does what is done relate to that purpose? How that question is answered provides some perspectives on graduate education.

SCHOLARLY RESEARCH: OXYMORON, REDUNDANCY, OR NECESSITY?

In a recent paper,[10] I suggested that specifying research as the essence of doctoral education was probably not sufficient. Research is done in many places and for many reasons. However, in a university

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[7]Bruce Alberts, as interviewed by Daniel S. Greenberg in *Science and Government Reports,* October 15, 1994.

[8]Committee on Science, Engineering, and Public Policy (COSEPUP) of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, *Reshaping the Graduate Education of Scientists and Engineers* (Washington, D.C.: National Academy Press, 1995).

[9]Robert M. Rosenzweig, *The Research Universities and Their Patrons* (Berkeley: University of California Press, 1982).

[10]Jules B. LaPidus, "Scholarship and the Future of Graduate Education in Science and Engineering," paper presented at Radcliffe/CPST Conference on Science Careers, Gender Equity, and the Changing Economy, American Institute of Physics, College Park, Md., October 1996.

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it is not enough to do research just to solve specific problems, or to find answers to specific questions. Research in universities has to extend what we know, help us to determine what questions to ask and what problems to solve, and in the process, educate people prepared to undertake these important tasks.

But the research experience has to extend beyond mere technical training. This has been expressed most clearly by John Ziman. "To be a member of a team directed by a distant and very busy leader, building just one technical link in a complicated experiment, is an inadequate apprenticeship to the art; it is as if the pupils of Rubens were to be accounted artists after five years of painting in the buttons on his larger compositions. High technical standards may be achieved by the student, without a grasp of the deeper intellectual issues."[11] The point is that graduate education must be more than a simple apprenticeship, and that research in this context must be more than a technical exercise for producing research results. It must be a vehicle for preparing scholars.

In thinking about these terms, it struck me that "research" is almost always used to refer to what scientists do and "scholarship" for what humanists do. Research is done in laboratories by people in white coats; scholarship is carried out in libraries by people in shabby tweed jackets with elbow patches. But these are Hollywood clichés; in most cases, the two activities—research and scholarship—are parts of a process so unified in the minds of its practitioners that they might view the term "scholarly research" as a redundancy. On the other hand, many scientists seem to be uncomfortable with the word "scholarship" and may think of research as "hard" and scholarship as "soft." To them, "scholarly research" probably would be an oxymoron.

I have concluded that there is a difference between the terms, and it is this: Research is something you do; scholarship is the way you think about it.[12] Another way to state this is that research is a process for obtaining information, and scholarship is a process for converting information into knowledge. That is why research can be done by teams and scholarship cannot, and why we have research assistants and not scholarship assistants. Research is a verb as well as a noun. You can do research; you can research a topic. Scholarship, on the other hand, is always a noun, taken to mean "the methods, discipline, and attainments of a scholar; learning; erudition." You don't do scholarship or "scholarship" a project.

John Armstrong, in a perceptive article entitled "Rethinking the Ph.D.," stated what I believe to be the essence of the issue: "Many new Ph.D.s have much too narrow a set of personal and career expectations. Most do not know what it is they know that is of most value. They think that what they know is how to solve certain highly technical and specialized problems. Of course, what they really know is how to formulate questions and partially answer them, starting from powerful and fundamental points of view."[13] To paraphrase, "They think that what they know is how to do research; what they really know is how to be scholars." At least that is what we hope they know.

Industrial research and technology managers who deal with a variety of different kinds of problems tend to agree with this point of view, recognizing that what has carried over from their own doctoral studies is not the specifics of their dissertation research but rather the generalizations of the scholarly process: how to read and listen critically, define and analyze problems, determine what the important questions are, decide what research needs to be done and how to do it, understand what the results mean, and learn from the entire experience. It is this process of scholarship that forms the irreducible core of

[11]John M. Ziman, *Public Knowledge: An Essay Concerning the Social Dimension of Science* (Cambridge: Cambridge University Press, 1968).
[12]Jules B. LaPidus, "Scholarship and Research: Gresham's Law Revisited," *CGS Communicator* 298 (January):3, 1996.
[13]John A. Armstrong, "Rethinking the Ph.D.," *Issues in Science and Technology* (Summer):19-22, 1994.

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graduate education. Academics, many of whom continue to work in areas closely related to their doctoral research, may not always remember this.

During the last few years, several reports and studies have suggested a variety of ways to improve the education of graduate students.[14-16] These include offering additional coursework in cognate areas, reinstituting minors in related fields, providing internship experiences in academia or in industry, and developing a greater number of branching options in graduate programs. Whatever is done must add to, rather than substitute for, intense involvement in the processes of scholarly inquiry. That is the structural element upon which graduate education is built and around which scientists and scholars are formed.

Universities provide a unique setting and a particular context for research, and that is what may well define the essence of doctoral education, which is more about education than training, more about knowledge than information, and ultimately, more about scholarship than research. My conclusion, then, is this: Scholarly research, not just research, is the critical component in graduate education. Scholarly research goes beyond finding answers to questions like, how fast? or how many? or how big? It must deal with, why? and what if? and so what? There is a balance that can easily shift too far in the direction of short-term answers and away from long-term questions. If this happens in graduate programs, universities will have turned from the education of scientists to the training of technicians, and society will be the ultimate loser.

DISCUSSION

Charles G. Moreland, North Carolina State University: I wanted to make a comment about the multidisciplinary team approach. I would like to suggest that not only should that team be made up of people from the university, but it also ought to include people from outside the university whenever possible. This broadens the educational component for the student. I am also not as sure as you that the team approach doesn't involve scholarship. People are not just gathering information separately. They are exchanging information and learning how to think like others on the team. So, they are developing a thought process that goes beyond what they know and how they put facts and concepts together.

Jules B. LaPidus: I have had similar discussions with a number of people about this particular point. I keep coming back to [Daniel] Boorstin's comment. I think that you end up engaging in discussion with your colleagues about information, and you learn much from the process. But at some point something has to happen inside the individual's head in terms of really knowing and understanding something. Nobody can give that to you; nobody can really tell it to you. You have to integrate information in your own head, and I see a parallel there in terms of scholarship and knowledge.

John T. Yates, Jr., University of Pittsburgh: I was really impressed by your emphasis on the need for coherence in the Ph.D. program and, of course, with the funding scenarios and the typical time constant of many U.S. funding scenarios that is becoming more and more difficult. I was in Denmark a

[14]Committee on Science, Engineering, and Public Policy (COSEPUP) of the National Academy of Sciences, the National Academy of Engineering, and the Institute of Medicine, *Reshaping the Graduate Education of Scientists and Engineers* (Washington, D.C.: National Academy Press, 1995).

[15]Sheila Tobias, Daryl E. Chubin, and Kevin D. Aylesworth, *Rethinking Science as a Career: Perceptions and Realities in the Physical Sciences* (Tuscon, Ariz.: Research Corporation, 1995).

[16]Roger Geiger, "Doctoral Education: The Short-Term Crisis vs. Long-Term Challenge," *The Review of Higher Education* 20 (Spring):239-251, 1997.

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couple of years ago and happened to be there when a major program in Denmark was being initiated. It was a 5-year program, and the first assignment to the group of people responsible for the program was to figure out what was going to happen for the next 5 years of the program. We should see more of that thoughtfulness in the United States.

Jules B. LaPidus: One of the interesting facts—I mentioned it briefly here—is that the so-called American model of graduate education is becoming more and more widespread. In 3 weeks, in Beijing, there is an international conference on graduate education. This is unusual in that, in most countries around the world, there haven't been conferences that have talked about graduate education generically, because in most places there haven't been a lot of people who think about graduate education generically. Graduate deans tend to do that because it is their job, but there haven't been graduate deans in many countries. There haven't even been graduate schools.

At last count there are now about 100 members in the U.K. Council for Graduate Education. The systems are getting bigger, and people are beginning to say, as they did in Denmark, that a coherent structure is needed. I was in Denmark about 4 or 5 years ago, as well as Sweden and Norway, talking about changes in the graduate education system in the United States and found very similar things going on there.

Charles Zukoski, University of Illinois at Urbana-Champaign: With a view from the trenches, I think I can point to Ph.D. research that covers the span that goes from training to scholarly research and that this isn't uncommon. What I took away from your comments was that we ought to redefine and narrow the Ph.D.—to go back to more of a classical definition, which is scholarly research, reduce the number of people who get that degree, and perhaps define a new degree category that involves advanced training. I would like your comments on that proposal.

Jules B. LaPidus: I don't know if I would buy that. You asked me what my view is. I don't think that is quite my view. There are a large number of people who are talking about this concept. The COSEPUP report, if you recall, says there should be three options. One is the traditional doctoral degree, another is to stop at a master's degree, and yet another is to define a more practice-oriented doctoral degree.

Roger Geiger in a recent article talked about the same sort of concept. I wouldn't say that we should forget about the training component and concentrate entirely on scholarly research. What I am saying is that if there is no part of the program that deals with anything broader than the training, then I think you have got a real problem. Then you are not involved in the Ph.D. enterprise anymore but are doing something different. You are training technicians.

I think every graduate program involves some training. One of the big questions in most other countries—take Britain, for example, or Australia—is whether or not the graduate student should do anything other than the dissertation topic. That is where this argument about research training comes in. A lot of people are saying you should know more about chemistry rather than just steroid chemistry. You should have a broader picture of what is going on in chemistry and in science, for that matter. You should have a broader view of that world.

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Assessing University-Industrial Interactions

Richard K. Koehn
University of Utah

The growth of university-industry-government collaborative research and development programs is the single most important development in the character of university technology transfer endeavors since the early 1980s.[1]

INTRODUCTION

There is no question of the widespread perception in both academia and business that partnerships between universities and industry, whether or not forged by the catalyst of government, are important new parts of the landscape of research universities.[2-4] Over the past several decades, we have seen the transformation of the U.S. economy from one reliant on agriculture and manufacturing to one opening up new industrial sectors, like information technology and biotechnology. These are industries that not only are driven by research and development, but also have their origins within the research communities of both universities and corporations. When we look to see where this wealth has developed most rapidly and forcefully, it is in places like Silicon Valley, the North Carolina Research Triangle, Massachusetts Route 128, and even the University of Utah Research Park—locations where major research universities have spawned new companies and industries and created new wealth for the local citizenry.

