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ADVANCED TECHNOLOGY PROGRAM

Managing Technical Risk:
Understanding Private Sector Decision Making on Early Stage, Technology-based Projects

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Managing Technical Risk

Understanding Private Sector Decision Making on Early Stage Technology-based Projects

Prepared for

U.S. Department of Commerce
Economic Assessment Office
Advanced Technology Program
National Institute of Standards and Technology

BY
Lewis M. Branscomb, Principal Investigator
Aetna Prof., Emeritus, in Public Policy and Corporate Management
Belfer Center for Science and Public Affairs,
John F. Kennedy School of Government, Harvard University

Kenneth P. Morse
Senior Lecturer and Managing Director
Entrepreneurship Center, Sloan School of Management
Massachusetts Institute of Technology

Michael J. Roberts, Consultant to the Project
Executive Director of Entrepreneurial Studies
Harvard Business School

Darin Boville
Project Director, Advanced Technology Program

APRIL 2000

U.S. Department of Commerce
William M. Daley, Secretary

Technology Administration
Dr. Cheryl L. Shavers, Under Secretary of Commerce for Technology

National Institute of Standards and Technology
Raymond G. Kammer, Director

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Harvard University

John F. Kennedy School of Government

79 John F. Kennedy Street, Cambridge MA 02138

Lewis M. Branscomb, Prof. Emeritus in
Public Policy and Corporate Management

Mr. Darin Boville
Project Manager, Advanced Technology Program
National Institute for Standards and Technology
Gaithersburg MD 20899

Dear Mr. Boville,

Enthusiasm and talent for innovation is a hallmark of the American economy, creating new industries and services that continue its growth, creating quality jobs and new opportunities with minimal environmental burdens. Our consumer culture accepts, even demands, novelty and change. American capital markets and business culture encourage risks to be taken when justified by the opportunities innovation may bring. These opportunities are made possible by a publicly supported scientific enterprise and system of higher education unmatched in the world. Nevertheless, the risks associated with science-based commercial innovations are real and often hard to quantify and circumscribe. These risks contribute to business failures, but more importantly to underinvestment in the early stages of research and to opportunities foregone.

The Advanced Technology Program, which chartered this study, was established by Congress to help the private sector minimize one significant source of risk in science-based innovation: the transition from an attractive new concept, based on new science, to a workable technology that enables product development and market entry. Such research typically lies beyond the scope of basic scientific research, but short of the target for venture capital investment. The ATP has clearly demonstrated its ability to help firms to bridge this "research gap," and thus enables a higher rate of innovation in areas most likely to bring broad economic benefits to the nation.

The participants in our workshops confirmed the existence of impediments to taking risks that can and should be lessened through both government and private action. Our study seeks to inform the decisions of both government managers and private entrepreneurs by exploring the way the technical dimensions of risk are viewed and managed by innovators, business executives, and venture investors. We believe this study will deepen understanding of the risks in science-based innovation and will enable programs like ATP to be further strengthened.

Sincerely,

Lewis M. Branscomb

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Preface

This report, and the work leading to it, were funded by the Advanced Technology Program of the National Institute of Standards and Technology, in a contract activated on May 5, 1999, to Harvard University's John F. Kennedy School of Government. Work under this contract involved a collaboration of the Science, Technology and Public Policy Program of the John F. Kennedy School of Government of Harvard University, the Entrepreneurship Center of the Sloan School of Management of MIT, and faculty members of the Harvard Business School.

The Principal Investigator is Professor Emeritus Lewis M. Branscomb, assisted by BCSIA Fellow Dr. Philip Auerswald; they bore primary responsibility for preparing the body of this report. The MIT team leader was Kenneth Morse, director of the Sloan School Entrepreneurship Center, in collaboration with Matthew Utterback. Dr. Michael Roberts coordinated the Harvard Business School faculty participation and, with MIT colleagues, made available to the project case studies of technical innovations.

The project was conceived by Darin Boville, NIST-ATP Project Director, who designed the goals and strategy for the research and monitored the progress of the work in fulfillment of requirements. During the course of the project, Boville provided valuable guidance, raising insightful questions that prompted further study. He offered many valuable suggestions (including editorial ones) about the structure of the project and the content of contributed papers which contributed significantly to the quality of the work.

We are especially appreciative of the contributions of both scholars and practitioners from the world of business and venture capital, many of whom are authors of papers reproduced in this report, others of whom contributed importantly to the discussion in our workshops. A list of participants follows.

Special thanks are due to supporting staff who supported many facets of the work: Andrew Russell and Beth Mathisen at the Kennedy School (KSG) and Audrey Dobek at Sloan School. Nora O'Neil (KSG) assisted with the financial and contractual arrangements. Albert George (KSG), Barbara Mack (KSG), and Obinna Oyeagoro (Andersen Consulting) assisted with workshops and contributed to our discussions, and Mack wrote the summary of the September workshop. Throughout the process David Hsu (Sloan) provided many comments and insights based on his independent work at MIT. Prof. Benjamin S. Bunney (Yale School of Medicine). Finbarr Livesey (KSG) and Dr. Peter Levin (TechnoVenture Management) provided helpful comments on an early draft of this report. A group of current and recent KSG and MIT students formed a working group during the final months of the project, carrying forward critical discussions of the issues and helping to organize the report. This working group included George, Hsu, Oyeagoro, Livesey and Mack as well as Sinan Aral (KSG), Brandon Mitchell (Sloan) and Robert Margolis (KSG). Our consulting editor for this report was Teresa Lawson, who contributed much to the clarity and readability of this document. As usual we are grateful for her high level of professionalism.

Lewis M. Branscomb
Philip E. Auerswald

List of Participants

Howard Anderson
Managing Director, The Yankee Group

Dr. Philip Auerswald
Postdoctoral Fellow, Kennedy School of Government

Dr. David Bernstein
President and CEO, Trexel

Darin Boville
Project Director, ATP

Prof. Lewis Branscomb
Aetna Professor of Public Policy and Corporate Management, emeritus, Kennedy School of Government

Mr. Richard M., Burnes
Charles River Ventures

Dr. Mark Chalek
Director, Office of Corporate Research, Beth Israel Deaconess Medical Center

Dr. Robert A. Charpie
Chairman, Ampersand Venture Management Corp.

Prof. Henry Chesbrough
Assistant Professor of Business Administration, Harvard Business School

Dr. Alexander V. D'Arbeloff
Chairman of the Corporation, Massachusetts Institute of Technology

Dr. David Edwards
President, Advanced Inhalation Research Inc.

Dr. Barry Eisenstein
Vice President, Office of Science and Technology, Beth Israel Deaconess Medical Center

Dr. Howard Frank
Dean, Robert H. Smith School of Business University of Maryland

Dr. Mary Good
Venture Capital Investors

Christopher Hansen
Economist, ATP

Dr. George C. Hartmann
Principal, Strategy and Innovation Group, Corporate Research and Technology, Xerox Corp.

David Hsu
Sloan School of Management, MIT

Prof. Marco Iansiti
Associate Professor of Business Administration, Harvard Business School

Prof. Adam Jaffe
Professor of Economics, Brandeis University

Dr. Larry Jarrett
Industrial Research Institute

Dr. Stephen Kent
Chief Scientist, BBN Systems and Technologies

Prof. Robert Langer
Germeshausen Professor of Chemical & Biomedical Engineering, MIT

Prof. Dorothy Leonard
William J. Abernathy Professor of Business Administration, Harvard Business School

Prof. Josh Lerner
Associate Professor of Business Administration, Harvard Business School

Dr. David L. Lewis
Vice President, General Manager of Chemical Products, Lord Corp.

James C. McGroddy
Ret. IBM Sr. VP Research; Chairman, MIQS Inc.

Terrance McGuire
General Partner, Polaris Venture Partners

David Morgenthaler
Founding Partner, Morgenthaler Ventures; Past President, National Venture Capital Association

Dr. Mark Myers
Senior VP, Xerox Research and Technology, Xerox Corp

John Preston
CEO, Quantum Energy Technologies

Prof. David Ragone
Ampersand Venture Management Corp.

Dr. Michael Roberts
Executive Director of Entrepreneurial Studies, Harvard Business School

Prof. Richard Rosenbloom
David Sarnoff Professor, Emeritus, Harvard Business School

Rosalie Ruegg
Director of Economic Assessment Office, ATP

Prof. F.M. Scherer
Aetna Professor of Public Policy and Corporate Management, Kennedy School of Government, Harvard

Susannah Schiller
Special Assistant to the Director of ATP

Prof. Scott Shane
Michael D. Dingman Center, Robert H. Smith School of Business, U. of Maryland

Mariah Tanner
Economist, ATP

Jonathan Tucker
Graduate Research Assistant, Institute of Public Policy, George Mason University.

Matthew Utterback
Program Manager, MIT Entrepreneurship Center

Overview

Killing the project minimizes risk but also eliminates reward.

—*James McGroddy, former Chief Technical Officer of IBM*

Motivation for the project

Decades of theoretical and empirical work on the process of innovation suggest that that commercial firms have inadequate incentives to undertake some varieties of early-stage, high-risk technology development projects that have potential to generate radically new products and processes.[1] In the late 1980s, the stimulus of Cold War military R&D was fading. Low cost, high-quality Asian production was eroding U.S. high-tech markets. Policy makers and corporate leaders alike became concerned that U.S. firms must not only improve their productivity, but could best sustain economic growth through new product and process innovation.[2] There was evidence that firms were systematically underinvesting in leading-edge technologies and failing to commercialize the products of their own research activities effectively.[3] These concerns, buttressed by academic arguments pointing to a potential market failure in the area of early-stage technological developments, motivated new proposals for the role of government in the innovation system. A key initiative that came out of this process was the creation of the Advanced Technology Program (ATP) through the passage of the Omnibus Trade and Competitiveness Act of 1988.

The ATP and its mission

The Advanced Technology Program (ATP) stimulates economic growth through the development of innovative technologies that, despite being high in technical risk, are "enabling" in the sense of having the potential to provide significant, broad-based benefits.[4] The program's mission is to "assist United States businesses in creating and applying generic technology and research results necessary to: (1) commercialize significant new scientific discoveries and technologies rapidly; and (2) refine manufacturing technologies...

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[1] Two seminal papers: Kenneth J. Arrow, "Economic welfare and the allocation of resources from invention," in *The Rate and Direction of Inventive Activity: Economic and Social Factors* (Princeton, N.J.: Princeton University Press, 1962); Richard R. Nelson, "The Simple Economics of Scientific Research," *Journal of Political Economy*, Vol. 68 (1959), pp. 297-306.

[2] Increases in aggregate productivity are, of course, driven as much by incremental changes in products or processes as they are by radical changes. Yet, while incremental technological change is clearly of vital importance both to the survival of individual firms and to current macroeconomic growth, it is not an important area for public investment; firms have every incentive to seek and implement relatively small changes to their products and processes on their own. See e.g. Robert M. Solow, *Learning from "Learning by Doing": Lessons for Economic Growth* (Stanford, CA: Stanford University Press, 1997); also Robert E. Lucas Jr., "Making a Miracle," *Econometrica*, 61(2): 251-272, March 1993.

[3] See, e.g., Michael L. Dertouzos, Robert M. Solow and Richard K. Lester, *Made In America: Regaining the Productive Edge* (Cambridge MA: MIT Press, 1989).

[4] Public officials characterize the selection criteria for research undertaken in ATP projects as "high risk." The notion of whether high risk is a positive attribute of radical, science-based innovations or rather should be seen as a negative characteristic in project selection pervaded this project and is discussed further in the report. Venture capitalists rarely see technical risk as a positive; some R&D managers use the term to imply a project with more than the usual uncertainties as to outcomes, which may nevertheless be justified because it has higher than normal prospects for "destabilizing" a market, that is, disrupting an old market and replacing it with a new one, a position which can be protected through exclusive ownership of intellectual property.

giving preference to technologies that have great economic potential."[5] Industry proposes research projects to ATP in fair, rigorous competitions, in which projects are selected for funding based on both their technical and their economic and business merit. Since its inception in 1990, the ATP has successfully completed 40 competitions involving over 1067 project participants and resulting in 468 awards to single companies and joint ventures. The ATP has awarded approximately $1,496 million, and industry has provided approximately $1,499 million in matching funds.[6]

The decision to establish the ATP was based on two premises. The first is that, under certain circumstances, firms may have incentives that are inadequate (from a social standpoint) to fund development projects that involve a high degree of new technical content, and that therefore have high outcome uncertainties. The second is that where the expected social return is sufficiently high, it is in the national interest for the government to support the development of such potentially neglected projects. Despite the decline in military research, the U.S. government supports a very broad and deep program of research in non-commercial institutions, including universities and national laboratories. There are serious questions about the effectiveness with which the commercial world gains access to the fruits of this work, providing yet another motivation for government to enhance the diffusion of new science to new markets.[7]

The bulk of analysis by academics on government support for technology development in general, and ATP in particular, has focused on the issue of social returns, and in particular the existence, measurability, and geographical localization of knowledge and market "spillovers" resulting from the success of high-risk technological ventures.[8] Far less attention has been paid to institutional, behavioral, and non-financial barriers to innovation that may inhibit economic actors—entrepreneurs, venture capitalists, or corporations—from undertaking projects with a high degree of inherent technical risk.

Goal of the project

What is known about the attitudes and behavior of those economic actors, faced with opportunities that are at once daunting and attractive, to engage in science-based innovations? The goal of this project, as articulated by Darin Boville in the Statement of Work, is as follows:

> The aim of this research is to...better understand: (1) the decision-making process within firms, and within outside financing sources, as it relates to the funding of early-stage, high-risk technology projects, and (2) how a deeper understanding of this process can help the ATP to better identify those projects—not undertaken or pursued less vigorously by industry—that are likely to offer both broad-based technical benefits and commercial success. The questions to be explored include the following:

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[5] The ATP statute originated in the Omnibus Trade and Competitiveness Act of 1988 (Public Law 100-418, 15 U.S.C. 278n), but was amended by the American Technology Preeminence Act of 1991 (Public Law 102-245). The full text of the ATP statute is available at <http://www.atp.nist.gov/eao/ir-6099/statute.htm>.

[6] Information provided by Darin Boville, NIST-ATP, January 4, 2000.

[7] Lewis M. Branscomb and James Keller, eds., *Investing in Innovation* (Cambridge MA: MIT Press, 1998).

[8] See e.g. the seminal article by Edwin Mansfield et al., "Social and Private Rates of Return from Industrial Innovations", *Quarterly Journal of Economics*, 91(2): pp. 221-240, May 1977, and the survey by Zvi Griliches, "The Search for R&D Spillovers," *Scandinavian Journal of Economics*, 94: pp. S29-S47, 1992.

- How do industrial managers make decisions on funding early-stage, high-risk technology projects?[9]

- What external factors, especially those controlled or influenced by government, can sufficiently reduce the risk factor of projects that appear otherwise to be attractive commercial opportunities for the firm, so that firms will invest in them and seek their commercialization?

- How can ATP better identify projects that would not be pursued or would be pursued less vigorously without ATP support and at the same time are likely to lead to commercial success—with broad public benefits—with that support?[10]

In the course of the work a number of other key questions arose, including:

- To what extent is purely technical risk separable from market risk?

- What role should the evaluation of market potential—even of an application area—play in determining the value of an early-stage research project?

How the project was conducted

The joint Harvard-MIT Project on Managing Technical Risk was initiated in the spring of 1999 under the sponsorship of the ATP, represented by Darin Boville. At that time, Lewis Branscomb of the Kennedy School of Government (the principal investigator for the project) and Ken Morse, Managing Director of the MIT Entrepreneurship Center, invited a group of experienced practitioners to join academic experts for two workshops on the management of technical risk. At the first workshop, held at the Sloan School on June 22, 1999, the practitioners shared their experiences with one another and with academic participants. Two detailed cases of high-tech innovation, prepared by the MIT and HBS entrepreneurship programs under the guidance of HBS Executive Director of Entrepreneurial Studies Michael Roberts, were evaluated with the participation of the innovators and investors in those cases. Summaries of the discussion were made available to all participants. The second workshop was held on September 17, 1999. Academic participants and practitioners presented commissioned papers. Subsequent to the workshop, the leadership team and our consulting editor, Teresa Lawson, reviewed the papers. All authors were then given the opportunity to revise their contributions to address issues raised during the review process.

The present report to NIST-ATP resulting from the workshops in June and September has two main sections: (i) the report of the project team and (ii) the collection of contributed papers. The report of the project team integrates comments from participants in the two workshops, insights from the contributed papers, and references to related empirical and theoretical literature. Both sections of the report are intended to complement, rather than substitute for, surveys and statistical studies of a more demonstrably representative nature. Our discussion is intended to be realistic and practical, bringing forward the best understanding of the issues from academic studies and raising for government officials issues relevant to policy formulation and program design.

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[9] At the risk of further propagating confusion about the term "high-risk," the term is used here not in the narrow sense of "likelihood of technical failure" but instead to encompass a variety of reasons that would cause a firm not to pursue an R&D project. (Footnote in the original quotation.)

[10] Statement of Work, attachment 1 to NIST/ATP solicitation 52SBNB8C1127, dated 10/14/98.

Structure of the report and major issues addressed

We begin, in Chapter 1 of this report, by distinguishing risk from uncertainty—two words often used ambiguously by academics and executives alike. Following Frank Knight,[11] we propose that risk is generically best understood as describing a known probability of an undesirable outcome—failure—while uncertainty refers to lack of knowledge about potential outcomes. The assessment of “risk” thus depends critically on the definition of failure. Definitions of failure in turn may depend on institutional and professional perspective. The magnitude of the risk of failure in a well-specified technical project depends above all on the competence of the project team: what may be daunting to a firm entering an area of technology for the first time might seem comfortably familiar to another firm with core competence in the technology. We end this first chapter with a summary of an economic view of technical risk-taking that owes much to F.M. Scherer (KSG), who (with M.J. Peck) first presented a model of the relationship of technical risks to other sources of business risk in 1962.[12]

The second chapter of the report explores the relationship between technical and market risks. These two sources of risk are coupled through product specifications set by market expectations but constrained by technical performance. As technical learning proceeds, marketable product function may change, requiring a readjustment of the business plan, which in turn changes the product specifications. Where product (or process) specifications are likely to change dynamically, allocation of the source of business risk to either technical or market uncertainty is difficult. This fact may have important implications for public policy.

The magnitude of technical risk that business managers and investors are prepared to assume in a given project depends not just on their assessment of the competence of the project team. It also depends on the managers’ and investors’ respective evaluations of potential rewards if the project is successful. Most industrial innovations are incremental: modest improvements in process technology may reduce costs and improve quality and performance, while improvements in design and technology may alter the product to reach new or broader markets. In such cases, both technical and market risks are usually nominal. In contrast, radical product innovations—those that have the capability to destabilize existing markets, create emergent markets not previously served, and generate profits far above the norm—are often (though not always) built upon significant technical breakthroughs.

A category of innovation of particular interest in the context of our report is the science-based innovation in which both new technology and new markets are being addressed. The empirical analysis of MIT's patent portfolio contributed to this volume by Scott Shane of the University of Maryland suggests that such “radical” innovations are relatively more likely to be commercialized via the mechanism of new firm creation. The contribution by David Morgenthaler discusses methods employed by venture capitalists to assess technical and other business risks within the context of potential reward, and circumstances under which the venture mode of financing is most likely to be employed in support of new firm creation. George Hartmann and Michael Myers illustrate how large corporations, such as Xerox, address the problem of the commercialization of radical innovations.

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[11] Frank Knight, *Risk and Uncertainty* (New York: Houghton Mifflin Co., 1921).

[12] M.J. Peck and F.M. Scherer, *The Weapons Acquisition Process: An Economic Analysis* (Harvard Business School Division of Research, 1962), p. 313.

In chapter three we focus on innovations in the corporate environment. We ask the following questions: How are the dynamically varying technical and market risks inherent in a given project shared by the individuals directly responsible for the project's success, and in particular, by the technologist driving the project and the business manager responsible for evaluating its success? How do technologists and business managers communicate across professional and technical language barriers? How do large firms differ from small and medium-size firms in the manner in which responsibilities for managing technical and market risks are delegated? What are the implications of these differences—if any—for the respective roles played in the innovation systems by firms of different sizes? We examine three environments:

- the large multinational firm with a strong tradition of scientific and engineering research in its corporate laboratories;
- the mid-size firm that defines its business by its core competence in an area of sophisticated technology; and
- the startup firm created to exploit a discovery or invention that might destabilize existing markets by providing protectable, unique technology for addressing new and potentially large markets.

When we look at the cultural, institutional, and informational barriers that prevent the technical and financial communities from reaching a common understanding of both technical and market risks, we find that the medium-size, technology-defined firm may have special advantages because it can literally internalize this "communication" within a single individual.[13]

In chapter four we address strategies for managing risk. An important fact in this context is that financial returns from innovation are typically highly skewed: a few projects in any given sample are huge winners, while the majority of projects fail financially or barely earn a standard market rate of return. As studied by Scherer and experienced by most venture capital (VC) firms, the presence of skewness implies that portfolio strategies may fail to immunize an organization from the downsides of project risks. Each potential investment in an early-stage, high-risk technological project must be assessed primarily on its own merits, and not in terms of its place in an overall project portfolio. Indeed, the success of the leading venture capital firms may be based less on the ability of those firms to *pick* winners and more on their ability to *create* winners by their direct and constructive engagement in the management of the firms in which they invest.

Optimal corporate strategies of larger firms will vary depending on the size of the firm and on the sectoral characteristics of the industry. Examination of experiences at IBM, Xerox, Witco, and Lord Corporation illustrate some of these strategies for firms no longer receiving VC support. While mature firms often have the technical resources to deal with scientific complexities, they may also lack motivation to undertake science-based innovations, especially those that are aimed at creating new markets and new technology concurrently.

The fifth chapter, entitled "Overcoming Barriers to Innovation," examines the idea of government as venture investor, the role of universities in commercial innovation, and several factors that may create artificial (but financially justifiable) distortions in sector or geography for VC investment, which might suggest areas for compensatory actions by

[13] Such an ability to internalize the market-technology trade-offs requires, of course, that market and technical knowledge be of sufficiently narrow scope that both can be mastered by a single executive.

government. We conclude by acknowledging that our picture, both of entrepreneurship and of public policy, is drawn mainly from the experience of the individuals who took part in this study. Only the foresight of practitioners and an understanding of the dynamics of the U.S. economic system by scholars will allow us to examine how circumstances may differ in the future.

Since public policy must be designed to meet future realities, even as they seek to address problems identified in the recent past, we end with a perspective by Mary Good, formerly Undersecretary of Commerce for Technology, under whose authority the NIST programs fell. Both this perspective and the stated views of the venture capitalists (see especially David Morgenthaler) make clear the existence of a serious gap between the public resources available for academic and national laboratory research and the ability of private venture investors to finance research to reduce the new technical ideas to commercial form. This is the "Valley of Death" in R&D about which Congressman Vernon Ehlers speaks so eloquently.[14] It will continue to be an important focus of public policy.

Following the essay are the papers contributed by participants, which, along with the record of the discussions at the two workshops, provide most of the basis for the discussion in the essay. Included as appendices to this report are the agendas from the two workshops and brief biographies of the participants.

Lessons Learned

The ATP program addresses, and this study explores, the significant gap between the creation of a commercially promising technical concept and the demonstration that the required technology can meet the requirements of an attractive market opportunity. Typically government funds research through the concept or "pre-commercial" phase but not beyond, while VC firms invest only at a stage well after the concept phase is complete and the technology has proved viable in a prototype production setting. What then are appropriate sources of support for research projects that fall in the gap between technical feasibility and marketability—that is, research that reduces the technology to practice? Our workshops indicate that hindrances to private investment in early stage, science-based innovations are as much institutional as economic.

Major changes are transforming the institutional structure of the high-tech industrial economy. Large corporations are increasingly focusing on their role as system integrators, low-cost producers, and distributors and marketers internationally, while outsourcing much of their innovation to mid-size and smaller, technically specialized firms in their supply chain. Where will those small-to-medium size firms get their insights into the art of the possible from new science, if not from the large firms they serve? Is this another reason for public programs like ATP? A study of the sources of new technical knowledge in those smaller firms might shed light on the alternatives.

Universities represent a vital source of new technical ideas for firms of all sizes. The ferment of industrial relationships pervades even the most elite academic institutions.[15] Are universities prepared to undertake research to explore the technologies required to reduce their inventions to commercial practice and prepare them for VC investment? In the two

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[14] See e.g. Vernon J. Ehlers, *Unlocking Our Future: Toward a New National Science Policy, A Report to Congress by the House Committee on Science* (Washington DC: GPO, 1998). <http://www.access.gpo.gov/congress/house/science/cp105-b/science105b.pdf>.

[15] See, for example, Lewis M. Branscomb, Fumio Kodama, and Richard Florida, eds. *Industrializing Knowledge: University-Industry Linkages in Japan and the United States* (Cambridge MA: MIT Press, 1999).

case studies reviewed in this project (Advanced Inhalation Research Inc. and Trexel Inc.) the technology required to make original inventions into commercially marketable products was developed in university settings (in these cases, MIT and Penn State) over the course of several years. VC firms were willing to make major investments only after technical risk was significantly reduced. If this is a useful pattern, should this area of "basic technological research" receive more explicit attention from public agencies that support research? An examination of the duration of university research from the time the first patent is filed on the scientific discovery, to the time when a new firm is created or the technology is funded by a firm that purchases a license, would provide further information to guide policy.

The ATP program measures its success by assessing not only technical success or failure in its projects, but also the dissemination of the technical learning and other technical assets (such as intellectual property) to the economy. The primary mechanism for such dissemination is successful commercialization; however, ATP also makes an effort to track the flow of technical knowledge (e.g. as evidenced by patent citations) from projects that are technical successes but commercially failures.[16] This is important, as valuable technical knowledge may be created in projects that are not immediate business successes. Policy objectives in the area of early stage, technology-based research would be clarified by a better understanding of the relationship of the commercialization of a technology and the broad dissemination of that technology.

A final question for the public policy maker concerns the widespread agreement among the practitioners that technical risk and product performance are interdependent. ATP evaluates the business case for the technical projects in which it participates. If the process of reduction to practice of the technology entails changes in product performance, the firm can report to its ATP partner the consequent changes in the market segment reached by the project, and thus the business case. However, if such changes are incremental and frequent, the required reporting might become an administrative burden on both the firm and the agency. If useful technical knowledge can be disseminated independent of the particular form of first market entry (or even as a result of a "constructive failure") evaluation criteria should allow flexibility on the specific form of initial economic success, recognizing that markets change and that the results of the technology development itself will inform a company's market strategy.

I. Defining and Quantifying Technical Risk

Technical risk and uncertainty

Properly speaking, the ability to describe the "risk" of failure inherent in some technical project implies some prior experience. It is not possible, for example, to talk meaningfully about a given project having a "10% probability of success" in the absence of some cumulated prior experience (e.g. a sample of similar projects of which nine in ten were failures). To the extent that a technical team is attempting to overcome a challenge that is truly novel, it may more properly be said to be facing uncertainty rather than risk.[17]

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[16] For additional information see the ATP publications webpage, <http://www.atp.nist.gov/atp/pubs.htm>.

[17] This distinction is due to Frank Knight. In this classic volume *Risk and Uncertainty*, Knight writes (p. 20): "Uncertainty must be taken in a sense radically distinct from the familiar notion of Risk, from which it has never properly been separated... [A] *measurable* uncertainty, or 'risk' proper, as we shall use the term, is so far different from an *unmeasurable* one that it is not in effect an uncertainty at all. We shall accordingly restrict the term 'uncertainty' to cases of the non-quantitative type."

The distinction is more than an academic one. Where probabilities of failure can be reliably calculated, conditional on observable facts, risks can be easily managed. If technical projects were mere spins of the roulette wheel, a few dozen trips to the table would suffice to yield a payoff for any given 'number' chosen at random.[18] Such is not, however, typically the case with early-stage, high-risk technical projects.[19]

Uncertainty describes the absence of sufficient information to predict the outcome of a project. Mark Myers (Senior Vice President, Xerox Research and Technology, Xerox Corp.) observed that "uncertainty [provides the motivation] ... to create options.... Uncertainty and risk are quite different. Risk offers great harm; uncertainty offers great opportunity. We see ourselves refining that uncertainty so that the risks are essentially removed." Where risk is quantification of potential failure, uncertainty is the context for the opportunities that drive innovation from the outset.

As pointed out by Larry Jarrett, Vice President of OrganoSilicones R&D of Witco Corporation, the quantification of technical risk is as much of an art as it is a science:

> The elements of technical risk are not easily characterized, since real technical risk involves a forecast of how science will pan out when real people conduct experimentation, interpret results, and apply them in real situations. The elements of technical risk are chaotic, in that they are dependent on people and environment, as well as the laws of science (some of which are known, and some of which are unknown at any point in time). And elements of technical risk are not independent of one another: actions to understand and mitigate risk are interrelated through the laws of science, patterns of rational processes, and the personalities of people involved. Risk can be characterized as a probability of success, but it is always a probability given a set of premises, an expected environment, and a pattern of response with a correlated expectation of success.

This said, numerous well-established methodologies exist for assessing technical risk. Jarrett's paper describes two broad categories: anchored scales and probabilistic methods. The contribution to the report by Hartmann and Myers describes in some details methods used at Xerox Corp. to quantify technical risk.[20] The consensus of the practitioners was that, while none of the methods for assessing risk are very successful, the effort to understand the sources of risk so that they can be dealt with systematically is very important to risk management.

The difficulty of quantifying the uncertainties associated with early-stage technical projects is only one of the conceptual difficulties with a statistically based definition of technical

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[18] Knight (1921), *op. cit.*, writes (p. 46): "While a single situation involving a known risk may be regarded as 'uncertain', this uncertainty is easily converted into effective certainty; for in a considerable number of cases the results become predictable in accordance with the laws of chance, and the error in such prediction approaches zero as the number of cases is increased."

[19] One reader—a leading scientist in the field of neuro-psychopharmacology—observes that different stages of the process of drug development through 'rational' design methods exhibit different magnitudes of risk as opposed to uncertainty. In the initial stages research occurs in the context of complex models constructed from fundamental molecular biological and biochemical principles. In the context of such models, researchers are able to arrive at informed conjectures regarding the relative "riskiness" of different research paths. In contrast, once development proceeds to the stage of clinical trials, no such model exists for reliably predicting the overall effects of introducing a given molecule into *human* subjects. This intrinsic uncertainty, as much as the daunting financial burden posed by the conduct of clinical trials, creates a significant barrier to entry particular to the pharmaceutical industry.

[20] See also George C. Hartmann and Ardras I. Lakatos, "Assessing Technology Risk: A Case Study," *Research-Technology Management* (March-April 1998), pp. 32-38.

"risk." A second difficulty is that technical projects tend to have binary outcomes: they are either terminated when they encounter severe obstacles or are supported all the way to market introduction (perhaps with modifications in both technology and market objective). As observed by IBM's former Director of Research, James McGroddy, at our June 1999, workshop:

> [Risk] is a statistical term, and therefore, I think, very inapplicable to single projects…. When you go to jump across the chasm, you either make it or you don't. It's not a continuous thing. And I think what risk management is about is identifying the points at which you can fall in the chasm, focusing your energy and focusing the rate at which you invest, consistent with the view that you've got to jump across this Grand Canyon on your motorcycle.

McGroddy observed that risk is the price of doing something that appears to be worthwhile. Risk is not desirable in itself, nor is risk necessarily something to be minimized. An important attribute of risk-taking is that it is deliberately undertaken because the rewards, multiplied by the (presumably known or estimable) probability of achieving those rewards, exceed the cost of taking the risk. After all, as McGroddy noted, killing the project minimizes risk but it also eliminates reward.

Risk of what? Defining failure and success

If technical risk describes the likelihood of failure in a technical project, we must ask: what constitutes both failure and success? Clearly, both failure and success are defined in terms of objectives. These objectives may be institutional, personal, or defined at the level of the project. Multiple objectives in a technical project directly imply multiple categories of failure and success.

INSTITUTIONAL OBJECTIVES

Consider first institutional objectives. A venture capitalist, for example, may define success of a technical project exclusively in terms of the expected return on invested capital, regardless of whether the firm abandons one particular set of specifications for another, or even changes the market objective altogether. Success to the VC will thus depend absolutely on the *commercial* viability of the technology in question. In contrast, a government technology project may, for example, emphasize specific national security needs, environmental objectives, and/or broad benefits to the economy (a.k.a. "knowledge spillovers") that may ensue from overcoming a particular technological challenge. In the last case—that of projects emphasizing spillover effects—the transfer of technical knowledge and generation of positive market dislocations (Schumpeterian "creative destruction") *may* occur through commercialization. However, knowledge spillovers *may also* occur through transfer of intellectual property created as result of the project (e.g. patent citations) or from the knowledge embodied in project researchers as they move forward to new research environments. At the June workshop, Dean Howard Frank (University of Maryland, Robert H. Smith School of Business, described the methods he used as a DARPA program director: "The level of specification of different technical projects…[was]…very loose, so that you could define success in many ways. You will never find an unsuccessful DARPA project." In this way he suggested that technical projects with sufficiently ambitious goals almost always produce useful technical knowledge and experience. The same cannot be said of investments measured by returns from sales in competitive markets.

The university, in turn, is defined by its own unique mission and objectives. Foremost among these are education and the advancement of knowledge—potential objectives for firms and government as well, to be sure, but ones that are at best secondary in those settings. During the June workshop, Prof. Robert Langer of MIT, out of whose laboratory more than 25 companies have been created, warned of evaluating university research laboratories by metrics similar to those used to evaluate commercial firms. Taking into account his primary responsibilities to his students and to the advancement of science, Langer stated of research projects conducted in his laboratory: "I have trouble identifying many failures by my standards as an MIT professor."

PERSONAL OBJECTIVES

The extent to which any institution—be it a corporation, a venture capital firm, or a university—is able to achieve its mission is dependent in large part on the harmonization of the objectives of the institution as a whole and those of individuals comprising the institution. In his chapter contributed to this volume, Josh Lerner discusses the importance of harmonizing personal and institutional objectives in the context of new firm formation and funding. If a new firm raises equity from outside investors, managers have an incentive to engage in wasteful expenditures because they do not bear their full cost; if instead the firm raises debt, managers have an incentive to decrease levels of risk. Furthermore, even if such problems can be mitigated so that the managers are fully motivated to maximize shareholder value (i.e. the objectives of investors and managers are fully harmonized), informational asymmetries may complicate efforts to raise capital. The fact that potential investors know less about the inner working of the firms they fund than the managers who run the firms can lead to problems for both groups. For example, managers will have an incentive to only offer new shares in the firm if the stock is overvalued; concerns over informational asymmetries may lead investors to offer funding under less than favorable conditions. Lerner views venture capitalists as financial intermediaries who are specialized in mitigating such generic problems arising out of imperfectly harmonized objectives of entrepreneur/managers and potential investor, and thereby minimizing financing constraints that exist on the funding of new firms.

A related, but distinct set of competing personal objectives defines the relationship of technology project managers (be they executives in a corporations or CEOs of start-up firms) and the technologists directly responsible for the work of the project team. The information asymmetry is nowhere greater than between the technical expert who champions the project and the financially responsible manager who must commit resources with an inadequate personal mastery of the technical challenges and means for their solution. Thus the nature of the communication, and most importantly the degree of trust between these two parties is probably the most critical element in the management of technical uncertainties.[21] Both parties must accept the reality of the uncertainties than can lead to failure. For the innovator they derive from the unpredictability of nature and uncertainty about how long the confidence of the investor can be sustained. For the investor or business executive the uncertainty about whether the innovator will be successful must be based on prior performance and trust.

In this situation both parties must face the possibility of failure. But it matters very much how that failure occurs. The technologist has at least two ways to fail. If nature proves unyielding, despite a well-organized and managed technical effort and good communications with investors, failure is honorable; if the team is ill-prepared, the effort poorly staffed,

[21] Later in this report we observe that for this reason the middle-sized, technology specialized firm may have intrinsic advantages from this point of view. The individuals who produce the innovative ideas and reduce them to practice may also have profit and loss responsibility in the firm, dramatically reducing the information and trust asymmetries.

knowledge of the state of the art or of the competition is inadequate and management feels deceived, then failure is dishonorable. Honorable failure will not markedly reduce the technologists' chance of being asked to direct future high-risk research efforts, whereas dishonorable failure has potential to be career ending. Similar distinctions between honorable and dishonorable failure exist for both technology managers and entrepreneurs. For technologist and managers alike, long run personal success will depend far more on cumulative reputation for effectiveness than on the outcome of any single project.