Our perception that wealth flows from university-industry partnerships is repeatedly reinforced. Former Vice President Walter Mondale, now U.S. ambassador to Japan, recently discussed this point in an editorial in *Science* (1996):[5]

> Among our conclusions was the not-so-startling fact that the primary advantage of the United States—the core of our economic competitiveness—is the unparalleled excellence of U.S. scientific research,

[1]Irwin Feller, "Technology Transfer from Universities," *Higher Education: Handbook of Theory and Research,* vol. XII, John C. Smart, ed. (New York: Agathon Press, 1997).
[2]Jack Miles, "A Modest Proposal for Saving University Research from the Budget Butcher," *Change* (Nov./Dec.):31-35, 1994.
[3]Donald S. Van Meter, "Blue Chip Investments: Assessing Higher Education's Contributions to Economic Vitality," *NACUBO Business Officer* (July):47-50, 1995.
[4]Eugene Wong, "An Economic Case for Basic Research," *Nature* 381:187-188, 1996.
[5]Walter Mondale, "America's Challenge," *Science* 274:899, 1996.

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> engirded by our entrepreneur system. We must recognize that the U.S. university research system is a technology generator for our entire country, creating new technologies that lead to new industries and good new jobs.

Universities generally, under increasing scrutiny to devise measures of productivity of the academic enterprise, have taken up the argument that universities (specifically through their research missions) have significant economic impact. A recent summary published by the National Association of State Universities and Land Grant Colleges[6] emphasizes that its member institutions foster new business, create new jobs, promote innovation, enhance the work force, and improve the quality (that is, the standard) of life—all positive economic forces that generally fall within the public service mission of these institutions. The data offered to support these points are unfortunately anecdotal and in some instances questionable.

In theory, assessing the value of research based on the productivity of university-industry partnerships should be relatively simple: the desirable metric should be in simple economic terms, consistently structured, unbiased by geography, and easily available. Unfortunately, if you search for hard and simple evidence of any universal metric, it cannot be found. In a recent publication of a government-university-industry research roundtable on the subject of developing performance standards and output measures for the research enterprise, we read: "Although measurement lies at the heart of the scientific method, no *universal* [emphasis mine] metrics exist for assessing research."[7] Recently, writing in the *New York Times,* William Broad noted:[8]

> Basic science, the kind that pursues fundamental knowledge for its own sake with no clear vision of how it might be practically applied, has long been considered a prime source of military and economic power. Yet, the exact relationship between science and innovation has been murky since the start of the industrial revolution.

If a metric is not obvious to use in measuring the economic productivity of research generally, that is certainly also the case for measures of the benefits of university research partnerships. For example, referring specifically to these collaborations, the Government-University-Industry Research Roundtable stated in its annual report: "There is surprisingly little empirical information available about the probability of satisfaction or the actual benefits realized by those who engage in collaboration in cross sectors."[9]

So there we have it. Despite our sense that university-industry partnerships have produced significant economic impact, there is disagreement both on the point and on how to measure. We have even heard in other presentations in this Chemical Sciences Roundtable that any claims for a valid metric are simply fiction—hopeful fiction, perhaps, but fiction nevertheless.

The main point that I make in this presentation is that although there is no universal metric for assessing university-industry research collaboration productivity, there are, I believe, some individual metrics that can be used to measure important aspects of these partnerships. To be sure, there are many

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[6]National Association of State Universities and Land-Grant Colleges, *For Every Dollar Invested . . . The Economic Impact of Public Universities* (Washington, D.C.: NASUSC, 1996).

[7]Government-University-Industry Research Roundtable, National Academy of Sciences, *The Costs of Research: Examining Patterns of Expenditures Across Research Sectors* (Washington, D.C.: National Academy Press, 1996).

[8]William J. Broad, "Study Finds Public Service Is Pillar of Industry," *New York Times,* May 13, 1997, pp. B7-B12.

[9]Government-University-Industry Research Roundtable, National Academy of Sciences, *The Costs of Research: Examining Patterns of Expenditures Across Research Sectors* (Washington, D.C.: National Academy Press, 1996).

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metrics that measure little or nothing, and I will discuss these briefly. However, there is emerging evidence that university-industry partnerships are indeed productive in a variety of ways, though little systematic information is provided by the participating industries or universities.

TRADITIONAL UNIVERSITY MEASURES

Each year, the Association of University Technology Managers produces statistics on various measures of technology transfer activity at its member institutions. These measures include the number of disclosures filed, patents issued, and licenses granted, as well as university royalty income from licenses. To my knowledge, these are the only "metrics" that produce standard information for a large number of universities on the possible intensity or purported success of institutional technology transfer programs.

Unfortunately, none of these parameters are accurate measures of economic impact. Therefore, they do not serve as adequate metrics for meaningful measures of productivity of university-industry partnerships. The number of disclosures varies widely among institutions, even differing greatly among years within a single institution. First, assuming all else to be equal (which it is not), the number of disclosures reflects the level of research funding; that is, it is a reflection of the intensity of discovery. A rule of thumb is one or two disclosures per $1 million of research. Second, the number of disclosures at a particular institution reflects to some degree the technology transfer policies of an institution. Does the institution encourage innovation? Does it encourage disclosing potential discoveries for economic reasons? Does it offer an incentive to disclose? If one were to offer a "bounty" for disclosures (say $50 cash to faculty making disclosures, as some institutions have done), the number of disclosures would rise dramatically, at least temporarily. Yet they would falsely represent the potential economic impact of discoveries that ultimately find their way to the marketplace. In short, while tabulating the number of disclosures might be useful for an institution to monitor its own activities, this cannot serve as an accurate metric of any significant variable among institutions.

The number of patents is often claimed to reflect an institution's involvement with industry. However, a closer look at patents shows that "patents are a limited measure of the extent to which technology, much less scientific and technological knowledge, is being transferred to university-industry."[10] A better metric might be the number of university patents paid for by industry. Only a small fraction of U.S. patents are issued to universities: about 3 percent currently, up from about 1 percent 25 years ago. More significantly, academic patents are concentrated in a few "utility classes" and have become more so in recent years with the emergence of biotechnology.

There is, of course, great variation in the cost of pursuing a patent, depending on its complexity. Although the rule of thumb might be a cost of about $15,000, the cold fusion patents pursued by the University of Utah in the late 1980s have cost many times that amount, and they were never issued in the United States! That is the point. The number of patents issued reflects not only an institution's commitment to intellectual property ownership, but also its financial capacity to secure that ownership. Institutions that are active in technology transfer have recognized the necessity of implementing a process intended to identify those innovations with the highest probability for commercial exploitation, in order to minimize institutional patent costs. The raw data on numbers of patents measures nothing of particular relevance to the productivity of industrial partnerships.

[10]Irwin Feller, "Technology Transfer from Universities," *Higher Education: Handbook of Theory and Research,* vol. XII, John C. Smart, ed. (New York: Agathon Press, 1997), p. 11.

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Again, statistics on the number of licenses to companies for university intellectual property reflect an institution's interest in and commitment to technology transfer, but these numbers do not constitute a metric for economic impacts of the research or technology. Most licenses never result in a marketable product. There is great variation in the length of time between the issuance of a license and any revenue that may be generated—usually many years (on average about 8 years), but longer for biotechnologies. Sometimes the license involves a complicated technology that cannot be commercialized. Sometimes the technology doesn't work, corporate priorities change, or the market window closes. There is tremendous variation in the value of technologies commercialized through license agreements.

The last purported metric of technology transfer that is commonly used is the royalty income realized by universities from the licensing of intellectual property. Here again, a look below the surface tells us that royalty revenue is not a precise metric for our interest (though it may be a valid measure for a single institution through time). For example, in recent years the top 10 royalty revenue recipient universities in the United States collectively received a large majority of all revenues paid to all universities. Even within this revenue stream, a very small number of patents at any institution produce the majority of revenues. The Wisconsin Alumni Research Foundation, one of the oldest and most successful technology transfer institutions, received 90 percent of its royalty income from ten patents, and one of these, Vitamin D, dominated the royalty income.[11] Similarly, at Stanford, seven individual patents produced more than three-quarters of the institution's royalty revenue in recent years—patents dominated by the Cohen-Boyer gene sequencing patent. For the University of Utah, the data are similar; a single license produced almost 25 percent of the royalties received in FY1996.

If these traditional university metrics that purportedly measure the productivity of industrial partnerships are not valid, what shall we use as an alternative? Not surprisingly, the answer depends on the focus of the question. For example, the metric applied by an individual faculty member to assess the potential benefits of a partnership with industry will differ from that applied by either the university or the state, regional, or federal government. The goals of each are very different.

To measure the potential benefits of a partnership with industry, an individual member of the faculty would measure the level of research support in relation to the workload. He or she might ask, What is the rate of publication and innovation from this project relative to the level of support? Is the project likely to create a significant outcome in scientific or technological terms? What could be the effect of this project on my scientific reputation? The metric here is reputation-enhancement, since research faculty operate on a credit economy and not necessarily a financial one. How a particular partnership plays into this credit economy is an important consideration and therefore an important metric to research faculty. There are other factors that influence this metric, including flexibility of the research support, exploratory or proprietary nature of the research, and deliverables that a sponsor might expect to receive from it.

The corporate university has very different values from those of the individual faculty who are part of it. Institutional measures of productivity are therefore different. Each institution will ultimately have to measure the *scholarly and/or economic* effects of these partnerships on the bottom line, either directly or indirectly. Despite the foregoing characterization, universities will undoubtedly continue to use the numbers of disclosures, patents, licenses, and royalty revenue in measuring institutional performance in technology transfer. However, I believe it is important to devise at least two new metrics that would be both more objective and more germane to the measure of university-industry partnership productivity.

[11] Irwin Feller, "Technology Transfer from Universities," *Higher Education: Handbook of Theory and Research,* vol. XII, John C. Smart, ed. (New York: Agathon Press, 1997), p. 13.

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First, a tabulation of the number of new companies created with university technologies, together with an objective measure of their economic impact, would provide a more accurate estimate of productivity as a metric directly comparable among institutions. It is the only metric that is important to local, regional, and state governments when they look to the university for technology innovation and a force in the economic development. The metric might be more sophisticated if it included measures of preproduction investment and jobs induced by these companies,[12] or even a highly comprehensive study of the economic impact of start-up companies such as been done for MIT by BankBoston.[13] A similar study on Silicon Valley[14] demonstrated that the number of jobs created and the tax revenue generated by such companies are important elements of an overall metric of economic impact.

Second, institutions need to develop metrics that reflect the amount of investment made in support of their own research from their partnerships with industry. A direct measure might be simply the amount of funds received for the support of research. More important indirect measures would include the creation of intramural funding programs for enhancing research and/or faculty competitiveness for extramural funds, jointly authored papers with industry, and so forth.

Research and education have been, and are likely to continue to be, the core mission of most universities. As such, the most efficient means of transferring technology is by graduating well-educated science and engineering students. Indeed, industry does not forge partnerships with universities so much for access to technologies as for access to students. Another metric for a university to apply to its industrial partnerships is the success that graduates have in entering the labor pool of those industries with which it collaborates.

There are, of course, many other issues that bear on a university's measure of the importance of industrial research partnerships—in particular, the trade-off between additional revenue from partnerships versus the additional management required for proper oversight.

University-industry partnerships have made the management of university research more complicated, including litigation over contractual disagreements, political exposure of faculty entrepreneurship in public universities, and faculty noncompliance with research misconduct and conflict-of-interest policies. Nevertheless, where the economic impact of the corporate spin-offs of university technology innovation has been carefully analyzed, research institutions are clearly forging highly productive partnerships with industry and, together, are forcefully driving economic growth of high-technology industrial sectors.

[12]L. Pressman, S.K. Gutennan, I. Abrams, D.E. Geist, and L.L. Nelsen. "Preproduction Investment and Jobs Induced by MIT Exclusive Patent Licenses, A Preliminary Model to Measure the Economic Impact of University Licensing," *J. Assoc. Univ. Tech. Managers* 7:49-82, 1995.

[13]Economics Department, BankBoston, *MIT: The Impact of Innovation,* special report (Boston: BankBoston, 1997).

[14]James F. Gibbons, "Silicon Valley: Startups, Strategies and the Stanford Connection," *MRS Bull.* (July):4-10, 1994.

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Panel Discussion: Academic Session

Ned D. Heindel, Lehigh University: Can you give me a feeling for how widely positioned equity ownership by universities and start-up companies is?

Richard K. Koehn: I cannot, because it is not a statistic that is made public by most institutions. I know many institutions do not hold equity positions because they have no mechanism to do so. In fact, we hold equity for the University of Utah through a subsidiary corporation, of which I am the president. The university had overcome the problem of taking equity, but when I arrived they had not overcome the problem of selling it.

And you can understand why. Taking equity in a new commercial venture is not nearly as politically charged as selling that equity. People would want to know why you sold it today, when the stock doubled in price the day after. We had to put into place a mechanism by which we could divest ourselves of a portion of the equity we held. There was approximately $8 million sitting there, doing nothing for the university. We are not in the real estate business; we are not in the brokerage business. We are in the research and education business, and we needed to turn those resources into strategic investments. That is how we generated those resources in the first place.

There are two programs at the University of Utah that are funded exclusively from the revenues generated by the divestitures of our equity positions: one provides $500,000 a year for a program called the Technology Innovation Incentive Grant. This program provides grants to university faculty for concepts with economic potential. The second program, which totals $1 million a year (on two cycles within each year, at $500,000 each cycle), provides seed money. These awards average $30,000 to $40,000 and are for new, innovative projects that will enhance our competitiveness for federal funds. As you know, the most successful people are always on the lookout for additional funding. The burden is on them to establish that whatever it is that they are seeking funds for represents a new direction.

Francis A. Via, Akzo-Nobel, Inc.: Focusing on the issues of metrics in the field of chemistry and chemical engineering has been a major challenge for most of us who are participating in university-industrial programs. The metrics for these programs are similar to those for internal industrial projects.

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In addition, we have found the most useful metric has been one of knowledge, training, and recruiting. With this metric, most of the programs are successful. There are, of course, many outstanding successes. Crest toothpaste is a direct result of a university interaction, and our largest-selling home pregnancy test kit came from a university-industry program. DuPont's success in changing from fluorocarbons to hydrofluorocarbons (HFCs) was reportedly accelerated through an association involving 10 universities and national laboratories in combination with DuPont's internal capabilities.

Despite these outstanding successes, we have found that we cannot always justify external programs based on new products and jobs alone. However, we can do so based on knowledge integration and increased productivity. Often we have used these associations to explore a high-risk research area and have gained knowledge that has affected internal research but not always led directly to new products. This effort continues to provide knowledge, people, flexibility, and high-risk program leverage. The secondary factors, as were mentioned yesterday by Dr. Jasinski at IBM and Dr. Mitchell at Lucent, are motivation and "lustre." Motivation is important for our scientists, who, because of globalization and decentralization, have moved to research programs with shorter time horizons.

Finally, we are very pleased to pay royalties to universities, but we are concerned about the emphasis in this area. A recently published listing for the 1994 top 50 U.S. research institutions and their top 10 patents demonstrates that only 2 of these were associated with chemistry. There was, as you would expect, a heavy concentration in electronics and biotechnology. Dr. Jack Yost, who is director of research at Pennsylvania State University, had been quite aggressive in working out intellectual property details on contracts with industrial programs. He now reports that the track record over the last 10 years at Penn State shows that royalties are primarily developed from their own research funded from other than industrial sources.

Richard K. Koehn: You have covered a lot of ground. Let me make a couple of very brief comments here. One is on the economics of royalties. No university ought to think about this in economic terms. The University of Utah receives $175 million a year in research funding. Our royalty is about $2 million! And we actually rank around tenth among the universities in royalty income. Basically, the university would be better off putting its money in CDs. By the way, our largest royalty generator is a chemistry patent.

Royalties aren't the reason to foster university-industry interactions. You encourage such interactions for other reasons, not for economic ones. And there are substantial other benefits. As you may know, Netscape was founded by one of our graduates, Tim Clark, after he left SGI. Netscape is doing well and, when they recently visited the university, we discussed a possible partnership. Do you know what Tim wanted? He wanted to hire all of our computer science graduates this year! That is an undeniable benefit—not just to the university, but to its graduates also.

We must be careful when establishing metrics for university-industry interactions. Use whatever metric seems appropriate; the main metric is that the partnership be beneficial for both parties involved.

Charles G. Moreland, North Carolina State University: My comments are a follow-up to the last question. When you talk about start-ups, especially start-ups that come directly from the universities, I would contend that industrial funding (especially large industrial funding) is a detriment. If the universities, in early-on negotiations with industry, give exclusive license to the large company, it will not be possible to form a start-up directly from the university. Then the question is, where is the start-up company coming from? Is it coming from the company to whom you licensed the technology, or is it coming directly from the university? One of the criticisms that you may have read in the report recently published about the Research Triangle area is that we have been overwhelmed by large industrial

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funding. The end result is that there are not a lot of start-up companies in our area, not nearly as many as you will find in Boston and Silicon Valley and so on.

Industrial funding can be a metric for economic development, but not necessarily the number of start-ups spun off, although depending on how the university structures its partnership to begin with, it may be possible to count start-ups. The real question is, How do we interact with more small companies? How do we provide incentives for them to interact with universities since they generally don't have much money to support their research base?

Richard K. Koehn: You have discussed two very different issues. One issue is contract research. The way Research Triangle has evolved has attracted a number of large corporations. The result is an opportunity for the companies and the universities in the Research Triangle area to interact in unique ways.

The issue of start-ups is a different issue. I like to joke—and I shouldn't because Professor Peter Stang is here, and he will go back and report to the faculty that I said this—but one of the reasons I think the University of Utah has been so successful in generating start-ups is because it underpays its faculty. There are very few research parks in the United States that are successful. The University of Utah research park turns out to be one of them, economically and programmatically. But unlike Research Triangle, virtually 90 percent of the companies that originated in that spot are from the university.

It is just a very different dynamic, and the way in which we as a university set up our policies and our interactions, our cultural incentives, is going to be different than it will be for an institution imbedded in large multinational corporations, which are looking for very different kinds of things than a small company starting up.

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Assessing the Value of Research at the Department of Energy: A Perspective from the Office of Basic Energy Sciences

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If you can look into the seeds of time,
And say which grain will grow and which will not,
Speak then to me, who neither beg nor fear
Your favors nor your hate

—Shakespeare, *Macbeth*, I:iii, 58-61.

INTRODUCTION

Efforts have intensified in recent years to define value as it relates to scientific research, to determine the tools and metrics by which that value can be quantified, and to assess the results of scientific research by using these tools and metrics. The incentive derives in part from a series of laws and executive orders, including the Government Performance and Results Act (GPRA) of 1993, that focus on performance management. This paper presents an overview of how these issues have been addressed within the Office of Basic Energy Sciences (BES) at the Department of Energy (DOE).

The BES program is prototypical of a large, diverse, and robust fundamental research program that exists within a mission agency. Part of its value derives from this special role, which is neither that of pure curiosity-driven research programs supported, for example, by the National Science Foundation nor that of applied research and development (R&D) programs supported by industry. Rather, the BES program supports fundamental research with a long-term objective. That this is not immediately seen as an oxymoron is testimony to the collective successes of the BES program and of other basic research programs that still exist within mission organizations.

This discussion relies on many thoughtful studies in performance measurement and assessment of basic science. Special acknowledgment is due to those who, in the early years of preparation for GPRA, spoke obvious truths about measurement and assessment of basic science while under pressure to derive measurement systems that tally results. Among these truths, several are of particular import: that the societal outcomes of basic research—new ideas and knowledge—are often unpredictable and may not be immediately apparent; that the measurement of a system will perturb it, often in ways that are unexpected; and that it is important to measure what matters, which frequently includes attributes that cannot be quantified.

This paper is not intended to be a scholarly review of performance measurement and assessment; therefore, references to important contributions made by others are not collected here. Rather, based on the literature and on personal interactions, the present discussion is a summary of the resulting philosophy, thoughts, and actions of those who manage the day-to-day activities of the large BES program.

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Even in the absence of GPRA and similar federal actions, performance measurement and assessment would exist within BES, because it is a part of good management practice. However, as with other organizations that support basic research, understanding and implementing performance management is most definitely a work in progress; thus, the obvious caveats apply.

THE BASIC ENERGY SCIENCES PROGRAM

With an FY 1997 appropriation of approximately $650 million, the BES program within the DOE's Office of Energy Research (ER) is one of the nation's primary sponsors of fundamental research in materials sciences, chemical sciences, geosciences, plant and microbial sciences, and engineering sciences. The program funds more than 2,400 researchers at 200 institutions nationwide and supports 17 major national user facilities. Included among these are the four major synchrotron radiation light sources; four neutron sources; and a number of specialized facilities for electron-beam micro-characterization, materials synthesis and processing, combustion research, pulsed radiolysis, and ion beam studies. Over 4,500 users, including hundreds of industrial scientists from about 100 U.S. companies, are accommodated at the major BES scientific user facilities each year. Detailed descriptions of the BES programs and the facilities as well as links to other related sites can be found on the BES home page at <http://www.er.doe.gov/production/bes/>.

In creating a portfolio of basic research in support of DOE's energy mission, BES has evolved an eclectic set of programs, many of which are cross-disciplinary or multidisciplinary, yet all of which have strong disciplinary grounding. In the area of the chemical sciences—the area addressed by this Chemical Sciences Roundtable workshop—BES research programs include topics such as analytical chemistry; atomic, molecular, and optical science; batteries and fuel cells; chemical kinetics; combustion dynamics; electrochemistry; heavy element chemistry; homogeneous and heterogeneous catalysis; organometallic chemistry; photochemistry; photosynthetic mechanisms; separations science; solar energy conversion; and thermophysical properties.