University professors may define their own success or failure in terms of any subset of an exceptionally large and varied set of professional objectives, including (but not limited to) pedagogy, research productivity, administrative effectiveness, aptitude for clinical work, ability to raise funds for research, and public service. Even in the absence of explicitly commercial incentives within the academic setting, there is an inherently entrepreneurial aspect to the U.S. academic culture. "It is amazing how much being a professor is like running a small business," remarks one faculty member quoted by Henry Etzkowitz in his article contributed to a recently published volume on university-industry relations. "The system forces you to be very entrepreneurial because everything is driven by financing your group." Another faculty entrepreneur observes: "What is the difference between financing a research group on campus and financing a research group off campus? You have a lot more options off campus, but if you go the federal proposal route, it is really very similar."[22] This inherent correspondence of academic and entrepreneurial cultures has become significantly reinforced in the past twenty years by both the passage of the Bayh-Dole act and the dramatic growth of the biotechnology industry, largely as the outcome of successful efforts to create new firms out of university research efforts. Incentive structures in university research laboratories have by both design and necessity become increasingly similar to those found in either corporate research laboratories or start-up firms. A current and ongoing concern for university administrators and policy makers alike is ensuring that universities as institutions, and university professors and researchers as individuals, receive their fair share of the direct monetary rewards from their innovative efforts while preserving the particular objectives which distinguish and define the university.

THE PROJECT

Informed by the above discussion of the many parallel objectives, both personal and institutional, by which success and/or failure may be defined, we can now turn our attention to the objectives of the technical project itself. Long before the market delivers its judgement on the value of a new technology, it must pass through a number of stages of development.

Any temporal partition of the innovation process is bound to be arbitrary and imperfect. A distinction that has the benefit of being often employed by practitioners (particularly in the life sciences) is that between "proof of principle" and "reduction to practice":

- *Proof of principle* means that a project team has demonstrated its ability, within a research setting, to meet a well-defined technological challenge. It involves the successful *application* of basic scientific principles to the solution of a specific problem.[23]

[22] Henry Etzkowitz, "Bridging the Gap: The Evolution of Industry-University Links in the United States", in Branscomb, Kodama, and Florida, eds. (1999), *op. cit.*, p. 218.

[23] In the life sciences, the term "proof of principle" is achieved "when a compound has shown the desired activity *in vitro* that supports a hypothesis or concept for use of compounds" (definition from Karo Bio AB <www.karobio.se>, a drug discovery company). Prof. Ron Burbank of the Stanford Computer Science Department at Stanford (<www-

- *Reduction to practice* means that a working model of a product has been developed in the context of well-defined and unchanging specifications. Product design and production processes can be defined that have sufficient "windows" for variability as to constitute a reliable product made through a high yield, stable process. In simple English, the technical risk has been sufficiently reduced when the innovator-entrepreneur can say to his managers and investors, "Yes, I can do that, and do it at a cost and on a schedule in which we can all have confidence."[24]

Failure at either of these stages may involve an unexpected technical problem that available skills and knowledge cannot solve. Alternatively, as in the example of superconducting Josephson technology as a possible replacement for silicon transistors cited by McGroddy, the technology may be said to fail, *despite* successful proof of principle and reduction to practice, because the pace of progress in the competing and better-established technology is seriously underestimated.

While there is value to clearly defining project success and failure as a prerequisite to evaluating incumbent risks, some technical managers in private firms may choose to leave the question of success or failure in suspension for a considerable period of time. David Lewis of Lord Corporation describes the strategy of burying a technology failure in "a shallow grave." A manager may stop the flow of funds to a project whose progress is blocked by an unresolvable technical difficulty, but retain both the technical knowledge and the awareness of market potential, pending a new idea that would justify resurrecting the project. Lewis further observed that the ability to quantify risk is dependent on how far the project is from the market: "The more that is known and understood about the total [market] area, the higher the probability of correctly assessing and dealing with the specific issue of technical risk. This is especially true during the market requirements phase."

At the June workshop, Larry Jarrett further observed that since failure is an outcome of the uncertainties associated with risk taking, failure is to be expected in an innovative organization. Furthermore, a persistent team can often turn a technical "failure" (in terms of original objectives) into an *ex post* market success. (This phenomenon is facetiously described in one company, as "If you can't fix it, feature it.") Jarrett and others noted that there exist many cases in which the final success is not the use originally intended. Value in failure, for established firms, may be found in residual technology values that are later used in as-yet-unforeseen markets, or the market and business learning from a failed project may contribute to success on the next venture. However, as Steve Kent of GTE-BBN Corp. observed, the extent to which failures are "useful" in this sense depends on firm size. Startup companies whose big projects fail are likely to just go out of business, in which case technology and business learning is preserved and transferred only by former employees who go to work elsewhere; big companies may be able to place failures into the portfolio for the future.

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db.stanford.edu/%7Eburback/>) describes the proof of principle phase in software development as follows: "[T]eams work simultaneously on all phases of the problem. The analysis team generates requirements. The design team discusses requirements and feeds back complexity issues to the requirement team and feeds critical implementation tasks to the implementation team. The testing team prepares and develops the testing environment based on the requirements... One of the goals of this stage is for the teams to convince themselves that a solution can be accomplished."

[24] In the software setting, Burbank (*op. cit.*) terms this the "prototype" stage, which he describes as follows: "The requirements and the requirement document are frozen and placed under change-order control. Changes in requirements are still allowed but should be very rare... One of the goals of this stage is for the team to convince non-team members that the solution can be accomplished."

Competence

Technical risk is not inherent in the technical processes being explored. As David Lewis noted "Managing and understanding the risk is really relative to how much you know.... The more familiar you are with the market requirements, etc.—even though the technology may be very difficult—your ability to put a risk factor on it, deal with it, and make the early decisions before you're well down the road, is much better."[25]

In his paper contributed to this report, Scott Shane presents a related finding: the most technically radical innovations are most successfully commercialized through the creation of new firms. An inference from this finding is that a given undertaking that might have been judged unacceptably risky by established firms may be acceptably risky for a new firm that has deliberately assembled more of the needed competencies.

One may also include within the concept of "competence" the information that is available to participants, much of which will have been garnered through prior experience. Until recently, theoreticians modeling entrepreneurship have assumed that all potential entrepreneurs would discover the same 'optimal' opportunities in response to a given technological change.[26] Shane's recent work (based on in-depth field work on entrepreneurs who exploit a certain MIT invention) shows that entrepreneurs do not discover the same opportunities in response to a given technological change, but rather tend to discover opportunities that are related to the information that they already possess.[27] Different entrepreneurs see different opportunities in a given new technology.

Modeling risks in new product innovation

If risk is hard to quantify, can the stages in the innovation process at least be modeled in such a way as to illustrate the different ways in which risk arises in a high-tech innovation?[28]

In his paper contributed to this report, David Lewis describes the way in which technical risk is manifest across three stages in the product development process: (i) basic invention/concept; (ii) achievement of market requirements; and (iii) robust commercialization. The first of these stages describes the type of work undertaken in a corporate or (increasingly) university research laboratory. This stage ends with a laboratory

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[25] At the same time, one leading scientist observes that the very fact of experience may bias successful research teams away from paths of inquiry that oppose conventional wisdom—even when such paths offer the prospect of major research breakthroughs.

[26] See, for example: D. Evans and B. Jovanovic (1989). "An estimated model of entrepreneurial choice under liquidity constraints." *Journal of Political Economy*, 97(4): 808-827; R. Khilsstrom and J. Laffont, (1979). "A general equilibrium entrepreneurial theory of firm formation based on risk aversion." *Journal of Political Economy*, 87(4): 719-784. References drawn from Scott Shane (2000). "Prior Knowledge and the Discovery of Entrepreneurial Opportunities", *Organization Science*, forthcoming.

[27] Scott Shane (2000) *op. cit*.

[28] There is a large literature on innovation models. A somewhat neglected literature has the virtue of recognizing the dynamic nature of science-based innovations, which change the environments within which they are launched and thus alter the nature of the risks encountered. This model, from an unpublished paper by Henry Ergas, comprises four stages: *generation* (all the R&D up to first entry to production), *application* (initial commercialization), *verticalization* (changes induced in the behavior or technology of suppliers, customers and end users), and *diffusion* (regulatory, environmental, even cultural changes brought about by the innovation). All four stages must run their course before the magnitude of returns and future prospects for growth can be ascertained. Small wonder that technical risk alone cannot predict the observed magnitude of skew in investment returns from such innovations. [Ergas's model is described in] Lewis M. Branscomb and Young Hwan Choi, *Korea at the Turning Point* (Greenwich CT: Praeger Press, 1996) p. 202.

demonstration of phenomena that, if commercialized, might offer attractive business opportunities. The second stage begins when a firm takes up the concept and begins to reduce it to practice—that is, to demonstrate the designs and processes necessary to achieve the assumed requirements of the market that make up the business case. The third phase, which Lewis characterizes as robust commercialization, encompasses the firm's response to a well-understood market opportunity with a full product line at competitive costs and quality. Note that these three stages are not intended to imply a linear model of innovation. Research activity in the first stage, for example, may be triggered by a "stage three" market discontinuity that signals potential opportunity. Reduction to practice (stage two) requires the satisfaction of technical specifications, regardless of how those specifications arose.

Lewis's model is consistent with the model advanced by Scherer and Peck in 1962 and summarized in Scherer's most recent book.[29] Scherer observes to begin with that "in an R&D project, uncertainties decline as spending accelerates". Figure 1 illustrates the relative rate of decline of uncertainty. The product will pass through a technical feasibility phase, a development phase, an introduction phase and a market acceptance phase; uncertainties concerning technical feasibility are resolved much earlier than those concerning cost and market acceptance. As risk falls, moving down on the axis, the firm accelerates spending; if the technical feasibility phase raises unexpected difficulties, the firm may choose not to accelerate spending.

In the event of technical difficulties that could not be foreseen, a project can be stopped at a time when only a fraction of the planned expense has been committed. This fact reduces the barrier that technical dimensions of risk otherwise pose.[30] The largest elements of business risk are referred to collectively as market risks: uncertainties attributable to competitors and consumer responses and by all the other factors that together determine business outcomes. Scherer hypothesizes that:

> The cheapest thing and the most important thing to do first is to demonstrate that the technology actually works in an environment that looks something like the manufacturing environment. Until you've done that it's pretty hard to demonstrate that the product function is what the conceiver of this program had in mind, and certainly to get some quantitative information about likely unit cost of production... even though the market risk is the surely the biggest... risk that one faces.

The Xerox innovation model is described by George Hartmann and Mark Myers., The invention phase (what Lewis refers to as the basic invention/concept stage) is seen as located in Corporate Research. The next stage, that of technology development, includes the

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[29] Scherer (1999), *op. cit*. Again, the Scherer-Peck diagram originates from a study of weapons research. Note that, in weapons research, the technical feasibility phase will have a longer lead-in time (hence longer curve), and although such technology does not necessarily attain "market acceptance" in the traditional sense of the term, quantities ordered vary widely, depending upon the weapon's effectiveness in meeting emerging mission needs, and weapons developed for one mission often turn out to have other unanticipated uses. (Note that product specifications in military programs are normally quite rigid, while commercial specifications may evolve constantly, as more is learned about the technology and about the market.) At the September 1999 meeting, Scherer identified in this context the example of the F105 fighter plane, originally intended for nuclear weapons delivery, which ultimately was used extensively in Vietnam because its design allowed for a relatively low-tech gun to be mounted on the fuselage.

[30] There may be a dilemma posed by this observation for public policy. A government research contract, bearing part of a firm's cost but imposing an obligation for a best faith effort to solve the technical problems, might serve both to reduce the technical uncertainties facing a project and also make halting the investment more difficult when trouble is encountered.

transfer of the invention to the product organization, and the selection of the technology required for the project and for product design. The following five stages, seen as post technology development, demonstrate, produce, launch and maintain the product. Like others in the workshop, Hartmann observes that "the process of refining the technology capabilities and customer requirements, which eventually evolve into a specification, is iterative ... and has a virtuous learning nature to it. A powerful technique for evolving and refining the specification is Quality Function Deployment (QFD), whose formalism emphasizes the intimate linkage between the technology characteristics and market requirements."

II. Technical Specifications and Markets

Is it really possible to separate technical risk from market risk? In a radical technical innovation, can one expect to define product and process specifications, then engage in research that is sufficient to reduce technical uncertainties to an acceptable level? The judgment of most of the practitioners was like that of Larry Jarrett: "Risk is defined ... with respect to a specification, and you don't know what the specification is—or what it should have been—when you begin."

Specifications are the link between technical challenges and the market. Specifications may be unstable for several reasons. In the most extreme situation, new information about the requirements of the customer may change, or may become revealed, during the execution of the product program. If the available technology cannot adapt to this change, the project may die (or be placed in what Lewis calls "a shallow grave") awaiting someone in the firm to make a discovery or invention that addresses the new requirement.

David Lewis described one case in which this happened:

> This is an example of a direct articulated need by a customer, in the general area of adhesives for auto assembly where Lord is currently a supplier. Specifically it was for an application that was both new to us and in some respects a major extension for our customer. What appeared to be a good technical invention was in place and we moved well down the path of specific product commercialization. Market requirements, however, soon became a major difficulty: the requirements were initially detailed by the customer but changed with time and understanding. Further final application testing was available only at the customer's location, and special tests were added during the protocol. We were thus vulnerable to surprises that came out of the customer's work, as testing went on and as the customer's understanding of requirements, and ours, evolved. Well into the project, a new test was put in place that our product could not pass. In previous instances, we had been able to modify our base technical approach to achieve success, but the new requirement was such that our base invention technology was now unsuitable for the application. It was a surprise to us, a curve ball that completely changed our original assessment of technical risk, because the market requirements were now different. It essentially put us back to square one, searching for a new technical innovation that could meet the new requirements. This is an example of a case where technical risk was considered and understood at project inception, but where technical risk changed drastically with changing understanding of market requirements.

More commonly, the specifications change when the performance of the technology is different from what was assumed at the beginning of the projects. Those differences require an adjustment in the specifications, which in turn requires that market estimates be adjusted, which in turn may suggest a further adjustment in product specifications. Mark Myers remarked that: "specifications are really where [markets and technology] interact, because ... you cannot make technologies fit to a market until you're really able to specify what the market requires. A major failure in programs is the interaction of technology maturation and change of specifications." Specifications in turn may change for a number of reasons, of which two dominate: (1) because competition causes a discontinuous change in the marketplace, and (2) because of 'spec. creep'—incremental revisions of project goals by the technical team in response to reinterpretations of market needs.[31]

Scherer observed that when you know what the technological possibilities are and understand what the consumer wants, you can go into the development process and write specifications with some degree of confidence. If that is not possible, you keep the spending low and explore the interaction between the technological possibilities and the needs expressed in the marketplace. Of course, by holding down spending, you may fall victim to a faster, more expensive competing project. If a competitor beats you in a small market, it's OK, but you don't want to lose in a big market. In that case, you will try to find an alternative strategy (e.g. parallel paths). Thus Scherer's model (Figure 1) is not a profile through time of work to meet the four goals—technical reduction to practice, verification that product function will meet specifications, determination of probable unit costs of production, and all other market and business risks. Instead, it is a representation of the allocation of R&D resources to the four goals, seen in hindsight; the actual work skips back and forth among the four tasks.

Interaction of technologists and executives or investors

Just as each of the actors—technologists, business executives, and investors—has different objectives[32], so do they have different perceptions of technical risk. Furthermore, these different actors may have different ways—even different language—for communicating about risk. When the technologist has little or no control over the capital required for a project, and the business executive or venture capital investor has little understanding of the details of the technology, their attempts to share their understanding of the business risks (both technical and financial) may be quite imperfect. Yet share they must, if the project is to proceed.

This is not a serious problem in the dominant case of incremental changes in the technology or in the target market. Prior experience will serve as a surrogate for understanding. But when a radical change in technology is proposed, especially if it is intended to create as well as address a new market, the way the innovators and investors share information becomes a critical factor to their success.

At the June 1999 workshop, Richard Rosenbloom observed that technical risk can only be defined in terms of specifications, which are defined by the marketplace and the business model employed to extract value. "One conjecture would be that one of the problems companies have in managing technical risk is that they leave it in the hands of the technical staff." But do they have a choice if the managers are not technically trained? Or if the innovation is sufficiently radical that the market it anticipates does not yet exist?

The technologist then has a special problem. The consequences of technical failure (and probably business failure too) rest on his or her shoulders. While failure to predict markets

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[31] Observation due to Finbarr Livesey.
[32] See above discussion of definitions of success and failure (p. 10).

or competition can be shared by the technologist and the business executive, only the technologist can address the reduction of technical risk.

Steve Kent of BBN/GTE observed that twenty years ago, technologists were in charge but they did not understand markets. Now business people are in charge, but they do not understand the details of the technologies. The charts and the spreadsheets have to look convincing. Executives within the corporation act as the venture capitalists selecting projects and expecting high returns. GTE is a multi-billion dollar company, so its executives, Kent suggests, are not interested in proposals that do not promise at least the possibility of yielding something like a half billion dollars in additional revenue. To get any of the (abundant) money for R&D you have to promise a lot of money.

If technical characteristics and market requirements have to be considered jointly, how should a firm organize itself to make these tradeoffs effectively? Jarrett suggested that it may be preferable to work with the marketing people so that they can perform this function: that is, train the marketing people to understand the technology, and try to get to the real market. David Lewis noted that at his firm, Lord Corporation, market managers are key people involved in definition of market specifications. When a small or medium-size company is participating in an integrated supply chain to a large firm, this sharing of market and technical understanding must also bridge the firms in the supply chain, making it even more difficult to achieve.

Radical (critical/emergent/disruptive) technologies

As evidenced by **Figure 3** the fact that a technology may be based on new science and be quite untested does not necessarily mean that the innovations envisaged are radical or that markets will be significantly disturbed by their introductions. New science flows into production processes to increase productivity or quality with little change in product utility. Similarly a clever market innovation, such as the application of the tools of one industry to destabilize markets in another, may well have a radical and disruptive market effect. The special case we explore here is represented by the diagonal path in **Figure 3**, in which there are concurrent technical and market innovations.

Every high-tech manufacturer wants to destabilize his competitors' markets by the introduction of a protectable innovation that creates its own market and displaces the established way of doing things. In such a situation all the risks are compounded: technical novelty, ambiguous specifications, an untried business model and, as in the case of the start-ups that Shane found are the best institutional model for such innovations, sometimes untried management. Technological support from an outside source can be very helpful in reducing the investor's concerns about risk, but it will not, according to many of the workshop participants, reduce the largest sources of risk substantially.

At the workshops and in their contributed paper, Mark Myers and George Hartmann (Principal, Strategy and Innovation Group, Xerox Corp. Research and Technology) described some of Xerox's experiences with radical technologies—the upper right hand quadrant in their technical-market risk typology.

- *The Xerox 8010 information system and 6085 professional workstation with ViewPoint icons and windowing software*: In 1981, the Xerox 8010 information system and 6085 workstation represented brand-new technology in an untried market. Competitive risk was low due to first-mover advantages, but intellectual property protection was weak. The market was not prepared to use the product, and no complementary industry existed. Customers had limited choices; nevertheless they could choose from three versions: network, remote, and stand-alone. The business plan was not clear. Xerox had the world's best computer scientists on the project, so the technical competency was high. But customer requirements were not well known, and product specifications were risky. Although several document-processing applications were offered, in hindsight, the "killer application" turned out to be the Lotus 1-2-3 spreadsheet that went out with the IBM personal computer. Xerox itself became a major user of the 6085, with tens of thousands of units installed throughout the company, but the product had limited commercial success, and it was later abandoned.

- *Hewlett Packard thermal inkjet printing*: Initially, HP launched this new technology into an existing market of pen plotters and dot-matrix printing: a technology displacement without high market risk. This fits in the "discontinuity" quadrant in **Figure 3**. After perfecting and refining the technology, HP moved into new markets of desktop printing and, more recently, into home photo-printing (examples of the leveraged base quadrant).

- *The Xerox Liveboard* provides another example of the radical quadrant, with a new technology in a new market. Liveboard was a computationally active whiteboard with remote communications capabilities using Unix. This was launched into a new market before working out a sound business model, in the belief that a market "had to be out there." The product price was high, and opportunities to develop manufacturing economies of scale were limited. Eventually Microsoft Windows was substituted for Unix because customers wanted compatibility with existing systems, which took away some proprietary technology opportunities. Following a short exploratory market probe, the product was withdrawn.

Markets, competitors and the pace of development

At the September workshop, Marco Iansiti of Harvard Business School observed that the paper by Hartmann and Myers underscores the point that the ability to clearly define a technical challenge depends on understanding of the form the technology will take when it reaches the market. Thus, while technical and market risks may be separable in a stable market, they will not be so in the sort of rapidly evolving market that accompanies the introduction of a radical technology. One factor determining levels of risk is a greater time between the introduction of the technology and the market acceptance of that technology. The faster technological development proceeds, the more difficult the task of separating technical from market risk.

At the September workshop, George Hartmann displayed the "Takanaka diagram," which originated in Fuji Xerox; it assists in framing the evolution of the development process by plotting the technologist's projections of the planned improvement of a performance or quality attribute against time. In this way, two kinds of risk—schedule and feasibility—are addressed. This plan may also be contrasted with improvements expected in the state-of-the-art of the same performance or quality attributes, enabled by technology advances of competitors across the industry. The research team on a given product has to be certain that it is aiming above that state-of-the art trend; this is known as "competitive technology trend analysis." In a fast-moving areas of new technology, innovators chase a moving target. Speed is of the essence, which requires the concurrent management of technical, product function and market risks.

With respect to the pace of development, Hartmann and Branscomb also contrasted to prototypical U.S. model to the Japanese model: the US tends to look at the top of the line machine at a big price and ties to capture the smaller applications and consumer markets later whereas the Japanese tend to aim at lower market segments, with a lesser regard for top quality and move to the high end with the most viable products: instead of a cost-learning curve they have a performance leaning curve: is essence the Japanese would rather be at the bottom of the market at 1/10th the cost.

Kenneth Morse, director of MIT's Entrepreneurship Center, noted that taking a portfolio approach to R&D may have the undesirable effect of making top management relax. He cited as an example Wave Division Multiplex technology, which was developing slowly at AT&T. Venture capitalists and MIT observed this pace of development and concluded that "Lucent is asleep." They decided to move quickly. Three competitor companies on Route 128 pushed AT&T into moving faster and succeeding. Competitive challenge can be a great stimulus to technical progress.

Decisions regarding the pace of development may be based as much on financial considerations as on technical and market assessments; in this context as in others we expect firms to adjust plans when the perceived rewards are greater than the costs. In the following two sections we review some of the fundamentals of *financial* risk, and then discuss some of the generic strategies that may employed by a firm to manage financial risks and reward.

The financial fundamentals of risk and reward[33]

In the case of a public security, the concept of risk—and the way in which risk influences desired returns—is clear. The required return on the stock is a function of the company's "beta"—the ratio of its volatility relative to the volatility of the stock market overall. This beta is, in turn, a function of the volatility of the firm's basic business, as well as the level of debt in the firm's capital structure (higher debt raises the volatility of the cash flows available to the equity holders).

In the case of an investment in a private firm, the beta cannot be derived from actual data. Thus, "risk" must be estimated by the investor. We would expect these estimates to vary according to the perceptions of the individual making them. Indeed, all other things being equal, we would expect that the individual who perceives the least amount of risk would be most likely to make the investment (or, willing to pay the most for a given share of the equity in the firm).

Note that, according to financial theory, investors are only compensated for taking systematic—or undiversifiable—risk. So, the theory goes, investors who had financed one company that was working on drug-delivery technology involving inhalation (see the AIR case discussed in Section IV below) could diversify away some of their risk by investing in other inhalation drug-delivery technologies. Therefore, investors should not be compensated for risk they can mitigate through diversification. However, in the case of private, venture capital–type investments, it seems that investors do get rewarded for taking unsystematic (business-specific) risk. Whether this is because the decision maker (venture capitalist) cannot make enough "bets" in one investment pool to truly diversify away the unsystematic risk, or because the market is inefficient and simply allows the venture capitalist to earn an excess return, is not clear.

What then are the factors that would influence a potential investor's perception of the risks in a fledgling high-tech venture? These factors would include perceptions of the probability

[33] This subsection is authored by Michael Roberts, Senior Lecturer at the Harvard Business School, consultant to the project.

of losing the entire investment; the amount of that investment (especially relative to the size of the pool of funds available for investment); and the level of uncertainty in the decision-maker's mind regarding the accuracy of the above estimates. Presumably, entrepreneurs themselves gauge risk in a similar manner, but include non-financial outcomes as well, including any detrimental impact on their career, status or professional reputation.

Having assessed the level of risk in the prospective investment, the investor may be unwilling to commit time or money to a venture either because the uncertainty seems too high or too costly to reduce—the entrepreneur or investor may simply feel that "I'm never going to be able to make a sound judgment about this"—or because the probability of losing money, or the amount of money at risk, is simply not a match with the investor's or entrepreneur's risk profile.

Alternatively, even when a decision is made to commit time or money to a venture, a perception of high risk results in a requirement for increased return, in the form of a higher equity share in the proposed venture, or some other mechanism for receiving a preferred return. In addition, investors seek to mitigate risk through some mechanism of control, including board seats and other governance mechanisms (e.g., shareholder approval required for issuance of new debt or equity securities).

Reward—or return—is the set of cash flows that accrue as the result of an investment. In practice this typically occurs all at once, either upon the sale of the firm, or upon the distribution of shares in the newly public firm to investors. Rarely does a start-up firm pay out cash dividends to its investors over multiple years.

The most common measure of return for investors is IRR (Internal Rate of Return) which is a function of the cash inflows and outflows, and the timing of these events.

The rewards for the entrepreneur are more complex, and—while they undoubtedly include financial returns—more personal dimensions also weigh heavily. Autonomy, control of one's destiny, the admiration of one's professional peers, and personal satisfaction of creating an enterprise are significant motivations.

Making Decisions: Weighing risk and reward[34]

The formation of any new enterprise represents a belief about the risk and reward equation: specifically, a belief that potential reward outweighs risk.

Investors and entrepreneurs simultaneously evaluate and attempt to manage the reward/risk equation. That is, it is not sufficient merely to judge that "this venture is risky because it will take a lot of money to get this technology to market." That is an important insight, but it leads immediately to the question of how the amount of money can be reduced. Can the technological hurdles that lie between proof of principle and reduction to practice be itemized and prioritized to minimize the likely expenditure of funds?

Once outside capital is raised, the primary lens through which risk and reward are evaluated—and decisions are made—is a financial one. In financial terms one manages the risk/reward equation by improving present value. This can be accomplished through several strategies:

[34] This subsection is authored by Michael Roberts.

- Obtaining more cash inflow at a given point in the future;
- Obtaining the same cash inflows, but sooner;
- Reducing the cash outflow (investment and cumulative operating losses);
- Making the same cash outflows (investments) but later;
- Reducing the risk (perceived uncertainty) of the cash inflows, thereby reducing the discount rate.

Weighing technical risk and market risk

The general consensus among practitioners at the workshops was that technical risks are, in general, more manageable than other sources of risk, in the sense that the research process for dealing with them is understood. Venture capital investors such as David Morgenthaler took the view that "Many of the good venture capital firms that we know… say that they would rather take a technical risk than a market risk. I think that's partly because we can evaluate technical risk better. To launch a fascinating technology out into a very uncertain market is an interesting experience and it's usually cost me a good deal of money." Richard Burnes of Charles River Ventures agreed: "We love technical risk. When we find a team that comes in where we see [technical] risk, typically we know where to get the people who can execute on that risk." Myers supported this view by noting that technical risks are much more accessible to deterministic tools than are some of the market risks at an early-stage in a new product innovation.

III. Institutional differences: Large, medium-size, and new firms

> *I believe quite simply that the small company of the future will be as much of a research organization as it is a manufacturing company, and that this new kind of company is the frontier for the next generation.*
>
> —*Edwin Land, founder of Polaroid (1944)*

Large corporations and the role of research labs

There is a widely held view that very large firms address technical risk quite differently from the way smaller firms or startups deal with it (see comments by Kent, McGroddy, Hartmann and Myers). They typically are better placed than smaller firms to address technical risk; they have corporate research laboratories with scientific staffs, superior access to capital, and often a long record of having introduced innovations into the market. Furthermore they are often more effective than smaller firms at incremental innovations and at process innovation through which production costs are lowered. What they may lack is the incentive to take significantly high risks in order to enter or create a new market. The problem is simply that the revenue and profit in the first five years or so is likely to be insignificant in the consolidated balance sheet. This reluctance is only ameliorated if the firm has a very strong commitment to a technology-based growth strategy over a long term future, and believes that internal innovation can compete with innovation by acquisition.

Of sales totaling about $1.7 billion/year (including Fuji Xerox), the Xerox Corporation, for example, spends about 6.5 percent on R&D each year. A fifth of this spending is on research and advanced technology development, while about 80% goes to product development. At Xerox, the management of these resources is a highly structured process. Sometimes a single product can involve half a billion dollars in development. The paper by Hartmann and Myers describes a sophisticated process for identifying sources of both technical and market risk and attempting to quantify them. The higher the risk, the more value a good quantification system would have, and of course, the more difficult it is to achieve. Their conclusion is that even if the effort at risk quantification is not fully successful, the process of attempting it has value in calling attention to key issues that must be managed.

James McGroddy (in his paper "Raising Mice in the Elephant's Cage") observed that large, established firms often fail to capture a significant share of the new opportunities in their industry, especially when they enjoy a strong, defensible position in some key sector; as a result they will lose market share to smaller, more agile enterprises. His explanation for this observation focuses on the different style demanded of those who would defend a known market with a set of loyal customers whose needs are well understood. This style he characterizes as like playing chess. The game is complex, but the rules are understood and the ability to look many moves ahead will be rewarded. Science-based innovation, on the other hand, is more analogous to the game of poker. "This willingness to place small bets in highly uncertain conditions, using intuition more than analysis, trusting one's own judgment, is an essential element of developing a strong early position in new areas of opportunity."

McGroddy observes that a large firm with deep technical roots has some advantages over the startup with limited resources. A promising new technology can be *incubated*, perhaps for several years, without the compulsion to move quickly to market. When the decision to commercialize is made, the depth of understanding of the technology reduces substantially the uncertainties surrounding the technical challenges. But when the time is right for the project to be *excubated*—that is, made subject to external forces such as customer feedback, competitive capabilities, market changes—the large firm too often finds it "safer" to house the project with the structure of the existing business—the "elephant's cage" of McGroddy's title. Thus, technical risk takes the form of mismatch between the potential of the technology and the opportunities in the market, rather than a question of endogenous difficulties in the science and engineering. It is not surprising that in such firms there is often quite a lot of tension between the creators and champions of a new technical concept and the senior engineers and business executives who are responsible for executing the product program with minimal risk to schedule and business success. Mid-size, technologically specialized firms may suffer less from this tension.

Do dominant market leaders impede or facilitate the development of radical technologies? It is conventional to believe radical product innovations are more likely to be found in small firms, even in startups, a finding consistent with Shane's work. Large manufacturers such as IBM, however, may well lead in process innovation, for productivity growth is crucial to their corporate strategy. As Lewis Branscomb has noted, large companies' alleged failure to innovate has been attributed to many conflicting explanations: both too much long-term focus and excessive concern with Wall Street's short-term focus; both a pace of development that was too slow, and an unwillingness to show the patience to stick with a slowly maturing new market; and so on. James Utterback looks at a broad set of radical innovations, and finds that the majority were developed by technological challengers (not market leaders).[35] Rosenbloom cited a number of counter-examples at the June workshop: the computer industry was started by IBM and Remington Rand, both established in accounting machines; the tire industry was changed dramatically by Michelin with the advent of the radial tire; integrated circuits were invented by Texas Instruments and Fairchild, both of whom were market leaders in semiconductors at the time.

Medium-size corporations (suppliers)

There are many mid-size companies that provide subsystems, components or services to the large original equipment manufacturers (OEMs). Many of these firms specialize in a core technical area, leveraging this special knowledge by addressing products used in a wide variety of markets. Lord Corporation, for example, specializes in technologies for controlling vibration and noise in mechanical systems; it sells subsystems and specialty polymers into many markets, from aviation, to auto assembly, to recreational vehicles. This business model is referred to as a technology-defined business model.[36] A senior executive of such a firm may be the leader of the technical team creating innovation opportunities and at the same time may have profit-and-loss responsibility with access to the company's capital. David Lewis plays such a role at Lord Corporation, as described in his paper in this report. The dialog between innovator and investor is in this case quite intimate, since both roles are played by a single individual. Often this will result in a greater capacity for understanding and evaluating technical and business risks, even when they are dynamically changing. David Lewis offered some observations regarding the relationship between the size of a company and its strategy with respect to the management of technical risk. In a medium-size company, he notes, the relationship between the technical team and the marketing team is a close one: "the discussion is on a continual basis." He emphasizes that "managing and understanding the technical risks depends on how much you really know about the total enterprise, not just the technical aspects. The more truly knowledgeable you are about the market requirements and other downstream issues, the better you can assess and deal with the technical risk."

Startup firms

> *The superior efficiency of smaller firms in the R&D process apparently reflects the superior quality of their technical personnel, greater cost consciousness, and better understanding of the problem to be solved resulting from closer contact with the firm's operations and better communications.*
>
> *Jacob Schmookler*
> *Testimony before the Senate Subcommittee on Antitrust and Monopoly, 1965*

The paper contributed to this report by Scott Shane demonstrates empirically that the newly-created firm is a particularly appropriate institutional form within which to make success of radical, science-based innovations. This might seem counter-intuitive since, as noted above, large established firms typically have much more extensive technical resources for reducing a radical technology to practice, while the startup is severely resource-constrained, must put most of its energy into creating a business structure where none

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[35] James Utterback, *Mastering the Dynamics of Innovation : How Companies Can Seize Opportunities in the Face of Technological Change* (Boston: Harvard Business School Press, 1994).

[36] The technology-defined business model is contrasted with market, product and system-focused models in Lewis M. Branscomb and Fumio Kodama, *Japanese Innovation Strategy: Technical Support for Business Visions* CSIA Occasional Paper Series (Lanham MD: University Press of America 1993)

existed before, and has very little latitude for falling behind the schedule of investment and expected commercialization.

Shane finds that a new firm is more likely to be created to commercialize a new technology in segmented markets with access to strong patent protection, and based on technologies that are observable-in-use. But new firm creation is less likely in older technical fields dependent on tacit knowledge where a dominant design characterizes the market and complementary assets play a large role in business success. Thus the case of science-based innovations that seek to create their own markets appear particularly strong candidates for new firm creation. One may infer, then, that under these circumstances, a startup offers a superior form for maximizing return in the face of all sources of risk.

The role of the university

The biotech industry was extensively nurtured by government-funded research. The Bayh-Dole Act, which allows agencies to grant title to inventions made with government funds in the universities that performed the work, helped to drive bio-tech industry's growth.[37] Small companies were able to keep afloat with government money. These public research investments led to a new allocation of technical risk between universities and other institutions. Mark Chalek of the Beth Israel Deaconess Medical Center noted that university technology transfer offices have become more professional, and while peer-review panels have not changed their standards much, clinical researchers have more influence now than they once did. To cite one example of the stimulus of university research to high-tech innovation, Professor Robert Langer's 400 patents at MIT have reportedly created over 25 new companies.

Barry Eisenstein (Vice President, Office of Science and Technology, Beth Israel Deaconess Medical Center) observed that the pharmaceutical industry has changed from a "chemical-driven" approach focused on "working around previous patents" to a biological industry based on innovation. Industry in this case has moved towards the university rather than *vice versa*. Shane's work, discussed above, is consistent: new firm creation has played a large role in the commercialization of university biomedical research.

Universities have also provided fertile soil for new firms based on digital electronics and computer networks, not so much because of their technical prowess as the low barriers to entry for Internet-related businesses and the extraordinarily levels of capitalizations many nascent business seem to have been able to attract. When combined with the impact of the massive and consistent investments by government in university-based biology and biomedical research, the impact has begun to change the culture of the research university. This cultural change is reflected in the career ambitions of the students, who appear to be prepared to forego the security of lifetime employment with a large, established firm in return for the opportunity to test their entrepreneurial skills. Similarly, faculty who were once content with consulting once a week are taking leave, or resigning their chairs to exploit their inventions. Thus the gap between traditionally risk-averse university community and the traditionally risk-prone business community appears to be closing.

Since government funding of university research, largely centered in NIH and NSF, is highly responsive to the demand of the research faculties, any trend toward faculty desire to carry

[37] Because the passage of Bayh-Dole and the early growth of biotech firms were concurrent, it is difficult to assess how important Bayh-Dole was in that growth. This is extensively investigated by David C. Mowery, Richard Nelson, Bhaven N. Sampat, and Arvids A. Ziedonis, "The Effects of the Bayh Dole Act on U.S. University Research and Technology Transfer," in Branscomb, Kodama and Florida, eds. (1999), *op cit.*

their research further toward proof of principle or even reduction to a practicable technology is likely to be rewarded with a shift in the willingness of agencies to fund such work and of peer review panels to give it support.

IV. How Startups Manage Risk: Lessons from Two Case Studies[38]

The workshop examined two cases—the Advanced Inhalation Research (AIR) and Trexel cases—written as part of a joint Harvard Business School–MIT/Sloan School initiative.[39] The objective of the case studies was to describe the evolution of start-up technology-based businesses and, in so doing, to better understand how entrepreneurs, and their financial partners, perceive and manage technical risk.