MISSION, GOALS, AND TENETS OF THE BES PROGRAM

The first step in performance management is defining the goals of the organization. As Yogi Berra allegedly said, "If you don't know where you are going, you won't know when you get there." Therefore, the BES approach to performance measurement and assessment relies heavily on the formulation of and subsequent adherence to the mission, goals, and tenets of the program.

The mission of the BES program, which derives both from the Energy Policy Act (EPACT) of 1992 and from the DOE and ER Strategic Plans, is as follows:

> To foster and support fundamental research in the natural sciences and engineering leading both to new and improved energy technologies and to an understanding and mitigation of the environmental impacts of energy technologies; and
>
> To plan, construct, and operate major scientific user facilities to serve researchers at universities, national laboratories, and industrial laboratories.

The goals of the BES program—which, in turn, derive from EPACT, the DOE and ER Strategic Plans, and the BES mission statement—are to do the following:

- To maintain U.S. world leadership in those areas of the natural sciences and engineering that are

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relevant to energy resources, production, conversion, and efficiency and to the mitigation of the adverse impacts of energy production and use;

- To foster and support the discovery, dissemination, and integration of the results of fundamental, innovative research in these areas;
- To provide world-class scientific user facilities for the nation; and
- To act as a steward of human resources, essential scientific disciplines, institutions, and premier scientific user facilities.

BES PERFORMANCE MEASUREMENT MATRIX

BES measures performance in four areas that together characterize its special role. The first three relate to the fundamental tenets or principles of BES, which correspond directly to the goals described above. These tenets are (1) excellence in basic research; (2) relevance to the energy mission of the agency and, moreover, to a comprehensive national energy agenda; and (3) stewardship of research performers, essential scientific disciplines, institutions, and scientific user facilities. Combining and sustaining these tenets are the management challenge—and it is a significant challenge—of BES. Therefore, the fourth area that BES evaluates is program management.

The principles embodied by the words *excellence*, *relevance*, and *stewardship* together determine the BES program portfolio, guide its funding choices, and ultimately provide the socioeconomic value of the program. Indeed, these three words—and the attendant principles—may well encompass the value of R&D activities performed in all sectors. Of course, each organization must determine its own set of activities within the three categories according to its mission and goals and must ultimately provide value to different constituencies. For example, industrially funded R&D activities provide value to shareholders, while federally funded R&D activities provide value to U.S. taxpayers—that is, to the nation as a whole. Research agendas, time scales, and risk factors are correspondingly different.

BES measures performance in the four areas described above in a number of ways, which separate naturally into four categories: (1) peer review; (2) indicators or metrics (that is, things that can be counted); (3) customer evaluation and stakeholder input; and (4) other assessments, which might include cost-benefit studies, case studies, historical retrospectives, and annual program highlights. The resulting BES performance measurement matrix is shown schematically in Figure 10.1.

During FY 1997, this matrix of "activities to be measured" versus "measurement techniques" was chosen as the framework by which BES would formalize performance measurement. A number of activities, sometimes quite unique from one another, can exist within an individual cell of the matrix. For example, the upper left cell shows peer review as a tool (in fact, it is *the* tool) for measuring excellence of basic research programs. Peer review can include mail peer review; panels of experts assembled to review simultaneously a number of proposals in a given subject area; site reviews by visiting committees; and special reviews overseen, for example, by the federally chartered Basic Energy Sciences Advisory Committee (BESAC) or by the National Research Council. Quite often more than one type of peer review method is used for a given program; this is particularly true of large programs and the scientific user facilities. Figure 10.1 also shows schematically that not every measurement technique is equally important to each activity. For example, peer review is very important for assessing excellence in basic research, but metrics and customer evaluation are not. Conversely, metrics and customer evaluation are very important tools for assessing the effectiveness of the scientific user facilities.

Ongoing planned and potential new activities related to performance measurement in BES are all accommodated by this four-column by four-row matrix construct. Furthermore, this matrix provides a

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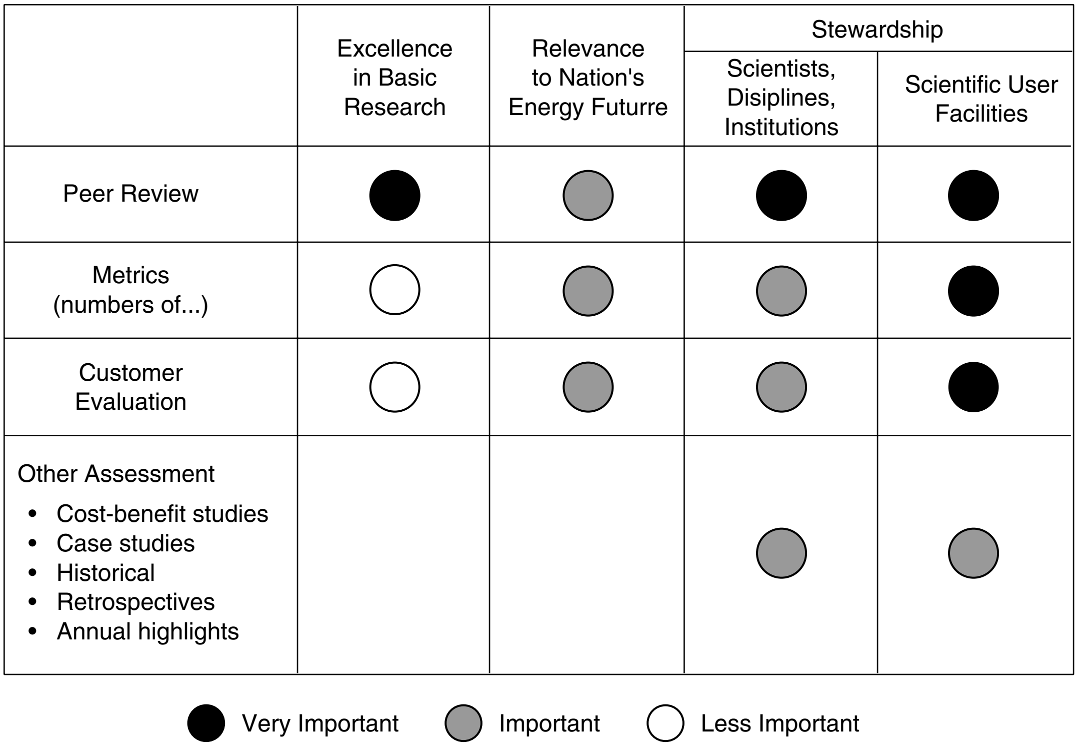

FIGURE 10.1 Office of Basic Energy Sciences performance measurement matrix.

comprehensive overview of performance measurement and allows a ready assessment of strengths, weaknesses, and gaps in performance measurement activities. The BES matrix is a modification of a simpler one derived by the Army Research Laboratory (ARL) for the measurement of its own performance. In July 1994, the Office of Management and Budget designated the ARL as a Pilot Project for Performance Measurement; as such, the ARL was the only R&D laboratory included in the GPRA pilot process. The mission, goals, and objectives of the ARL are different from those of BES. As a result, the row and column headings of the ARL matrix are naturally different from those adopted by BES.

A number of activities that might be considered essential or "foundation" performance measurement activities have long been in place in BES. These include peer review of research programs and customer surveys of the scientific user facilities. However, literally dozens of other activities and indicators can be envisioned for inclusion in the performance measurement activities. It quickly becomes apparent that an important management challenge is choosing a few significant items to target for special attention. Different activities and indicators may be added to the matrix or may be emphasized in successive years. In this way, a balanced system of performance measurement will evolve. For the next few years, BES has committed to select a few activities each year that address different aspects of performance measurement and that, taken together with ongoing activities, will strengthen performance measurement.

For example, during FY 1997, BES began two major activities designed to strengthen and formalize performance measurement in the future. These were (1) codification of the peer review process for research at the DOE laboratories, using a process analogous to that described in 10 CFR 605 (the Code of Federal Regulations) for the university grant program and (2) the development of a new survey tool

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for the 17 BES scientific user facilities, in collaboration with the facility directors and the facility user coordinators. In addition, a number of other activities related to performance and to the management of basic research programs were conducted; for example, BES initiated a pilot study to assess the culture that promotes excellence in basic research at the DOE laboratories.

Details of some of these and other activities relating to performance measurement in the four areas to be measured are given below. However, this is not meant to be an exhaustive discussion of each cell within the matrix.

Fostering and Assessing Excellence

Excellent basic research produces new knowledge and ideas *that endure, that change the way people think, and that are widely used by others*. Intuitively, we suspect that there is value associated with the production of new ideas and knowledge, particularly as knowledge and ideas affect products and processes. Many studies have been done that link outputs of basic research with positive societal outcomes. More recently, economic theories have been proffered that suggest that ideas and knowledge are even more powerful drivers of change and of socioeconomic value than previously thought. Professor Paul M. Romer of Stanford University's Graduate School of Business is well known for his work in this area.

Managing for excellence in basic research, that is, for new knowledge and ideas, is tantamount to managing for the unexpected. Every major organization that supports basic research has faced this conundrum. Over the years, peer review has emerged as the dominant (and perhaps the only valid) tool for measuring the technical excellence of basic research. The formal peer-review process used by the BES program is that used by the Office of Energy Research for its extramural grant program; this process is summarized in 10 CFR 605. The BES program has adapted 10 CFR 605 so that it may also be applied to the research programs and the scientific user facilities in the DOE laboratory system. First among the criteria used for selection of proposals is scientific merit. BES will soon begin reviews of its own management to assess the implementation of the peer-review system, including timeliness of decision, methods of review (mail, preproposals, panels, combinations), and demographics of reviewers.

The BES program not only assesses the degree of excellence of the basic research that it supports by using the standard techniques encompassed by peer review, but it also seeks to understand and foster the culture that promotes excellence in basic research. In a formal study headed by Gretchen Jordan of Sandia National Laboratories (SNL), the BES program is seeking to identify and assess the institutional and other factors that foster an environment for excellence in research in the DOE laboratories. The short-term goals of this study were first to identify the factors that promote excellence in basic research and then to develop a tool (a survey) to assess and improve the environment. These steps have already been completed by a panel of working scientists from SNL and managers from both SNL and BES. The resulting self-assessment survey has been beta tested on two groups at SNL. Perhaps not surprisingly, the most important factor determined by the panel was to hire and retain the best. Longer-range goals for this study include exploring the differences in the environment required to foster excellence in basic research, applied research, and technology development, and to compare and contrast research environments in national laboratories, universities, industries, and other types of U.S. institutions. A not-so-subtle hidden agenda was to remind researchers and managers that excellence in basic research is a BES priority. An unanticipated result of this effort has been a very high level of interest at other DOE laboratories, with several volunteering to participate in the study. As with all work supported by the BES

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program, it is expected that this study will be published in the archival literature and will contribute to the body of knowledge directed at conducting and managing basic research.

Fostering and Assessing Relevance

Making basic research relevant requires that the BES program set basic research directions in keeping with the DOE missions. The program must also promote the transfer of the results of basic research to contribute to DOE missions in areas of energy efficiency, renewable energy resources, improved use of fossil fuels, reduced environmental impacts of energy production and use, science-based stockpile stewardship, and future fusion energy sources.

To bring about research relevance, BES sets strategic research directions through working relationships with other DOE programs; research workshops involving input from the scientific and technical communities; the promotion of open information transfer and exchange of ideas between the basic and applied research communities; and, finally, the sponsorship of selected high-impact research collaborations and partnerships. Individual research projects are funded based on peer review, as discussed above.

DOE's national laboratory system plays a special role in the ability of BES to effectively integrate basic and applied research by providing opportunities to co-locate activities at these sites. For example, about one-third of the scientists supported by the BES program at the DOE national laboratories also receive support from at least one of DOE's technology programs. BES also aggressively fosters the integration of basic and applied research through the formation of "real" and "virtual" laboratories that bring together researchers with different backgrounds, expertise, and problems.

An example of a "virtual" laboratory is the Center of Excellence for the Synthesis and Processing of Advanced Materials. The center involves 12 DOE laboratories as well as several universities and industries. The eight projects currently under the center's umbrella (which includes topics such as metals forming, metals joining, and high-efficiency photovoltaics) involve many disciplines and require that each participant bring unique expertise and talents to the collaborations. The Center Steering Committee is composed entirely of representatives of industry and the DOE technology offices.

An example of a "real" laboratory is the Combustion Research Facility (CRF) at Sandia National Laboratories, Livermore, California. The CRF, which is one of the BES scientific user facilities and is operated by the BES Chemical Sciences Division, houses 20 laboratories. These include the Turbulent Diffusion Flame Facility, the Burner Engineering Laboratory, the Multi-fuel Combustor, and numerous laboratories for chemical dynamics, chemical kinetics, imaging of turbulent reacting flows, spray combustion, internal combustion engine studies, and coal research. Projects under way range from fundamental studies of combustion-generated pollutants, to development of new laser diagnostic techniques, and applied studies of processes in internal combustion engines. Basic research programs at the CRF are supported by BES, and applied research programs are supported by various programs in the DOE technology offices, including the Offices of Energy Efficiency and Renewable Energy, Fossil Energy, and Defense Programs. About 25 percent of CRF users are from U.S. corporations; the other 75 percent come from universities and national laboratories.

Another way that BES fosters integration of basic and applied research is through the newly initiated Partnerships for Academic Industrial Research (PAIR) program. This program is designed to encourage and facilitate research partnerships between academic researchers, their students, and industrial researchers. As discussed above, the BES program, through support of basic research co-located with applied research at the DOE laboratories, has had considerable opportunity to observe that both basic and applied researchers contribute to problem definition, discovery, and understanding and that

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the transition from discovery to development and deployment is not linear. The PAIR program encourages similar interactions between basic and applied researchers in academia and industry. Additionally, the PAIR program is intended to encourage universities to consider novel research activities and to foster faculty participation in nontraditional partnerships, which may have been discouraged in the past. The PAIR program requires evidence of a working relationship between the academic and industrial researchers and further requires that a graduate or postgraduate student spend at least 4 weeks a year in the industrial setting. Research funds are provided to the academic partner only.

As shown in Figure 10.1, assessment of these efforts incorporates the full range of measurement tools. For example, peer review of programs may involve representatives of the DOE technology programs and industry; BES advisory groups and steering committees will have similar membership. BES tracks the number of cooperative research and development agreements (CRADAs) that have resulted from BES-supported work; for example, BES funding has led to 120 CRADAs, which extend the basic research to applications and development. In addition, there are literally hundreds of collaborations between BES researchers and industrial researchers.

BES also supports a formal study led by Professors Barry Bozeman and David Roessner of the Georgia Institute of Technology to identify and measure the value to industry of research supported by BES. This study uses an exploratory approach known as R&D Value Mapping, which involves measuring a variety of project attributes (for example, resources devoted to the project, or number of industrial participants) against outcomes. In particular, the work employs a modified case study design to identify the impacts and benefits that industry experiences after interacting with basic research projects supported by BES. Moreover, the work identifies the industry impacts. Benefits will be causally linked to a series of upstream factors that can be influenced by DOE program managers, such as project funding mix and choice of mechanism for industry interaction. This method combines the strengths of case studies with those of systematic, quantitative analysis. This work, which identifies the factors that foster relevance in basic research, is philosophically analogous to the study described above, which seeks to determine the factors that foster excellence in basic research.

Finally, as an adjunct to the formal measurement and assessment techniques discussed above, it is also appropriate to use more qualitative retrospective analyses and annual highlights. When properly executed, these narratives can be compelling summaries of the relationships between the outputs of basic science and the outcomes that affect society. For example, based on a collection of more than 800 summaries of interactions involving BES researchers and industrial researchers (BES funds almost no industrial research directly), BES recently published a booklet, *Basic Energy Sciences—Serving the Present, Shaping the Future,* that gives an overview of the many areas in which basic research affects U.S. industry.

Fostering and Assessing Stewardship

Stewardship requires that BES establish and maintain stable, essential research communities, institutions, and scientific user facilities. For example, BES serves as the nation's primary or sole supporter of a number of important subdisciplines such as heavy element chemistry, natural and artificial solar energy conversion, catalysis, organometallic chemistry, combustion-related science, separations science, neutron science, radiation chemistry, and radiation effects in materials. Maintaining these communities is an important responsibility of BES.

Furthermore, BES has a major responsibility for the planning, construction, and operation of major national user facilities and for encouraging the use of these facilities in areas important to BES activities, and also in areas that extend beyond the scope of BES activities, such as structural biology, environmen-

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tal science, medical imaging, rational drug design, micromachining, and industrial technologies. Approximately 40 percent of the total BES budget is given to the operation of its 17 user facilities. Considerable additional funds go to support research at the facilities and to develop the next generation of tools, instruments, and facilities themselves.

Over the years, formalized processes have evolved to plan for new or upgraded facilities and to assess the progress of facility construction. These processes, for which ER and BES are well known, involve substantial participation from the scientific and technical communities. The planning process for new facilities frequently involves multiple workshops and symposia over many months or years and may involve hundreds of participants from the scientific community. Similarly, construction project reviews involve participation from the scientific and technical communities throughout the life of the construction project.

As an example, ER recently conducted a review of the BES-supported Conceptual Design Report (CDR) for the $1.333 billion Spallation Neutron Source project proposed for construction at Oak Ridge National Laboratory. This project, which will produce a next-generation spallation neutron source for neutron scattering, had been recommended in the mid-1980s as one of four major scientific user facilities needed for materials science and related disciplines. The need for and justification of this project have been validated numerous times since then by committees at both the National Research Council and DOE. The purpose of the CDR review was to assess the technical feasibility of achieving the proposed design and goals, the credibility of the associated cost and schedule estimate, and the adequacy of present and planned management arrangements to accomplish the scope of work. The DOE committee included 60 experts in the areas of project management, accelerator physics, front ends, linac systems, ring systems, neutron sources and targets, experimental systems, conventional facilities, environment and safety, and cost and schedules. This review was conducted by a group independent of BES and reported to the director of ER. These reviews (known as “Lehman Reviews” after Dr. Daniel Lehman, the director of ER’s Division of Construction Project Management) are emulated by many other organizations that oversee large construction projects.

For those facilities in operation, BES uses the full range of measurement tools to assess performance. Peer review is conducted both by BES and, when warranted, by special panels charged by BESAC. In addition to peer review, both metrics and customer evaluation are important tools for evaluating operating facilities. Because of the increased interest in the large BES facilities (the synchrotron radiation light sources and the neutron sources) during the past few years by the Office of Management and Budget (OMB), the Office of Science and Technology Policy (OSTP), and the user community, it became apparent that a comprehensive, standardized survey tool was needed for the BES facilities. That tool was developed during FY 1997 in collaboration with the directors and user coordinators of the 17 user facilities. Not surprisingly, it was extremely difficult to agree on an ideal survey—one that was comprehensive, relevant to all the facilities, had standard definitions (for example, an appropriate definition of a “user” in a time when many users have collaborators and when increasing numbers of users gain access to facilities via remote electronic connections), and was sensitive to the increased effort needed by facilities and their users to complete such a survey.

The resulting BES survey has several parts: (1) a user-satisfaction minisurvey that includes questions on availability, reliability, dependability, and service provided by the facility; the outputs of research from the facility (papers, patents, students trained, new collaborations formed, and so on); and other impressions, such as safety-related issues; (2) user demographics; (3) areas of research (that is, scientific disciplines) and funding sources for the research; and (4) budget and operation data of the facility, including hours of operation, fraction of the facility in use, and beam line statistics for those facilities that have beam lines. The survey will be beta tested for FY 1997 (the facilities will provide as

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much information as they can) and put into effect in FY 1998. Thereafter, it will be completed annually at the end of each fiscal year.

SUMMARY AND CONCLUSIONS

The characteristics of individual basic research projects supported by Basic Energy Sciences are often indistinguishable from those of projects supported by the National Science Foundation. For both organizations, achieving scientific quality is the primary goal, and merit review based on peer evaluation is the predominant vehicle for assessing excellence. To understand the differences between the two science programs, we need to compare the groupings of research projects within the broad scientific disciplines. As discussed in this paper, the rationale of how the BES program portfolio contributes to the Department of Energy's mission areas of energy efficiency, energy resources, environmental quality, and national security becomes clearer when we examine the specific mechanisms by which research areas are determined within BES and the performance measures by which the success of the BES program is ultimately determined.

A large part of BES's value is derived from the explicit goal of integrating the basic research supported by BES with applied R&D activities supported by other parts of DOE or by industry. In managing this difficult challenge, the BES program also shoulders a strong stewardship responsibility for maintaining stable, essential research communities, institutions, and scientific user facilities that are needed for new and improved energy technologies.