Trexel was founded in 1982 by a scientist from MIT to exploit various plastics technologies. After pursuing several different technologies over a twelve-year period, the company decided to focus on MuCell, a microcellular plastic technology licensed from MIT. The process is based on mixing a super-critical fluid with molten plastic under pressure. When the pressure is released, microscopic air bubbles are introduced and "frozen" in the plastic, at very uniform spacing and density. The technology offers the promise of reducing cost by reducing the amount of plastic material required by many applications. The case describes the new management team's efforts to commercialize the technology, and the difficulties encountered as they attempt to perfect the technology in various applications. The case also describes the various rounds of "angel" financing that support the company, as well as various types of partnerships and licensing arrangements between the firm and plastics manufacturers.

AIR was founded to pursue a drug-delivery technology licensed from MIT. Its technology is based on a large, light, porous particle which is manufactured from lactose and delivers molecules of a drug into the lung. The particles are inhaled and—because they are large—they offer more sustained release of the drug. The case describes the initial research carried on at MIT and Penn State, and the early attempts to refine and commercialize the technology. In addition, the case describes the venture-capital financing of the company, as well as AIR's early business development deals with pharmaceutical companies, which generate both revenues and credibility for the firm.

AIR and Trexel are similar businesses in several ways. Each is attempting to exploit a platform technology: a drug-delivery technology in the case of AIR, and a plastic foaming technology in the case of Trexel. Both companies obtained a relatively small amount of venture financing to advance the technology, and each aimed to work with partners to develop, manufacture, and sell some products that flow from the technology. Thus, both companies have engaged in a series of licensing deals with different companies for different products. Both companies plan to use the proceeds from these licensing deals as a way of "bootstrapping" their way towards the development of proprietary products themselves. Even though they are both utilizing a licensing partnership strategy early on, neither wishes to be

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[38] This chapter is authored by Michael Roberts.

[39] *Trexel*, Harvard Business School Case No. 9-899-101, by Michael J. Roberts and Matthew C. Lieb; and *Advanced Inhalation Research, Inc.*, Harvard Business School Case No. 9-899-292 by Michael J. Roberts and Diana Gardner. Page references below in parentheses refer to these two cases. Copies of the case studies are available from the Harvard Business School Publishing.

wed to this strategy. Both companies want to capture the increased value—and independence—that come from manufacturing and distributing their own products.

Obtaining more cash at a given point in the future

Both Trexel and AIR are focused on two kinds of future events: the manufacture of their own proprietary products, and some liquidity event that will allow investors to recoup their cash. Ultimately this could entail a public offering or the sale of the company. Each firm took several steps to maximize the potential inflow of cash associated with each of these possibilities. Each company limits the scope of specific development deals and agreements it crafts with partners. AIR's strategy, for example, was to make "molecule-specific deals" (p. 11 of the HBS case study). The company entered into an agreement in 1998 with "Beta Pharmaceuticals" (a pseudonym) to conduct a feasibility study regarding systematic delivery of a particular protein. This partnership was followed later the same year by two separate agreements with two additional companies, each regarding specific protein molecules.

Trexel made deals that "offered exclusive use of [its] MuCell process for a specific product application ... over a three to five-year period assuming the customer achieved production levels sufficient to generate a minimum royalty payment and garner a minimum share of the specific product market" (pp. 5–6). These terms were designed to insure that Trexel would not be "stuck" with a partner who does not utilize the technology in the market.

Each company, by limiting the number and scope of licenses, retained the rights to all other applications of the technology. AIR, for instance, was free to pursue inhalation drug delivery for all drugs other than the specific protein molecules it had agreed to develop jointly with its three partners. Trexel could continue to develop MuCell for any applications other than those it had agreed to pursue under joint venture agreements. Thus, each maximizes the amount of cash it may be able to generate from its own proprietary production. Moreover, by maintaining this large pool of "options" to pursue the applications it has not licensed, each company maximizes its potential value to an acquirer, or in a public offering of its own stock.

Obtaining the same cash inflows, but sooner

Simply getting the same amount of cash—but sooner—improves present value and returns. Of course, this is hard to do, but focusing on getting cash sooner is a common approach to managing risk. Both Trexel and AIR accelerated the inflow of cash (relative to the scenario of developing and distributing their own products) through licensing deals and partnerships.

Reducing cash outflows

In each case, the companies—and their backers—use several strategies to minimize cash outflows. One strategy for reducing the investment required—and for delaying it until at least some of the risks have been wrung out from the process—is to delay the formal start of the firm. In both the Trexel and AIR cases, substantial work took place in the university setting, with university research funding.

The venture capitalist who helped found AIR (McGuire) noted that he invested relatively little money ($250,000) up-front, but maintained the option of investing additional funds (p. 2). He also pointed out that cash from the company's corporate partnerships allowed the business to minimize its ongoing funding requirements, and that this strategy continues to offer these benefits, and minimize future dilution of shares (p. 2).

Trexel's CEO employs a similar model, “bootstrapping” via development agreements and using the money thus obtained to fund internal projects (p. 4). Here too, this approach minimizes the equity financing required and thus minimizes dilution.

Thus, each company’s strategy is based—from the very beginning—on one explicit approach to trading off risk and reward: during the earliest phase of the company’s existence, when risk is highest and financing most expensive (in terms of dilution of the owner’s interest), the companies minimize the amount of equity financing required by getting cash from another source: selling a claim on a specific application of the technology. This approach preserves the options for each company to pursue the unlicensed applications on its own. In addition, the partner-financed projects—to the extent they are successful—demonstrate the viability of the technology platform, thus lowering perceived risk and the cost (dilution) of future financing. Similarly, by signing licensing deals, especially with well-known partners, the firms demonstrate the effectiveness of the technology to potential customers, and thus increase the perceived upside of the technology and the venture.

Obtaining the same cash outflows— but later

Sophisticated investors generally invest in a staged manner. That is, they do not provide all of the funding sufficient to take a venture to cash positive operations, but dole out capital in moderate-sized tranches. The objective of such a staged capital commitment process is two-fold:

- First, to the extent that there are multiple sources of risk along the path to proof of principle and reduction to practice, this allows the efforts at accomplishing these tasks to be tackled one by one. If a hurdle cannot be surmounted, further investment is truncated, saving the capital that would otherwise have been invested in the venture.

- And, if the hurdles are surmounted, the staging has the effect of moving the “average-weighted” time of investment back—closer to an ultimate liquidity event and thus, improving IRR.

AIR’s investor (McGuire) explained that “...of our total investment, $1.25 million went in after we had a corporate partner. This dramatically reduced the risk.”[40]

Reducing the risk of the cash inflows

Both companies also do their best to reduce the risk—both real and perceived—of their business models. They do this by identifying the obstacles to the success of the technology and tackling them one by one. These obstacles are easy to see in AIR's case, because the FDA and the medical model are quite explicit about the various hurdles the company must surmount in creating a new drug-delivery method. AIR made very specific efforts to:

- prove the basic science by using the technology in an animal model (delivery of testosterone and insulin in rats);

- validate this approach by subjecting the research to the scrutiny of a peer-reviewed journal (publication in *Science*);

[40] Remarks at June 1999 MTR workshop.

- prove that particles could be manufactured at commercial scale and at a reasonable cost (via spray-drying experiments at Penn State);

- prove that this commercial manufacturing process—spray drying—would be sufficiently robust to work with the chemistry of specific drugs that might ultimately be used.

In Trexel's case, the risk seems to lie less with the science that underlies the technology, and more with the application of the company's technology to specific products and manufacturing processes. Thus, the evolution of Trexel's strategy can be seen as an attempt to reduce risk by reducing the expenditure of time and effort on projects that are unlikely to reach commercial scale. Trexel does this by:

- First, canceling development deals that seem unlikely to be fruitful (p. 5);

- then, focusing on more practical projects that meet Trexel's specifications and objectives (p. 8);

- then, pursuing "fast-track" development deals that focus even more narrowly on Trexel's specifications and which also have tighter time frames, further reducing the risk of an unfruitful effort. Trexel has refined its criteria to focus on "materials and applications that we understand and can transfer with little effort [where the]...customer is capable of working independently...[and where] the target product represents an interesting market opportunity" (p. 10);

- finally, narrowing the company's focus even further, in an effort to "control every step in the process" (Trexel CEO David Bernstein's remarks at June 1999 MTR workshop).

In each case, the perception of risk is a function of the context of the particular technology. In AIR, the medical model outlines the risks quite clearly: "To me, what were the risks? Safety and efficiency, ultimately in humans" (remarks of company founder Robert Langer at June 1999 MTR workshop). The FDA imposes well-defined hurdles, and uncertainty over whether these hurdles can be surmounted becomes a source of risk. In Trexel's situation, the uncertainties were more varied, as a function of the specifics of the material and product that was being manufactured, as well as the production process employed. Indeed, whereas AIR was able to perfect a single production technique (spray-drying), Trexel attempted to get its plastics technology to work in a wide variety of production processes (such as extrusion and injection molding). This additional complexity and uncertainty undoubtedly contributed to its difficulties.

One of the keys to managing risk lies in mapping the expenditure of funds sequentially against the perceived risks. Thus, if the technical risks can be pulled apart into a series of experiments, and each experiment funded separately and sequentially, then the investor's risk is reduced because the investor has an opportunity to exit from a "failed" project before spending the sum that would be required for the entire project. AIR, for example, was able to separate the technology into a series of discrete elements:

- Can a large particle be made?

- Can it be inhaled?

- Does it achieve sustained release of the drug in the lung?

- Can it be made at commercial scale and cost?

- Will it break apart and lose its functionality during shipping?

Other technologies, for example Trexel's, may be harder to pull apart into a set of discrete technical challenges that could then be solved sequentially. Trexel's production of bench samples of the product at laboratory scale represents an attempt to prove out the basic underlying premises of the technology, but Trexel's technology seemed to encounter more problems scaling to commercial proportions (a challenge not yet faced by AIR's technology.)

Other lessons from the cases

In addition to the issues that the cases highlight regarding the relationship between a new venture and its financial backers, the cases also shed light on the efforts of new ventures to attract the interest of another key constituency—partners. Trexel and AIR were both dependent upon development partners to commercialize their technology, and both faced challenges in doing so. Yet, in spite of their similarities, differences emerge in the two stories in terms of the success with which AIR seemed to be working with its partners, and the great difficulties Trexel was experiencing in this regard. There are several possible underlying causes of these differences.

RESEARCH AND DEVELOPMENT MINDSET OF THE INDUSTRY

In many ways, AIR may have had an easier time dealing with its customers/partners in the drug industry because they were used to dealing with technical risk. The deals have milestones built in that assume there is some possibility that the technology will not work, and that give each party the option of pulling the plug. Moreover, AIR was most likely interacting with the Research and Development or Development piece of its partners' organizations. In contrast, Trexel was dealing with the manufacturing organization, which is less used to dealing with technical risk, and where the pressure for current revenue is greater.

MODULARITY OF THE TECHNOLOGY

AIR's technology (like, perhaps, most drug-delivery technologies) is more "modular," in the sense that the company's model is to find a drug with known efficiency, and then embed it in the company's specialized large delivery particle. This required a relatively minimal amount of coordination between the partners. While Trexel's technology might be similarly described as "give us your plastic and we will put the bubbles in," in practice, far closer integration was required between Trexel and its partners. This imposes administrative and coordination issues on top of the technological ones.

KEY EXTERNAL CONSTITUENCIES

One of the key steps in the process of managing risk appears to be working early with key external constituencies. In many cases, this would include customers, but in the Trexel and AIR cases, the constituencies were development partners. AIR investor and executive Terry McGuire said that one of his key contributions to the company was to "get the company in early to see top people" at potential pharmaceutical partners (remarks at June 1999 MTR workshop). Robert Langer makes a similar point when he says that one of the ways to success in all medical-oriented businesses is to "get to the clinic early [and] get a real result" (remarks at June 1999 MTR workshop). The value of this early involvement is suggested by comments of AIR founder David Edwards at the June 1999 MTR workshop, when he noted that AIR's potential partners were actively involved in identifying many of the early risks:

“There were many risks.... would the particles break apart during transport?... would the chemistry of particle formation work with both fat and water-based drugs? These, and many other questions, were posed by potential partners during meetings.”

In the Trexel case, too, key learning took place when the company started working with its partners/customers. Trexel CEO Bernstein notes that “it is never a product until it is a product in the customer's product and process” (remarks at June 1999 MTR workshop).

Trexel's key angel investor Alex D’Arbeloff relates a more fundamental point:

> You have to work with the technology to know what it is about.... Applying technology to a market is trying to hit a moving target. Until you are in the market, you are not progressing.... the market may move in a direction that is unpredictable. So, the key is to get in quickly. When you are dealing with a technology, you have to hang in until you understand its advantages and applications, so troll... spend as little as possible so you can hang in long enough to find your focus.

FOCUS

One of the key themes in the Trexel case—and one that was amplified during the discussion—relates to the issue of focus. Alex D’Arbeloff talks about the initial stage of a venture as the “trolling phase” where you are hoping that some customer will bite. This is the phase where you are learning about the technology. The key challenge, according to both D'Arbeloff and Bernstein, is focus. In D’Arbeloff’s words, “companies that succeed, focus.” But one challenge lies in knowing when to focus. Bernstein relates that he is glad Trexel did not choose to focus all its energies on two projects that the company's partners proposed—garden hose and coated wire—projects that were ultimately unsuccessful.

Summary of lessons from case studies

Although both Trexel and AIR utilized private funding in their early stages, government funding can play an analogous role. Government research and development funding, particularly if it occurs early in the life of a company when other forms of funding may not be available, is similarly bearing a good deal of risk. Yet, two factors make the “reward side” of the equation different from the private funding scenario.

First, government funding may seek some non-economic rewards, or at least, rewards that are not easily measurable in simple dollar terms at the time of a liquidity event. The fact that these rewards may be difficult to measure may make them appear to be less tangible and/or less valuable, when in fact, it is simply that they are difficult to measure.

The second difference is that the levers government can use to manage risk and reward differ from those used by private investors. Because government funders seem destined to be less involved in the business than the private investors we observed in the Trexel and AIR cases, government programs may attempt to manage by requiring companies to pre-commit to particular plans, products, or processes. Yet, as we have seen, success often demands entering the market with a rather rudimentary product, seeking customer input, and making significant changes to the initial plan. This flexibility has great benefit in allowing the firm to learn about the market and those segments of it that will value the potential project most highly. Rigid adherence to a plan formulated in advance of such experimentation actually increases risk, rather than reducing it.

The effective management of technical risk is a complex calculus that involves weighing and trading off risk *and* reward to arrive at a satisfactory ratio of the two. The active management of that ratio involves a variety of tools that can be used either to reduce risk or increase reward. As long as the aims of technology are defined in terms of satisfying a customer's needs, it is almost impossible to separate technical risk from market risk. The process of working with real or potential customers is key to refining the technology.

V. Strategies for Managing Risk

Skewness of returns and its implications

> *Spectacular prizes much greater than would have been necessary to call forth the particular effort are thrown to a small minority of winners, thus propelling much more efficaciously than a more equal and more "just" distribution would the activity of that large majority of businessmen who... do their utmost because they have the big prizes before their eyes and overrate their chances of doing equally well.*
>
> *Josheph A. Schumpeter*
> Capitalism, Socialism and Democracy, *1942*

Firms must attempt to manage technical risk at the project level. But investors can also attempt to deal with risk by attempting to ensure that, of several projects, at least one or two are sufficiently successful to compensate for the disappointments. This approach is referred to as portfolio management. Looking at the payoffs from venture capital investment and return on IPOs, we see a repeated pattern of skewness in that a few dominant products garner a disproportionate percentage of the total market profits. Scherer provides a number of examples:

- *Example 1*: Of 670 venture partnership investments studied by Horsley Keogh Associates and analyzed by Scherer, 385 lost money or at best broke even.[41] We see that when the venture partnership is liquidated, half of the investments are losers and the top 5% have 42% of the terminal value.

- *Example 2*: Of 99 new pharmaceutical chemical entities introduced into the U. S. marketplace in the 1970s, 60 percent were market failures and the top 10% contributed 55% of the total discounted present value.[42] Here we see a group of products that are all technically feasible (since they have been through FDA approval), but there is significant skewness in the returns.

- *Example 3*: Scherer and his colleagues tracked changes in the value of a hypothetical $1000 investment in each of ten firms selected from 131 IPOs from 1986–1996.[43] Here, each company is at a more mature point, with at least one product on the market and potentially more in the pipeline. Again we see a wide divergence, as the top 10% account for 62% of the capital value of the terminal portfolio. Perhaps more interesting, a selection from the 131 of any five firms at random showed that they were essentially indistinguishable; most had gone nowhere.

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[41] F.M. Scherer, *New Perspectives on Economic Growth* (Washington DC: Brookings Institution Press, 1999), Figure 5-8.
[42] Ibid., p. 61.
[43] Ibid., p. 79.

Thus skewness in returns from early-stage investments is replicated at the mature stage.

Scherer used a Monte Carlo model to investigate the effect of a diverse portfolio on returns year by year. He randomly selected 18 new pharmaceutical chemical entities each year to introduce to market and gave each a 21-year lifespan. His results for market return year by year for 9 runs of the Monte Carlo model show a wide divergence of returns (for example between $1.6 billion and $2.6 billion). Scherer's conclusion from this exercise: In the presence of high degrees of skew in returns, portfolio strategies will remove some variance, but cannot bound the variability to any significant level.

Morgenthaler observed that, as a venture investor, his firm "definitely does not take a 'portfolio approach.' Instead, each project must stand on its own, and we would not knowingly undertake a number of projects—each of which had an unacceptably high risk on its own—merely with the hope that one or more would win by 5–20 times the investment and make up for all the losers."

Strategies used by venture capitalists

> *Venture capital investing is like a horse race. The technology is the horse. The management team is the jockey. The market and the competitive conditions are the race. Wonderful horse, lousy jockey—the jockey falls off the horse. That's what happens to us, about 60% of our failures.... Lousy horse, wonderful jockey—the jockey gets all the ride there is out of the horse, but the technology hasn't got it. If you're using the second or third best technology in the industry, you're not going to win. Wonderful horse, wonderful jockey—but he's running at the county fair. They win easily, but the prize is $50. That is the small-market problem. Very good horse, very good jockey, and he's running in the Kentucky Derby. Wonderful prize—millions in stud fees, all the rest of it—but he's running against the best horses in the world, and the best jockeys in the world, and if he isn't absolutely world class in both of them, no hope.*
>
> *—David Morgenthaler, Morgenthaler Ventures*

At the June workshop Richard Burnes noted that Charles River Ventures (CRV) has spent 30 years working to reduce risks in its investments. This effort, he reported, has been successful. Statistically, risks have been reduced and results have gone up. CRV has raised nine different partnerships totaling a little under $500 million, and has invested in 265 different, mostly early-stage, companies. (In the last ten years 80% of investments have been in raw startups.) Of the 265 investments, 55 have led to IPOs, and 45 companies remain in the portfolio. In the last year, CRV has invested $45 million in seventeen companies new to the portfolio. CRV started out very eclectic, investing in everything from electric cars to biotechnology. Starting in the 1980s, CRV developed substantial expertise in the areas of software and communications. That shift has been very positive in terms of the results of the firm.

In his paper contributed to this report, David Morgenthaler posed the rhetorical question "When should a venture capitalist invest?" His answer: A venture capitalists should

- *never* invest to discover new scientific phenomena;
- *almost never* invest to prove the scientific principle;
- *rarely* invest to develop an enabling technology;
- *often* invest to use a new technology to develop a product;

- *very often* invest to revise and improve a product;
- *very often* invest to produce a later-generation product;
- *very often* invest to broaden a product line; and
- *very often* invest to apply a product to another application.

Virtually all the venture firm executives who participated in this project agreed on one central issue: their primary strategy for limiting risk is the selection of people in whom they have confidence, both in the technical and the business dimensions of the business. Because their options are sometimes less than ideal, the firms take an active role in firm management, including using their financial position to require changes in leadership from time to time. “I can remember no case where we intervened to replace a CEO too soon,” Morgenthaler said.

Strategies used by corporations

At the June workshop, David Lewis noted that while firms are always looking for protectable, radical innovations that offer the possibility of destabilizing existing markets, the company can, to some extent, manage risk by holding a diversified portfolio of R&D projects: a “mix of flyers, mid-risk projects, and low-risk.” As noted above, and spelled out in his paper, Lewis emphasizes the vital importance of knowledge of the technology and understanding of the market. Where market knowledge is deep, technical risk is easier to manage, because, as discussed earlier in this report, one has confidence in one’s understanding of the requirements of the market. If you know what the product specifications have to be, you will know when the technology will not support them, and you can stop, at least temporarily. Halting a project that is doomed to disappointment is a key element of risk management. Failing to pursue a project whose requirements are as yet undefined and are a function of both technical and market uncertainties is to fail in technical risk management. This is the failure to which large firms are often prey, as discussed by McGroddy.

Hartmann and Myers describe the Xerox innovation system. Xerox has on the order of 300 investments at a time, at different stages through the pipeline. These investments have to be spread over a portfolio of new or existing markets and technology in order to balance the risks. (see **Figure 3**):

- *Evolutionary business offerings* (existing markets, existing technology): Lowest risk, but also limited economic potential;
- *Leverage base extensions* (new markets, existing technologies): For a global company, opportunities of this type tend to be geographical;
- *Discontinuities in technology* (existing markets, new technology): This is what we are most familiar with: technological substitution;
- *Radical innovations* (new markets, new technologies): Low probability outcome, but holds greatest opportunity.

These differences do not fully describe the differences in risk to be found within the firm’s portfolio of projects. Other significant parameters of the risk equation, as Hartmann and Myers describe them, include:

- *Competence*: "Sometimes you have access to the technology, but a big failure is that you're not competent as an organization [to be a player in the area.]"

- *Specifications*: "Specifications are ... where these two sides interact.... you cannot make technologies fit to a market until you're really able to specify what the market requires. A major failure in programs is the interaction of technology maturation and change of specifications. Why do specifications change? Because competition causes a disruption in the marketplace."

- *Complementary assets*: "Are there other people in the industry helping to develop the complementary parts of the technology that you need, particularly at a systems level?"

- *Value chain*: "Do you have access to the complete value chain to serve the market?"[44]

- *Market preparedness*: Is there a customer base prepared to use the technology?

- *Business concept*: How do you make money? Corporations move away from their business concepts very reluctantly, as discussed in the paper by Henry Chesbrough and Richard Rosenbloom.

Do individual technology managers tend to be more risk-averse than would be ideal from the perspective of the organization as a whole and, if so, does this lead to an overall bias against undertaking enough high-risk projects? Myers replied that products following the evolutionary path in the innovation typology presented in **Figure 3** tend to become commodities, but "a technology company does not want to work in commodity space." Consequently, there are strong incentives within a large corporation to invest in projects that represent at least discontinuities, if not radical innovations. This is what David Lewis refers to as products that "destabilize the market."

Xerox makes a series of investments in speculative research projects where it is not always clear what will come out of the work, funded by corporate headquarters from protected funds. Technology development begins with technology concept initiation (a negotiated collaboration between corporate research and the business divisions). Product concept initiation then begins commitment to product generation. At the end of phase 2, the Concept phase, Hartmann and Myers observe, "the technology has been shown to be capable of meeting the performance requirements, to be manufacturable, and to be sufficiently robust that it is ready to begin product design, which usually requires a significant ramp-up of product development resources." McGroddy identifies three phases in technology project management:

1. *Discovery and invention phase*: often pursued in a corporate research laboratory; the most fun, at least for the scientists.

2. *Incubation*: Investing in the technology, protected from normal marketplace values. The firm does not insist on making money at this stage, but early exposure to the intended marketplace helps direct the research.

3. Ramp up production within the company or develop the technology outside the company through "excubation" (which McGroddy contrasts with "sheltered incubation" of a new

[44] The value chain comprises all the elements of a sale that contribute to customer satisfaction and value, which may go far beyond function, quality and price of the product, to include service, parts supply, training in use, environmentally acceptability, user safety, trusted relationship with the vendor, etc.

technology). At this point the possibility of "fratricide" (competition with one of the company's established products) arises. As an example of "excubation," McGroddy cites the very successful experience of IBM in its joint venture with Toshiba to manufacture and market flat panel displays, which would, if developed internally, be seen as competing with technologically inferior IBM gas-panel displays.[45]

The company has to manage technical risk differently in each of the above three phases.

In the first phase the primary failure is "the stuff won't work." In the second phase you start to engage the question of whether the technology will or will not meet a market need.

In the third phase, the manager has to either match the product with the existing activities of the companies, or move to develop the product outside the company. There are several reasons to develop the product outside the company:

- The product may be very important to the company in the long run. (IBM examples: flat panel displays, lack of router business.)
- "You may make a lot of money out of [businesses started outside the company]." (Example of laser business in Zurich that made $150 million profit over six years before it was sold by IBM.)
- "You owe it to the people.... You can hire a different set of people—you can hire these people who are entrepreneurs, who want to make something happen in the marketplace—if you build that kind of an image for your company."
- It is important to expose technology early to surrogates for what the market will be. Of course, one has to be careful in the selection of the surrogates; the government is normally a poor surrogate because its market requirements are more arbitrary and may not represent leading-edge user needs.
- Basic research should be exposed to potential applications very early on. As a rule, researchers are enthusiastic about this.
- Going outside may help in understanding the internal conflicts within the company, such as displacement threat, competition for resources, distraction to the customers in the marketplace.

Finally, firms must beware of intellectual arrogance: when technology "fails," the team concludes that the task must be impossible because "we, the smartest people in the industry, couldn't make it work." As David Morgenthaler said, "We look at availability of alternative technical solutions. If this one fails, what's our alternative? We compare [our technology] with the competitive technology. Why are we so smart? Why are we better off than IBM and Xerox and all the other people who have been out there putting a lot of effort on it? I find that when I'm smarter than anybody, I'd better go back and re-examine what we're doing."

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[45] McGroddy illustrates with the example of routers: IBM built the backbone of the Internet, but McGroddy could not convince IBM top management to get into the router business. The perceived problem was that IBM routers would have competed with established IBM business in SNA controllers. Two years later Cisco, which now dominates the router market, had half of IBM's market capitalization; it now exceeds IBM's market capitalization.

VI. Overcoming Barriers to Innovation

> *In the twentieth century... the individual inventor is becoming rare; men with the power of originating are largely absorbed into research institutions of one kind or another, where they must have expensive equipment for their work. Useful invention is to an ever-increasing degree issuing from the research laboratories of large firms which alone can afford to operate on an appropriate scale... Invention has become more automatic, less the result of intuition or genius and more a matter of deliberate design.*
>
> *John Jewes, David Sawers and Richard Stillerman,*
> The Sources of Invention, *1959*[46]

Overcoming institutional barriers to radical innovation

The first requirement for innovation is a team, led by a champion, prepared to put together an organization to exploit an opportunity. As Mark Chalek (Technology Transfer, Beth Israel Deaconess Medical Center) stated at the September workshop:

> What typically impedes our technology from becoming commercialized is [the absence of] some mitigating, facilitating entity—whether you want to call it an incubator, whether you want call it a group of facilitators, or group of managers, or all of the above—who have the ability to both validate the technology from a commercial perspective and take it to that next stage which includes getting it ready for financing and also getting it ready for commercialization.

The second requirement is an enterprise free from the constraints of conflict with existing products and markets, as discussed in the previous section. Kent observed that BBN had an experience similar to that of IBM with respect to the router market. Routers competed with BBN's successful X25 packet business. The entire marketing organization—which had an important say in how research money would be spent—argued there was no future in routers. This was no surprise, as BBN's customers were exactly those people who wanted to buy X25 packets.

The constraint of the familiar business model

A third obstacle to innovation is the constraint of the business models with which the firm is familiar and in which it is experienced. Chesbrough and Rosenbloom's paper notes that a bias against business models that do not fit their core business is characteristic of large firms and start-ups alike (which creates a role for medium-size firms). Design of the business model may not contain any tight definitions, but instead will seek to:

- identify the market segments;
- articulate the value proposition;
- define the structure of the value chain;
- estimate the cost structure and profit potential;

[46] Rhodes (1999), *op. cit.*, p. 212.

- position the firm within the value network; and
- formulate a competitive strategy in the prospective marketplace.

The business model maps technical inputs to economic outputs, and is, like the other issues explored in this report, at the heart of the issue of managing technical risk. Success with a particular business model may provide an industry leader not only with market dominance, but potentially with the foundation of corporate identity. However, when new technologies provide the potential for discontinuities in the marketplace, past success and the firmly entrenched competencies that success may engender may put the established firm at a disadvantage. The incumbent firm may struggle to shed yesterday's business model and reconstruct itself to match new opportunities.

Failure to connect to the market early

A fourth barrier to innovation is the failure to gain sufficient market knowledge soon enough: to "excubate" a project, as McGroddy put it, by exposing it to influences outside the firm. Branscomb noted that the explosive growth of innovative businesses on the Internet is at least in substantial part due to the low cost of entry. One may try any or all of the business models: rent space (domains), sell products, sell advertising, etc. One may test consumer acceptance by offering the product or service free of charge, all at very low cost. This experience with the new IT firms appears to confirm the intimate connection between confidence in one's understanding of one's market and business model, and the ability to see what is required to manage the technical risks.

Sectoral concentration of innovative effort

The "dot-com" phenomenon has distracted many venture capitalists (VCs). Some even go so far as to claim that they will not be looking at biotech at all this year, in an environment where they can pull their internal rate of return out of the dot-coms in 5-6 months, while it could take up to five years to get their money out of the biotech firm. The great variations from year to year in the sectors in which VC firms invest suggests a degree of "faddism" in this industry. The response of one VC executive to the suggestion that they "only look for pennies under the street light" is that "our business is dominated by our ability to raise capital from the investment bankers on Wall Street, who take our properties public." If Wall Street pursues fads, so will the VC industry. Of course it is also well known that the "social capital" required for nourishing emerging industries calls for concentrations of activity and a high level of effective communications. It may be that focused investing in certain emerging technologies is the most economically efficient course: this may leave governments with an appropriate space to operate, by compensating, to a degree, in the interest of keeping many doors open to economic growth.

Time to market

An illustration of the obstacles posed by the time-to-market lag is found within the biotech industry, in the requirements for clinical trials and regulatory approval. Once the product is proven in the lab (a result enjoyed by only approximately 25 of the original 50 in the pipeline), it must be proven in human clinical trials. Only about 1 in 7 of those that make it to clinical trial is approved by the FDA (in the United States). The numbers are quite disturbing from a VC standpoint: 60% do not even pay back the company's R&D expenses, to say nothing of royalties. Although it was not discussed at this session, there are initiatives seeking agreement for the United States and the European Union, and perhaps even Japan, to accept one another's clinical trials more readily. If such cross-acceptance should become more common, it will substantially reduce the time to market of potentially profitable pharmaceutical products.

Geographic concentration of innovative effort

The Rust Belt has given way to the High Tech sector, and the geographic concentration of successful firms has shifted too. Universities in Ohio may be good, for example, but Silicon Valley dominates where finance beyond the university is concerned.[47] In an example comparing Cleveland with Palo Alto, it was shown that VCs evaluated the average small high-tech startup in California at $12 million, whereas a similar enterprise in Cleveland was valued locally at just $4 million. One reason for such a discrepancy is the higher probability of value in Silicon Valley, given the strength of the talent pool and infrastructure there. A bidding war among VCs in Silicon Valley might also be helping to drive evaluations up.

The perception of risk depends in part on social capital, which manifests itself, in part, in these clusters of opportunity in Silicon Valley. So the question emerges: should ATP target geographic areas that fall outside of the recognized cluster? Or if they do so, are they merely lowering their own probability of success? Government programs do not share the concerns of private VCs that all stages of the financing process including the exit strategy should be maximally efficient, independent of issues of geographical equity.

Public sector incentives

> *We [government] shouldn't invest in products that were too specific to marketplace; we shouldn't invest in incredibly long range technologies (because they took too much willpower for people who needed to last throughout several administrations). We should invest in groundbreaking sort of major technological thrusts that will change the way we do business between 5–10 years from now, but not 20–30 years from now.*
>
> —*Howard Frank*

Robert Charpie, chairman of Ampersand Ventures, observed that, from the point of view of a venture capitalist, "technical risk is the easiest" sort of risk to work with. "At the same time," he said,

> it is natural for the government to focus on technical risk, because that's the sort of risk that is familiar to the government which has extensive experience with large technical projects. There is no role from my point of view for government as an equity investor in startups. I don't want an investor who isn't interested in making money. It's hard enough to organize and create a successful business—to discipline a company, to drive people who are all anxious to be successful, to work hard, to make a lot of money. I can't tolerate the handicap of having an investor sitting at the table who's interest is in something else, like promoting the development of a technology in ways beyond the needs of the company.

The motives of private investors and government technology agencies are quite different, yet in the ATP model, for example, both are sharing costs and risks of high technology ventures. Josh Lerner explores the government programs, taking the view that these activities, even

[47] Michael S. Fogarty and Amit K. Sinha, "Why Older Regions Can't Generalize from Route 128 and Silicon Valley," in Lewis M. Branscomb, Fumio Kodama and Richard Florida, eds., *Industrializing Knowledge* (Cambridge MA: MIT Press 1999) pp. 473–509.

when focused on technology spillovers, can be viewed as an alternative form of venture investment.

Josh Lerner noted that venture capitalists, in the aggregate, make a disproportionate contribution to innovation and growth. Venture capital has undergone a lot of change and growth, yet it is still just a fraction of a percent of public equity markets. The kind of firms backed by venture capitalists often have a difficult time getting funded from more traditional financial sources. The reasons include: uncertainty; information gaps and asymmetries; intangibility of assets; and shifts in market conditions.

Venture capitalists address particular problems associated with funding high-risk, early-stage technology firms using three sets of tools, which include sorting; governance; and certification/stamp of approval. (For more detail, see Josh Lerner, "When Bureaucrats Meet Entrepreneurs.")

With regard to "public venture" programs, some of the questions/problems that arise are:

- Venture industry itself is highly focused in a few areas. Does it make sense for the government to target these areas, or should it perhaps look at other areas? How should government balance the competing social goals of achieving geographic diversity of development and achieving a high rate of return to the economy?

- How well suited are companies that are generally involved in contract research to the task of developing new commercial technologies?[48] Can or should a program like ATP avoid the contract research firms in favor of more entrepreneurial ones, or is it appropriate to favor those who make a business by developing technical knowledge?

- ATP and other public programs are oriented to funding pre-commercial work; this orientation may not match up well with the rush to market typical in the entrepreneurial setting.

Lerner's view is that if ATP and SBIR are viewed as "public venture capitalist" programs, they will have all of the problems that conventional VCs have, and in addition, a number of problems that private VCs do not have:

- The government agency may not have access to proprietary information at the level of detail that would permit it to perform the appropriate level of due diligence before the 'investment';

- An inappropriate 'investment' tool (grant or contract instead of equity investment) may be Congressionally mandated;

- The degree of government oversight of the enterprise receiving the finds is limited by the traditional reluctance of government to micromanage its commercial contractors;

- The agencies are required to document their decisions, which might make agencies resist changes in plans, or cause firms to be reluctant to request such changes, even when the market would require it;

[48] A frequently expressed policy concern about the SBIR program is the apparent success of "SBIR mills," firms whose business model seems to be competing for government R&D projects rather than focusing on commercialization.

- There may be external distortions, such as pressures for regional distribution, or "gaming" behavior by repeat winners; [49]

- Since their goal is expressly to address market failures, agencies cannot completely duplicate the strategies of private industry, but instead must project and factor into their equation social returns on capital;

- If their mission precludes them from funding a company that can gain access to private sources of capital without government help, they may be driven to fund poorly managed firms or firms which have structural problems.

Thus Lerner's examination of the consequences of measuring ATP success by the tests of the venture investor—successful market entry, equity value growth and return on investment—serves to remind us that this is not a realistic model for a government program of this kind, and if it were, a better strategy might be for government to share financial risks with private sources of equity investment which are not constrained in the way government is. If the program is viewed as a research program, with success measured by the creation of useful technical knowledge that might provide the nutrients for future economic growth, government evaluates the extent of successful commercialization of research as a measure of technology diffusion. In this case business success is an important mechanism, but not a necessary condition, for diffusion success; a project that was technically successful but failed in the market might be published and used by others in more promising markets.

Long-term technological trends

At the June 1999 workshop Mark Myers observed that in recent years the velocity of the market for information based products has accelerated dramatically: 50–60% of revenue in large technology driven companies now comes from products that have been in the market less than two years. The challenge for a company like Xerox is "how to create $2–3 billion of new revenue every year" in new areas. Xerox has increased its rate of product introduction from 30 products in 1994 to 95 products in 1998. "To really make this work, you have to create what I think of as an 'innovation system,' so this is not an *ad hoc* process. The front end of this process is creating investible options so that the innovation system can support the business."