DISCUSSION

Thomas A. Manuel, Air Products & Chemicals Inc.: I would like to pick up on Dr. Dehmer's statement about what do you do if someone challenges you to say the world will end if you cut my budget 5 percent. This is not the end of the world. This sort of thing has been going on in the chemical industry for a long, long time. In fact, it is actually a healthy thing, because it doesn't talk about cutting 5 percent out of the objectives or goals. What it talks about is getting 5 percent more efficient in attaining those goals. So, it drives the argument from outcomes, which are long term and difficult to measure, as we have been saying, down to the work process itself. I think that it is healthy for government, as well as industry, to have this kind of challenge and to respond to it. From my experience in much smaller organizations and aggregates than BES, there is easily 5 percent inefficiency every year in many of the things we do.

Patricia Dehmer: I agree. At least, I think I agree. First of all, we separate the impacts of budget reductions into two categories. One is for the facilities, and the other is for the principal investigator programs. For the facilities, it is fairly easy to quantify what is going to happen with an *n* percent budget decrease, because it is usually very nonlinear, and an *n* percent decrease is usually a 2*n* or 3*n* decrease in the number of operating hours, and so forth. The facility operations is 40 percent of our budget. For the individual investigator program, it is more difficult to determine the impact of a budget reduction.

I tend to agree that most large programs have 5 percent inefficiency. But they do not have that year after year, and that is what has been happening for the last several years. During this period, Basic Energy Sciences has had an essentially flat budget, and, given inflation, we have had to make such reductions every year. Complicating the situation is "congressional direction" (more colloquially known as "pork"), which also forces us to make reductions in our peer-reviewed programs. The effect is usually

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small but not insignificant. For example, this year we effectively had a 3 percent cut because of congressional direction.

So the system does have flexibility and you can make rational cuts, but you can't do it on an ongoing basis without severely affecting the programs. I think many basic research programs in the federal government are at the point where they can no longer make perturbative cuts to get rid of inefficiencies. Instead, you will start seeing entire programs eliminated. That is essentially where we are right now.

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Assessing the Value of Research at the National Science Foundation

Judith S. Sunley
National Science Foundation

INTRODUCTION

The passage of the Government Performance and Results Act (GPRA, or the Results Act) in 1993 and its imminent implementation with the development of the FY 1999 budget request has made all federal agencies more sensitive to the importance of assessing the results of their activities. This presentation reflects the wide-ranging thinking and discussion that have gone into developing the National Science Foundation's (NSF's) response to the Results Act and includes information taken from public elements of NSF's strategic and performance plans. Any opinions are those of the author, rather than official agency positions.

As scientists, we know that there are many different ways of "measuring" things, and, in fact, there are whole fields of science devoted to measurement and evaluation. Key elements in any assessment of research activities include who is doing the assessment and what their expectations are for program outcomes. We know that different constituencies may attach different values to the same characteristics and may have quite different ideas about which dimensions of an effort merit consideration during an assessment. Equally important is the level of aggregation at which the assessment is made. We evaluate the results of a specific research project quite differently from the results of a broad program of activity. Finally, the stage at which a set of research activities is assessed is important in determining reasonable expectations for the assessment.

The multidimensional character of the contributions of research means that absolute valuations are difficult, particularly given the precision to which the individual measurements can be made. Precision is particularly problematic with assessments of quality, which are essential for research. This introduces some fuzziness in assessing the value of research that makes many outside science and engineering uncomfortable. The lack of precision requires the use of expert judgment in making effective assessments.

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THE NSF CONTEXT FOR ASSESSING THE VALUE OF RESEARCH

GPRA requires the development of a strategic plan that guides annual performance plans and reports. Key factors of the strategic plan are statements of mission and general goals. These provide the context for assessing agency performance. NSF's continuing mission is stated in the preamble to the National Science Foundation Act of 1950 (Public Law 810507): "To promote the progress of science; to advance the national health, prosperity, and welfare; to secure the national defense; and for other purposes."

GPRA authorizes and directs NSF to initiate and support the following:

- Basic scientific research and research fundamental to the engineering process,
- Programs to strengthen scientific and engineering research potential,
- Science and engineering education programs at all levels and in all the various fields of science and engineering, and
- An information base for science and engineering appropriate for development of national and international policy.

NSF works toward its mission through the support of research, infrastructure development, and education and training, largely at academic institutions. When we assess our programs, we are thus assessing the results and outcomes of the investments we make. We examine the outcomes of aggregate collections of awards over time frames appropriate to our expectations for results.

NSF has established its outcome goals by determining what types of observable outcomes from its programs advance the progress of science and engineering. These include:

- Discoveries at and across the frontier of science and engineering;
- Connections between discoveries and their use in service to society;
- A diverse, globally oriented work force of scientists and engineers;
- Improved achievement in mathematics and science skills needed by all Americans; and
- Meaningful information on the national and international science and engineering enterprise.

The first three outcome goals are most relevant to the assessment of research and are the focus of the remainder of this discussion.

ASSESSING PROGRESS TOWARD OUTCOME GOALS

Because the timing of outcomes from NSF's activities is unpredictable and annual change in the award outputs is not an accurate indicator of progress toward outcome goals, NSF has developed performance goals for outcomes against which we expect to assess progress on a continuing basis. The stream of data and information on the products of NSF's investments will be combined with the expert judgment of external panels to assess NSF's performance over time and to provide a management tool for initiating changes in direction, where needed.

These continuing performance goals take advantage of GPRA's option for the use of an alternative format where quantitative annual performance goals are impossible or inappropriate. They are based on descriptive standards that convey the characteristics of the types of results NSF seeks. The successful performance standards for the outcome goals most closely related to valuing research are listed below.

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- *Discoveries at and across the frontier of science and engineering*—NSF is successful in making progress toward this outcome goal when, in the aggregate, NSF grantees make important discoveries; uncover new knowledge and techniques, both expected and unexpected, within and across traditional disciplinary boundaries; and forge new high-potential links across those boundaries.
- *Connections between discoveries and their use in service to society*—NSF is successful in making progress toward this outcome goal when, in the aggregate, the results of NSF-supported work are rapidly and readily available through publication and other interaction among researchers, educators, and potential users; and when new applications are based on knowledge generated by NSF grantees.
- *Diverse, globally oriented science and engineering work force*—NSF is successful in making progress toward this outcome goal when, in the aggregate, NSF programs provide a wide range of opportunities to promising investigators; expose students and scientists and engineers to world-class professional practices and increase their international experiences; strengthen the skills of the instructional work force in science and technology; ensure access to modern technologies; enhance flexibility in training to suit an increasingly broad set of roles for scientists, engineers, and technologists; when business and industry recognize the quality of students prepared for the technological work force through NSF-sponsored programs; and when the participation of underrepresented groups in NSF-sponsored projects and programs increases

In addition to these successful performance standards, NSF has developed similar descriptions for exceptional performance and unacceptable performance. The descriptive standards include terms that require expert judgment, but we have attempted to limit these to concepts routinely judged through the merit review system,which gathers advice to inform project selection by program officers. Each of the descriptions will be accompanied by related output indicators that will provide hard information for the exercise of expert judgment.

The descriptive performance standards will be used at several levels of aggregation and by various groups as evaluative tools in NSF's management process. We expect each program to report on its performance annually based on an internal evaluation. Senior management will examine these reports and integrate them to develop reports for NSF as a whole.

This regular internal assessment cycle will be complemented and validated through external assessment using modifications to our existing Committee of Visitors process on a 3-year rolling cycle. We are already beginning to experiment with these modifications, including changes in the composition of the panels themselves. By FY 1999, all Committees of Visitors will give judgments of program effectiveness using the descriptive performance standards for outcomes. They will also address other performance issues, including those for the merit review system that these committees currently address. We anticipate that advisory committees and the National Science Board will also play important roles.

A critical factor in NSF's ability to conduct these reviews will be implementation of a revised project reporting system. This is currently being tested and will be fully implemented over the next year. We must rely on the research community for complete, accurate reporting of results.

DISCUSSION

Judith S. Sunley: I would like to add to Dr. Dehmer's earlier reply. The point that Dr. Manuel made is a good one. However, it is not clear to me that GPRA is a tool that really helps us deal with those questions, except to the extent that it may take it out of a 1-year view of "what do I do if my budget is cut 5 percent this year?" and put it into the context of a strategic plan that the agency has developed. This

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plan could certainly cover the long-term implications of a 5 percent cut this year with no future cuts, or a 5 percent cut this year, another 5 percent cut next year, and another 5 percent cut the year after that.

In an agency like NSF, there is frequently a feeling outside the government that, if it takes a 5 percent cut, it has that much fat in its salaries and expense pool for the people inside the agency. In an agency like NSF, our total salaries and expense budget is less than 5 percent of the agency's budget. So any significant cuts must come directly out of investments that we would otherwise make in research and development.

On the other hand, who knows (1) which 5 percent is the right 5 percent to cut out of those investments, and (2) whether some researchers could, in fact, do with less than they have actually asked for? Our program officers tend to be fiscally tightfisted in general (as many of you know). I think such cuts really do lead to a decrease in the number of people who are funded.

Andrew Kaldor, Exxon Research and Development Corp.: I was impressed by both the DOE and NSF presentations. It is very satisfying to see government program managers actually use some of these tools and matrices. I have one specific concern—the second outcome given by NSF, which dealt with applications of the technology. Unless you address that outcome with a very sophisticated time-averaged methodology, I honestly don't see how you can measure the outcome on an annual basis. In fact, if you start using less sophisticated measurement techniques, you will find yourself driven to short-term impact research, and I can't believe you can sustain the excellence that you are noted for under those circumstances.

Judith S. Sunley: I should have been a little clearer. One of the real difficulties that NSF has is that if you look, for example, at FY 1997 and you tried to establish a performance plan for FY 1997, based on the funds we distributed in that year, to be reviewed and reported on in FY 1998, you would not find much that came from those FY 1997 investments. So we have to go back and look at our accomplishment retrospectively, over an appropriate period of time.

There will be a variety of mechanisms for "measuring" our performance in these areas, both for the discoveries and for the connections to outcomes goals. For example, we could take a look at a set of selected investments made in, say, 1990 or 1992 and see what the total output of that set of investments is in 1997. Or, we could take some key technology result from 1997 and try to trace it back and see what investments NSF made that had an impact on those technology developments.

There are a variety of different ways of approaching this problem and, in the experiments that we are doing now with our Committees of Visitors, we are investigating a number of different options. Dr. Dehmer indicated that similar activities were under way in Basic Energy Sciences (BES). There is clearly not a single methodology that we would use in all cases.

Andrew Kaldor: I view this part as one of the most dangerous areas in reality, because the opportunity for misunderstanding, misinterpretation, and misdirection as a result is huge. NSF, for better or worse, has under its control the best science-generating machine in the world. It is a trust that has been given to NSF and Basic Energy Sciences, and that trust is worth a tremendous amount to us as a country. So when you talk about these measurements—measurements that could take this capability and diminish, if not destroy, it—I view the process as being extremely dangerous. It is critical that NSF makes sure GPRA doesn't adversely affect its mission, which is to support the best science.

Judith S. Sunley: That is something that we all are very concerned about.

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Jack Halpern, University of Chicago: I wanted to return to the 5 percent reduction question. I think this is the crux of the matter, at least as far as the level of investment in science is concerned. This issue—how much to invest in science—is what the government struggles with each year, whether to increase or decrease the budget by a few percent.

Having been through this exercise in the Academy for the last few years, of just trying to become more efficient, there are limits to what you can do. You soon reach a point where you really can't cut administrative costs much more. However, if you look then at the science support budget—what I have to say applies particularly to NSF, which is supporting extramural research and largely basic research—there are no strategic programs that you can measure against. Let's say NSF supports 20 percent of the requests that it gets. The real question is, What do we support at 21 or 19 percent? The managers at NSF are making arbitrary cutoffs, and it is not obvious what the consequences are of making a 1 percent shift from 19 to 20 or 20 to 21 percent—how detrimental the decrease, or beneficial the increase, is going to be.

It seems to me that the GPRA activities that we have been hearing about provide an opportunity to calibrate how sensitive the consequences are to the cutoff. Granted, it is very difficult to identify quantitative measures of performance, but NSF has identified a particular set of criteria that they are proposing to use in assessing the effectiveness of their programs. I agree that you can't do this project by project, but if you can do it across programs, I think it would be useful to take NSF's ranking of its projects and apply these criteria to the top 20 percent and the middle cut and the bottom 10 percent. This would allow you to do a couple of things. One is to assess the effectiveness of the criteria that you are using. If they don't distinguish between what you think are your most highly ranked programs and your most poorly ranked programs, then it suggests that your criteria are not very useful.

To turn that around, if you have faith in your criteria, or evidence that they allow you to assess the effectiveness of your peer-review ranking of programs, such a study would also allow you to see significant differences between your top-ranked programs and your lower-ranked ones. At the very least, it seems to me that you ought to be applying these criteria by strata, even if not to individual projects. It would also give you some appreciation about what will happen if you have to cut out the lowest 5 percent. How much less effective are they by your criteria than the others?

Judith S. Sunley: That is a very interesting idea. I will try to get someone to explore some of those ideas further.

Richard K. Koehn, University of Utah: I would like to expand on the point that Professor Halpern raised. Why wouldn't you include in your assessment those projects that were not funded? By selecting projects to fund and, implicitly, others not to fund, you have made choices of where to invest and where not to invest. It would be worthwhile to know that those projects you didn't invest in haven't in fact produced positive outcomes by your criteria.

Judith S. Sunley: I don't think we have attempted to do that in the past. One of the things we are very concerned about is the overall burden both on the scientific community and on NSF staff, as well as the expense, in exploring these options. So we would have to estimate what value we thought we could get out of going that much farther in terms of detail in the assessments.

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National Institutes of Health Response to the Government Performance and Results Act

Mary Groesch
National Institutes of Health

INTRODUCTION

At the National Institutes of Health (NIH), the response to the Government Performance and Results Act (GPRA) is being developed and coordinated by the Office of Science Policy, within the Office of the Director. The GPRA was enacted to strengthen public confidence in government; to improve program management, effectiveness, and accountability by focusing on results; and to facilitate and improve congressional decision making with respect to program budgets. This paper describes some of the actions NIH is taking to implement the GPRA.

There are three required components of the GPRA. The first is an agency strategic plan—a 5-or-more-year plan that states the agency's mission and its long-term goals. The plan must be updated at least every 3 years. For GPRA reporting purposes, the agency that is required to put forward a strategic plan is the Department of Health and Human Services, not NIH. We are, however, included within the HHS strategic plan. In addition, we have been working on our own GPRA strategic plan to guide the development of our performance plan.

The performance plan is the second component of the GPRA. In this case, NIH and HHS both are required to develop a plan. A significant amount of our effort has therefore gone into developing an annual performance plan, which is submitted along with the yearly budget request. The performance plan outlines our expected program outcomes and means and sets forth performance goals and performance indicators for each. The third component of the GPRA is the performance report, which includes an assessment of how well we did in meeting our goals.

The NIH strategic plan for GPRA is straightforward in concept, although the implementation of these concepts is anything but simple. Our mission is to sponsor and conduct research that leads to better health for all Americans. This is reflected in two long-term goals, to advance medical knowledge through research, and to enhance and maintain our research capacity.

We accomplish these goals through the activities of three core programs, and this is the level at which the results are aggregated. We decided to aggregate the programs from our 24 institutes, centers,

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and divisions into three core programs: Research, Research Training and Career Development, and Facilities.

The GPRA allows an agency to either aggregate, desegregate, or consolidate its program activities in the way most appropriate to performance reporting. We consider it essential that the activities of our 24 different institutes, centers, and divisions be aggregated. Our rationale for this with respect to NIH's research programs is simple. Disease is typically systemic. It is influenced by multiple factors and affects more than one organ or body system. Therefore, expertise across a wide range of disciplines is necessary to establish the mechanisms and etiology of disease and to develop strategies for diagnosis, treatment, and prevention. Multidisciplinary expertise necessarily cuts across institute boundaries; therefore, we have to combine the efforts of many of our institutes to make progress on any particular disease or disability. Following the same line of reasoning, training and facility needs are also not unique to any one particular institute or center. Therefore, we must coordinate and collaborate these efforts across NIH to ensure that we can meet our long-term goals.

ESTABLISHMENT OF PROGRAM OUTCOMES AND MEANS

To assess the performance of the three aggregated programs, we have established for each a framework of expected outcomes and program means. As the name implies, the expected program outcomes are the expected, tangible results of NIH programs. The program means reflect the process and management activities that we undertake to support the conduct of our programs and to enable us to achieve our goals. For each of the expected program outcomes and program means, we have set performance goals that identify what we expect to accomplish. Each goal is accompanied by a set of performance indicators, which will be used to measure our success in achieving it. I will discuss a few examples of the expected program outcomes, the program means, and the performance goals and indicators that we are considering for our research program. I use the term "considering" because everything we are doing for the GPRA is very much in a state of development. What I tell you today may not necessarily be what will soon be forwarded to Congress.

We have defined two broad expected program outcomes that are expected. The first is increased understanding of normal and abnormal biological functions and behavior. The second is improved prevention, diagnosis, and treatment of diseases and disabilities. For each expected program outcome in the research program, we have defined performance goals, many of which are not assessable by quantitative measures.

As previous speakers have pointed out, agencies whose missions encompass fundamental science face unique challenges in implementing the quantitative evaluations that seem to be preferred under the GPRA. This is an issue to which we have devoted much time and effort. We have had extensive internal discussions and have also sought substantial input from outside groups. In particular, the National Science Foundation has been very helpful to us. They have generally been somewhat ahead of NIH in the GPRA process, and we have benefited from their insights, experiences, and lessons learned as they, too, have grappled with GPRA.

First and foremost, we concluded that just because something can be counted does not mean it should be reported. We decided that a combination of both qualitative and quantitative performance goals and indicators is the most meaningful response for NIH. We think that strictly numerical results are neither feasible nor sufficient to capture the breadth and impact of NIH's research activities. Although conventional scientific and research metrics can be relevant, they measure only some of the dimensions of output. These metrics provide important data, but they alone cannot assess the full scope of the quality of our work, its relevance, and the impact of our research program on human health that

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is necessary for evaluating NIH. For these reasons, qualitative measures will play a major role in how we report on NIH's performance.

For example, we are developing narrative descriptions of our research accomplishments. This places a specific incremental advance into its larger context—that is, describes what was previously known and unknown, the nature of the accomplishment, its contribution to understanding and improving human health, its significance to advancing a field or fields of science, the next steps in the research, and, where possible, the economic impact of the advance.

Nonetheless, we plan on using quantitative goals and indicators whenever possible. Genomic research, for example, lends itself to this approach better than much of the other research that we conduct and support. For example, one of our goals is to complete the development of important genomic resources, such as the DNA sequence of the human genome and the genomes of several other important model organisms. Quantifiable indicators for this goal include our progress toward completing the sequence of the human genome by 2005; progress toward our goal of completing the sequence of zebrafish by 2002; progress in sequence tagging the genes expressed during tumor development; the number of new gene sequences that have been added to the GenBank; and progress in completion of the rat genomic libraries, genetic map, and expressed sequence tag map, with a target date of 1999.

I have touched on the expected program outcomes portion of the research program and the goals and indicators we have developed for those. The other portion of our performance plan is program means: the way that we get to our expected outcomes—that is, the management activities. The HHS and OMB have encouraged us to provide specifics, stressing the use of metrics wherever possible for this portion of GPRA reporting.

In terms of the program means, the types of means goals and indicators that we are considering include priority setting, grants administration and peer review, communication of results, technology transfer, management and administration, and collaboration and coordination. Some of the specific performance goals for grant administration and peer review, for example, could be to ensure that submitted grant applications receive fair and appropriate review, to improve and enhance the electronic research administration, to improve and enhance communication with the extramural community, and to improve customer service by expediting and processing the award of grant applications. Some of the indicators for improving customer service include expediting and processing the award of applications, implementing pilot projects of different ways to process applications, implementing on-time procedures to eliminate unnecessary submission of administrative information, and developing pilot studies for referring grant applications to program offices.

ASSESSMENT OF NIH'S PROGRAMS

The third component of GPRA is assessing NIH's performance. The law calls for an external review of our performance. Although this section sounds like it could be the most relevant to the discussion for this meeting, unfortunately it is the part that we cannot address because we are still at the drawing board. We are exploring who will do the assessment and how it will be done. However, I can at least describe some of the ideas that we are considering.

Our current plans call for NIH program assessment to be conducted by a subcommittee of the advisory committee to the director of NIH. Such an assessment subcommittee might include one or two individuals from the advisory committee as liaisons, as well as grantees, members of the lay public, patient advocacy groups, professional societies, other federal agencies, and various members of the NIH staff.

How the assessment group will function and what form its report will take are still under discussion.

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For example, what will the external reviewers assess every 3 years—all NIH research, or several selected research programs or areas? Or, will they look at areas of research emphasis established by the NIH director? These include six broad categories: the biology of brain disorders, new avenues for development of therapeutics, genetic medicine, new approaches to pathogenesis, new preventive strategies against disease, and advanced instrumentation and computers in medicine and research. Another possibility is a combination of the above.

Although NIH is well along the path to developing a response to GPRA, a number of issues have yet to be decided.

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Panel Discussion: Government Session

Charles Zukoski, University of Illinois at Urbana-Champaign: How do the individual agencies view education as part of the role when they are funding research initiatives in academic institutions? Clearly, NSF puts a lot of thought into this, but I am curious about the other agencies.