McGroddy observed that growth is the fundamental issue. "Half of growth these days in information technology (IT) (five-year time period) is coming from 'new stuff.' A mechanism is needed to capture the new stuff. Who got the revenue for the new stuff in the past five years? Overwhelmingly it is captured by companies that you didn't hear about five years ago. Old companies are not going to make it on old stuff." He pointed out that one can, to some extent, project technical trends into the future by tracing out the trajectory of progress for underlying technologies—e.g. Moore's Law for processing speed. But as Kent noted, it is difficult to predict the particular form of product or service that will come to dominate the

[49] Of course, it is well known that in government funded R&D programs the applicants' understanding of the agency priorities and processes is essential to a successful application. This is especially true of the ATP, whose rigorous, competitive selection process, while praised as a model of a well run government program, presents to the inexperienced firm a steep learning curve. Indeed, the NSF established the EPSCOR program specifically to compensate for the relative lack of competitive experience on the part of the more isolated and less research experienced universities. Lerner's concern in this regard illustrates again that it makes a big difference whether programs like ATP should be viewed and evaluated as R&D programs or as venture capital investments in commercialization of new technical ideas—that is, whether the proper metric for success is broad-based benefit to the economy or direct, monetary return on investment.

market. It may well come from an industry with a quite different business model, a different set of interfaces to other industries, and a different way of delivering value to customers. (An example could be the competition between Xerox Dynabook and the currently successful Palm Pilots.)

McGroddy summarized the case for both corporate and public policy to pay even more attention to technological innovation in the future:

> All the great waves of technology that we've seen [have the property] that while there's usually exponential progress in some parametric characterization of the goodness of the technology, that continuous exponential improvement in the technology causes discontinuous things to happen high in the value chain. At a certain point, the horsepower per weight is enough that you can make this thing called a flying machine. And it opens this huge amount of opportunity... There's a huge amount of work that's driving forward this exponential progress. In the case of integrated circuit technology... there's at least two orders of magnitude to go in the next ten years... So it's a perfectly reasonable behavior on the part of society to focus on the applications of stuff.

Nonetheless, as Burnes noted, the opportunities for radical, science-based innovations are still rare, compared to the new possibilities from steadily evolving advanced technologies. "The problem is today that a lot of the projects that come in don't have any real differentiation. That is a shift from the environment that we were in the 1970s and 1980s, where we did see a lot of technically sophisticated projects, where there was technical risk and we took that risk. In fact, I would say that we may very well be going through a phase in the evolution of venture capital, where there are many, many opportunities that the existing technologies can address and create major companies."

Any evaluation of the options for government policy must, therefore, take into account not only the changing patterns of entrepreneurship, of the roles and relationships of firms in the supply chain, and all of the changing features of world markets; policies must also examine the justification for government intervention to keep science-based innovation a driving force in the U.S. economy. Dr. Mary Good, who served as UnderSecretary for Technology and director of the Technology Administration in the Department of Commerce, concludes our discussion with an essay that explores the issues public policy must face in future.

VII. Will industry fund the science and technology base for the 21st century?

Dr. Mary Good

> *Dr. Mary L. Good is Donaghey University Professor at the University of Arkansas at Little Rock, Managing Member of Venture Capital Investors, LLC, of Little Rock, Arkansas, and the former UnderSecretary for Technology and director of the Technology Administration in the Department of Commerce.*

I spent four years in the Clinton Administration as the Under Secretary for Technology in the U.S. Department of Commerce (1993–97), where I was responsible for the oversight of the National Institute of Standards and Technology (NIST), which manages the Advanced Technology Program (ATP). The first two years of my tenure I was able to discuss the attributes of ATP and request significant funding levels for it. The program grew from about $60 million to over $200 million in that period. During the last two years, however, I spent a great deal of time explaining the program, discussing the assessments being done, and defending the management of the program. Beginning with the new Congress elected for the 1994 term, our objective was to maintain the program at its 1994 level and prevent, to the extent that we could, the politicization of the program. Today however, I can look back objectively on the program from the perspective of the private citizen who has an interest in the health of the innovation infrastructure of the country and who worries about our technology base for 2010 and beyond, when my grandchildren will be responsible for the prosperity and standard of living in the United States.

Thus, this paper reflects my observations and assumptions of the value of ATP, based on thirteen years as an industrial research manager in technology-intensive companies (Allied Signal and its predecessor companies); four years of intense involvement (and research) with the civilian technology base in the United States and abroad; and two years as an active member of a group of investors who seek to stimulate start-up and early-stage, technology-intensive companies in Arkansas and the mid-South, an area that has a very poor track record for these activities. Thus my remarks present an opinion based on close study in several arenas: the art of technology development in an industry dependent on technology for profits and growth; technology development stimulated by government programs; and technology development that leads to a venture-fundable business start-up.

I participated in ATP as a grantee, managing an ATP project on metallic glasses for Allied Signal, before I came to the government. My vision of the program then was to leverage ATP support to increase my chances of getting internal company support for a technology program I thought had significant long-term commercial potential for our company. Our ATP grant allowed us to contract with some experts in the field at two different universities and to develop a prototype for a commercial process to make metallic glasses. The grant fit all of the rhetorical conditions we have come to associate with ATP: early technology development, enabling technology, collaborative work with university research groups, and the creation of a truly new material. The program was a success from the company's point of view, although we did not accomplish all of the technical goals we had originally set. The company has commercialized the process and now sells significant products in a financially successful business. Since the technology was not considered as part of our core business at the time, the ATP grant made it possible to pursue what became a successful product line. It was government support for a large company whose expensive equipment and experienced technologists that made the development possible. In my view, it would have been beyond the resource limits and capability of most small firms.

I have also reviewed all of the many studies of ATP outcomes. Clearly the program has produced winners in a number of cases, from enabling technology licensed to a wide variety of users, to the establishment of companies marketing truly new products. In fact, the number of failures is perhaps lower than one might like to see because it indicates that the level of risk-taking may be sub-optimal. For example, the success rate is better than that experienced by venture-funded start-ups and early-stage technology companies. However, I believe that most objective observers would agree that ATP has been a successful program in supporting technology research that provides a pathway to commercial innovation. Thus in my mind the question is not whether the program works, but why it is needed.

The most frequent argument used against ATP is that the private sector should provide the resources to do any and all research beyond fundamental university research, either using current company resources or by the acquisition of capital support from such sources as venture capital firms and angel investors. Our attention then should be on studies that can determine the probability that the nation's innovation system is being adequately addressed by the private sector. An historical study would suggest that early governmental support has been a factor in many of our major innovations; the telegraph and aviation are early and dramatic examples. The many studies already done on the origins of our current exploitation of information technology clearly point out the government role, particularly that of DARPA, in its funding of early technology research to develop the underlying technologies for the Internet and computer systems. The technologies that made the agricultural revolution possible in the United States were almost all developed through technology research programs in the Department of Agriculture.

True, there have been catastrophic failures in government programs. The one most cited (and, I might add, most used to argue against ATP) is the Department of Energy syn-fuel program in the mid-1970s. Knowing something about that program, I would suggest that its biggest failure was to allow the politics of the day to dictate costly “quick fix” demonstration projects, rather than focusing on the technology research that needed to be done prior to the design of large, complex pilot plants.

In any case, a good analysis of government support for technology research in this century would show a return on investment probably equal or superior to that enjoyed by our innovative companies in the same period. In addition, that support has largely been at the early stage of a technology, where the private sector is least likely to provide the seed money to get it from a promising idea to recognizable commercial potential. The real success of our system has been the ability of our established companies and our entrepreneur and venture-capital communities to discern quickly where commercial opportunities may arise from early-technology research, and to make the investment necessary to assess the market fully and do the innovation necessary to bring the technology to a successful commercial outcome. Thus, ATP and sister programs should be judged on where they fit in to the innovation system of the nation, including an evaluation of the country’s complex portfolio of research and development in the private sector, the not-for-profit-sector, and the government.

The Council on Competitiveness has recently done such as analysis; a close read of its report on innovation would provide both feelings of comfort about our current development of commercial products and of concern about whether our investment in next-generation technology is adequate to provide the United States with the opportunity to be leaders in the next global business cycle.[50] The investments by the National Institutes of Heath (NIH) and the pharmaceutical industry have clearly given us a commanding lead in biotechnology in almost all of its aspects. This lead can be expected to persist for quite some time, both because of the magnitude of our lead, and because NIH continues to fund basic science that is close to the potential technology. Moreover (although this is not a highly publicized fact), NIH funds early-technology development that moves quickly into commercialization processes of the pharma-firms. Today U.S. research in the life sciences is roughly $26–28 billion annually, with NIH funding about half of that. Of all the models that indicate the values of government research, both fundamental and applied, the successes of the biotechnology and pharmaceutical industries are stellar examples.

However, the Council finds that in other technology-based commercial areas, the commitment to research is much weaker both in the industry and in the government. Research and development in information technology is dominated by the development of new applications for technology that is already beyond the true research stage. Indeed, most of the venture capital money in Silicon Valley now goes to new companies with innovative ideas for exploiting today's technologies. Both David Morgenthaler of Morgenthaler Ventures and Richard Burnes of Charles River Ventures made comments to that effect at the MTR

[50] Council on Competitiveness, *Going Global: The New Shape of American Innovation* (Washington, D.C., 1998).

practitioner's workshop in June 1999. Mr. Morgenthaler made the point that venture capitalists do not develop enabling technologies, and that their fortes are in the use of enabling technologies to develop a product, the revision and improvement of a product, or the creative metamorphosis of a product into an application in a new arena. Mr. Burnes made the point that in the 1970s and 1980s, venture capitalists did see "technically sophisticated projects, where there was a lot of technical risk, and they took that risk." However, he followed that comment with the observation that venture capital activities are now in a phase "where there are many, many opportunities that the existing technologies can address and create major companies."

The question then becomes: who is financing the early-technology research that will lead to new technologies? Apart from James McGroddy of IBM, the panel of business executives at the June MTR workshop did not create the impression that the industries they represent would support the early-technology research necessary to develop a proof of principle or a "bread-board" prototype. They talked about "process systems" that let one assess risk before an investment is made, about how to determine if potential products would fit in their companies' business plans, and about the need to determine whether the project will have a potential market of a size to be of interest to a large company (maybe $50 million is the cut-off). None of these comments gives you the feeling that these will be the avenues for truly new technology development. One very promising theme of their remarks, however, was the realization that innovation is a "people" activity and that the involvement of really good people, both technical and managerial, is the key to the successful commercial exploitation of research of any kind.

The remarks of the business executives are very much in line with the results of on-going analysis of current industry R&D trends by the Industrial Research Institute, the National Science Foundation, and others. All of these studies indicate that the percentage of industrial R&D devoted to basic or applied research is small compared to the resources expended for product and process improvement and technical services. In industries where new products move rapidly into the market, such as personal computers, companies rarely have significant technology investments beyond the next model to be released. In the current global environment where quality, price, and time-to-market are the differentiating business parameters, this use of technical talent and capital resources is neither unexpected nor necessarily bad. It just means that the fundamental technology pool available for true innovations is not being replenished by these firms.

A very good overview of the innovation in industry was presented in the *Economist* magazine of February 20, 1999. Several specific assessments in that article are relevant to the present discussion:

- One-third of all of the world's venture capital today goes to nurturing innovation in Silicon Valley. Most of the money is raised there, most of the entrepreneurs have moved there, and most of the wealth created stays there.

- The *Economist* argues that the most likely rival to Silicon Valley is Israel with its immigrant technical workforce, competitive environment, respect for learning, and willingness to take risks. It reports that Israel has 135 engineers and technicians per 10,000 people, compared to only 18 in the United States.

- The typical strategy for venture-funded businesses has changed over the last year or two: going public is less common, as more firms are being bought out by established companies seeking to shore up their innovation product streams.

- The international competition in innovation is heating up. Of the world's top 300 international companies, those headquartered in Denmark, Sweden, Canada, and the United States have increased their industrial R&D spending by 17–26 % from 1996 to 1997. During the same period, similar firms in Britain and Italy increased their average R&D spending by only 3–5%. In Finland, notably, comparable companies spend an average of over 10% of sales on R&D, while U.S. firms in the same cohort spend about 5% of sales. (Perhaps the success of Finland's Nokia cellular telephone company is no fluke.)

- The *Economist* article argues that "innovation has more to do with the pragmatic search for opportunity than with romantic ideas about serendipity or lonely pioneers pursuing their vision against all odds." It states that the new industrial cycle fueled by information technology has probably run through two-thirds of its life-cycle, leaving only a 5–10 year window before some new wave of technology begins.

- The Stevenson-Wydler Technology Innovation Act and the Bayh-Dole Act have had a significant impact on American innovation, says the *Economist*, because they have fostered government-industry-university interactions, which have speeded up the exploitation of not-for-profit research, allowed private companies to partner with government, and let university researchers "cash in" on their government-funded expertise.

These reports, reviews, and economic studies, along with personal observations, lead me to believe that the current industrial structure will not provide the same level of technology pool for the country's innovators to draw from in the future as has been provided in the past fifty years. That technology pool was a mix of heavily funded corporate laboratories that created more technology than they used in-house, and government funding of fundamental research and early technology research, especially at DARPA and NASA. On the government side, the current climate is not conducive to replacing DARPA and NASA in their role in technology research, and the defense budget continues to de-emphasize this type of activity.

On the industry side, there are some significant bright spots where industry is carrying out research on truly new technologies. Two that come to mind immediately are Xerox PARC and Lucent's Bell Laboratories. A review of Xerox's web page indicates the breadth of its "out-in-front" new technology research, ranging from next-generation technology for document handling to truly new technology like MEMS (Micro-Electro-Mechanical-Systems), "smart matter," and nanotechnology. Bell Labs' new venture has created new business structures to capture promising new technologies discovered by Bell Labs researchers. Other similar industrial laboratories have also done significant work in the development of truly new technologies. However, they are not likely to create spin-offs like those that resulted from the technologies that escaped from Xerox PARC in the 1970s or that created Intel from Shockley’s transistor, first conceived at Bell Labs. The new generations of research managers and business executives at these companies have worked hard to shape the forefront research so that they can capture most of the value. Thus the pool of new start-ups in new technologies outside of the biotech area will come primarily from the academic laboratories, the government laboratories, and the government-sponsored partnerships like ATP, and perhaps from continuing programs from NASA and the Defense Department.

There are many justifications for programs like ATP, and most of them have been well articulated in the past few years. Two really important reasons that have not received attention from ATP or the policy community, however, are the needs to provide opportunity to entrepreneurs in all parts of the country, and the need to support subject areas not currently considered fashionable by the usual providers of capital. Good ideas do not arise only in Silicon Valley and Austin, Texas. They occur all over the country and, if properly nourished, they could be the seedlings of new centers of innovation activity. The same is true for innovative ideas in areas other than information technology and biotechnology, which absorb most of the current venture capital resources. The concept of nurturing new technology to the point where its potential commercial value can be determined should be a priority of the national innovation policy. It should be supported through incentives to state governments, programs like SBIR (Small Business Innovation Research) and ATP, and incentives for investors to place at least some of their risk capital in early-stage technology research which has high risk but also high potential to become a "change agent" product or process.

ATP has been studied more than any comparable program ever! It has been shown to deliver results from a well-conceived and well-run rigorous review process that is not subject to political bias. However, it has been plagued by ideological debates, identification as a "Clinton" program, and year-to-year funding that resembles the "perils of Pauline." I would hope that in the next year or two, it can be stabilized with funding in the $500 million per year range, and that it could be focused on technologies that are not fundable (or at least not funded) in other agencies and are not in vogue in the private capital community. The projects could be selected very much as they are now and the mix of large companies, startup, or early-stage companies and consortia could be controlled to be sure adequate attention is given to small and embryonic companies with good ideas. Larger companies should not be eliminated, however, because they can bring a range of opportunities which they would not pursue on their own but where they have resources and expertise to bring projects to a satisfactory conclusion with some wins and some losses.

If appropriate research and policy strategy could position ATP as a strategic piece of the government's research portfolio, to provide opportunity for entrepreneurs in any location and in areas of corporate and government neglect, I believe bipartisan political support could be achieved. The quality of the peer review, the vision of new technology development, and the business incentive process could all still be maintained. This would clearly circumvent any political attempt to reposition the program to fund politically popular research areas or to rework the management and selection criteria each year. This rationale could also be used to appeal to the states to provide matching funds, create incentives for local investors, and get congressional delegations on board.

I am an advocate of a balanced federal R&D portfolio that includes: first, fundamental research that is not targeted to any foreseeable commercial use; second, applied research designed to provide answers to specific scientific and technical questions, and needed to carry out certain government missions in defense, energy, space, the environment, and underlying national interests in the commercial sector such as standards and meteorology; and third, technology research that provides incentives for the development of new technologies before the usual market forces will focus on them, and that will provide significant additions to the country's technology pool. A portfolio of this mix will provide universities with funding for knowledge creation research, an opportunity to partner with industry in applied areas, and support for the education and training of the next generation of our technical workforce. It allows mission agencies to meet the new demands on their knowledge base, and it creates new technologies that create new businesses. The portfolio also provides private-sector investors with a variety of opportunities to create growth for both new and existing businesses.

What I am truly advocating is a research policy for federal investments that will serve as well during the next fifty years as federally-funded health and national security R&D have served us over the past fifty years. After about fifty years of experience in investing in research and people in a rather opportunistic way, it is time the public had an innovation investment policy that is as good as that practiced at Xerox, Lucent, and other forefront companies who will be players in the next technology wave, whatever it turns out to be.

Concluding Remarks

Government policies intended to promote innovation in the American economy must reflect future as well as current realities of the innovation system. The policies in the 1988 Trade and Competitiveness Act and other legislation—much of it in response to a serious challenge to U.S. high-tech preeminence in the 1980s—reflected the institutional and economic conditions of the time. Our discussion of technical risk management has been cast largely in the framework of familiar institutions: newly formed, technically specialized, firms; large companies with deep resources; universities supplying new concepts to innovators; government agencies funding and conducting research. The principal actors are familiar too: technical innovators with visions of new technologies of commercial promise; business executives seeking to allocate their resources across a broad range of opportunities; investors eager to multiply their equity through creation of new, successful enterprises.

As the new century begins, massive changes are sweeping through the U.S. system of innovation. There have been major shifts in the sources of ideas, technical knowledge and capital. As a consequence of these shifts and the quickening pace of competition in global markets, the institutions in the innovation system are relating differently to one another in response.

A powerful upsurge in private sector R&D investment has occurred in the last decade. The capitalization of enterprises based on both market and technology-based innovation has soared. The innovation process is increasingly led by small, medium and newly created firms. The role of government as an industrial driver through military procurement has faded in comparison to more robust commercial markets.

The evidence for this is clear: in 1988, the year the ATP legislation was enacted, private sector funded R&D expenditures were 50.2 percent of the national total. In 1999, NSF estimates this fraction will have fallen to 24.7 percent. According to NSF estimates, industry spent $185.9 billion in 1999. Of that total only 10.7 percent is estimated to have come from government sources, in contrast with 32 percent in 1987. Thus the historic equality of the split of R&D investment between public and private sources has been broken, driven by the strong growth in the commercial economy and the draw down of defense and NASA R&D resources. Thus overall federal investment in R&D has been essentially flat since 1988, but with big shifts away from funding to industry, compensated by growth in academic research, especially in health related areas.

At the same time the availability of venture capital has soared.[51] Traditionally a highly cyclical component of risk investment, the funds available appear to exceed substantially the number of projects that meet the venture investor's risk-avoidance thresholds.

Does this happy, although perhaps transient, circumstance imply that the government's concern over the research support for commercial innovation is now allayed, and other areas of investment should take priority? Not at all. As stated graphically in the Congressional Report authored by Congressman Vernon Ehlers

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[51] Between1980 to 1995, disbursement from U.S. venture capital firms fluctuated from $608 million (1980), to $3.2 billion (1986), $1.3 billion (1991), and $3.8 billion (1995). Since 1995, disbursements have soared, growing by better than 50% each year, and reaching a total $28.6 billion in just the first three quarters of 1999. (All figures in U.S. dollars, unadjusted. Sources: Venture Economics and the National Venture Capital Association.)

> Concern has been raised that companies are focusing their research efforts on technologies that are closest to being marketable—and hence are likely to be profitable sooner—instead of on projects which will require a more substantial research investment. This approach is of questionable long-term sustainability. The deployment of industry scientists on research problems that address largely—or entirely—projects for which there are expected near-term payoffs suggests that these scientists will work on a series of short-term research projects and not be encouraged to take part in longer-term, more exploratory research. This would represent a clear loss for the overall research enterprise.
>
> At the same time, the limited resources of the federal government and thus the need for the government to focus on its irreplaceable role in funding basic research, has led to a widening gap between federally funded basic research and industry-funded applied research and development. This gap, which always existed but is becoming wider and deeper, has been referred to as the "Valley of Death." A number of mechanisms are needed to help span this Valley and should be considered.[52]

The degree of general agreement among technical entrepreneurs, high-tech business managers, and venture capital investors was striking on this point. There is a significant gap between the creation of a technical concept with potential for commercialization (proof of concept), and the establishment of designs and processes that can be shown to meet an attractive market opportunity (reduction to practice). Given evidence that government funds research through the concept phase, and VC firms typically invest only after this phase is complete, what are the appropriate sources of support for research in this gap, which aims to reduce the technology to practice?

The consensus view was articulated by David Morgenthaler:

> I do not see how the government can help very much in the process of evaluation of venture capital investment opportunities. However, it does seem that early stage help by the government in developing platform technologies and financing scientific discoveries is directed exactly at the areas where institutional venture capitalists cannot and will not go. In the analogy of the horse race, the role of government can be to improve the bloodlines of the horses and give them some preliminary schooling.[53]

Congress appears to have intended the ATP to be a mechanism for addressing the "Valley of Death" area of research, which has been described by Charles Vest, president of MIT, as "mid-level" research, and by Branscomb as "basic technology research".[54]

If such a gap is real, the fact that growth of commercial R&D is outstripping the growth of government-funded (Morgenthaler's bloodlines and preliminary schooling) only means that the gap may be growing, not shrinking, as public funding of early stage research fails to keep pace with commercial development.

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[52] U.S. House of Representatives, Committee on Science, "Unlocking our Future: Toward a New National Science Policy" (Washington D.C.: GPO, 1998), pp. 39-40.
[53] David Morgenthaler, "Assessing Technical Risk" (pp 103-107) in this volume.
[54] Lewis M. Branscomb, "From Science Policy to Research Policy" chapter 5 in Lewis M. Branscomb and James Keller, eds, *Investing in Innovation: Creating a Research and Innovation Policy that Works* (Cambridge MA: MIT Press,1998) pp 112-139.

Large corporations are increasingly focusing on their role as system integrators, low cost producers, distributors and marketers internationally, while outsourcing much of their innovation to the mid-size and smaller technically specialized firms in their supply chain. Those smaller firms are often highly innovative but do not have a tradition of research. New ventures are often exploiting university inventions. Where will those small-to-medium firms get their insights into "the art of the (scientifically) possible," if government funding is not keeping pace and the large firms they serve are expecting their supply chain to produce their own innovations?

Thus, if there is a growing gap between proof of concept and reduction to practice, and there is at least a limited consensus that public research funding can contribute to closing it, the key issue is how that investment can most effectively contribute to reducing the risks that inhibit the creation of new opportunities for private investment and economic growth? Answering this question was the motivation for our study.

Government is moving away from its historic focus on R&D and procurement to find ways to enable higher rates of innovation in the economy.[55] Industry is increasingly using the new more powerful tools of science to master complex, custom-designed technologies, and doing so with shorter product cycles. University cultures are beginning to embrace a new role in the economy, as they find themselves the sources of these new tools.[56] Universities, firms and investors are all putting their talent and equity into innovative new firms. What will be the changes in the way technical risk reduction will be sought?

Our discussions revealed that many of the barriers to taking technical risks, despite attractive market possibilities, derive from the institutional and management-environment differences these actors encounter. The evolving role of the university seems particularly important, both as the source of intellectual capital for the private sector, but also as a place to nurture the "mid-level" or "basic technology" research that fills the gap between proof of concept and reduction to practice of a new technology. In the two cases studies we discussed with the principals, several years of additional research in a university setting was necessary before the new firms to exploit it could be created. A number of universities are even creating affiliated venture funds to accelerate the exploitation of the intellectual assets they produce.[57]

How far universities wish to go to support entrepreneurship is being hotly debated. They will have to determine the balance to be struck between preserving the traditional reluctance of academia to engage in relationships with commercial institutions and the opportunity both to serve society by participating in the creation of new technology and to benefit financially from doing so. But even if universities continue on their current path, which appears to embrace partnerships with private sector firms with some eagerness, including taking equity interests in those they help create, the question remains, Who will fund this work in the universities?

Private investors appear ready to provide some early research funding for promising university projects for pharmaceutical and software markets. But in the broad array of other technologies, they do not appear ready to do so. Thus there appears to be a need for partnerships between firms and universities, in which the federal research agencies may

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[55] The case for this shift from Science and Technology Policy to Research and Innovation Policy is explored in Lewis M. Branscomb and James Keller, eds., *Investing in Innovation* (Cambridge MA: MIT Press 1998).

[56] See Lewis M. Branscomb, Fumio Kodama, and Richard Florida, eds, *Industrializing Knowledge* (Cambridge MA: MIT Press 1999).

[57] Josh Lerner, "Venture Capital and Academic Technology" in *Industrializing Knowledge*, loc. Cit. Table I page 387.

participate, that can close the gap by allowing new science to be converted to useful technology, guided by the collective vision of technical entrepreneurs, business mangers and venture investors.

The widespread attention given to new ventures and smaller firms as the most effective vehicles for science-based innovations (see Shane) suggests that more attention should be paid to the relationships between innovators, managers and investors and the obstacles they face in creating high-risk, high-potential innovations. This is especially true when the innovator may come from a university; the manager may run a technology-focused company interested in exploiting the innovator's ideas; and the investor may be a principal in a VC firm tracking the evolution of the technology to a point where he can prudently invest.

The research required to reduce a promising technology to practice is often—perhaps usually—intimately and interactively related to product specifications that reflect the market opportunity that carries the business case for investment. This means that if firms and investors are to define the opportunities and put up the capital for their realization, agencies who seek to share in the research agenda must be flexible, agile, and responsive to change. For this reason it is highly appropriate that ATP projects are managed as Cooperative Agreements, which can provide that flexibility.

Large firms have both financial and technical resources for radical innovations but often lack the incentive to pursue them. Partnerships with smaller firms or universities, or incentives to spin off the technology to a new firm, can help overcome this reluctance. Their participation in consortia centered on a common interest in opening up new areas of promising technology is encouraged by ATP for this reason.

Our project also reveals a unique position for the mid-sized to smaller established firm that builds its business model around an area of technology in which they specialize. In these firms the ability of the technical innovators and those with responsibility for committing the resources to share their understanding of technical and business risks may be particularly favorable. It is not surprising that they form key sources of innovation in the supply chains of the large companies they serve.

National and private interests are different in such partnerships. That difference is reflected in the interest of the government in realizing a commercially viable new technology in the economy while the interest of the firm, and that of their investors, is to create value regardless of the research strategy required to deliver it. As Lerner correctly argues, the government is not in a position to behave like a venture capital investor. The ATP program does not try to do so, for the requirements for venture capital investment success lead to a degree of participation in company management based on a strong equity position that is wholly inappropriate for government. The government program must be seen as a strategic investment in technology creation, going beyond the creation of new concepts but stopping short of participating in new product development.

This leads naturally to the conclusion that government should measure its own progress by evaluation of the technology created, the future economic potential of that technology and its diffusion through a significant segment of the economy. It is, of course, quite reasonable to accept evidence of successful commercialization by the government's partners as evidence of that potential and its diffusion as competitors and partners also take advantage of the new technology. Having seen that the barriers to technical risk-taking in science-based innovations are not all economic but have substantial institutional components, any government program seeking to lower these barriers must seek to understand the way these firms who might seek to partner with government deal with the technical elements of risk. We hope that this study has opened a useful exploration of this issue.

Figures

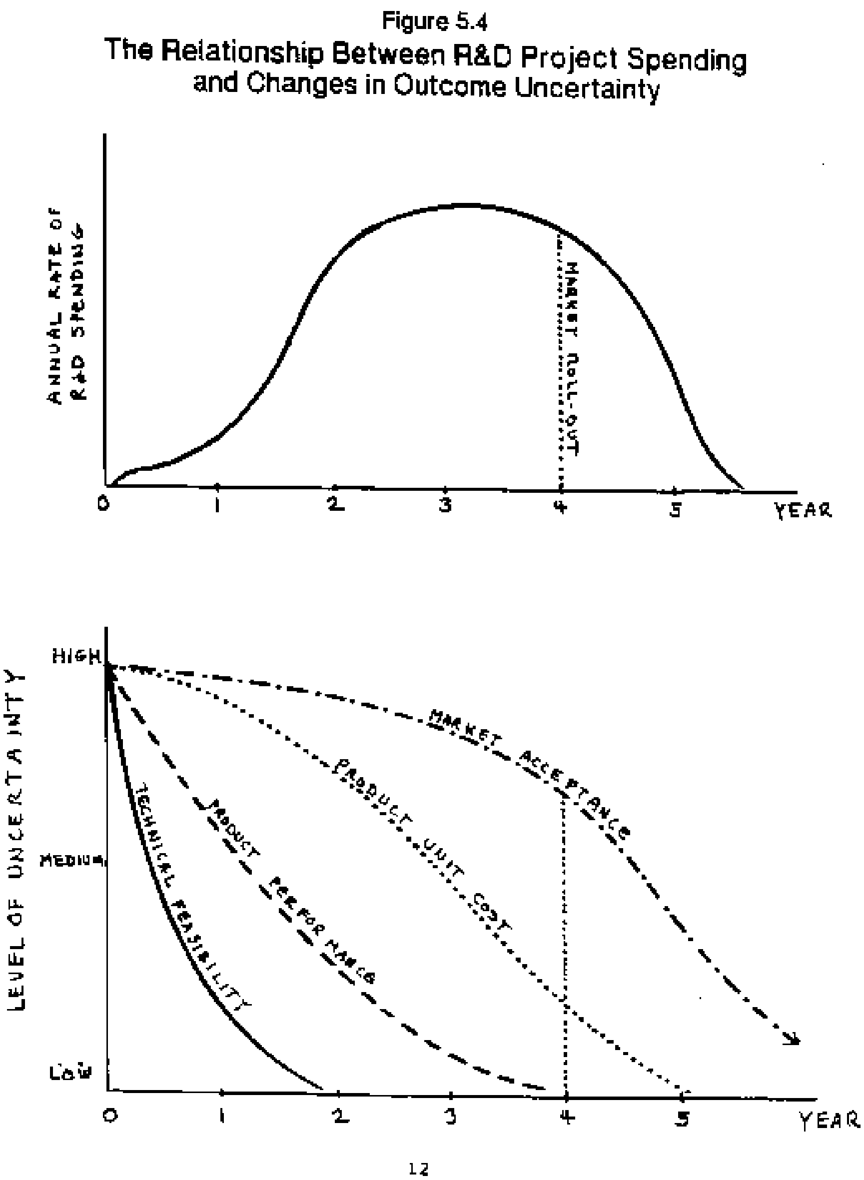

Figure 1. **Relationship between R&D project spending and changes in outcome uncertainty.**

Source: F.M. Scherer, *New Perspectives on Economic Growth and Technological Innovation*. (Washington, DC: Brookings Institution Press, 1999), p. 66. Adapted from M.J. Peck and F.M. Scherer, *The Weapons Acquisition Process: An Economic Analysis* (Harvard Business School Division of Research, 1962), p. 313.

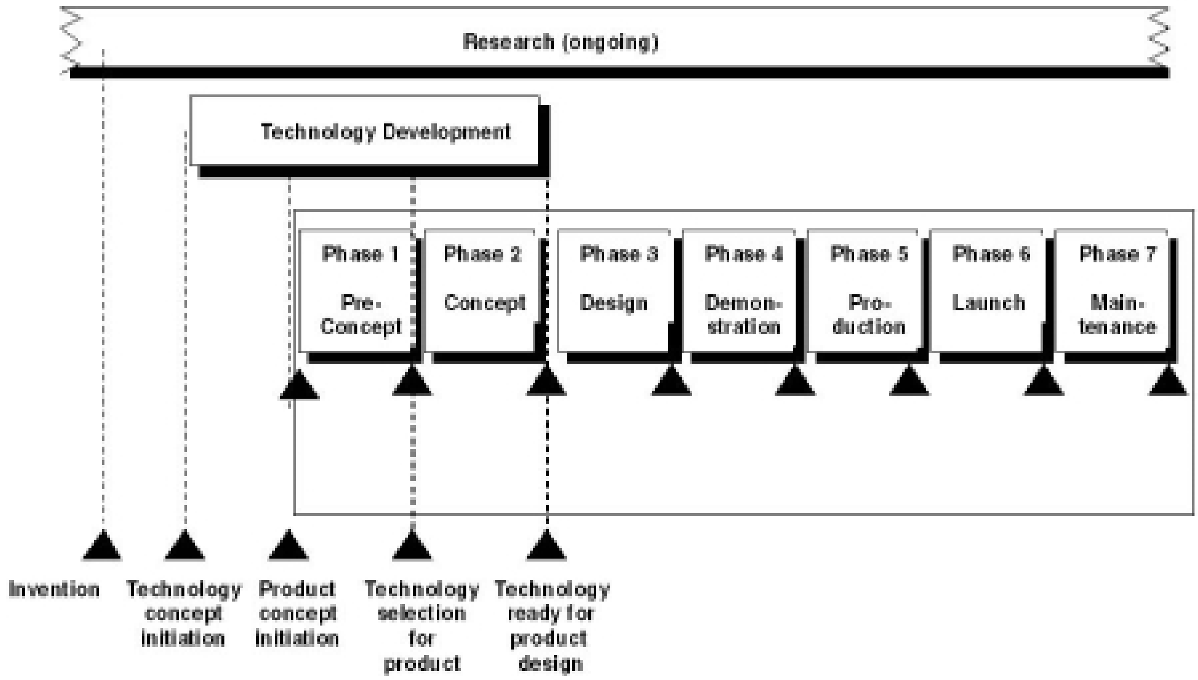

Figure 2. **The Xerox product development pipeline.**

Source: George C. Hartmann and Mark B. Myers, "Technical Risk, Product Specifications, and Market Risk", in this report.

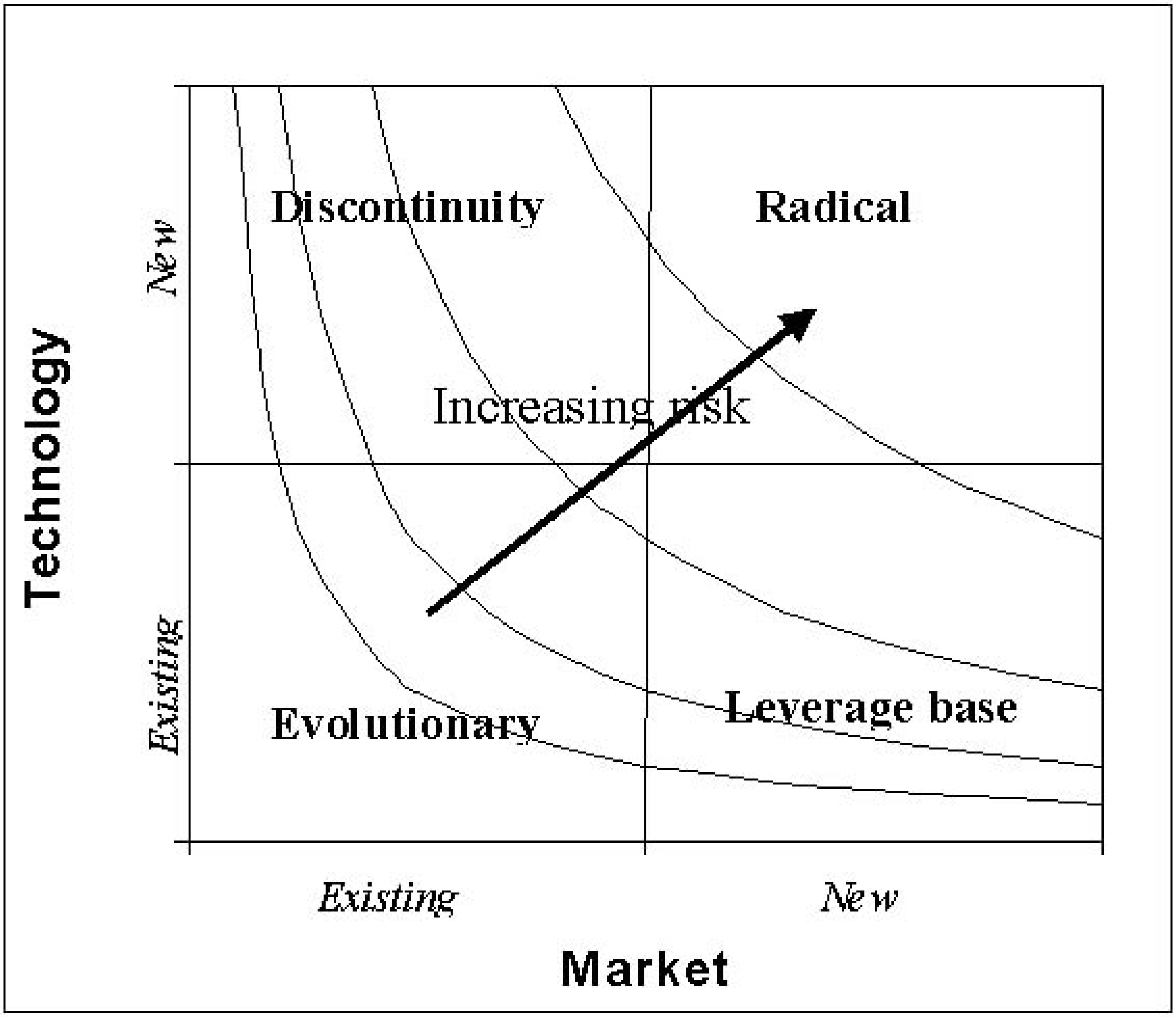

Figure 3. **Quadrants of risk.**

The solid lines represent constant overall risk.