Patrica M. Dehmer: Education is not a prime mission in terms of the Energy Policy Act for the Department of Energy, but we take this issue very seriously. The national laboratories all have large educational activities, which have been regrettably cut by Congress in the last year. But we track and monitor the number of undergraduate students, graduate students, and postdoctoral fellows that we support, and it is rare that we would fund a grant that doesn't have those components to them.

We also believe that the scientific user facilities that we operate are one of the best training grounds for students. So we aggressively try to maintain high student populations of these facilities.

Mary Groesch: At NIH, education also is important, but it is a small part of what we do. There are four components within the Office of Science Policy, one of which is the Office of Science Education. A number of the activities of that office, one of which is the Mini-Med School that you may be familiar with, have been very popular. The Mini-Med School is now going on the road to other parts of the country to help educate the public about the implications of scientific advances for health care.

Each institute also sponsors activities that relate to education and outreach to schools. They prepare a lot of material for students and for their parents and teachers. But, again, this is a fairly small portion of our overall activities.

Beverly K. Hartline, Office of Science and Technology Policy: Doesn't NIH still have a major traineeship program at the graduate level? Contributing to the flow of medical research professionals is certainly a mission of NIH.

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Mary Groesch: Absolutely. And I wasn't including the research training, which is a major effort for NIH.

Beverly K. Hartline: One of the things that we have been doing in the Office of Science and Technology Policy is working with the agencies and Dr. Horrigan and her colleagues at OMB. I wanted to take this opportunity to share with the group here—to the best of my recollection—the gist of a memorandum that went from Dr. Jack Gibbons, the director of the Office of Science and Technology Policy, to all of the agencies that have science and technology elements.

The memo noted that OSTP is working with OMB to review the performance and strategic plans of the agencies with respect to R&D to ensure that these plans don't totally miss out on some element required by GPRA. We want to make sure that all agencies treat their science and technology elements in some visible way in their strategic plans. You heard today from agencies for whom science and technology are major components and can't be missed. However, there are many other federal agencies where science and technology are smaller, but we believe are nonetheless extremely important—for example, the Environmental Protection Agency, which has a $7 billion budget and only about $500 million or so that is in science and technology.

Of course, if you ask the highest levels at Commerce, Interior, and the other agencies what they are about, science is probably not the first word out of their mouths. And yet the science and technology activities that are supported in those agencies are essential to the regulatory and management activities that are the primary mission of the agency.

So we intend to make sure that all of the agencies include their science and technology programs in their strategic plans, so it can't be missed or mismanaged. We are also interested in making sure that (1) they address coordination within and among the agencies; (2) their performance measures accurately reflect the performance, contribution, and value of that agency in a way that even nonexperts can appreciate; and (3) the performance plan will lead to both meaningful numerical and alternative or qualitative types of performance goals and measures that can be accountable and to some extent audited. I use those words in the most broad definition; that is, they can't just be hand waved around, but they aren't only counting beans either.

We are interested in making sure that the documents have the potential to be useful to the agency managers and staff and, last but not least, we hope that they promote and do not interfere with scientific excellence, creativity, and innovation.

When you are doing things like GPRA, which look very prescriptive, the fear is that they will make everybody just go through a set of steps and check off some boxes. You could lose the excellence, the creativity, and the innovation that are certainly the hallmark of scientific research and the reason that the taxpayer puts money into these three agencies, and into many others as well.

Judith S. Sunley: I heard a rumor to the effect that the strategic plan from the Department of Defense had one page that referred to research and development. Given the large effort that the Department of Defense has in that area, it seems a little bit out of balance if you take the $70 billion, in our federal R&D effort, and recognize that half of that is defense oriented. I don't know if that rumor is true or not, but it illustrates the difficulty that OSTP has because there are a lot of other things on the mind of the Department of Defense as well.

Beverly K. Hartline: I don't know if that rumor is true either. Nonetheless, Judy's remarks are very cogent. The Department of Defense does spend some $35 billion a year in science in R&D, most of

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which is in development and applied to specific weapons systems. However, $3 billion or $4 billion is in basic and applied research. That part of the agency is particularly vulnerable, because the Joint Chiefs of Staff don't see that investment translating into battlefield superiority on any time scale that is within their watch.

With the Department of Defense budget being under pressure and the numbers of forces, ships, and airplanes being reduced, I have heard unfortunate news about decisions on certain elements of research laboratories' budgets, which I hope isn't true. But I don't know.

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Appendixes

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APPENDIX A

Workshop Participants

Joan Adams, Pacific Northwest National Laboratory
Richard C. Adams, Battelle Memorial Institute
Richard C. Alkire, University of Illinois at Urbana-Champaign
Paul S. Anderson, Dupont-Merck Pharmaceuticals
Jeffrey Banacato, National Science Foundation
Eric C. Beckman, University of Pittsburgh
Henry N. Blount III, National Science Foundation
Robert Brown, The Gillette Company
Donald M. Burland, National Science Foundation
Daryle Busch, University of Kansas
Phillip Certain, University of Wisconsin-Madison
Jeff Conrad, U.S. Department of Agriculture
Carol Creutz, Brookhaven National Laboratory
Anne Datko, U.S. Department of Agriculture
Patricia M. Dehmer, U.S. Department of Energy
Brian Dougherty, American Institute of Chemical Engineers
Michael P. Doyle, Research Corporation
Lawrence H. Dubois, Defense Advanced Research Projects Agency
Thom H. Dunning, Jr., Pacific Northwest National Laboratory
Karolyn K. Eisenstein, National Science Foundation
Raymond E. Fornes, North Carolina State University
James Fry, University of Toledo
Jean H. Futrell, University of Delaware
Judith C. Giordan, International Flavors and Fragrances
Harold C. Graboske, Jr., Lawrence Livermore National Laboratory
Mary Groesch, National Institutes of Health
Jack Halpern, University of Chicago

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Fred Heineken, National Science Foundation
Sarah G. Horrigan, Office of Management and Budget
David A. Hounshell, Carnegie Mellon University
Madeleine Jacobs, American Chemical Society
Joseph M. Jasinski, IBM Research Center
Paul Wyn Jennings, National Science Foundation
Andy Kaldor, Exxon Research and Development Corp.
Don E. Kash, George Mason University
Jack G. Kay, Drexel University
Jack A. Kaye, National Aeronautics and Space Administration
William Koch, National Institute of Standards and Technology
Richard K. Koehn, University of Utah
Jules B. Lapidus, Council of Graduate Schools
Robert L. Lichter, Camille and Henry Dreyfus Foundation
Andrew J. Lovinger, National Science Foundation
Thomas A. Manuel, Air Products and Chemicals Inc.
Robert S. Marianelli, U.S. Department of Energy
L.E. McNeese, Oak Ridge National Laboratory
James W. Mitchell, Lucent Technologies
Charles G. Moreland, North Carolina State University
Francis Narin, CHI Research, Inc.
Janet G. Osteryoung, National Science Foundation
Trueman Parish, Eastman Chemical Company
Tom Picraux, Sandia National Laboratories
Stephen Piotrowicz, National Oceanic and Atmospheric Administration
Gintaras V. Reklaitis, Purdue University
Mihail C. Roco, National Science Foundation
Michael E. Rogers, National Institutes of Health
Ronald W. Rousseau, Georgia Institute of Technology
K. Barbara Schowen, University of Kansas
Peter J. Stang, University of Utah
Gerald M. Stokes, Pacific Northwest National Laboratory
B. Ray Stults, Pacific Northwest National Laboratory
Judith S. Sunley, National Science Foundation
Janis Tabor, Council on Chemical Research
Kathleen C. Taylor, General Motors
Michael Thompson, Pacific Northwest National Laboratory
Marion C. Thurmauer, Argonne National Laboratory
Frank P. Tully, Sandia National Laboratories
David L. Venezky, Naval Research Laboratory
Francis A. Via, Akzo-Nobel Chemicals, Inc.
Isiah M. Warner, Louisiana State University
Vern W. Weekman, Mobil R&D Corporation (retired)
Patrick Windham, Windham Consulting

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Staff

Douglas J. Raber
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APPENDIX B

Origin of and Information on the Chemical Sciences Roundtable

In April 1994, the American Chemical Society (ACS) held an Interactive Presidential Colloquium entitled "Shaping the Future: The Chemical Research Environment in the Next Century."[1] The report from this colloquium identified several objectives, including the need to ensure communication on key issues among government, industry, and university representatives. The rapidly changing environment in the United States for science and technology has created a number of stresses on the chemical enterprise. The stresses are particularly important with regard to the chemical industry, which is a major segment of U.S. industry, makes a strong, positive contribution to the U.S. balance of trade, and provides major employment opportunities for a technical work force. A neutral and credible forum for communication among all segments of the enterprise could enhance the future well-being of chemical science and technology.

After the report was issued, a formal request for such a roundtable activity was transmitted to Dr. Bruce M. Alberts, chairman of the National Research Council (NRC), by the Federal Interagency Chemistry Representatives (FICR), an informal organization of representatives from the various federal agencies that support chemical research. As part of the NRC, the Board on Chemical Sciences and Technology (BCST) can provide an intellectual focus on issues and fundamentals of science and technology across the broad fields of chemistry and chemical engineering. In the winter of 1996, Dr. Alberts asked BCST to establish the Chemical Sciences Roundtable to provide a mechanism for initiating and maintaining the dialogue envisioned in the ACS report.

The mission of the Chemical Sciences Roundtable is to provide a science-oriented, apolitical forum to enhance understanding of the critical issues in chemical science and technology affecting the government, industrial, and academic sectors. To support this mission, the Chemical Sciences Roundtable will do the following:

[1] *Shaping the Future: The Chemical Research Environment in the Next Century,* American Chemical Society Report from the Interactive Presidential Colloquium, April 7-9, 1994, Washington, D.C.

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- Identify topics of importance to the chemical science and technology community by holding periodic discussions and presentations, and gathering input from the broadest possible set of constituencies involved in chemical science and technology.
- Organize workshops and symposia, and publish reports on topics important to the continuing health and advancement of chemical science and technology.
- Disseminate the information and knowledge gained in the workshops and reports to the chemical science and technology community through discussions with, presentations to, and engagement of other forums and organizations.
- Bring topics deserving further, in-depth study to the attention of the NRC's Board on Chemical Sciences and Technology. The roundtable itself will not attempt to resolve the issues and problems that it identifies—it will make no recommendations, nor provide any specific guidance. Rather, the goal of the roundtable is to ensure a full and meaningful discussion of the identified topics so that the participants in the workshops and the community as a whole can determine the best courses of action.

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