Source: George C. Hartmann and Mark B. Myers, “Technical Risk, Product Specifications, and Market Risk”, in this report.

The Dual-Edged Role of the Business Model In Leveraging Corporate Technology Investments

Henry Chesbrough and Richard S. Rosenbloom

Henry Chesbrough is Assistant Professor and Class of 1961 Fellow, and Richard S. Rosenbloom is David Sarnoff Professor of Business Administration, Emeritus, at the Harvard Business School.

Abstract

This paper defines the concept of a business model and describes its role in focusing resources and attention within the firm on certain technologies, while implicitly discouraging investment in others. We argue that companies are biased towards investments in technologies that can be deployed within familiar business models, sometimes to the point of overinvestment. Companies are biased against making investments in technologies that do not fit with their established business models. These ideas are illustrated through a series of examples: (1) The pharmaceutical industry illustrates the ability of firms to continue high levels of investment in commercializing technology when the business model remains viable and relevant, despite technological change; (2) the origins of the Xerox 914 copier show the limitations of applying an established business model to a new opportunity, and the rewards generated by utilizing a creative new model; (3) DuPont's polymer innovations illustrate the leverage provided by a robust business model that is applicable to a wide range of technologies, as well as the risk of overinvestment that can ensue. We close with a brief comment on "technology push," where technology is commercialized in the absence of any defined business model.

Organized R&D in both public and private laboratories in the industrial world continually produces scientific discoveries and breakthrough inventions that open up manifold opportunities for commercial exploitation. Each of these events embodies the kernel of a potential innovation, whose realization requires investment both to shape the nascent technology to fit specific uses and to create the organizational capabilities necessary to bring it to routine commercial use. Those investments must be made—often many years in advance—in the face of significant uncertainties about the eventual commercial outcomes to be realized by the investing organization.

There is a common opinion that established businesses exhibit a systematic bias toward underinvestment in commercialization of novel emerging technologies, while startup firms are believed to exhibit less of this bias.[58] However, the readiness of new ventures to bring novel technologies to market is accompanied by a disinclination or inability to invest in the initial discovery research and early development of those technologies. This puts more importance, from a national point of view, on the innovative behavior of established firms, especially the large ones capable of pioneering work in new technology.

[58] See, for example, Richard N. Foster, *Innovation: The Attacker's Advantage* (New York: Summit Books, 1986).

Thus the sources of the bias of established firms against commercializing new technologies must be clearly articulated so that policy prescriptions can be formulated to offset them.[59] The social dynamics of large organizations sometimes play a role. In addition, the economics of "cannibalization" of established profit streams can deter worthy ventures. But other factors are also likely involved, especially uncertainty, and the business model, as we discuss below.

In the MTR Practitioners' Workshop in June 1999, most attention was paid to the uncertainties inherent in the situation. Clearly they are substantial and multi-dimensional. They affect estimates of key parameters, especially product performance, long-run costs, time-to-market, market acceptance, and competitive response. But uncertainty is only one element of the investment calculus. Investments are motivated by the expectation of future reward. Profit-seeking investors will commit resources in the face of substantial uncertainty if the potential payoff is correspondingly large in relation to the investment. In the end, the decision to invest is governed by the perception of that reward, its nature and magnitudes, adjusted for the perceived uncertainties and the expected time to realize it.

We argue that successful firms tend to interpret the potential value of nascent technologies in the context of the dominant business model already established in the firm. The reward to be expected from any innovative venture must be assessed within the framework of a specific business model, which will specify how revenues will be generated, from whom, and what costs will be incurred in so doing. In other words, technology does not create value in a vacuum. The established model may or may not be appropriate to the opportunities inherent in the new technology. If not, its use will lead to inaccurate analysis and underinvestment. That is one source of the bias exhibited by successful firms facing novel technologies, and it is the one to which we devote the rest of our discussion.

The business model concept

This term "Business Model" is widely used, but seldom well defined. In our usage, the functions of a Business Model are to:

- identify a *market segment*, that is, the users to whom the technology is useful and for what purpose;

- articulate the *value proposition*, that is, the value created for users by the offering based on the technology;

- define the structure of the *value chain*, that is, the network of activities within the firm required to create and distribute the products or services offered to customers;

- estimate the *cost structure* and *profit potential* of producing the offering, given the value proposition and value chain structure chosen;

- describe the position of the firm within the *value network* linking suppliers and customers, including identification of potential complementors and competitors;[60]

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[59] Note that we do not attribute this bias to firms of any particular size, only to those having substantial experience in a given marketplace. Size does seem related, however, to willingness (or ability) to invest in invention or discovery and in pre-commercial development of novel technologies. There, large firms are predominant.

[60] The term "value network" is used in different ways by Clayton M. Christensen and Richard S. Rosenbloom, "Explaining the Attacker's Advantage," *Research Policy*, 24:233-257 (1995); and by Adam M. Brandenburger and

- formulate the *competitive strategy* by which the innovating firm will gain and hold advantage over rivals.

Defining a business model to commercialize a new technology begins with articulating a value proposition inherent in the new technology. The model must also specify a group of customers or a market segment to whom the proposition will be appealing and from whom resources will flow. Value, of course, is an economic concept, not primarily measured in physical performance attributes, but rather what a buyer will pay for a product or service. A customer can value a technology according to its ability to reduce the cost of a solution to an existing problem, or its ability to create new possibilities. One challenging aspect of defining the business model for technology managers is that it requires linking the physical domain of inputs to an economic domain of outputs, sometimes in face of great uncertainty.

Value thus derives from the structure of the situation, rather than from some inherent characteristic of the technology itself. Increasingly, realizing value also involves third parties. The value network created around a given business shapes the role that suppliers and customers play in influencing the value captured from commercialization of an innovation. The parties in the value network can benefit from coordination if that increases the value of the network for all participants.

A market focus is needed to begin the process in order to know what technological attributes to target in the development, and how to resolve the many trade-offs that arise in the course of development, e.g. cost vs. performance, or weight vs. power. Technical uncertainty is a function of market focus and will vary with the dynamics of change in the marketplace.

Identification of a market is also required to define the “architecture of the revenues”—how a customer will pay, how much to charge, and how the value created will be apportioned among customers, firm, and suppliers. Options here cover a wide range including outright sale, renting, charging by the transaction, advertising and subscription models, licensing, or even giving away the product and selling after-sale support and services.[61]

> Having a sense of price and cost yields target profit margins for the opportunity. Target margins provide the justification for the real and financial assets required to realize the value proposition. The margins and assets together establish the threshold for financial scalability of the technology into a viable business. In order for the business to grow, it must offer investors the credible prospect of an attractive return on the assets required to create and expand the model.

Barry J. Nalebuff, *Co-opetition* (New York: Doubleday, 1997). The former emphasizes the extended supply chain from supplier to customer; the latter focuses on rivals and allies in the “game” of competition. Both frameworks are relevant for our purposes.

[61] The technology sector is witnessing a proliferation of business models as a result of the Internet. Models may be based on providing internet access to viewers, luring viewers with free content in order to sell advertising, selling subscriptions to viewers, providing them with utilities, aggregating viewers and effectively "reselling" them to other content providers, selling products and services, or mediating market transactions between viewers. Some firms such as AOL blend multiple models together. AOL is currently an access provider, a portal, and a content provider, and is also becoming a market mediator. A newly emerging variant of this appears to be what is driving the “open source” software development model that has propelled Linux to prominence in network servers, where the code is given away, and supporting services are the source of revenues. This is a virtual analogue of the Xerox model used to market its 914 copier, which we discuss below. Our thanks to our colleague, Tom Eisenmann, for characterizing the different emerging Internet business models. See also Peter Cohan, *Net Profit: How to Invest and Compete in the Real World of Internet Business* (San Francisco: Jossey-Bass, 1999).

Case illustrations

We provide brief case examples to illustrate our argument, and to show how the business model concept can inform our understanding when private firms can sustain high levels of investment (pharmaceuticals), when they underinvest in a new technology due to the use of an inappropriate business model (the Xerox copier), and when they overinvest in technologies due the reliance upon a previously successful business model (DuPont). We begin with the pharmaceutical industry.

THE PHARMACEUTICAL INDUSTRY: SUSTAINED PRIVATE INVESTMENT, DESPITE ENORMOUS TECHNICAL CHANGE

One might think that the pharmaceutical industry would shows signs of strain from new "disruptive" technologies, because the underlying science for most drug discovery has recently been revolutionized, from organic and synthetic chemistry to genetic science. “Designer drugs” now flow from scientific laboratories in companies that a generation ago found their products through random screening. However, the industry provides many examples of large investments in new technology by its dominant firms. Many of the firms that led the industry twenty years ago (Merck, Pfizer, Lilly, Abbott) continue to be at the forefront of the industry today. While there has been noticeable entry by young startup firms, particularly in the biotechnology area, the overall structure of the industry is relatively unperturbed, especially by comparison with what has happened to leading organizations in the information technology sector.

Why has so much new technology created such little disruption? The plausible answer here is that the business model of the pharmaceutical industry has not changed much, despite the scientific revolution that has transformed the flow of new products. The value proposition for most ethical drugs is little different than it was decades ago, even though the science base and manufacturing processes for these drugs has greatly changed. Patents remain essential; the necessary FDA approval still defines the development path to commercialization; physicians remain the “customers” who specify the drugs to be consumed by their patients; and marketing channels to reach these physicians are still vital. These elements of the pharmaceutical business model have remained stable, enabling the industry to finance the commercialization of exotic technologies that draw from completely new areas of science.

XEROX 914: A NEW BUSINESS MODEL REQUIRED TO COMMERCIALIZE A "DISRUPTIVE TECHNOLOGY"

The introduction of the Xerox 914, the first plain-paper high-quality office copier, provides a classic example of our argument, both of investments deterred by the initial inappropriate application of an established business model, and of rewards amplified by the creation of a novel model in its place.

Xerography surely ranks as one of the most significant new technologies of the mid-20th century, yet its commercial success came only after it had been rejected by several leading firms, including Kodak and IBM.[62] Chester Carlson, a graduate in physics from Cal Tech who

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[62] Sources for this brief history include a talk given by C. Peter McColough, then Chairman of Xerox, printed as “The Birth of Xerox,” *Agenda*, No. 20 (Rochester, N.Y.: Xerox Corporation, May 1984); Arthur D. Little, Inc., "Report to International Business Machines Corporation: Investigation of Two Haloid-Xerox Machines as New Product Opportunities in the Office Reproducing Equipment Field,” December 1, 1958 [C-61613]; Robert W. Gundlach, “Xerography from the Beginning,” *Xerox World*, Vol. 7 No. 3 (Fall/Winter 1988), pp. 6–9; Eric Pell, *From*

became a patent attorney during the Great Depression, made the core invention working in his kitchen in the late 1930s. After Carlson filed his first patent in 1937, numerous corporations expressed interest in the novel technology, but none was willing to invest in bringing it from concept to practical reality. In 1944, he approached Battelle Memorial Institute, which soon entered into a partnership, investing in further development and acting as his agent. Commercialization was the work of Haloid Corporation, which approached Battelle in 1946 after learning of its work in xerography. Haloid, a small enterprise operating in Rochester, New York, in the shadow of mighty Eastman Kodak, served a niche market with high quality cameras and photographic papers for copying important documents. Its CEO, Joseph Wilson, driven to find a growth vehicle for his failing enterprise, “bet the company” in the 1950s on Carlson’s invention.[63]

It was not obvious *a priori* what would be the best economic use of the powerful capabilities inherent in xerographic technology. Haloid first designed a machine to produce offset masters. This generated a modest revenue stream in the early 1950s. But Wilson saw the potential for massive revenues in office copying, for which the desk-size Haloid 914 copier was designed. At that time, copies were made for business use either by “wet” photographic methods, or by low-quality dry thermal processes. Both methods required special paper or supplies, creating an aftermarket revenue stream for vendors. Typical office copying machines sold for $300. The average machine in use produced 15–20 copies per day, and 90% were used for fewer than 100 copies per day. The existing business model called for charging customers the full price of the initial equipment, and charging them again for supplies as needed. The new 914 copier, which produced high-quality images on plain paper, had a manufacturing cost estimated at $2,000.

Haloid sought vainly to find a strong marketing partner for the expensive new machine, but was rebuffed by Kodak and others. IBM rejected the 914 after a careful and highly professional market analysis by the respected consulting firm Arthur D. Little and Co. (ADL). ADL could not conceive a successful business model, in part because they could not identify a salient value proposition. They reported that:

[because] the Model 914 ... has considerable versatility, it has been extremely difficult to identify particular applications for which it is unusually well suited in comparison with other available equipment.... Perhaps the very lack of a specific purpose or purposes is the model 914’s greatest single weakness.[64]

Failing to recognize the radical character of xerographic technology, ADL analysts essentially assumed the 914 would be offered within the business model then extant in the office copy machine industry. Skeptical that customers would invest thousands of dollars to acquire a copier that would, after all, only be used to make a few hundred copies a month, they concluded: “Although it may be admirably suited for a few specialized copying applications, the Model 914 has no future in the office-copying-equipment market.”

Having failed to find a partner, on September 26, 1959, Haloid brought the 914 to market by itself. It surmounted the obstacles of high cost by using an innovative business model. A customer needed to pay only $95 per month to lease the machine, and to pay four cents per copy beyond the first 2,000 copies each month. Haloid (soon to be renamed Xerox) would

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Dream to Riches – The Story of Xerography (privately printed), 1998; and Carol Kennedy, “Xerox Charts a New Direction,” *Long Range Planning*, Vol. 22, No. 1 (1989), pp. 10–17.

[63] Wilson spent $12.5 million on development in the 1950s, more than the company’s profits for the decade.

[64] Arthur D. Little, Inc., "Report to IBM".

provide all required service and support, and the lease could be cancelled on just 15 days notice.

This was an attractive value proposition for customers. This business model imposed most of the risk on tiny Haloid Corporation: customers were only committed to the monthly lease payment, and did not pay anything more unless the quality and convenience of the 914 led them to make more than 2,000 copies per month. This let Haloid offer the 914 at a low entry price, to lure more customers. Only if the 914 were to lead to greatly increased volumes of copying would this business model pay off for Haloid.

Haloid's model essentially acknowledged that the ADL analysis was right, but was incomplete. Joe Wilson bet that ADL's conclusion could be reversed by a different business model. It proved to be a smart bet. Once installed, the appeal of the machine was intense; users averaged 2,000 copies per *day* (not per month), generating revenues far beyond even Joe Wilson's most optimistic expectations.[65] The business model established for the 914 copier powered compound growth at an astonishing 41% rate for a dozen years, turning $30 million Haloid Corporation into a global enterprise with $2.5 billion in revenues by 1972.[66] This was an early demonstration of a proposition now more widely recognized: that technologies that make little or no business sense in a traditional business model may yield great value when brought to market with a different model.

The story of Xerography in the 1950s is an archetype of what our colleague, Clayton Christensen, calls a "disruptive" technology. A technology is "disruptive" when it "bring[s] to market a very different value proposition than had been available previously."[67] Successful businesses, such as IBM and Kodak, have difficulty coping with such situations. Such companies, however, invest in many technologies, some radically novel, that are not disruptive. Christensen calls these "sustaining technologies" because they support growth in established businesses, reinforcing the complementary assets that serve those businesses. Successful businesses invest heavily in R&D for those technologies that they expect will fit within their established business models, as we saw with pharmaceuticals above.

DUPONT POLYMERS: NEW TECHNOLOGIES IN OLD BUSINESS MODELS

Another example of the leverage to be gained by exploiting novel technologies through established business models can be found in the history of DuPont's many innovations in synthetic polymers. The DuPont story, however, also shows how the intoxication of growth through continued exploitation of a winning model can lead to an unhealthy overinvestment in commercializing new technology.[68]

DuPont diversification in the 1920s built highly successful businesses in rayon fibers and cellophane films. DuPont sold these products only to fabricators who turned them into finished products. As part of its strategy, DuPont established expensive technical support organizations to assist customers in utilizing new products. DuPont promoted cellophane—where patents gave it a proprietary position—to end users to "pull" the product through its fabricator channels.

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[65] Kennedy, "Xerox Charts a New Direction."

[66] In the 1950s, antitrust pressures forced Xerox to offer machines for sale and competitive pressures squeezed margins. The company moved to a different business model, creating an "annuity stream" from placements based on sale of paper and supplies and on service contracts. Hence revenues continued to reflect copies made.

[67] Clayton Christensen, *The Innovator's Dilemma* (Boston: Harvard Business School Press, 1998), p. xv.

[68] The DuPont story is brilliantly recounted by David A. Hounshell and John Kenly Smith, Jr., *Science and Corporate Strategy: DuPont R&D, 1902–1980* (Cambridge: Cambridge University Press, 1988).

This model was readily adapted to the commercialization of nylon, the first synthetic fiber, in 1939. At the time, the Rayon Department was the largest and most profitable in the company. To develop demand for nylon, DuPont helped hosiery companies develop replacements for silk hosiery, an application that sustained a premium price for the fiber. Similar technical support helped carpet producers and tire makers to introduce the new material in their products. The size, breadth, and scope of applications allowed DuPont to make significant investments in facilities for nylon, which, in turn, yielded lower costs, enabling development of further applications. This created a reinforcing cycle of increasing demand, which led to additional capital investment in production, which spurred further cost reduction, which enabled further applications.

The awesome commercial success of nylon inspired research activity in search of “new nylons,” yielding the discovery of new polymers that were routinely commercialized within a similar business model. The first were Orlon and Dacron, brought to market in the early 1950s. Despite some concerns about cannibalization of nylon revenues, management wisely thought it better to manage the risk than to miss the opportunity.[69] By the 1960s, enthusiasm for this approach had spawned a host of new materials brought to market in ventures following the established pattern. Some, such as Lycra, proved highly profitable, but others were later seen as poor investments. Corfam, a leather substitute, was a highly publicized failure; Kevlar, despite “miraculous” properties, was characterized by *Fortune* as “a miracle in search of a market,” nearly a decade after its commercial launch.[70]

Implications

These cases suggest that the biases introduced by an established business model can cut two ways. First, as noted earlier, they can mask the potential for reward inherent in a valuable new technology to which the model is inappropriately applied. On the other hand, a model that has been notably successful in a series of new businesses can result in exaggerated expectations of the rewards from an innovation that has received insufficient scrutiny for that reason. The latter effect is similar to the force familiarly known as “technology push.” In such cases, enthusiasm for a novel technology, especially when combined with hunger for revenue growth, can lead to investments in commercializing innovations without sufficient scrutiny of their true economic potential. DuPont’s aggressive and insufficiently profitable “new products” push in the 1960s is a classic example of “technology push,” fueled by hubris derived from highly successful research in the context of a powerful and profitable business model. A variant of this is the move to commercialization on the basis of enthusiasm for the technology itself, expecting that an appropriate business model will reveal itself in time.

In the June 1999 MTR Workshop, Dr. Mark Myers described Liveboard, a failed Xerox venture of the mid-1990s that well illustrates this trap. Liveboard is essentially an electronic version of a whiteboard that is interactive and networked with other whiteboards and computers. An outgrowth of research on collaborative workgroups at Xerox PARC, it was soon recognized as a useful tool and quickly adopted for use within PARC and elsewhere in the company. A new venture organization was formed to bring it to market. As Dr. Myers described it: “we thought we would work out the business concept someplace after we got to market.... We knew there had to be [a market] out there... [but we] couldn’t figure out how to make money.” The venture was terminated in early 1997, after Xerox had invested tens of millions of dollars in attempting to build the business.

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[69] Hounshell and Smith, *Science and Corporate Strategy*, p. 420.
[70] Lee Smith, “A Miracle in Search of a Market,” *Fortune*, December 1, 1980, pp. 92–5.

The Xerox Liveboard experience cautions companies to devote more effort and investment to identifying a business model when pursuing a promising technology. Unless a viable path to commercialization can be identified, money spent on such technology-push projects is unlikely to yield a positive return. In turn, the government should be wary of inadvertently subsidizing ill-advised technology-push investments.

We conclude by noting three implications of our analysis. One is that executives in successful firms weighing investment options involving novel technologies need to be careful to ensure that the intended business model is appropriate both to the technology and to the sponsoring firm. Applying an inappropriate model, simply because it is familiar (as IBM and ADL did with xerography), or proceeding without a clear business model (as Xerox did with Liveboard), will not produce happy results.

Second, disruptive technologies are sure to challenge the capabilities of established firms. An organization cannot simply shift its capabilities to suit a novel business model if and when a new technology demands it. Organizations will have to become more creative and more willing to experiment with non-traditional organizational approaches in order to respond to the challenge of disruptive technologies. In the meantime, visionary risk-takers like Xerox's Joe Wilson will continue to find opportunities to profit from disruptive technologies.

Third, government programs such as ATP need to look beyond technology-push-based applications for technology funding by the private sector. As the pharmaceutical industry shows, private industry is likely to finance even very expensive discovery-oriented research initiatives when those initiatives can be commercialized through a viable business model. As the DuPont example shows, a strong business model may even motivate private industry to finance these initiatives past the point where they are economically justified. We believe that currently, in the high-tech industries, the private venture-capital sector provides substantial enough support for the exploration of new approaches to commercializing promising technologies that depend upon novel business models.[71] This suggests a potential role for ATP: one that is focused on early-stage, discovery-oriented research and development, since venture firms do not usually invest at that stage. A potential ancillary role would be to support organizations that might experiment with commercializing technology through new business models in the many industries that are not now well served by private venture capital.

[71] One very positive attribute of the decentralized exploration used in the VC sector to commercialize new technologies is that multiple parties will pursue many different commercialization paths and business models. This creates enormous diversity in ways to capture value from a technology, and allows the system to select from a wide range of business models. Particularly in the case of disruptive technologies, it is far from obvious what the "right" business model will be to create value. In these cases, a system that fosters diversity is more likely to find a "better" model than a system where few models are explored.

Technical Risk, Product Specifications, and Market Risk *

George C. Hartmann and Mark B. Myers

George C. Hartmann, Technology Strategy & Planning, Xerox Research & Technology, and Mark B. Myers, Senior Vice President, Xerox Research and Technology, Xerox Corporation, P.O. Box 1600, Stamford, Connecticut.

Xerox is a multinational corporation with $19.4 billion annual revenues. In addition, Fuji Xerox, jointly owned by Xerox and Fuji Photo Film Co., Ltd., has annual revenues of $6.8 billion, giving the company revenues on a worldwide basis of $26.2 billion. Fuji Xerox manufactures and distributes products in Japan and the Pacific Rim. Xerox Corporation offers products and services related to documents and associated information technologies. An ongoing challenge is creation of new products to refresh the product line as well as to grow revenue, requiring the generation of more than $3 billion additional revenue each year. To accomplish this, Xerox spends approximately 6% of revenue on research, development, and engineering (RD&E) annually, and uses a disciplined innovation process and product delivery system. About four-fifths of the RD&E budget is invested in product engineering and manufacturing, the remaining one-fifth is invested in research and advanced technology development.

The product development and time-to-market process, illustrated in Figure 1, includes research, technology development, and product development activities, each of which drives risk down. Research is an on-going activity that spawns ideas, inventions, and new technologies that must be reduced to practice. If promising, a new technology must then be developed, often concurrently with other sub-systems, for an envisioned market application. An objective of the technology development activity, illustrated by the middle box in Figure 1, is to demonstrate the performance potential of the technology and address robustness and manufacturing issues to reduce technology risk. A second objective is to refine the customer requirements to reduce market risk, and to evolve specifications.

The six boxes labeled Phase 1 to Phase 6 in the lower part of Figure 1 illustrate the product development process, which delivers final product specifications, product design, factory design and product manufacture, and product launch infrastructure. Technology development may occur concurrently in all three types of activities. Typically, decisions about the degree of concurrency depend on the objectives of the product program, and how much risk the product chief engineer is willing to accept. In many situations, it is best to demonstrate technology feasibility before committing to an expensive and time-sensitive product development effort. At any one time, on the order of 300 projects may be underway in various stages of the pipeline, from research to product launch. Over 90 products are launched annually.

A key mission of the Xerox Research and Technology (XRT) organization is to create options in the form of technology opportunities matched to markets, consistent with the strategic direction of the corporation. A second mission is to reduce the technical and market risk inherent in these new technology opportunities. The market risk is strongly linked to the

* We wish to acknowledge contributions of Mark Bernstein, Curt Fey, Herve Gallaire, Tim Jacobs, Tom Kavassalis, Rick Koehler, Andras Lakatos, Juris Pirvics, Gil Porter, and Filomena U.

technology through the customer requirements, which may be explicitly known, or stated as a working assumption in the early phases.

As technology development proceeds, eventually these requirements must be restated in technical terms in the form of a specification, with target performance goals and a specified product launch date. The process of refining the technology capabilities and the customer requirements, which eventually evolve into a specification, is iterative, as depicted in Figure 2.[72] We often use Quality Function Deployment (QFD), a powerful technique for evolving and refining the specification. The formalism of QFD emphasizes the intimate linkage between the technology characteristics and market requirements.[73]

Elements of risk

The importance of describing and managing the market and technical risks of emergent technologies has been emphasized in books dealing with management of technology, and techniques for risk quantification are discussed there.[74] Investments in research and technology have to be placed into a portfolio of risk, assessed in terms of markets and technology. As Figure 3 shows, one can distinguish four quadrants according to the degree of market and technology risk:

- *Evolutionary (existing markets, existing technology):* lowest risk, but possibly limited economic potential.

- *Leverage base (new markets, existing technology):* somewhat higher risk. For a global company, opportunities of this type tend to be geographical.

- *Discontinuities (existing markets, new technology):* somewhat higher risk. This case refers to technology substitution, a familiar situation.

- *Radical (new markets, new technology):* highest risk. If the market is large, this may offer the greatest opportunity.

Several examples illustrate these risk categories. The Xerox 8010 information system and 6085 professional workstation with ViewPoint icons and windowing software is an example of the *radical* quadrant. In 1981, this was a brand-new technology in an untried market. Competitive risk was low due to first-mover advantages, but intellectual property protection was weak. The market was not prepared to use the product, and no complementary industry existed. Customers had limited choices; nevertheless they could choose from three versions: network, remote, and stand-alone. The business plan was not clear. Xerox had the world's best computer scientists on the project, so the technical competency was high. But customer requirements were not well known, and product specifications were risky. Although several document-processing applications were offered, in hindsight, the "killer

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[72] Mark B. Myers, "Research and Change Management in Xerox," Richard S. Rosenbloom and William J. Spencer, eds., *Engines of Innovation: U.S. Industrial Research at the End of an Era* (Boston, Mass.: Harvard Business School Press, 1996), p. 142.
[73] Yoji Akao, Quality Function Deployment: Integrating Customer Requirements into Product Design (Cambridge, Mass.: Productivity Press, 1990) (English translation).
[74] Phillip A. Roussel, Kamal N. Saad, and Tamara J. Erickson, *Third Generation R&D: Managing the Link to Corporate Strategy* (Boston, Mass.: Harvard Business School Press, 1991), see chapter 5, "Evaluating Risks and Rewards," pp. 67 et seq.; Preston G. Smith and Donald G. Reinertsen, *Developing Products in Half the Time*, 2d ed. (New York: John Wiley & Sons, 1998), see chapter 12, "Managing Risk Proactively," pp. 221 et seq.; Michael E. McGrath, Michael T. Anthony, and Amram R. Shapiro, *Product Development, Success Through Product and Cycle-time Excellence* (Newton, Mass.: Butterworth-Heinemann Press, 1992).

application" turned out to be the Lotus 1-2-3 spreadsheet that went out with the IBM personal computer. Xerox itself became a major user of the 6085, with tens of thousands of units installed throughout the company, but the product had limited commercial success, and it was later abandoned.

Hewlett Packard's thermal ink-jet printing provides examples in two quadrants. Initially, HP launched this new technology into an existing market of pen plotters and dot-matrix printing: a technology displacement without high market risk. This fits in the *discontinuity* quadrant. After perfecting and refining the technology, HP moved into new markets of desktop printing and, more recently, into home photo-printing (examples of the *leveraged base* quadrant).

Xerox's Liveboard provides another example of the *radical* quadrant, with a new technology in a new market. Liveboard was a computationally active whiteboard with remote communications capabilities using Unix. This was launched into a new market before working out a sound business model, in the belief that a market "had to be out there." Product price was high, and opportunities to develop manufacturing economies of scale were limited. Eventually Microsoft Windows was substituted for Unix because customers wanted compatibility with existing systems, which took away some proprietary technology opportunities. Following a short exploratory market probe, the product was withdrawn.

Quantification of risk—An example

Risk comes in many forms, often difficult to enumerate, much less quantify. Our discussion is limited to the nature of technologies undertaken by Xerox: technologies that involve electromechanical systems, electronics, digital image processing, control systems, document management tools, and information management tools.

Our approach is to identify major contributors to technology and market risk. For each contributing element, an anchored scale is constructed with a score that provides an approximate measure of the probability of success. Six contributors to risk are identified: three each for technology risk and market risk. Components of technology risk include the risk of being able to resolve any remaining technical problems adequately, the risk of having available the necessary competencies and complementary technologies required for commercialization, and the risk of achieving the technical specifications necessary to meet customer expectations. Components of market risk include the risk of having value chain elements (such as engineering, manufacturing, marketing, distribution, and sales) available for delivery, the risk that the product will provide vectors of differentiation sufficient to distinguish it from competitive offerings, and the risk that the proposed business model will be successful in the market.

TECHNOLOGY RISK

Three types of technology risk are quantified in Table 1 and described below.

Technology Risk Elements			Probability of success (for each element)
Technical risk (P_1)	Availability of competencies & complementary technologies required to deliver the technology (P_2)	Specification achieveability (P_3)	
Incremental extension of existing in-house technology	Technology and advanced development competencies are available, complementary technologies exist	Modest extension of existing specifications & performance requirements	0.9
Incremental extension of existing outside technology	Technology competency not available. Advanced development competency and complementary technologies are available	Major extension of specifications/perform ance	0.7
New technology, feasibility demonstrated	Technology competency and complementary technologies available, advanced development competency is not	New specification in a new performance domain	0.5
New technology, feasibility not demonstrated	Technology or advanced development competencies are available elsewhere. Complementary technologies not available	Some specifications unknown or unknowable	0.3
New invention, not reduced to practice	Neither technology & advanced development competencies, nor complementary technologies are available anywhere	No specification known	0.1

Table 1. Technology Risk Quantification Model

Technical Risk. Technical risk refers to the set of technical problems associated with a new or emerging technology. The characterization of technical risk in physical systems (as opposed to software) has been discussed elsewhere; we summarize it here.[75] With a new or emerging technology, many types of "technology problems" will be encountered. "Technology problems" can arise from application of a new process, material, or subsystem before fully understanding the parameters that control performance, cost, safe operating latitudes, or

[75] George C. Hartmann and Andras I. Lakatos, "Assessing Technology Risk: A Case Study," *Journal of Research Technology Management*, May–April 1998, p. 32.

failure modes. They can occur if a previously commercialized technology is extended outside the known domains of the pertinent design rules. They can also occur from unexpected interactions arising from a new or unique combination of subsystems or components. An example is the requirement for much more precise motion quality when digital imaging subsystems are substituted into hardware that was previously based on analog technology.

Periodically during the technology development process, "technology reviews" should be conducted in which technology champions and a peer group of subject-matter experts participate. These reviews enable a list of anticipated or known technology problems to be generated and tracked over time. Each technology problem can be rated using a uniform method, such as the "technical risk" algorithm shown in Figure 4. This information can be aggregated to create a risk profile for the new technology that can be followed over time, and to position the new technology on the scale in Table 1.

As technologies move from the research bench to product development, there is an inherent tension between the technology champions and the product chief engineer. The technologist creates new concepts, new surprises, and new risks. He or she is optimistic, is successful if his or her ideas are adopted, and may overstate the merits. The chief engineer, on the other hand, tries to solve problems, avoid surprises, and minimize risk; he or she is successful if the product meets the specification on schedule, irrespective of the technology used. The technical risk approach outlined here is intended to provide a framework for managing this inherent tension, to help identify the risk as soon as possible so that appropriate measures can be taken. As Richard Feynman said during the investigation of the Challenger disaster, "for a successful technology, reality must take precedence over public relations, for Nature cannot be fooled."[76]

Availability of competencies and complementary technologies required. Development of a new technology may require new technical skills, tools, and processes, or may require access to skills and tools already committed to other technology and product development efforts. Complementary technologies may be required to work in concert with the new technology, but may not be ready or implemented. In some instances, a critical resource is the technical know-how necessary to integrate the new technology into an existing system. Systems integration and systems engineering skills are usually in high demand and often not available. If the critical skills must be acquired outside the corporation, for example through a development contract, appropriate interfaces and partnerships must be devised. If the required skills simply do not exist, they must be developed concurrently with technology and product development, which introduces additional risk. Table 1 provides a guide for judging these dimensions of risk.

Specification achieveability. As new technology moves toward product, performance must eventually be quantified and characterized in terms of the targeted product specification. What we are referring to here is not the risk that the target specification has been properly selected based on the customer need and market requirements, but the risk that the technology performance is insufficient to meet the target specification. Examples include the possibility of shortfalls in parameters related to quality, speed, reliability, and cost. These problems are difficult to nail down until the product specification and design intent have been identified. Moreover, the assessment of this risk factor is often entangled with the technical risk above, depending on the newness of the envisioned product concept.

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[76] Richard P. Feynman, *What Do You Care What Other People Think? Further Adventures of a Curious Character* (New York: W.W. Norton Company, 1989), Appendix F, "Personal Observations on the Reliability of the Shuttle," p. 237.

MARKET RISK

The market risk is separated into three factors, described below and listed in Table 2.

Market Risk Elements			Probability of success (for each element)
Availability of value chain elements (P_4)	Product vector of differentiation (P_5)	Market acceptance and business model (P_6)	
Value chain is available within the company	Product is best in class in all attributes	Company is currently in the market	0.9
Major elements of companies value chain must be developed	Product is best in some attributes, but not all	Company has contact with customers, but is not in the market	0.7
Company value chain is broken, many elements not available	Product offers advantages in one or two attributes	Company is active in a closely related market	0.5
No value chain elements exist within the company	Product has same profile as competitors	Market exists, but only as a ''niche'' market. Business model not established	0.3
Critical value chain elements do not exist anywhere	Product offers advantage in one or two attributes, but is worse in all others	Market and business model does not exist	0.1

Table 2. Market Risk Quantification Model

Availability of value chain elements. Market success of a new technology requires many things to fall in place, in addition to the technology. For example, the corporation needs to have the engagement of product engineering, manufacturing, marketing, distribution, and sales organizations. For a new technology, especially in a product offering in a new market, many of these elements may not be in place, or if they are, may not be prepared to deal with the new product. Consequently this area represents a significant risk. Table 2 offers a guide for this dimension of risk.

Product vector of differentiation. New products may offer some compelling combination of product functions, features, or economics to differentiate them from existing products. Some of these product capabilities may be enabled by the new technology. There are several risks. For example, when the product specifications were created, product planning may have underestimated how rapidly competition would raise the bar, and in the worst case, the product would offer capability at launch less than competitive offerings. More likely, product planning would respond before product launch by modifying the target specification during product development. “Specification creep” can push the technology into difficult performance regimes, increasing the risk and/or delaying the schedule. In the meantime, the competition advances again. Another risk is that the customer does not perceive the performance or feature enabled by the technology as an advantage. More than one technologist has been disappointed when customers simply did not care about the marvels of the technology embedded in the product.

Market acceptance. In some instances, the product may flow into a market in which the corporation is active, and where it has a business model, understands the customers, competition, and market dynamics. Products introduced into new markets offer higher risk; for example, less may be known about the customers. In some instances, a business model may simply not exist, and no one has any idea of the potential size of the market. Table 2 suggests a method of quantifying this risk.

Application

All research and technology activities in Xerox Research & Technology were scored by subject domain experts, using the scales in Tables 1 and 2. Fifty-five technologies were scored. The scores were consolidated using:

$$\text{Technology risk} = 1 - (P_1 \exists\ P_2 \exists P_3)$$

$$\text{Market risk} = 1 - (P_4 \exists P_5 \exists P_6)$$

The consolidated scores are displayed in Figure 5, in which the bubble area is proportional to the investment. A corresponding chart, not shown here, can be made in which the bubble area is proportional to the estimated market value of each technology project.

The overall probability of success for each technology project can be estimated:

$$\text{Overall probability of success} = P_1 \exists P_2 \exists P_3 \exists P_4 \exists P_5 \exists P_6$$

This information can be summarized as shown in Figure 6, which illustrates the cumulative investment plotted against the overall probability of success. This plot shows the overall risk profile of the research and technology investment. Projects with the smallest investments tend to have small probabilities of success, and vice versa, illustrated by the density of points on different regions of the curve. Once again, a corresponding chart can be made in which the cumulative market value of each project is plotted as a function of the overall probability of success.

The four charts just described (two of which are shown in Figs. 5 and 6) provide information useful for understanding the risks and potential rewards of the research and technology investment stream.

This information and the techniques described by Roussel, Saad, and Erickson can be used to help manage the risk.[77] The technology and product decision makers must work together continuously to drive the risk down, and track progress in risk reduction over time.

[77] Roussel, Saad, and Erickson, *Third Generation R&D.*

Concluding remarks

As others have pointed out, there are inherent difficulties with risk analysis. Admittedly it is impossible to know if all the risks have been identified, or whether an adequate measure of each identified risk has been constructed. Another aspect is that management can become too comfortable and forget the "real" risk, which includes things as yet unknown. We advocate a balance between a purely analytic approach and an intuitive one, and endorse an approach that explicitly deals with the risk arising from the interplay between technology and market.

Figure 1. **Product development and time-to-market process.**

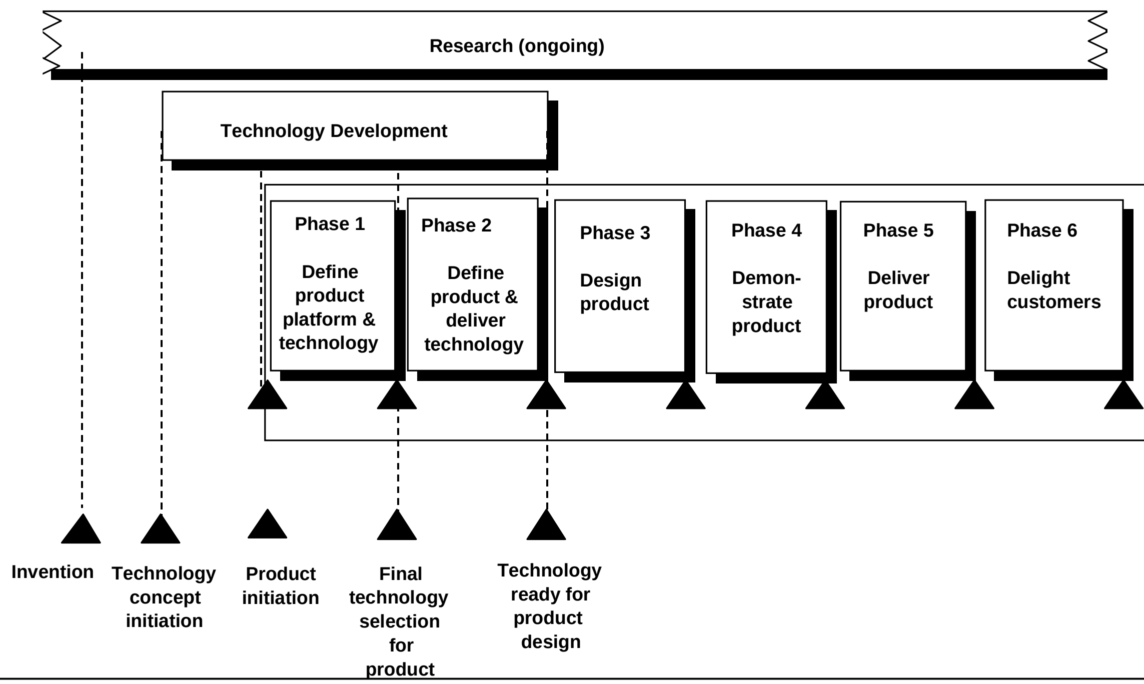


Figure 2. Iterative innovation for creation of new business value.

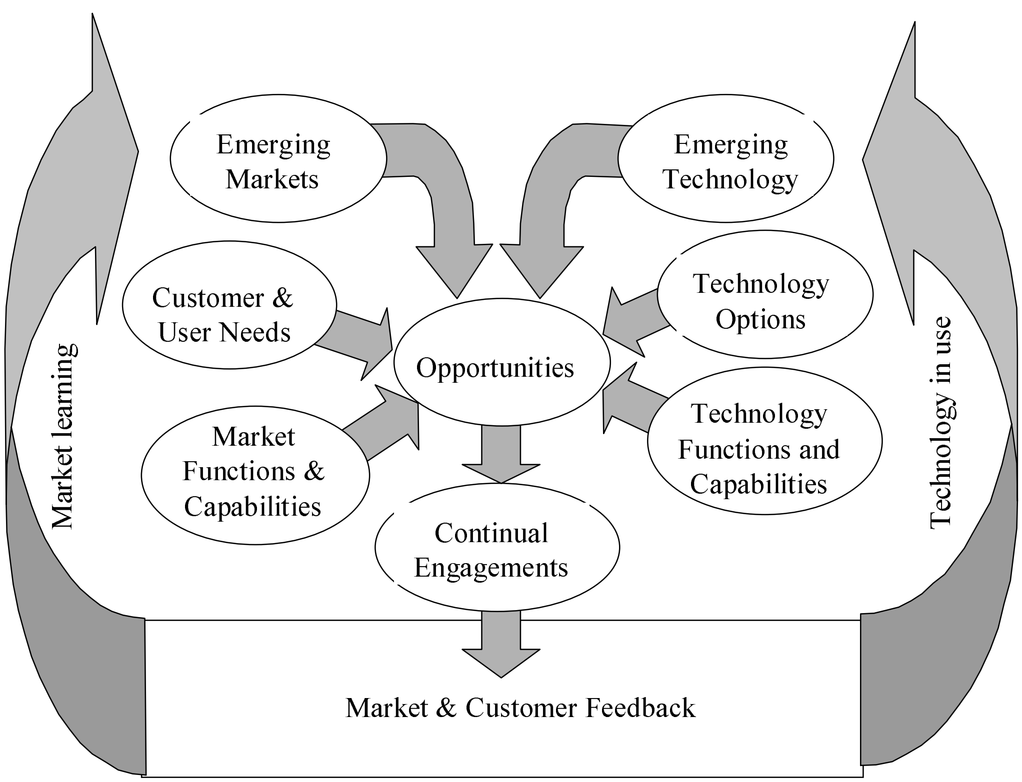


Figure 3. **Quadrants of Risk. The solid lines represent constant overall risk.**

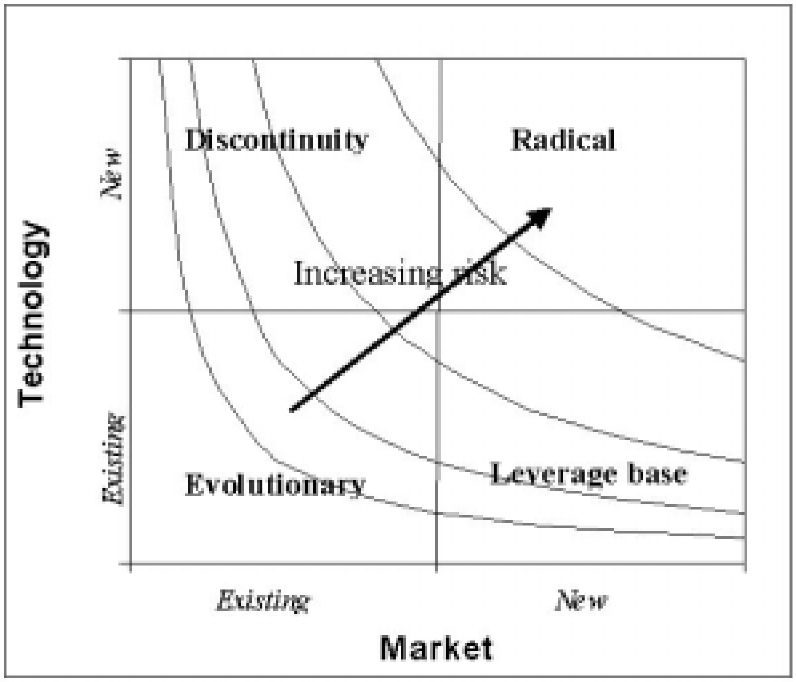


Figure 4. **Algorithm for assigning technical risk in physical systems.**

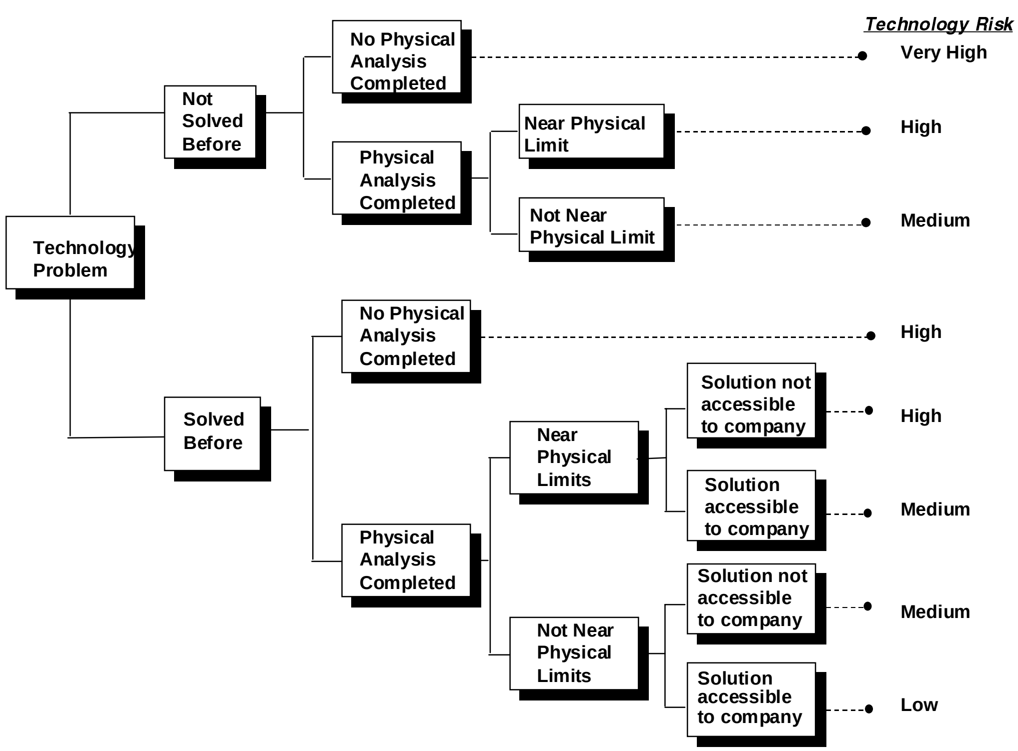


Figure 5. Consolidated risk profile of research and technology projects.

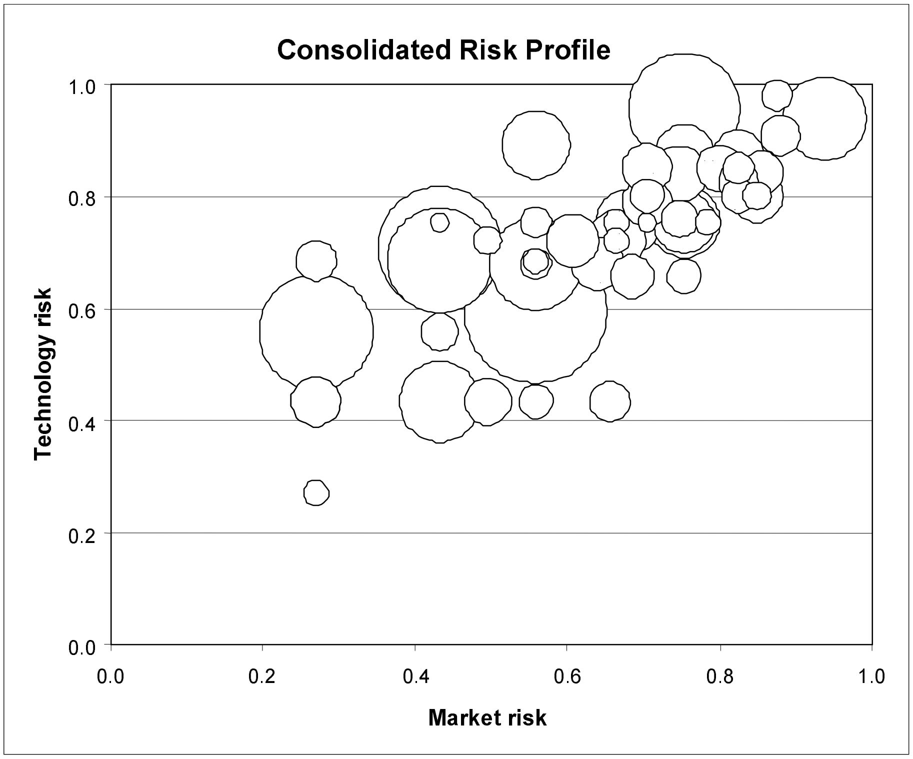
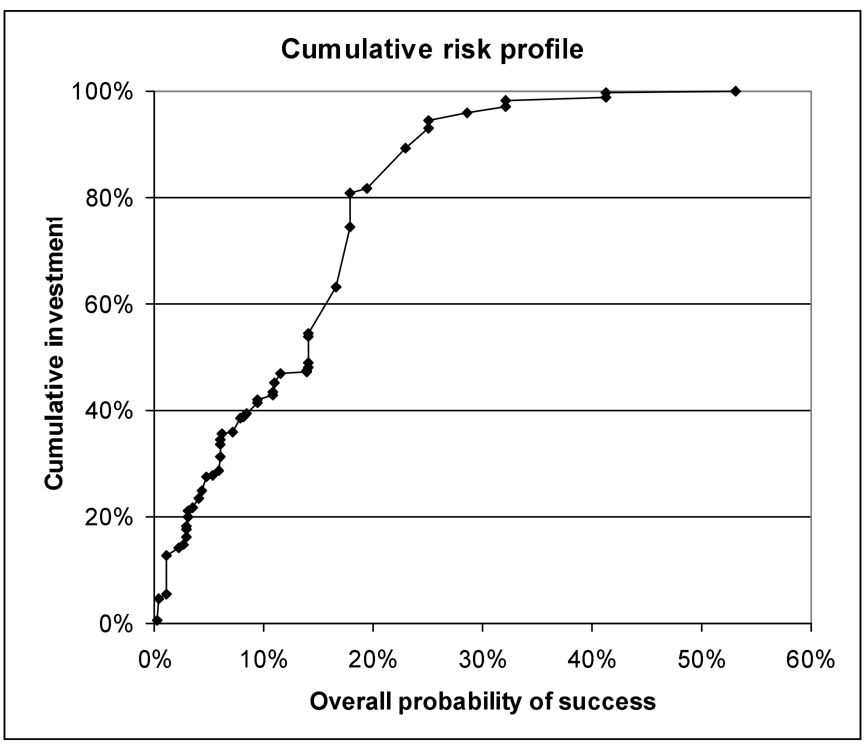


Figure 6. Cumulative risk profile for research and technology projects.



Effect of Technical Elements of Business Risk on Decision Making

E. L. Jarrett

Larry Jarrett is a Director of the Industrial Research Institute and, until recently, chaired its "Research on Research" Committee, which has about a dozen active research projects studying the innovation and portfolio management processes. Over the years, he has helped to build decision processes for several new business development activities. Dr. Jarrett holds a B.S. in Chemical Engineering from West Virginia University and a Ph.D. from Ohio State; he serves on departmental advisory boards at both universities. He has held a wide variety of R&D positions during his career with Union Carbide Corporation, OSi Specialties, Inc., and Witco Corporation. Over the years, his research teams have received key awards for innovation, including the R&D 100 award from Research and Development magazine and the Kirkpatrick Award from Chemical Engineering magazine.

The acceptance of risk for the potential of reward is at the heart of the entrepreneurial free-market system. Risk is an essential component of business decisions. Risk arises because it impossible to predict outcomes: uncertainty is always with us. Risk is the potential for adverse impact of areas of uncertainty on a decision or action path. It is reflected in the "efficient frontier" of the apparent risk versus yield for the usual range of investments—from low-risk government bonds to equity stocks to high-risk unsecured investments. Risk offers the opportunity for exceptional rewards for exceptional wisdom in understanding and characterizing uncertainty, and exceptional creativity inaccommodating, overcoming, or mitigating potential adversity. Elements of risk range from the trivial (there will be no material effect no matter how the area of uncertainty pans out) to profound (the future of the enterprise depends entirely on what happens in the area of uncertainty).

Technical risk is often among the most profound of risks, because technical failure is a "show-stopper." It is impossible to sell a new product, or to implement a new manufacturing process, if key technical components fail.

One cannot talk about risk without thinking about project failure and developing an appropriate attitude toward it. Project failure shows that real risk has been accepted, for the chance at extraordinary returns. Project failure has a portfolio role, just as bad stocks do. One can stop losses, and shift resources: one need not ride a failure all the way down. Failures that are clipped early should be celebrated: if there is technical advancement, there is always some potential for future alternative application.

Obviously success is better than failure, and overcoming adversity to achieve success is good. But trying too long and too hard is not good. A taste of the serenity prayer is useful: "let me change the things I can, accept the things I can't, and have the wisdom to know the difference." How do decision makers in firms go about "knowing the difference"?

Risk characterization

The elements of technical risk are not easily characterized, since real technical risk involves a forecast of how science will pan out when real people conduct experimentation, interpret results, and apply them in real situations. The elements of technical risk are chaotic, in that they are dependent on people and environment, as well as the laws of science (some of which are known, and some of which are unknown at any point in time). And elements of technical risk are not independent of one another: actions to understand and mitigate risk are interrelated through the laws of science, patterns of rational processes, and the personalities of people involved. Risk can be characterized as a probability of success, but it is always a probability *given* a set of premises, an expected environment, and a pattern of response with a correlated expectation of success. Risk must always be defined with respect to the risk tolerance of the decision maker.

One way to deal with risk is to perform a purely subjective analysis, considering the sources of risk, the probability of occurrence (and the factors controlling that probability), the impact of occurrence, means of mitigation, and the likelihood of success in mitigation. A wise, experienced manager can then develop a “comfort factor” (with respect to the subjective risk tolerance of the manager or the organization) and place the riskiness of a potential project in its proper place within a portfolio of projects.

However, it is difficult to model risk quantitatively. Here, I describe two approaches to doing so, but as we will see, their apparent rigor is undercut by the many subjective decisions that necessarily go into these models.

ANCHORED SCALES

One rational (and very popular) approach is to use “anchored scales” to forecast probability of success. Elements of technical risk are scored as to their eventual beneficial or adverse impact on project success. To provide consistency across a portfolio of projects, the scales for these scores are “anchored” by describing levels of risk in terms as unambiguous as possible. The aggregate impact on the potential for success is derived by aggregating the various scores mathematically, most commonly by a linear combination, or by a weighted linear combination where the weighting indicates importance.

Often a two-part appraisal is performed: First, the probability of technical success is assessed. This is estimated by comparing the capabilities of the organization (and the assets to which it has access) to the challenge posed by the project, including the unknown and the chance for serendipity. Then, the probability of commercial success (given technical success) is assessed. This likewise compares the capabilities of the organization and the predicted merits of the project outcome with the needs and challenges related to commercialization: “If it works, can we sell it or use it?” The overall probability of success for a project is then approximated by the product of these two probabilities (although it isn’t perfectly so from a statistical standpoint, because there is a bit of correlation of the factors making up the two probabilities).

The aggregate score for Probability of Success can be related to a percentage probability by subjective or objective means: Subjectively, the score can be associated with a percentage scale developed by consensus of those experienced in the area; or objectively, the score can be correlated with a percentage scale by comparison of past project success rates.

The NewProd™ computer model developed at McMaster University and the PACE™ Complexity model developed by PRTM provide this kind of correlation with built-in databases.

PROBABILISTIC ANALYSIS

An alternative approach, which appears to offer even more rigor, is to model the whole process of research, development, and commercialization statistically. In this approach, the key elements of the whole work process, from scientific invention through development and commercial implementation, are related through a process map, which becomes a model system in which blocks represent key transformations, and connections between the blocks represent physical flows or knowledge flows. The output from a block is modelled as the probability distribution of outcomes as related to a set of inputs. For example, the probability distribution estimating the degree of achievement of a certain key technical objective may be related to resourcing and the availability of certain enabling technologies.

When the blocks are completed and connected (in a computer simulation) and the system is complete, Monte Carlo simulation may be computed by using the probability models for the system inputs. Enough runs can be completed to develop an overall probability distribution of outcomes, given these probability models for system inputs. The result is a forecast probability distribution of the degree of success that might be achieved.

The apparent rigor of the simulation approach, however, is subverted by the extreme subjectivity of all the probability distributions that must be constructed for the many blocks, and by the simplifications needed to construct such a process map in the first place. Such an effort might require many man-months, and it is not a popular or practical approach to business decision making. Undertaking the effort main shows that there is a lot of uncertainty when you look at all the details; there is no way to model all the decision contingencies; and one cannot model (other than in aggregate terms) how everything will respond to adversity and advancement.

Making real-world decisions in a risky environment

The executive is the agent (either an individual or a team) of the organization which deals with risk and makes decisions. Executives depend both on risk analysis and trust in those who provide the analysis. Trust is a “people thing"; personal knowledge on the part of an executive is good, but consensus and confidence within the team is even better.

Even if it were possible to develop complex representations of risk, such as those discussed above, accurately, it is difficult for the executive to deal with them. Instead, the executive is able to deal with a few scenarios and possible cases, and only with three general levels of conceptual risk associated with them:

- High Risk, where risk must be reduced before a project can become part of the thinking about the future. There is really not a lot of difference between a 5% probability of success and a 15% probability of success; it’s still a long shot.

- Medium Risk, where risk and mitigation must be well understood before a project can proceed: this is where decisions are difficult. There is really not a lot of difference between a 35% probability of success and a 60% probability of success; there must be a clear understanding of how to reduce risk so that the project can be depended on.

- Low Risk, where risk is not a significant factor in going ahead. There is really not a lot of difference between an 85% probability of success and 95% probability of success; the project is being depended on to produce results as much as any other real project (excluding “sure things” which have essentially no risk).

The most important thing in risk analysis is the process of analyzing risk, not the summary result. Aggregate scores or forecast probabilities of success are far less important than the development of a shared understanding of the elements of technical and business risk among the technical and management teams conducting the analysis. During the analysis, the organization can understand, and potentially improve, the capability of the organization and its individuals to respond to challenges and mitigate unexpected events. It is this understanding that forms the basis for decision by the executive, whether that is an individual or team.

Risk: Diversifying the project portfolio

One element of assessing and managing risk is its context within a firm's project portfolio. To deal with any significant risk (medium or high), there must be diversification—a portfolio whose components have a scale much smaller than the enterprise. Most firms cannot afford to “bet the farm” on a horse race, even if they think they know the jockeys and the horses.

In addition to diversification, other kinds of balance may be sought, to ensure continuity and health for the enterprise. These might include investing some money in lower-risk projects that provide a near certain return and viability for the enterprise, or investing some money in higher-risk projects, which is the proven path to “really big hits” providing extraordinary return.

It is difficult to achieve such diversification and balance in a small firm. The risk can be pooled through alliances and consortia, or the risk can be “bought down” by inviting venture capital or other funding (with the costs and ties that it brings) into the firm.

Risk and funding

Does the source of funding make a difference in the executive’s view of risk? Within an enterprise, all sources of funding are equivalent—there is no such thing as “free” money. The enterprise owns all the resources.

However, to encourage certain behaviors and directions, enterprises establish funding policies in the form of rules and rewards for kinds of funds. Executive actions and specific decisions should represent a genuine attempt to comply with the intent of these policies.

Funding from outside the enterprise comes with its own set of incentives, costs, and rules. Accepting such funding really merges those into the funding policies of the enterprise. There still is no “free” money, even if it comes from the outside.

Summary

There are a few important messages about risk.

- Risk is at the heart of value creation.
- Project failure demonstrates the acceptance of risk, and is inevitable in an aggressive portfolio.
- Risk can be characterized, or even modeled, but not with any real accuracy. Executives can really only deal with three general levels of project risk: high, medium, low.

- Risk analysis is an important process which builds understanding of issues and appropriate confidence in dealing with them. It is essential to decision making. The benefit comes from doing the analysis, not from any few aggregate metrics which might result.

- Funding policy within an enterprise is just a means of incenting behavior and direction; there is no "free" money, even if it comes from outside the enterprise.

When Bureaucrats Meet Entrepreneurs: The Design of Effective "Public Venture Capital" Programs[*]

Josh Lerner

Josh Lerner is a Professor of Business Administration at Harvard University's Graduate School of Business Administration and a Research Associate at the National Bureau of Economic Research.

Abstract

Within the past few years, public efforts to finance small high-technology firms have proliferated. This article reviews the motivations for these efforts and makes some preliminary observations about their design. It explores the underlying challenges that the financing of young growth firms poses, the ways that specialized financial intermediaries address them, and the rationales for public efforts to finance these companies. The final section makes a set of observations about the ways in which the structure of these efforts can most effectively complement private sector activity. In particular, I highlight that a frequent fault in program design is the presumption that technological criteria can be divorced from business considerations when evaluating firms.

The federal government has played an active role in financing new firms, particularly in high-technology industries, since the Soviet Union's launch of the Sputnik satellite. In recent years, European and Asian nations and many U.S. states have adopted similar initiatives. While these programs' precise structures have differed, the efforts have been predicated on two shared assumptions: *(i)* that the private sector provides insufficient capital to new firms, and *(ii)* that the government either can identify investments which will ultimately yield high social and/or private returns or can encourage financial intermediaries to do so.[78] In contrast to many forms of government intervention designed to boost economic growth, such as privatization programs, these claims have received little scrutiny by economists.

The neglect of these questions is unfortunate. While the sums of money involved are modest relative to public expenditures on defense procurement or retiree benefits, these programs are very substantial when compared to contemporaneous private investments in new firms. Several examples, documented in Gompers and Lerner (1997), underscore this point:

- The Small Business Investment Company (SBIC) program led to the provision of more than $3 billion to young firms between 1958 and 1969, more than three times the total private venture capital investment during these years (Noone and Rubel [1970]).

[*] This is based in part on conversations with Zoltan Acs, Lewis Branscomb, Ken Flamm, Paul Gompers, Adam Jaffe, Bill Sahlman, Greg Udell, and Chuck Wessner. Helpful comments were provided by participants in the September 1999 workshop on "Managing Technical Risk" at the Harvard University's Kennedy School of Government. Parts of this article are adapted from Lerner (1998), Lerner (1999), and Gompers and Lerner (1997). Financial support was provided by Harvard Business School's Division of Research. All errors are my own.

[78] It is striking to note the similar emphasis on these rationales in, for instance, the statement of Senator John Sparkman (1958) upon the passage of the Small Business Investment Act and the recent testimony of Dr. Mary Good, Under Secretary for Technology at the U.S. Department of Commerce (1995). The rationales for such programs are discussed in depth in U.S. Congressional Budget Office (1985).

- In 1995, the sum of the equity financing provided through and guaranteed by federal and state small business financing programs was $2.4 billion, more than 60% of the amount disbursed by traditional venture funds in that year. Perhaps more significantly, the bulk of the public funds went to early-stage firms (e.g., those not yet shipping products), which in the past decade had accounted for only about 30% of the disbursements by independent venture capital funds (Venture Economics [1996]).

- Some of America's most dynamic technology companies received support through the SBIC and Small Business Innovation Research (SBIR) programs while they were still privately held entities; these include Apple Computer, Chiron, Compaq, and Intel.

- Public venture capital programs have also had a significant impact overseas: e.g., Germany has created about 800 federal and state government financing programs for new firms over the past two decades, which provide the bulk of the financing for technology-intensive start-ups (Organization for Economic Cooperation and Development [1995]).

Government programs in this arena have been divided between those efforts that directly fund entrepreneurial firms and those that encourage or subsidize the development of outside investors.

While these efforts have proliferated, a consensus as to how to structure these programs remains elusive. While the design of regulatory agencies has been extensively studied from a theoretical and empirical perspective, little work has been done on how to structure these programs—which may be referred to as public venture capital programs[79]—to insure their greatest effectiveness and to avoid political distortions. As we discuss below, a number of these programs appear predicated on a premise that is at odds with what we know about the financing process: that technologies in entrepreneurial firms can be evaluated without considering the business prospects of the firm.

This article provides an overview of the motivations for these efforts, as well as a brief consideration of design questions. In Section 2, the underlying challenges that the financing of young growth firms poses are discussed, as well as the ways that specialized financial intermediaries address them. The rationales for public programs are explored in Section 3. Section 4 concludes the paper and raises a set of issues about the design of these efforts.

1. Venture capitalists and the financing challenge

The initial reaction of a financial economist to the argument that the government needs to invest in growth firms is likely to be skepticism. A lengthy literature has highlighted the role of financial intermediaries in alleviating moral hazard and information asymmetries. Young high-technology firms are often characterized by considerable uncertainty and information asymmetries, which permit opportunistic behavior by entrepreneurs. Why one would want to encourage public officials instead of specialized financial intermediaries (venture capital organizations) as a source of capital in this setting is not immediately obvious.

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[79] The phrase "public venture capital programs" is used to refer to programs that make equity or equity-like investments (e.g., without a fixed repayment schedule, as seen in debt contracts) in young firms, or encourage other intermediaries to make such investments. In some such programs, such as the Advanced Technology Program and the Small Business Innovation Research programs discussed below, the funds are provided as a contract or outright grant.

A. THE CHALLENGE OF FINANCING YOUNG HIGH-TECHNOLOGY FIRMS

Jensen and Meckling (1976) demonstrate that agency conflicts between managers and investors can affect the willingness of both debt and equity holders to provide capital. If the firm raises equity from outside investors, the manager has an incentive to engage in wasteful expenditures (e.g., lavish offices) because he or she does not bear their entire cost. Similarly, if the firm raises debt, the manager may increase risk to undesirable levels. Because providers of capital recognize these problems, outside investors demand a higher rate of return than would be the case if the funds were internally generated.

Even if the manager is motivated to maximize shareholder value, informational asymmetries may make raising external capital more expensive or even preclude it entirely. Myers and Majluf (1984) and Greenwald, Stiglitz, and Weiss (1984) demonstrate that equity offerings of firms may be associated with a “lemons” problem (first identified by Akerlof [1970]). If the managers are better informed about the investment opportunities of their firms than the investors and act in the interest of their current shareholders, then such managers will issue new shares only when the company’s stock is overvalued. Indeed, numerous studies have documented that stock prices decline upon the announcement of equity issues, largely because of the negative signal sent to the market.

These information problems have also been shown to exist in debt markets. Stiglitz and Weiss (1981) show that if banks find it difficult to discriminate among companies, raising interest rates can have perverse selection effects. In particular, the high interest rates discourage all but the highest-risk borrowers, so the quality of the loan pool declines markedly. To address this problem, banks may restrict the amount of lending rather than increasing interest rates.

These problems in the debt and equity markets are a consequence of the information gaps between entrepreneurs and investors. If the information asymmetries could be eliminated, financing constraints would disappear. Financial economists argue that specialized financial intermediaries can address these problems. By intensively scrutinizing firms before providing capital, and monitoring them afterwards, they can alleviate some of the information gaps and reduce capital constraints.

B. RESPONSES BY VENTURE CAPITALISTS

The financial intermediary that specializes in funding young high-technology firms is the venture capital organization. The first modern venture capital firm, American Research and Development (ARD), was formed in 1946 by MIT President Karl Compton, Harvard Business School Professor Georges F. Doriot, and local business leaders. A small group of venture capitalists made high-risk investments in emerging companies that were formed to commercialize technology developed for World War II. The success of the investments ranged widely: almost half of ARD's profits during its 26-year existence as an independent entity came from its $70,000 investment in Digital Equipment Company (DEC) in 1957, which ultimately grew in value to $355 million. Because institutional investors were reluctant to invest, ARD was structured as a publicly traded closed-end fund and marketed mostly to individuals (Liles [1977]). The few other venture organizations begun in the decade after ARD's formation were also structured as closed-end funds.

The first venture capital limited partnership, Draper, Gaither, and Anderson, was formed in 1958. Imitators soon followed, but limited partnerships accounted for a minority of the venture pool during the 1960s and 1970s. Most venture organizations raised money either through closed-end funds or small business investment companies (SBICs), federally guaranteed risk capital pools that proliferated during the 1960s. While investor demand for

SBICs in the late 1960s and early 1970s was strong, incentive problems ultimately led to the collapse of the sector.[80] The annual flow of money into venture capital during its first three decades never exceeded a few hundred million dollars and usually was substantially less.

Activity in the venture industry increased dramatically in late 1970s and early 1980s. Industry observers attributed much of the shift to the U.S. Department of Labor's clarification of ERISA's "prudent man" rule in 1979. Prior to that year, the Employee Retirement Income Security Act (ERISA) restrained pension funds from investing substantial amounts of money in venture capital or other high-risk asset classes. The Department of Labor's clarification of the rule explicitly allowed pension managers to invest in high-risk assets, including venture capital under specified constraints. In 1978, when $424 million was invested in new venture capital funds, individuals accounted for the largest share (32 percent). Pension funds supplied just 15 percent. Eight years later, when more than $4 billion was raised, pension funds accounted for more than half of all contributions. (These annual commitments represent pledges of capital to venture funds raised in a given year. This money is typically invested over three to five years starting in the year the fund is formed.)

The subsequent years saw both good and trying times for venture capitalists. On the one hand, during the 1980s and 1990s venture capitalists have backed many of the most successful high-technology companies, including Apple Computer, Cisco Systems, Genentech, Netscape, and Sun Microsystems. A substantial number of service firms (including Staples, Starbucks, and TCBY) have also received venture financing. At the same time, commitments to the venture capital industry were very uneven. The annual flow of money into venture funds increased by a factor of ten during the early 1980s, peaking at just under six billion 1996 dollars. From 1987 through 1991, however, fund-raising steadily declined. Over the past decade, the pattern has been reversed; 1998 represented a record fund-raising year, in which $25 billion was raised by venture capitalists. This process of rapid growth and decline has created a great deal of instability in the industry.

To address the information problems that discourage other investors in small high-technology firms, the partners at venture capital organizations employ a variety of mechanisms. First, business plans are intensively scrutinized: of those firms that submit business plans to venture capital organizations, historically only 1% have been funded (Fenn, Liang, and Prowse [1995]).

In evaluating a high-technology company, the venture capitalists employ several criteria. To be sure, the promise of the firm's technology is important. But this evaluation is inexorably linked with the evaluation of the firm's management. Venture capitalists are well aware that many promising technologies do not ultimately fill market needs. As a result, most place the greatest emphasize on the experience and flexibility of the management team and the size of the potential market. Even if the business does not evolve as predicted, a firm with a sophisticated team may be able to find an attractive opportunity. The decision to invest is frequently made conditional on the identification of a syndication partner who agrees that this is an attractive investment (Lerner [1994]). In exchange for their capital, the venture capital investors demand preferred stock with numerous restrictive covenants, and representation on the board of directors.

[80] In particular, many SBICs made investments in ineffective or corrupt firms. Observers noted that SBIC managers' incentives to screen or monitor portfolio firms was greatly reduced by the presence of government guarantees that limited their exposure to unsuccessful investments.

Once the decision to invest is made, the venture capitalists frequently disburse funds in stages. Managers of these venture-backed firms are forced to return repeatedly to their financiers for additional capital so that the latter have an opportunity to ensure that the money is not squandered on unprofitable projects. In addition, venture capitalists intensively monitor managers, often contacting firms on a daily basis and holding monthly board meetings during which extensive reviews of every aspect of the firm are conducted. (Various aspects of the oversight role played by venture capitalists are documented in Gompers and Lerner [1999].)

It is important to note that, even with these many mechanisms, the most likely primary outcome of a venture-backed investment is failure, or at best modest success. Gompers (1995) documents that out of a sample of 794 venture capital investments made over three decades, only 22.5% ultimately succeeded in going public, the avenue through which venture capitalists typically exit their successful investments. (A Venture Economics study [1988] finds that a $1 investment in a firm that goes public provides an average cash return to venture capitalists of $1.95 in excess of the initial investment, with an average holding period of 4.2 years. The next best alternative, a similar investment in an acquired firm, yields a cash return of only 40 cents over a 3.7-year mean holding period.) Similar results emerge from Huntsman and Hoban's (1980) analysis of the returns from 110 investments by three venture capital organizations. About one in six investments was a complete loss, while 45% were either losses or simply broke even. The elimination of the top-performing 9% of the investments was sufficient to turn a 19% gross rate of return into a negative return.

In short, the environment in which venture organizations operate is extremely difficult. Difficult conditions that have frequently deterred or defeated traditional investors such as banks can be addressed by the mechanisms that are bundled with the venture capitalists' funds. These tools have led to the emergence of venture capital organizations as the dominant form of equity financing for privately held technology-intensive businesses.[81]

2. Rationales for public programs

There are reasons to believe that despite the presence of venture capital funds, there still might be a role for public venture capital programs. In this section, I assess these claims. I highlight two arguments: that public venture capital programs may play an important role by certifying firms to outside investors, and that these programs may encourage technological spillovers.

A. THE CERTIFICATION HYPOTHESIS

A growing body of empirical research suggests that new firms, especially technology-intensive ones, may receive insufficient capital to fund all positive net present value projects due to the information problems discussed in the previous section.[82] If public venture capital awards could certify that firms are of high quality, these information problems could be overcome and investors could confidently invest in these firms.

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[81] While evidence is imprecise, Freear and Wetzel's [1990] survey suggests that venture capital accounts for about two-thirds of the external equity financing raised by privately held technology-intensive businesses from private-sector sources.

[82] The literature on capital constraints (reviewed in Hubbard [1998]) documents that an inability to obtain external financing limits many forms of business investment. Particularly relevant are works by Hall (1992), Hao and Jaffe (1993), and Himmelberg and Petersen (1994). These show that capital constraints appear to limit research-and-development expenditures, especially in smaller firms.

As discussed above, venture capitalists specialize in financing these types of firms. They address these information problems through a variety of mechanisms. Many of the studies that document capital-raising problems examine firms during the 1970s and early 1980s, when the venture capital pool was relatively modest in size. Since the pool of venture capital funds has grown dramatically in recent years (Gompers and Lerner [1998]), even if small high-technology firms had numerous value-creating projects that they could not finance in the past, one might argue that it is not clear this problem remains today.

A response to this argument emphasizes the limitations of the venture capital industry. Venture capitalists back only a tiny fraction of the technology-oriented businesses begun each year. In 1996, a record year for venture disbursements, 628 companies received venture financing for the first time (VentureOne [1997]); to put this in perspective, the Small Business Administration estimates that in recent years close to one million businesses have been started annually. Furthermore, these funds have been very concentrated: 49% of venture funding in 1996 went to companies based in either California or Massachusetts, and 82% went to firms specializing in information technology and the life sciences (VentureOne [1997]).

Several contradictory conclusions can be drawn from these funding patterns. Concentrating investments in such a manner may well be an appropriate response to the nature of opportunities. Consider, for instance, the geographic concentration of awards. Recent models of economic growth—building on earlier works by economic geographers—have emphasized powerful reasons why successful high-technology firms may be very concentrated. The literature highlights several factors that lead similar firms to cluster in particular regions, including knowledge spillovers, specialized labor markets, and the presence of critical intermediate goods producers.[83] Case studies of the development of high-technology regions (e.g., Saxenian [1994]) have emphasized the importance of such intermediaries as venture capitalists, lawyers, and accountants in facilitating this clustering.

A related argument for public investments is that the structure of venture investments may make them inappropriate for many young firms. Venture funds tend to make quite substantial investments, even in young firms; the mean venture investment in a start-up or early-stage business between 1961 and 1992 (expressed in 1996 dollars) was $2.0 million (Gompers [1995]). The substantial size of these investments may be partially a consequence of the demands of institutional investors. The typical venture organization raises a fund (structured as a limited partnership) every few years. Because investments in partnerships are often time-consuming to negotiate and monitor, institutions (limited partners) prefer making relatively large investments in venture funds, typically $10 million or more. Furthermore, governance and regulatory considerations lead institutions to limit the share of any fund that any one limited partner holds.[84] As a consequence, venture organizations typically raise substantial funds of $100 million or more. Because each firm in his or her portfolio must be closely scrutinized, the individual venture capitalist is typically responsible for no more than a dozen investments. Venture organizations are consequently unwilling to invest in very young firms that require only small capital infusions.[85]

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[83] The theoretical rationales for such effects are summarized in Krugman (1991).

[84] The structure of venture partnerships is discussed in detail in Gompers and Lerner (1999).

[85] There are two primary reasons that venture funds do not simply hire more partners if they raise additional capital. First, the supply of venture capitalists is quite inelastic. The effective oversight of young companies requires highly specialized skills that can only be developed with years of experience. A second important factor is the economics of venture partnerships. The typical venture fund receives a substantial share of its compensation from the annual fee, which is typically between 2% and 3% of the capital under management. This motivates venture organizations to increase the capital that each partner manages.

This problem may be increasing in severity with the growth of the venture industry, as discussed above. As the number of dollars per venture fund and dollars per venture partner have grown, so too has the size of venture investments. For instance, the mean financing round for a start-up firm has climbed (in 1996 dollars) from $1.6 million in 1991 to $3.2 million in 1996 (VentureOne [1997]).

Again, it is not clear how to interpret these financing patterns. Venture capitalists may have eschewed small investments because they were simply not profitable, either because of the high costs associated with these transactions or because of the poor prospects of the thinly capitalized firms.[86] If so, then encouraging public investments in small firms may be counter-productive and socially wasteful if the financial returns are unsatisfactory and the companies financed are not viable. Support for these claims is found in recent work on the long-run performance of initial public offerings (IPOs). Brav and Gompers (1997) show that IPOs that had previously received equity financing from venture capitalists outperform other offerings. These findings underscore concerns about policies which seek to encourage public investments in companies that are rejected by professional investors.

Furthermore, it appears that in 1997 there were a number of financial innovations to address the needs of early-stage entrepreneurs. These included the creation of incubators and "entrepreneur-in-residence" programs by established venture organizations such as Mayfield and Mohr Davidow. Other examples are innovative efforts to direct the resources of individual investors to small venture capital funds (an example is Next Generation Partners, a "fund-of-funds" for wealthy families developed by FLAG Venture Partners). Finally, some institutional investors are displaying an increased willingness to provide capital to first time and seed venture funds. Thus, market forces may be addressing whatever problem has existed.

B. THE PRESENCE OF R&D SPILLOVERS

A second rationale emerges from the literature on R&D spillovers. An extensive literature (reviewed in Griliches [1992] and Jaffe [1996]) has documented the presence of R&D spillovers. These spillovers take several forms. For instance, the rents associated with innovations may accrue to competitors who rapidly introduce imitations, developers of complementary products, or to the consumers of these products. Whatever the mechanism of such spillovers, however, the consequence is the same: the firm invests below the social optimum in R&D.

After reviewing a wide variety of studies, Griliches estimates that the gap between the private and social rate of return is substantial: the gap is probably equal to between 50% and 100% of the private rate of return. While few studies have examined how these gaps vary with firm characteristics, a number of case-based analyses (Jewkes [1958], Mansfield, *et al.* (1977]) suggest that spillover problems are particularly severe among small firms. These organizations may be particularly unlikely to defend their intellectual property positions effectively or to extract most of the rents in the product market.

Public finance theory emphasizes that subsidies are an appropriate response in the case of activities that generate positive externalities. Such investments as R&D expenditures and pollution control equipment purchases may have positive spillovers that help other firms or

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[86] For a theoretical discussion of why poorly capitalized firms are less likely to be successful, see Bolton and Scharfstein (1990).

society as a whole. Because the firms making the investments are unlikely to capture all the benefits, public subsidies may be appropriate.

Even if these problems are substantial, however, the government may not be able to address them appropriately. An extensive political economy and public finance literature has emphasized the possible distortion that may result from government subsidies as particular interest groups or politicians seek to direct subsidies in a manner that benefits themselves. As articulated by Olson (1965) and Stigler (1971), and formally modeled in works such as Peltzman (1976) and Becker (1983), the theory of regulatory capture suggests that direct and indirect subsidies will be captured by parties whose joint political activity such as lobbying is not too difficult to arrange (*i.e.*, when "free-riding" by coalition members is not too large a problem).

These distortions may manifest themselves in several ways. One possibility (discussed, for instance, in Eisinger [1988]), is that firms may seek transfer payments that directly increase their profits. Politicians may acquiesce in such transfers in the case of companies that are politically connected. A more subtle distortion is discussed by Cohen and Noll (1991) and Wallsten (1997): officials may seek to select firms based on their likely success, and fund them regardless of whether the government funds are needed. In this case, they can claim credit for the firms' ultimate success even if the marginal contribution of the public funds was very low.

The presence of these distortions is likely to vary with program design. In particular, one of the reasons that has been suggested for why the SBIR program is relatively effective (as documented in Lerner [1999]) is that the decision makers are highly dispersed. In particular, the federal program managers are scattered across many sub-agencies, and are responsible for many other tasks in addition to SBIR awards. Thus, the costs of identifying and influencing these decision makers is high. In programs where a central group makes highly visible awards, the dangers of political distortions are likely to be higher.

3. The challenge of program design

An immense literature in regulatory economics and industrial organization has considered the structure of regulatory bodies. The different ways in which regulators can monitor and shape industry behavior—and Congress can in turn monitor the regulators—has been explored in detail. (For an overview, see Laffont and Tirole [1993].)

Other areas of interactions between government officials and firms, however, have been much less well scrutinized. Not only is the theoretical foundation much less well developed, but the empirical literature is at a much earlier stage. (Klette, Moen, and Griliches [1999] provide an overview of the current state of empirical research.) Thus, our observations must be necessarily tentative in nature.

My colleague Paul Gompers and I recently (1997) looked at the design of efforts to assist high-technology entrepreneurs in one program, the Advanced Technology Program (ATP) run by the Department of Commerce. Between its inception in 1990 and 1997, the program awarded nearly one billion dollars in research and development funding to approximately 300 technology-based projects conducted by American companies and industry-led joint ventures. From 1990 to 1997, 36 percent of ATP funding went to small businesses, with an additional 10 percent going to joint ventures led by small businesses.

In particular, we asked how the public sector could interact with the venture community and other providers of capital to entrepreneurial firms in order to advance the innovation process most effectively. Reflecting the early state of knowledge and lack of a theoretical foundation, we did not analyze these challenging questions through a large-sample analysis. Rather, we relied on seven case studies of ATP firms, complemented by a review of the secondary literature and our own empirical and field-based study of other “public venture capital” programs.

As part of this analysis, we highlighted four key recommendations, which are likely to be more generally applicable to public venture capital programs. In this section, we will review each of these recommendations. I particularly highlight our final recommendation, which emphasizes the premise that technologies in entrepreneurial firms can be evaluated without considering the business prospects of the firm.

First, there is a strong need for public officials to invest in building relationships with and understanding the U.S. venture capital industry. Financing small entrepreneurial firms is exceedingly challenging. The venture capital industry employs a variety of important mechanisms to address these challenges, which empirical evidence suggests are quite effective. Because of the magnitude and success of venture capital financing, it is important that administrators view their actions in light of lessons learned by this type of financial institution.

A corollary to this first point is that public venture capital investments should be made with an eye to the narrow technological focus and uneven levels of venture capital investments. Venture investments tend to be very focused into a few areas of technology that are perceived to have particularly great potential for profit. Increases in venture fundraising—which are driven by factors such as shifts in capital gains tax rates—appear more likely to lead to more intense price competition for transactions within an existing set of technologies than to greater diversity in the types of companies funded. Administrators may wish to respond to these industries conditions by *(i)* focusing on technologies which are not currently popular among venture investors and *(ii)* providing follow-on capital to firms already funded by venture capitalists during periods when venture inflows are falling.

A third point is that federal officials must appreciate the need for flexibility that is central to the venture capital investment process. Venture capitalists make investments into young firms in settings with tremendous technological, product market, and management uncertainties. Rather than undertaking the (often impossible) task of addressing all the uncertainties in advance, they remain actively involved after the investment, using their contractually specified control rights to guide the firm in response to changing conditions. These changes—which often involve shifts in product market strategy and the management team—are an integral part of the investment process. In our case studies, it appeared that ATP administrators often appear to view these shifts as troubling indications that awardees are deviating from plan, rather than as a natural part of their evolution.

Fourth, just as the venture capital community carefully analyzes the track record of entrepreneurs it is considering funding, government officials should examine the track record of the firms receiving public venture awards. As it is now, public venture capital programs are often characterized by a considerable number of underachieving firms. (The presence of “SBIR mills” who have won large numbers of awards apparently by cultivating relationships with federal officials is a manifestation of this phenomenon in another federal program, as Lerner [1999] discusses.) In particular, certain company characteristics—attributes that may not be adequately considered in the selection process of these programs—appear to be highly correlated with a company’s ability to achieve its research and commercialization goals. These include the experience of the management team, the presence of a clear product market strategy, and a strong desire to seek private financing.

By devising new methods to search for such factors, government officials would be better able to distinguish between high-performing and underachieving firms.

Our research indicates that a prevalent characteristic among underachieving companies is the existence of research grants from numerous government sources, with few, if any, tangible results to show from previous R&D awards. Because a lack of results can easily be attributed to the high-risk nature of technology development, many of these companies can avoid accountability indefinitely. As a result, these government grant–oriented research organizations are able to drift from one federal contract to the next. For such companies, it appeared that public venture capital funds were treated in exactly the same manner as other government research grants: It did not appear that ATP funding showed any notable returns or that the unique program goals were particularly well-served.

Adding to the problem is the fact that companies with substantial government grant experience appear to have several advantages over other firms when applying for future public awards. Past grants, regardless of project outcomes, help a company gain legitimacy in a particular area of research, as well as acquire the equipment and personnel needed to do future work. There is also a tendency for some government programs to try to “piggyback” on other government programs, hoping to leverage their grant dollars. In addition, firms gain considerable insight on the grant application process with each proposal they submit. Because of all of these factors, these firms frequently have a greater chance of being awarded future government grants than other firms. The end result can be a stream of government funding being awarded to companies that consistently underachieve.

The problem of close relationships between applicants and government officials appeared to be an important issue in our case studies of the Advanced Technology Program. The companies in our sample indicated that after submitting multiple ATP proposals and completing an ATP project, they gained a significantly better understanding of how to appeal to the ATP’s unique selection criteria. In fact, one interviewee frequently advises first-time applicants on how to write and structure ATP proposals. In addition, past ATP award recipients may develop relationships with ATP evaluators and managers that at least indirectly aided in the selection process.

To level the playing field, our research suggests that public venture capital providers should more closely scrutinize the amount of funding a company has received from prior government sources. A greater number of underachieving firms could be weeded out if government officials conducted a more comprehensive evaluation of a company’s past performance and examined the tangible progress attributable to each government grant the firm has received.Large inflows of prior government funding without significant product development may indicate that a particular company is unlikely to generate significant commercialization of new technologies.

Another telltale characteristic of underachieving firms was the existence of factors outside the scope of the publicly funded projects that undermined their ability to successfully complete and later commercialize government-funded technology. Legal troubles, for instance, can divert substantial amounts of human and financial resources away from a company’s R&D projects. For early-stage firms, legal problems may even cause dramatic changes in the size and structure of the company. And when a firm is ready to commercialize its technology, the liability concerns associated with pending legal battles will often drastically impair the company’s ability to attract venture capital investment dollars.

The existence of resource-draining auxiliary research projects can also potentially undermine a company’s performance. One company in our sample, for instance, was involved in a project that was only distantly related to the company’s core (and ATP-funded) technology. Although the ATP grant was not used to fund this auxiliary project, it appeared that a substantial amount of the company's time, energy, and capital was diverted toward this tangential research. This, in turn, diluted the company's focus on its ATP-funded research project, and thus slowed down the development of its core technology.[87] The existence of unrelated R&D projects, especially for smaller companies, can cause a company's resources to be spread too thin.

For early-stage companies, additional limiting factors frequently involve managers who lack experience in running small companies. Although some of these managers may have accumulated business experience as consultants or as members of large organizations, the successful operation of early-stage companies can demand very different management skills. It thus comes as no surprise that when a venture capitalist sinks substantial funds into a company, it will often place its own hand-picked manager in charge—typically an individual who has already been successful in managing an early-stage company in a similar industry. Because many of the skills needed for managing start-up companies comes through experience, the existence of managers who do not have this background can significantly undermine a company's ability to carry out its commercialization plans.

In a broader context, each of these performance-undermining factors emphasizes the need for the government officials to evaluate critically whether a particular company is a viable vehicle for accomplishing its commercialization goals. This goes far beyond a simple assessment of the feasibility of a business plan. In fact, many of these potentially limiting factors will not even be discussed in a company's written proposal to the government. It is tempting, of course, to attribute the failures resulting from such factors to the high-risk nature of the technology. But to a large extent, companies exhibiting a high potential for underachievement could be more thoroughly weeded out by placing a greater emphasis on these factors during the selection process. The R&D project itself may be high-risk, but the risks of turning the technology into a product should be minimized. Regardless of how innovative or enabling a technology may be, or how well a business plan is constructed, if these undermining factors are substantial, a company will be hard pressed to overcome such roadblocks. In short, the claim that technological projects can be assessed in entrepreneurial firms without consideration of business issues appears to be profoundly mistaken.

A broader implication is that administrators of public venture capital programs must think carefully about the validity of the concept of "pre-commercial research" in an entrepreneurial setting. An extensive body of entrepreneurship research has highlighted the unpredictability of the entrepreneurial process. Very few entrepreneurs, whether in high- or low-technology settings, commercialize what they initially set to develop in their original time-frame. Rather, successful entrepreneurs gather signals from the marketplace in response to their initial efforts, and adjust their plans accordingly. Once they identify an opportunity, they move very rapidly to take advantage of it before major corporations can respond. Yet many federal agencies, leery of being seen as "picking winners," push entrepreneurs to devote public funds to purely pre-commercial research. While these actions may insulate them from criticism that they are engaging in "industrial policy," they may be seriously detrimental to the firm. These directions may lead to them ignoring an essential source of information: *i.e.*, feedback from customers. Even more detrimental have been instances where companies—having identified an attractive commercial opportunity—are afraid to pursue it rapidly, lest they jeopardize their public funds (which they are relying on as a key source of financing) on

[87] Part of the problem in this instance was the lack of corporate discipline. If a venture capital firm had invested in this company, it likely would have provided this discipline by closely monitoring the company, and limiting the company's R&D activities to areas that were directly related to its core technology.

the grounds that they are pursuing commercial research. While well-intentioned, such policies may have the perverse effect of punishing success. One potential change would be to allow firms that rapidly commercialize publicly funded projects to use the funds to pursue another project.

Much is still to be learned about the design of these programs. While the literature on the design of regulatory agencies and the problem of political distortions in subsidy programs has yet to consider public venture capital programs in much depth, one can be optimistic that this will be a topic of increasing interest to researchers. With the help of these theoretical insights—as well as the willingness of program administrators to encourage dispassionate analyses of their strengths and weaknesses—our ability to say more about the design of these programs should grow.

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Technical Risk and the Mid-Size Company

David L. Lewis

David L. Lewis is Vice President and General Manager of the Chemical Products Division at Lord Corporation in Cary, North Carolina.

Lord Corporation is a $400-million, privately-held, diversified company that designs, formulates, manufactures, and markets adhesives and coatings, and devices and systems to manage mechanical motion and control noise. Lord has three major operation divisions: Chemical Products Division, Mechanical Products Division, and Materials Division. The Corporation has facilities in seven states and ten countries and employs over 2000. World headquarters is in Cary, North Carolina.

The corporation emphasizes four core technologies: material science, electro-mechanical dynamic systems, chemical synthesis and polymerization; and surface science. It applies these technologies to develop, manufacture and market unique, high-quality products that bring high value to its customers in selected niche markets.

When viewing the overall concept of technical risk, our experience is there are three key stages or activities, each with its own unique aspects of technical risk and evaluation. The three areas of activity, which generally occur sequentially in time, are:

- Basic invention/concept: The classic light bulb or eureka moment when a basic scientific or engineering approach that appears to hold commercial promise is conceived and demonstrated.

- Achievement of market requirements: Reduction to practice in the laboratory of the invention which is shown to meet all the eventual customers' specific needs.

- Robust commercialization: Demonstrated capability to reproducibly manufacture commercial quantities at a cost capable of generating an acceptable profit/return.

In this paper I describe the nature of technical risk moving through each of these three stages of activity. Particularly in the first stage, that of basic invention or concept, I draw a distinction between projects prompted by "technology push," and those driven by "market pull." I illustrate these concepts with three cases from my company's experience: one "technology push" case in which risk, though present, was well understood; a second in which technical risk changed drastically with changing information during the market requirements phase, and a third "market pull" example that was cancelled in the invention phase. I conclude by pointing out that technical risk exists in all phases, not just the invention phase, and that the more that is known and understood about the total area, the higher the probability of correctly assessing and dealing with the specific issue of technical risk. This is especially true during the market requirements phase.

Basic invention/concept stage

The basic invention or basic concept stage is typically black-or-white for technical risk; risk is either very high or very low. I distinguish between situations of "technical push" and "market pull." Technical push is an invention looking for a need, where technical risk is low; market pull is an opportunity looking for a breakthrough invention, where technical risk is high to infinite. In a technical push case, a scientist or engineer has the concept and brings it out as a technical invention with a degree of certainty from preliminary experimentation. Now the question facing the company is how to move down the chain toward something that is really commercializable. Technical risk can be well summarized and primarily relates to issues in the later stages of achieving market requirements and robust commercialization.

In the market pull case, there is a requirement from a specific customer or market which, if it can be met, will have high potential in the identified market. In this situation, you go to the scientist or engineer and say, in effect, “crank up your innovation” and ask what concepts or ideas the scientist or engineer might have to solve this requirement. This is, obviously, a case where technical risk is very high and could in fact be considered un-quantifiable.

Achievement of market requirements

The stage of achieving market requirements comes after the invention; it is in essence a question of how well the invention can match up to the real requirements of the marketplace. The ability to understand, estimate, and manage technical risk at this stage depends greatly on how close the company is to the market in the everyday sense. In areas where a company is a major supplier or player, it understands the requirements; in some cases, it may understand the requirements even better than the customer. The ability to estimate and manage technical risk is very high in that scenario.

In other areas, however, especially a new market area, companies may find that they do not quite understand the real requirements. They may think they do, and they may have consulted various users or experts, but in such a setting it is almost a certainty that they will be surprised as they proceed to find out "what the market really wants.” The ability to estimate and to manage technical merit or technical risk in this instance is uncertain, as the target appears to be continually moving.

Robust commercialization

For most companies, especially of our size or larger, the stage of robust commercialization is their bread and butter. Having successfully arrived at this late stage of commercialization, you usually feel pretty comfortable that you can do it, or at least understand the technical risk in detail. Robust scale-up is something done every day and it is something our technologists have the best handle on. There may be milestones that create problems to be solved, but there is usually a good estimate of the level of difficulty and the probability of success. One risky area is where the process strays far from what the company normally does. As with the above example of moving into new markets, robust commercialization of technologies that are far afield from the company's normal experience and capability has a high potential for nasty surprises and should be viewed as higher risk.

Cases

I present three cases from my company's experience where technical risk was evaluated and projects initiated.

CASE A: TECHNOLOGY PUSH WITH UNDERSTOOD MARKET REQUIREMENTS AND CONTROLLED COMMERCIALIZATION

Case A is an example of "technology push" in the basic concept stage. A major project that evolved from an in-house technical invention, it involved the invention and commercialization of a major new product line of environmentally acceptable adhesives for bonding rubber to metal. The concept had been developed by one of our scientists and was a classic technical-push situation. The technology was in an area of high familiarity to the company: Lord has been the global leader in the technology for bonding rubber to metal for over forty years, and probably has a better knowledge of the market requirements and in-house testing than many of its customers. Thus, the company had a tremendous capability to understand technical risk of later phases from the outset. We knew that it would be the most difficult technical project the company had had in many, many years, and that it would have the highest expense of any project to date (the total project cost, including building a stand-alone plant, represented the largest investment the Chemical Products Division had made in its history).

Fortunately in this case the company also had the best handle on technical risk at each stage that it had ever had. Concept risk was minimal, as this was a case of technology push. We could go through steps such as in-house testing that allowed us to minimize the risk exposure for the Corporation at every level while verifying our ability to meet market requirements. Commercialization of both the production process and the product introductions were done via avenues in which the company had considerably experience.

This was a good example of what from the outside appeared to be a very high technical risk project, but one that could in fact be managed and controlled very well, due to the company's very strong technical knowledge base in all three phases. Unlike most technical push projects, which face major market requirement questions, our first-hand knowledge of market requirements reduced those issues significantly.

CASE B: MARKET PULL WITH AVAILABLE INVENTION, POORLY DEFINED MARKET REQUIREMENTS, AND CAPABLE COMMERCIALIZATION

Case B illustrates how the extent of a company's understanding of market requirements can have a major impact on technical risk. This is an example of a direct articulated need by a customer, in the general area of adhesives for auto assembly where Lord is currently a supplier. Specifically it was for an application that was both new to us and in some respects a major extension for our customer. What appeared to be a good technical invention was in place and we moved well down the path of specific product commercialization. Market requirements, however, soon became a major difficulty: the requirements were initially detailed by the customer but changed with time and understanding. Further final application testing was available only at the customer's location, and special tests were added during the protocol. We were thus vulnerable to surprises that came out of the customer's work, as testing went on and as the customer's understanding of requirements, and ours, evolved. Well into the project, a new test was put in place that our product could not pass. In previous instances, we had been able to modify our base technical approach to achieve success, but the new requirement was such that our base invention technology was now unsuitable for the application. It was a surprise to us, a curve ball that completely changed our original assessment of technical risk, because the market requirements were now different. It essentially put us back to square one, searching for a new technical innovation that could meet the new requirements. This is an example of a case where technical risk was considered and understood at project inception, but where technical risk changed drastically with changing understanding of market requirements.

CASE C: MARKET PULL WITH HIGH TECHNICAL RISK AT THE INVENTION PHASE

Case C relates to an effort to develop a breakthrough approach to commercial floor coatings. Unlike Case A, which was technology push, this is an example of market pull. This is a classic situation for a company that is an established player in a market and feels that they can have a very, very successful product if they can make a major breakthrough in one or two key technical areas. In this case, the "pot of gold" was large enough that we were willing to take a technical flyer on some ideas and essentially fund an applied research program without a truly identified technical solution at the onset of the project. Because of the large commercial potential, a significant effort was felt justifiable and was in fact mounted. We did not, in the end, develop the basic invention breakthrough we had hoped for, but that was part of the calculation: we were willing to spend a certain amount of money trying some unusual approaches to solve the problem. This is an example where technical risk was large, and in some respects unquantifiable, because of the unknowns involved in seeking out a new technical invention. That was balanced by what was perceived to be a low risk in both market requirements and commercialization if we were to get to those points.

It is important to note that, despite its initial lack of success, we have buried this experiment in a shallow grave. This is a familiar type of situation in the laboratory, and if one of our scientists comes up with a better idea further down the road, it's something we will resurrect.

Conclusion

A theme that flows through each of these cases is that managing and understanding the technical risk depends on how much you really know about the total enterprise, not just the technical aspects of the initial invention. The more truly knowledgeable you are about the technical market requirements and other downstream issues, the better you can assess and deal with the technical risks that occur in later phases. Technical risk does not exist in isolation, but rather in a close partnership with other aspects of the total project enterprise, and is highly influenced by in-house capability and experience to understand and deal with changes in those areas. There is no question that the ability to estimate and manage technical risk in the later phases (market requirements and robust commercialization) is highly dependent upon a correct and detailed understanding of the specific technical market requirements that will govern the final phases of commercialization.

Table 1

	Case A	**Case B**	**Case C**
Source of formal project	In-house technical invention.	Customer request.	Customer request/market need.
Requirements	Product and process use well known; experience established previously in-house. In-house understanding is equivalent to customer or better.	Partially known by customer but still developing; overall non-articulated requirements not well understood in-house. Customer understanding of conceptual need greater than in-house.	Reasonably well known in-house.
Testing—initial	Capability equivalent to customer.	Limited capability past general testing. Specific tests only at customer. Tests evolving	Base capability in-house.
Testing—Beta	Multiple potentials with well established relationships.	Single situation; relationship with customer good, but not deep.	Available through well-established customer relationships
Base chemistry	Groundbreaking base chemistry identification part of initial invention.	New to Lord but known chemistry.	Utilizing base chemistries known to Lord.
Formulation	Area well known to Lord. Many tricks of the trade transferable from previous experience.	New in specific but similar to other situations. Not demonstrated prior to project, but strong conceptual validity was available.	No strong preconceived concept for success. Create quantum leap without preconceived notion of how to do it.
Production	Major pieces of new capability needed.	Similar to on-going production processes; potential for drop in to current production scheme with minor changes.	
Use	Established and well understood.	In flux.	Known.
Size	Well understood.	Overall size understood	Overall size understood
Price	Situation well understood.	Dynamics between price and performance not well established.	Dynamics between price and performance not well established.
Result	Commercialized	Cancelled	"Shallow grave"

Raising Mice in the Elephants' Cage

James C. McGroddy

Jim McGroddy headed IBM's Research efforts from 1989 to the end of 1995. During his 31-year career at IBM, he drove significant change in the structure of relations between IBM's Research efforts and commercialization. In at least a few cases he was instrumental in his drive to create new commercial entities, the largest being DTI, a multi-billion dollar joint venture between Toshiba and IBM which is one of the world's leading suppliers of flat-panel displays. Since retiring from IBM in 1996, he has served in a number of pro-bono positions, chairing the NAS study on the effectiveness with which Defense uses IT in warfighting, chairing the Visiting Committee at NIST, and serving as Chairperson at Phelps Memorial Hospital Center in Sleepy Hollow NY. He serves as a boardmember or board chair on a number of corporate and academic boards, and advises a number of universities and governmental organizations.

The history of the last fifty or so years has provided numerous examples of industries in which opportunities opened up by major technological change have not been captured by the in-place major players, but rather are exploited by entirely new companies. This phenomenon, and the underlying causes, have been the subject of a number of studies and publications, prominent among which are Richard Foster's 1986 book *Innovation, the Attacker's Advantage*, and more recently, Clayton Christensen's *The Innovator's Dilemma*. The increasing pace of technological evolution only exacerbates the potential for successful companies to miss major opportunity for growth. The focus of this paper, which is based on many years of personal participation in and observation of the information technology industry, is hinted at by the title. The central thesis is that the success of large enterprises in their dominant businesses is based on a culture and set of processes which are ill adapted to dealing with rapid and radical change in technology and opportunity. As a result, these enterprises more often than not fail to capture a proportionate share of opportunity in new products and services in their industry sectors—opportunity which in many cases grows to dominant proportions. Success requires that these new opportunities—the mice—be nurtured in a radically different environment from that appropriate to the large base businesses, the elephants. There is more than ample evidence that mice are unlikely to survive and prosper when raised in the elephants' cage.

Growth and opportunity

The information technology industry, including its large component of communications, has for the past thirty years been a major driver of change and growth in the world economy, and is mid-stream in transforming at least the operational aspects of every institution in society. This growth of the information technology industry, in aggregate in the range of 15–20% per year, will continue for at least the next two decades, as the technology continues to surge forward in its capability, and the application and exploitation of these technology advances lag another five to ten years behind the raw technology advance. The improvement in the key underlying functional capabilities—processor power, memory chip capacity, disk storage density, communications data rates, and other closely related fundamental capabilities—will continue to advance at a rate of ten times each five years, a hundred times in ten years. The capability we have today is thus ten percent of what we will have in five years, one percent of what we will have ten years hence. This phenomenal growth is a near certainty, since precursors of these advances can be seen in research laboratories around the world. These large factors of improvement guarantee the creation of major new opportunities at every level of the information technology value chain, as well as "disruptive" change, in Christensen's terminology.

If history is a faithful guide, much of this new opportunity measured in revenue terms will be captured by today's major players, albeit with a very nonuniform distribution among the players. But a major portion of the revenue growth, perhaps half of the industry growth over a five-year period (during which the total will likely more than double), will be captured by newly emerging, previously unrecognized players, offering new products and services, building on new business models. More striking, these emergent players are likely to capture an even larger portion of the newly created market valuation. A look backward over any recent five-year period will confirm the plausibility of this view. The most recent five-year period has been dominated by the explosion of the internet and electronic commerce, both between businesses and other businesses, and between businesses and consumers, and the beginning of the major wave of pervasive personal use. Major new players and products have achieved dominant positions, including Cisco, Netscape, Amazon, America On Line, and the Palm organizer, to name but a few. In earlier eras one would have pointed to the growth of the workstation and the PC, with the consequent spawning and explosive growth of Microsoft, Intel, Apple, Sun, Compaq, Dell, and others. Even earlier one would point to the rise of the minicomputer and to Digital, Wang, Prime, and Data General as the leaders. Many of those key players are now either absorbed by others or otherwise greatly diminished. And it is important to note that in most cases the ultimate dominators of the newly emergent segments developed that dominance when the segment was tiny, early on in its development. Later entrants, usually larger industry players, often struggled without success for years in attempts to displace the early leader.

A key issue for today's successful companies is how to capture a larger portion of this new opportunity, opportunity which is barely visible at the beginning of a five-year period, often unnamed at that point, included if at all under "other" in market segmentations. The rate and magnitude of revenue growth in these new opportunity segments are such that without significant participation in them, large players, particularly those without a very defensible dominance of some key sector, will tend to fail by a large margin to grow at the pace of their industry. In an industry sense, they will lose market share. They will miss enormous opportunity to create value for their shareholders. Their failure to deal with the changed opportunity will, as history shows, lead to major business failures by more than a few firms.

Over a fairly long career in the information technology industry, I have watched many companies deal with these challenges, and I have had the opportunity to test the strengths and weaknesses of various approaches and to discuss them with a number of industry leaders. These experiences has led me to formulate a set of principles that are useful for thinking about this hard problem and developing guidelines and business processes to increase success.

Why this is a hard problem: Chess players at the poker table

Large companies—their cultures and their processes—are organized to succeed in doing what they do well: managing and growing large businesses, usually with a well-defined set of customers. In my parlance, they are very good at raising elephants. The processes used to do this are thoughtful and deliberate, rational, analytical, and quantitative. And because of the relative continuity of most large business sectors, the processes are designed to look ahead a number of years and develop plans that have a high degree of certainty of execution. This is a chess style of management. A good chess player does not make a move before understanding the likely sequence of the next ten or more moves. Analysis plays a large role and uncertainty is minimized. The experience with IBM's chess-playing computer, Deep Blue, demonstrates the degree to which this analysis can be codified and systematized.

What later proves to be major new opportunity is rarely wrapped in mystery or secrecy in its early stages; rather it is usually visible to all. However, the ultimate winners are not definitively labeled as such: they are mixed in a much larger pool of what will ultimately prove to be losers. As pointed out above, these winners usually develop a dominant position very early in the evolution of a new segment. What is very clear is that they do not do this with a chess-like set of processes; rather, they manage in a style which is much more akin to poker.

One cannot play poker without being comfortable with placing bets in situations with large uncertainty. The pace is fast, and one cannot take time out, or hire consultants, to get accurate estimates of the two cards yet to be dealt, nor can one learn much about the hands of competitors. The dealer assumes that the hesitant player has dropped out for that hand, and deals right by him. This willingness to place (small) bets in highly uncertain conditions, using intuition more than analysis and trusting one's own judgment, is an essential element of developing a strong early position in new areas of opportunity.

One of the major difficulties for the large successful company is the unwillingness of most chess players to play poker and their total discomfort with every aspect of the game. The chess processes, which have been proven to be effective in the main part of the business, prevent the person at the poker table from putting up any chips at the pace the hands are played. In my view, a large information technology company that wants to participate fully in the growth of the industry must, unless it dominates some major rapidly growing sector, recognize the need to implement a separate, poker-like, set of processes for capturing new opportunity either from an internal base, or partnering and investing in emerging external companies. The chess process will almost always come to "no," and even that will be at a slow pace.

There are many other reasons why emerging opportunity is not pursued in large successful companies, but in many cases they are results of dealing with potential opportunity by using chess processes. The new product, service, or technology is often seen as confusing to the understood customer set. It is often potentially damaging to an existing business model. In any case, the opportunity is clearly not large in the next few years, and the uncertainty is high. Besides, it is usually clear from the chess analysis that the investment required will clearly provide greater returns in the planning horizon if aimed instead at improving existing businesses incrementally.

In some cases the decision is made, despite the above inhibitors, to proceed with the development of a new technology, the creation of a new product, or the launching of a new business. As in the case of making the decision to pursue a new and uncertain opportunity, the typical large-company business processes and culture can be a major inhibitor of success in getting from concept and commitment to success in the marketplace. On the flip side, there are major advantages that are enjoyed by large companies in this process. The key is to develop, for each case, a trajectory of progress which builds on the advantages and avoids the pitfalls along the path of progress.

For an internally developed idea and proposal, even if the proposal is being pursued by others (as is almost always the case), one can usually identify two major phases of progress between concept and marketplace success. The first phase consists of invention, reduction to practice, the building of the first prototypes, and initial interaction with a few leading-edge customers. For a new technology, this can be a period of several years, whereas for a new application of existing technology the period is much shorter. It is in this phase that the mature company with deep technical roots has a major advantage over the pure startup with limited resources. I call this the *incubation* phase. In this phase the new concept is not subjected to the pressures of a going business, and it is nurtured and supported in its environment. The primary measures are of progress toward the prototype goals, aimed at first exposure of the concept to customers and markets. Typically the organizational focus of this phase is in a central research and development organization rather than in a line of business, and it is this phase that many of the large information technology companies have traditionally excelled.

Yet, having achieved success in this incubation phase, the new concept or product is still far from becoming a factor in the marketplace, and even farther from having the potential to be the base of major growth of an already-large base. Success is required in the second phase, bringing the innovation to market, and the challenges in this phase are quite different from those of the invention and reduction to practice phase. The level of resources, the types of skills, the actions that must be taken within and outside the company change dramatically, and often the level of risk, or at least of perceived risk, increases dramatically. It is often at this point, when the business players see both the potential for market disruption and the demand for significant resources, that the internal environment can change from being supportive and nurturing to being either overtly or covertly hostile. It is in this phase that many companies fail, and it is here that the incompatibility of the culture and processes of the ongoing businesses with what is needed for success in the new is the root cause of the failure.

The internal vs. the external path: The case for excubation

The critical decision to be made at this point, where the prototype and customers' reaction to have proven sufficiently compelling to drive a decision to make a major push toward the marketplace, is whether to proceed with an internal entity or to make a major move toward separation. I call this move to an entity with major independence from the parent company "excubation," a term designed to indicate the contrast with the incubation phase which it follows.

Incubation implies major, even excessive, nurturing and monitoring, as well as protection from many of the forces of the real world. From what I have seen, there comes a time when continued incubation dramatically increases the likelihood of failure. In addition to its overhead and its prevention of the creation of a competitively strong team, incubation often focuses the new technology on too-narrow targets of opportunity, those within the limited interest of the parent company. And it causes enormous waste of resources since Darwinian principles are not at work. An excubated entity, with major equity participation by its parent, can have the best of both worlds, but only if the control from the parent is restricted to that exercised by parent company Board members in their Board role. Experience shows that large companies do not easily come to the conclusion that excubation is the right path. Unless the new thrust is so clearly in the white space relative to the business unit's market and product ambitions that it has no interest (a rare case in my experience), the chess players will typically attempt to embed the new thrust in an existing organization, based on proposed synergies and economies.

I refer to this approach so commonly followed as *attempting to raise the mice in the elephants' cage*. The argument for doing so points out that the cage has plenty of room, and that it makes no sense to develop a new cage for the mice; besides, there is plenty of straw and food around, so it can be done without much additional expense; and certainly the elephant keepers will not be burdened by the additional responsibility. In reality, however, the behavior of the elephants will usually result in the demise of the mice. It is not that the elephants are behaving badly, but that in going about their normal business they are likely to either trample the mice (who rustle about in the night and really do annoy the elephants), or suffocate them with their randomly placed, but substantial, droppings.

While it is true that being embedded in a large company provides a number of sources of support not easily available to a small stand-alone company, there are several areas, key to success, where the embedded company will be far weaker than the stand-alone company with which it is likely to be competing. Key among these are the following. First, an embedded company, and its best people, are inevitably distracted by the monitoring and other processes that are part of that culture. This limits the ability to focus maniacally on a single goal. Second, the culture and value system of the larger company will typically inhibit the ability to build the strongest possible team. The creators of the idea are usually a good base for the technical team, but to get a first-rate marketer or business development person to join something which is tiny and not obviously central to the company's interests is likely to be impossible. The scale of the project and the size of the team affect the perceived value of a position in this culture, with the likely result that the startup effort can only recruit from the second and third teams. Third, decision-making is inevitably slowed by the management hierarchy. Fourth, the pressure to get to market, provided in the stand-alone environment by the need to manage cash, is lessened. And finally, the ability to use market-value creation as a tool to both benefit the stakeholders and to attract a top team is eliminated. The net result is that often a team and environment are created which are markedly inferior to what can be obtained in a stand-alone environment.

All this is not to say that there is no value in having the right connections to the parent. Among other things, the parent can provide, on a very limited basis and on request, specific help in key areas such as the management of intellectual property or access to key expertise or tools. In addition, often there is the opportunity for the parent to be one of the leading-edge users of the excubated company's technology and products.

To get to a statistically significant data base to support the above assertions would be a major project. There is, however, in my view, more than enough evidence of the failure of major players to capture the benefit of opportunities where the key technologies and products have been incubated into an early leadership position to make the case. One example with an element of currency is the router business. In this case IBM developed three generations of products internally in its research division, and used these to build the early T1 and T3 internet backbones. Yet the anticipated conflict with the mainstream systems network architecture (SNA) products led IBM to pass on this opportunity. Recently, the last act of this drama has been played out with the licensing of IBM's technology in this area to Cisco, which grew to dominate the area, and which has a current market valuation which is substantially larger than that of IBM. An example of success for IBM is its creation in the late 1980s of a new company, with Toshiba as an equal partner, to enter the then-emerging market for flat panel displays. This company, Display Technologies Inc., is one of the major manufacturers and probably the technical leader in this multibillion dollar market, and IBM's linkage with DTI was one of the keys to the successful position which IBM has developed in the laptop market with its Thinkpad line of products.

With years of emerging opportunity ahead of us, and an increasing premium placed on velocity and being the early leader, it becomes increasingly important for highly successful companies such as IBM, Lucent, Motorola and the like to develop mechanisms for successfully creating new, major businesses from the results of their massive investments in research and advanced development. They must develop parallel methodologies to track new entities created outside their boundaries and build the substantive early linkages which create benefit for large company stockholders and customers. In my view there is a huge, underexploited opportunity to improve both these processes by recognizing the cultural and business process differences required to succeed in raising mice, some of which will, over time, become the next generation of elephants.

Assessing Technical Risk

David Morgenthaler

David Morgenthaler, a veteran of 31 years in venture capital plus 23 years as a manager of small entrepreneurial companies was an early President and Chairman of the National Venture Capital Association, and was the first recipient of its Lifetime Achievement Award. He was elected to the Private Equity Analysts Venture Capital Hall of Fame. He has been a director of many companies, President or Chairman of several, and was President of the Chief Executives Organization and the first Senior Vice President-International of the Young Presidents' Organization.

Comments during the discussions in the MTR workshop on assessing technical risks suggested that some defining of terms would lead to clearer communication about the subject. My ideas are expressed from the viewpoint of a long-time venture capitalist whose firm invests in information technology (which we regard as medium technical risk), life sciences (which can be very high technical risk), and middle-market buyouts (which usually have no technical risk).

The workshop discussions raised definitional distinctions between *risk* and *uncertainty*. Some seemed to feel that risk represented the possibility of large losses or some other kind of severe damage to the financing source undertaking a project. In contrast, our firm (and, I believe, most venture capital firms) would use the term "risk" to mean the likelihood that an individual project would not have a satisfactory financial outcome anywhere near the projected one. Most venture firms (not all) would not commit more than 5–10 percent of a fund to a single investment, especially in an early stage where the outcome is highly uncertain. Fortunately, it is rare for a fund's liability for an investment to exceed its capital invested—this has never happened to us—but it can, for example, if there is a lawsuit. It is true that many funds will be disappointed in perhaps 10–25 percent (numerically) of their projects if they do early-stage investing. But the aggregate money lost by unsuccessful investments is usually a fraction of the money made by the very successful ones. This is especially true if a venture fund has several investments with a cash-on-cash return that is a significant multiple of the investment (the "skew" referred to by Lewis Branscomb). This is usually the case with the successful funds.

However, in evaluating each investment, we definitely do not take a "portfolio approach." Instead, each project must stand on its own, and we would not knowingly undertake a number of projects —each of which had an unacceptably high risk on its own—merely with the hope that one or more would win by 5–20 times the investment and make up for all the losers. Venture capitalists must consider not only the loss of the initial capital plus the additional investments that will almost inevitably be made in the hope of salvaging the company; also important is the time of their professionals which would be inordinately consumed by a sick investment. This is particularly true if the venture capital firm presents itself to entrepreneurs and to investors as a "value adding" firm which constructively influences managements. Such ailing companies require large amounts of venture-capital partner time in planning new strategies, recruiting new management, making new strategic partnerships, and raising new financing under very difficult circumstances. For working purposes therefore we (and probably most) venture capital firms view *risk* as the likelihood that the specific project will have a financially unpleasant outcome, and we never knowingly enter into anything that threatens our entire fund.

We evaluate risk by dividing it into three components: technical, market, and management. A few years ago I separated those projects that had disappointed us financially into these three categories, a highly subjective exercise. To my surprise, only about 10 percent of the poor performers were caused by technical failure; I was astonished it was that low. About 30 percent were hit by market or other factors exogenous to the investment or beyond its control. In the remaining 60 percent, the technology had not failed and nothing unexpected happened in the market. These disappointments could be clearly traced (with the benefit of hindsight) to poor management decisions, or failure to execute in some way on what appeared to have been an attainable program. This confirmed the conventional wisdom that in venture capital investing the management people are the most important ingredient.

I have not taken the time to do the analysis more recently, but have no reason to think that the outcome would be very different. We have renewed our efforts in the obvious direction of increasing our due diligence as much as possible, and insisting on bringing in qualified managers even where this risks antagonizing the founding entrepreneurs to the point where we may lose the opportunity to make the investment. We require detailed plans against which we carefully monitor performance, and we coach managers, while trying to be tough-minded about replacing those individuals in whom we lose faith. Our experience is that, with hindsight, we see that we have never replaced a manager too soon, while there are numerous examples where we clearly waited too long.

How then do we evaluate technical risk, if we judge it is present? First, we consult our own staff, some of whom may have extensive relevant experience from previous operating jobs or previous investments. Second, we go to the appropriate technical people of both present and past investments, which is often a considerable network of resources. Third, we ascertain who are the leading technical experts in the relevant fields and hire appropriate specialists to consult for us on the subject. Part of our own skill is the ability to find and attract such people, and the judgment to weigh their often conflicting opinions appropriately. We look at the availability of alternative technology if the initial technology should fail. We compare the proposed technology with competitive technologies on the market. We look at the record of the technical team in solving similar problems. Finally, we may negotiate for a staged investment where the amounts risked are kept low until milestones proving feasibility have been attained. While this process results in a series of highly subjective judgments, it seems to work well enough that we don't regard assessing technical risk as a major problem in the venture business.

Some seemingly conflicting statements were made in MTR workshop discussions about risk. I think I made the statement that I hate the words "risk" and "grants." Sometimes inexperienced people come up and say "I hear you *like* to take risks," or "tell me how your grants work." As a serious founder and builder of companies, I want to calculate *all* risks carefully, and avoid investing in projects where we cannot be reasonably confident we can overcome the uncertainties. We do not make "research grants": we invest in what we believe will become successful businesses.

Some people said they "like risk." They feel they are equipped to evaluate it, and to find ways and people to overcome the uncertainties. The reward for overcoming these risks results in the possibility of high financial returns that attract people to venture capital. From this standpoint, we too like the risk-reward ratio that makes venture capital a viable business; this is the reason we are willing to make risky investments. However, I want to avoid any impression that we deliberately gamble on a favorable outcome by taking a number of individually unacceptable risks in a wide portfolio. In my experience, risks are very real: if you take enough of them, you will sooner or later lose some. I was an operating manager in several small private companies until I was nearly 49 and I never lost on a single one of these investments; perhaps I can be pardoned for thinking I had a golden touch. But then I went into the venture capital business, invested in enough projects so that I could not be involved on a day-to-day basis on each one, and discovered I could—and did—lose.

A useful way of looking at technical risk is to assess at what stage of development of a technology an institutional venture capitalist should invest:

- Venture capitalists should *never* invest to discover new scientific phenomena.
- Venture capitalists should *almost never* invest to prove the scientific principle.
- Venture capitalists should *rarely* invest to develop an enabling technology.
- Venture capitalists should *often* invest to use a new technology to develop a product.
- Venture capitalists should *very often* invest to revise and improve a product.
- Venture capitalists should *very often* invest to produce a later-generation product.
- Venture capitalists should *very often* invest to broaden a product line.
- Venture capitalists should *very often* invest to apply a product to another application.

This suggests that there is a need for support by the government in the earlier stages of research and development; this should be considered in ATP planning. Developing broad platform technologies would be a major contribution by government, but I see little or no role for government in the later stages of development.

One case that illustrates this recommendation is a biotech investment in which I am involved. A scientific discovery was made which won a Nobel Prize. It opened up radically new possibilities for a spectrum of therapeutics, which should both be highly effective and have minimum adverse side effects. The discovery was made at a university. An industrial company was partially funding the laboratory and got rights to a very basic patent. However, the process of turning this discovery into an enabling technology proved to be much longer and more expensive than was expected. The company did not seek government help in financing this broad platform technology development, but with hindsight it is clear that it should have.

Venture capital and biotech do not mix well because venture capital funds are usually formed with an expected life of ten years, plus two or three years of wind-down. However, the limited partners expect an even shorter 5–7 year investment cycle from they time that they commit their cash until it is returned to them. This mismatch of time scales presents severe problems for venture capitalists investing in drug discovery and development, which in the best case usually requires 10–12 years or more, and $100 million or more out-of-pocket cash, *if the drug is successful.* Counting the failures, the cost of successful drugs probably averages $250 million or more. Such time and cost requirements are beyond the capabilities of most venture capital funds. Anything that government can do to help develop *enabling* technologies will encourage institutional venture capitalists to invest in this field.

There are some common misunderstandings about the investment process that is followed by institutional venture investors. People say to us, “you just invest in exciting technology," but that is far from the whole story. Or they say, “You just invest in people," but this is also not true. If we were limited to just one factor, we would base investing on the people involved, but there is no such limitation. We can consider everything, and we do. This is shown by our checklist for new venture investing, which may clarify the process by which we consider the various elements of risk:

- What is the size of the market? (what is the *need* the world has?)
- What is the technology or the product by which we hope to fill this need?
- What is the plan for building this organization, developing this product, and filling this need?
- Do we have or can we get the people who will implement the plan?
- Can the plan be financed?
- With reasonable expectations, will the internal rate of return (IRR) be well above the minimums our firm's goals require?
- What is our realistic method of exit from the investment?

Venture capital investing is like a horse race: the technology or business concept is the horse, and when the race is run, you are limited to what you can get out of the horse, or make the horse into. The management team is the jockey. The market, including the competitive conditions within it, is the race. Consider the combinations. Wonderful horse, lousy jockey: the jockey lets the horse get boxed in, get hurt, or worst of all falls off the horse. That describes about 60 percent of our disappointments. Or lousy horse, wonderful jockey: the jockey gets all there is out of the horse, but it is not enough against the competition, and you lose. Wonderful horse, wonderful jockey, but you're running at the county fair. You win easily, but the prize is trivial. That is the small-market problem, and why market size is the first item on our checklist. Finally, consider a very good horse, a very good jockey, and the race is the Kentucky Derby with a potential for a huge prize and millions in stud fees. However, the best horses and the best jockeys are the competitors, and if your horse and your rider are not world class, you have little hope. The founders of Apple knew from the beginning that when the market was proven, IBM would move in and ultimately dominate.

These comments are meant to clarify the process and the role institutional venture capital plays in funding and developing new enterprises in the world and especially in the United States today. The process that the venture capitalist goes through in evaluating and investing in new technology is a result of a number of factors. These include first the necessary evaluations of the three kinds of risks listed above. Then come the financial requirements of the limited partners who furnish the capital to the institutional venture capitalists, which have to be weighed against the alternative investment opportunities available to these limited partners. If the time required is too long to permit an attractive internal rate of return (IRR) to the limited partners, they will insist on later-stage investments where the time required to exit the investment is shorter (and where frequently the risk is reduced because the development process has proceeded further). These factors put pressure on venture capitalists to invest in technology at later stages of development than may be optimal from the standpoint of the nation in the creation of desired new products.

I do not see how the government can help very much in the process of evaluation of venture-capital investment opportunities. However, it does seem that early stage help by the government in developing platform technologies and financing scientific discoveries is directed exactly at the areas where institutional venture capitalists cannot and will not go. In the analogy of the horse race, the role of the government can be to improve the bloodlines of the horses and give them some preliminary schooling.

Technology Regime and New Firm Formation*

Scott Shane

Scott Shane is associate professor of entrepreneurship in the Robert H. Smith School of Business and director of research at the Dingman Center for Entrepreneurship at the University of Maryland. His recent research focuses on the creation of new high technology companies, particularly out of universities.

Abstract

At least since Schumpeter (1934) and (1942), researchers have argued that entrepreneurs are more likely to establish new firms to commercialize technology when they are operating in a technological regimes of “creative destruction” (a technology regime in which new firms routinely replace large, established firms) than in one of "creative accumulation” (a technology regime in which large, established firms maintain their competitive positions despite competition from new firms). Despite considerable conceptual work on this question, data limitations have precluded previous researchers from directly examining how features of the technological regime influence the propensity of entrepreneurs to establish new firms. However, I was able to use data on the 1397 patents assigned to the Massachusetts Institute of Technology during the 1980–96 period to show that eight dimensions of the technology regime influence the propensity of entrepreneurs to commercialize new technologies through the creation of new firms: age of the technical field, the importance of market segmentation, the importance of dominant design, the importance of complementary assets in manufacturing, the strength of patents as a competitive advantage, the tacitness of knowledge, the observability of knowledge in use, and the independence of research and development. These results suggest several implications for public policy: (1) intellectual property policy should be assessed at the industry level; (2) government policies toward monopoly should be examined at the industry level; (3) government policy toward income distribution will be influenced by technology regimes; and (4) the government should adopt different policies to encourage the commercialization of technology in different industries.

When will people found new firms to commercialize new technologies that they have developed? In *The Theory of Economic Development*, Schumpeter (1912) presented an argument that new firm formation will be society’s primary mechanism for the commercialization of new technologies. Entrepreneurs, responding to exogenously developed inventions, will discover new products, processes, raw materials, and ways of organizing. By forming new firms to exploit these developments, entrepreneurs will usher in a wave of "creative destruction" that will replace existing firms in the market place.

Having observed the rise of the major research corporation in the period since his earlier work, Schumpeter formulated an alternative argument in *Capitalism, Socialism, and Democracy*. Innovation by the large, established firm, he argued, will provide society’s primary mechanism for the commercialization of new technologies. Under this scenario of "creative accumulation," established firms will commercialize technology by exploiting

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existing stocks of knowledge, accumulated financial resources, and well-honed competencies.

Which of these two perspectives—which we may call "Schumpeter Vers. 1.0" and "Schumpeter Vers. 2.0"—best depicts the reality of technology commercialization, varies across industries (Winter, 1984; Malerba and Orsenigo, 1997). Some industries, such as computer hardware, display the creative destruction pattern of Schumpeter Version 1.0: entrepreneurs frequently found new firms and replace established organizations. Other industries, such as pharmaceuticals, display the creative accumulation pattern of Schumpeter Version 2.0: established firms repeatedly withstand attempts by entrepreneurs to displace them.

For forty years, researchers have sought to explain this cross-industry variation in Schumpeterian patterns of innovation (Cohen and Levin, 1989). Using the theories and tools of static equilibrium analysis, economists have looked to monopoly power and market structure for the answer (Kamien and Schwartz, 1982). This effort to explain cross-industry variation in Schumpeterian patterns of innovation on the basis of monopoly power and market structure has been, at best, inconclusive (Cohen and Levin, 1989).

Observers have suggested a variety of other factors that might affect new firm formation. Several researchers have argued that the failure to explain cross-industry variation in the mode of technology commercialization is the result of an inappropriate emphasis on static equilibrium models (Nelson and Winter, 1982; Malerba and Orsenigo, 1997). The equilibrium focus has kept researchers from examining three important dimensions of industries which influence the propensity of entrepreneurs to exploit technological opportunities through firm formation: the nature of technology life cycles (Utterback and Abernathy, 1975; Gort and Klepper, 1982); appropriability conditions (Levin et al, 1985; Nelson and Winter, 1982); and the nature of knowledge accumulation in a particular industry (Winter, 1984).

Evolutionary economists have proposed explanations for the variation in patterns of innovation across industries that incorporate these concepts (Winter, 1985; Teece, 1986; Audretsch, 1997; Klevorick et al, 1995). However, empirical tests of their arguments have been limited by methodological obstacles.

This study overcomes these methodological problems to directly test the effect of technology life cycles, appropriability conditions, and the nature of knowledge accumulation on the propensity of entrepreneurs to form new firms to commercialize new technologies. By exploring data on the 1397 patents assigned to the Massachusetts Institute of Technology during the 1980–96 period, the study shows that eight dimensions of the technology regime influence the propensity of entrepreneurs to commercialize new technologies through the creation of new firms. These eight dimensions were tested as hypotheses derived from the work described above; they are: the age of the technical field, the importance of market segmentation, the importance of dominant design, the importance of complementary assets in manufacturing, the strength of patents as a competitive advantage, the tacitness of knowledge, the observability of knowledge in use, and the independence of research and development.

These results suggest several implications for public policy: (1) intellectual property policy should be assessed at the industry level; (2) government policies toward monopoly should be examined at the industry level; (3) government policy toward income distribution will be influenced by technology regimes; and (4) the government should adopt different policies to encourage the commercialization of technology in different industries.

The article proceeds as follows: In the next section, I review the literature on technology regimes and develop the eight specific hypotheses for why technology life cycles, appropriability conditions, and the nature of knowledge accumulation should influence the likelihood that an invention will be commercialized through firm formation. In the third section, I describe the dataset and the methods used for analysis. In the final section, I summarize the findings and discuss their implications for technology policy.

Theoretical development

In this section, I review the existing literature in the areas of industry life-cycle, appropriability conditions, and the nature of knowledge accumulation to develop eight testable hypotheses about when new firms will form.

INDUSTRY LIFE-CYCLE

Industry life-cycle theories argue that industries evolve over time from regimes of creative destruction to regimes of creative accumulation. This process of technological evolution influences the propensity of people to found firms to commercialize new technology. In particular, the propensity of entrepreneurs to found firms is higher when technology is young, when a dominant design has not yet emerged to block entry of new radical technologies, and when markets are segmented to allow entry of new firms with radical technologies.

Age of the Technology

Research on the technology life cycle argues that new firm formation is more common when a technical field is young than when it is mature (Utterback and Abernathy, 1975). For example, Gort and Klepper (1982) showed that a wide variety of markets display high levels of entry, which level off, and then contract, as the market matures. Similarly, research on industry life-cycles shows that most new industries experience waves of new entry, followed by a plateau in entry rates as the industry matures (Geroski, 1995). Four different arguments have been put forth to explain this life-cycle pattern. First, in the early stages of a new technology, markets are small and cannot provide sufficient returns to justify the investment by large, established firms. Instead, independent entrepreneurs with low opportunity-cost tend to exploit the new market. Over time, as markets grow in size, large firms become attracted to them.

Second, technical knowledge is cumulative. At founding of an industry, all firms in the industry are new, providing no advantage to incumbency. However, as the technology matures, firms that entered first develop learning curve advantages (Nelson, 1995). As a result, independent entrepreneurs find themselves at an increasing knowledge disadvantage over time.

Third, the maturation of technology changes the basis for competition in an industry. As technology matures, product innovation becomes less important, and the reduction of production costs and scale economies become more important (Pavitt and Wald, 1971). Therefore, over time, competitive advantage shifts to those things at which established firms are advantaged, at the expense of those at which independent entrepreneurs excel.

Fourth, complementary assets are important to competition in many industries. As technologies mature, these assets are brought under the control of incumbent firms to reduce contracting problems. As the industry matures, the tendency of established firms to obtain control over complementary assets makes entry more difficult for independent entrepreneurs (Teece, 1986).

These arguments lead to the first hypothesis:

> H1: The older the technological field, the lower the likelihood that a firm will be founded to commercialize a new technology.

Dominant Design

Life-cycle theories also argue that new technologies generally begin with a period of experimentation during which new firms adopt different technical designs. Through the combined effects of economic and social factors, one of these designs typically emerges as dominant (Utterback, 1994). Once a dominant design has emerged in an industry, alternative technology paths tend to be abandoned (Tushman and Anderson, 1990). Radical technological change becomes difficult to implement, and technological change becomes confined to incremental extension of the technological trajectory (Dosi, 1982).

After a design becomes dominant, the basis of competition shifts towards complementary assets and scale economies because the development of a dominant design reduces uncertainty and allows firms to invest in the reduction of production costs (Teece, 1986). As Suarez and Utterback (1995:418) explain, "prior to the appearance of a dominant design, economies of scale will have little effect, because a large number of variants of a product will be produced by the many competing entrants in any industry, with each producing at a relatively small scale. Once a dominant design is created, economies of scale can come into play with powerful effect, leading to rapid growth of those firms which most competently master the development of products based on the dominant design, to the detriment of those firms which are slower to adapt."

Dominant designs emerge in some industries, but not in others (Teece, 1986). The tendency of an industry toward dominant design has an important implications for new firm formation. Given the nature of competition after the development of a dominant design, entrepreneurs who have developed radical technologies will be less likely to form new firms in industries which tend toward dominant designs. This argument leads to the second hypothesis:

> H2: The more the industry tends toward a dominant design, the lower the likelihood that a firm will be founded to commercialize a radical new technology.

Market Segmentation

Life-cycle theories also argue that radical technologies tend to be exploited first by new firms in small market segments. The lack of performance reliability and high costs mean that new technology will tend to start in small markets where its unique performance advantages are critical (Utterback and Kim, 1984). New firms often provide these radical technologies because large firms allocate resources for innovation to satisfy the demands of their major customers (Christiansen and Bower, 1996). Since new technology that addresses the needs of a small segment of customers generally does not provide sufficient revenues to justify investment by established companies, these firms cede niche markets to new firms with radical technologies.

The ability of entrepreneurs to commercialize new technology through this niche strategy depends on structure of the market. Radical new technologies are more likely to be commercialized through the creation of new firms in industries which tend toward market segmentation than in industries which do not. In segmented markets, entrepreneurs can obtain a foothold for the radical technology before being faced with competition from established firms. This argument leads to the third hypothesis:

> H3: The more the industry tends toward market segmentation, the greater the likelihood that a firm will be founded to commercialize a radical new technology.

APPROPRIABILITY

Appropriability theories hold that the propensity of entrepreneurs to commercialize new technologies through firm formation is greater when patents are more important and complementary assets are less important to the generation of competitive advantage in an industry. When an entrepreneur founds a firm in response to the development of a new technology, the new firm typically does not yet possess complementary assets, such as a distribution system or specialized manufacturing that provide a competitive advantage in that industry (Teece, 1986). The more effectively entrepreneurs in an industry can protect a new technology against appropriation by competitors during the development of complementary assets, the more likely they will be to found new firms to commercialize new technologies. The ability to protect a new technology against appropriation, in turn, depends on the strength of intellectual property protection in that industry, and the magnitude of the complementary assets that need to be developed.

Strength of Patents

Strong patent protection increases the likelihood that an entrepreneur will commercialize a new technology through the creation of a new firm. Patents provide a legal right to prevent others from imitating a technological development. However, research has shown that the strength of patent protection varies across industries (Levin et al, 1987). Some patents can be "invented around" at low cost, while others provide strong protection for their duration (Teece, 1986). In industries where patent protection is weak, new firms have difficulty reaping the benefits of technology development because their new knowledge dissipates quickly to competitors who are better able to exploit it quickly (Von Hippel, 1982).

Strong patent protection provides several advantages to new firms. First, strong patent protection allows the developer of the new technology to create additional competitive advantages before the knowledge of a new technology dissipates to competitors (Teece, 1986). In particular, the possession of a strong patent position provides time to raise money from capital markets (Lerner, 1994). Since new firms lack cash flow to finance investment, this time window is more important for the efforts of new firms to commercialize technologies than for similar efforts by established firms.

Second, strong patent protection provides the innovator with the time to adapt the new technology to market needs. New technologies that turn out to have significant value may initially be commercialized for the wrong market segment or with the wrong design. Tight patent protection allows an innovator to come to market with the wrong product or market, but have the time to alter the technology to market needs before competitors can imitate it (Teece, 1986).

Third, strong patent protection allows a new firm to compete on the basis of innovation rather than on the basis of costs. When patent protection is weak, an imitator can copy an innovator's novel design. This ability to copy allows large, established firms to shift competition to manufacturing costs, at which they generally have an advantage. However, when patent protection is strong, the new firm can offset the imitator's manufacturing cost advantage by maintaining competition on the basis of its superior innovation. This argument leads to the fourth hypothesis:

H4: The more that patents provide a competitive advantage in an industry, the greater the likelihood that a firm will be founded to commercialize a new technology.

Complementary Assets

The magnitude of the complementary assets that have to be developed to compete in an industry also influences the likelihood that a new firm will be formed to commercialize a new technology. New technologies are often intermediate goods that need to be packaged into products or services to be sold to end users (Teece, 1998). For example, innovations in automobile design take on value only because the innovators have access to manufacturing capabilities. Teece (1986:191) explained that "the successful commercialization of an innovation requires that the know-how in question be utilized in conjunction with other capabilities or assets ... such as marketing, competitive manufacturing, and after-sales support."

Often these complementary assets are co-specialized with the innovative technology. For example, pharmaceutical firms often develop specialized sales forces who sell their drugs to physicians rather than rely on the distribution channels that are used for other products. These sales forces are co-specialized because they have value to the pharmaceutical companies largely because they have drugs to sell to physicians. Co-specialization makes it important to bring both assets both under the control of a single firm to mitigate bargaining problems. The tendency of established firms to acquire control over complementary assets to mitigate transaction costs makes it difficult for new firms to contract for these assets in the market place (Teece, 1986). Therefore, where specialized complementary assets are important in an industry, entrepreneurs will be less likely to establish new firms to commercialize a technology. This argument leads to the fifth hypothesis:

H5: The more that important that complementary assets are in an industry, the lower the likelihood that a firm will be founded to commercialize a new technology.

NATURE OF KNOWLEDGE ACCUMULATION

Several researchers have argued that variation in rates of new firm formation across industries depends on the nature of knowledge accumulation in those industries (Nelson and Winter, 1982). The development of a new technology involves the incorporation of prior technical knowledge because technical developments are cumulative (Dosi, 1982). This cumulativeness requires the innovator to obtain access to a repository of prior knowledge. The way in which this prior knowledge is best gathered by an innovator depends on the tacitness, observability, and independence of knowledge in an industry. These dimensions of knowledge accumulation, in turn, influence the tendency for firms to be founded to commercialize new technology.

Tacitness of Knowledge

Knowledge can be codified or tacit. Tacit knowledge is knowledge that can be transferred, but not articulated easily. The ease of knowledge transfer depends on its codification. The more that information is codified, the easier it is to transmit without direct communication (Teece, 1986). Because codified knowledge is transmitted well in written form, it can be obtained without face-to-face interaction (Cohen and Levin, 1989). Tacit knowledge, in contrast, demands face-to-face contact, in which people engage in a discussion or demonstration of the solutions to technical problems.

To gather tacit knowledge, firms develop formal and informal communication mechanisms (Henderson and Clark, 1990). Since useful knowledge may not be held exclusively within the boundary of the firm, but also by users, suppliers, or academic researchers, these communication mechanisms often extend across firm borders (Von Hippel, 1988). Therefore, to obtain tacit knowledge, firms invest in human interactions that span firm boundaries (Tripsas, 1997).

The tacitness of knowledge decreases the likelihood that new technology will be commercialized through firm formation. Since tacit knowledge cannot be obtained without the investment in human communication, new firms are disadvantaged relative to established firms in the exploitation of tacit knowledge. First, existing firms can more easily engage in know-how exchange with other firms because established firms can reciprocate the exchange of information. The symmetry of this relationship enhances its stability and facilitates its functioning. Second, firms must develop information trading mechanisms long before they need to use them for a particular purpose (Tripsas, 1997). The development of assets in advance of their use is more costly for new firms than for established firms: the negative cash flow of new firms means that underutilization of assets may threaten their survival. Therefore, new firms cannot access tacit information from competitors as easily as established firms can. This argument leads to the sixth hypothesis:

H6: The more that knowledge in an industry is tacit (i.e., the less it can be codified), the lower the likelihood that a firm will be founded to commercialize a new technology.

Observability-In-Use

The knowledge about prior technological developments that is important for subsequent innovation can be obtained through public or private channels. Public channels of knowledge transfer are those are made available to other economic actors. Examples include the sale of a product in the marketplace or disclosure in a patent document. Private channels of knowledge transfer are those that transfer information despite efforts to keep the information from entering the public domain (Winter, 1984). Examples of private channels of information transfer are the leakage of trade secrets, or the transfer of personnel from one organization to another.

Although knowledge about all innovations diffuses through both public and private channels, the relative importance of the two channels varies across technologies. For example, knowledge transfer for chemical processes tends to be more private than knowledge transfer for software code.

One factor that accounts for variance in the relative salience of public and private channels of knowledge transfer is the degree to which a technology is "observable-in-use." Observability-in-use refers to the degree to which technical knowledge is disclosed through observation of the good or service in which the technology is used. Observability-in-use is increased by the embodiment of the technical change in the product or service sold in the market place. When new technical knowledge is embodied in a product or service, the transaction of selling or renting the product or service makes the technology transfer public. By purchasing devices that embody technological developments and taking them apart, trained engineers often gather valuable information about prior technological developments and can incorporate them into their innovations.

Observability-in-use increases the likelihood that an innovator will commercialize a new technology through firm formation. Information that is observable-in-use is equally available to independent entrepreneurs and established firms. Anyone can buy a product and reverse-engineer it. Accessing information that is not observable-in-use is more costly for new firms than for established firms. First, accessing private information requires an investment in absorptive capacity, or the ability to understand the information developed by others (Cohen and Levinthal, 1990). Absorptive capacity has a high fixed cost of entry, resulting in scale economies that are lower on a per-unit basis for larger firms than for smaller firms. Second, new firms pay a risk premium to obtain capital because they lack the positive cash flow of existing firms. Consequently, new firms pay a higher price than established firms to obtain the assets necessary to monitor the technical developments of their competitors. These arguments lead to the seventh hypothesis:

> H7: The more that knowledge is observable-in-use, the greater the likelihood that a firm will be founded to commercialize a new technology.

Independence of Technology Development

The independence of technology development also increases the likelihood that new technology will be commercialized through firm formation. Teece and Pisano (1994:540) explain that technological development is rarely "stand-alone," because organizational routines cannot be assembled immediately upon entry of a new firm into a market. Rather, technological development often involves interdependencies between the development of routines in technology, manufacturing, production, and distribution that require interaction between people in different parts of an organization. For this reason, firms enjoy significant benefits in technology commercialization when they have established an ongoing relationship between research and development and production and distribution (Mowery, 1983). This relationship provides a flow of valuable information between the research laboratory and marketing and production units.

Interdependencies between development of technology and other organizational routines reduce the likelihood that a new firm will be created to commercialize a new technology because they make it difficult to commercialize a new technology through independent research and development alone. As Winter (1984:318) explains, "the problem facing the aspiring entrepreneur is that his key idea must be complemented with other elements to constitute a functioning routine, and the persistent innovative efforts of established firms have given them enough of an edge in these complementary elements to outweigh the advantage of his key idea."

However, the importance of interdependencies between technology development and routines in manufacturing, production, or distribution varies across industries. As Nelson (1985:173) explains, the concept of interdependencies "applies well to aircraft and semiconductors. Pharmaceuticals, on the other hand, can be found and tailored or constructed virtually exclusively in a laboratory, by scientists who know little about how pharmaceuticals are mass produced and marketed."

Variation between industries in how important interdependencies between technology development and other routines are suggests that new firm formation will be a more common mode of technology commercialization where the technology development is more independent of other activities. Winter (1984:318) explains that the difficulty of commercializing a new technology through firm formation is "lessened if the entrepreneur could enter the market for an isolated *component* of the product or product line offered by established firms, a component in which his key idea played a much larger relative role. The feasibility of this course of action depends on the isolatability of the component, both intrinsically and as a result of the deliberate policies of the established firm" (Winter, 1984:318). This argument leads to the eighth hypothesis:

H8: The more independent research and development is from other firm activities in the industry, the greater the likelihood that a firm will be founded to commercialize a new technology.

Methodology

This study explores the likelihood of firm formation for the population of 1397 patents issued to the Massachusetts Institute of Technology for inventions made by faculty, staff or students of the university between 1980 and 1996. This population includes all patents for inventions that made material use of university property during their development.

DEPENDENT VARIABLE: FIRM FORMATION

I measured firm formation through the use of a dummy variable of one if the invention was licensed to a firm that did not exist as a legal entity prior to the receipt of the license. The MIT Technology Licensing Office maintains records of its inventions and its licensees, and I was able to use these records to code this variable. Through the use of event-history analysis, I predict the likelihood of firm formation on the basis of several factors, which are specified below.

YALE MEASURES

This study makes use of several variables developed from the Yale study on industrial research and development. Therefore, I summarize briefly the methodology used to collect data for that study. (Further information is available in Levin et al., 1987.) Levin et al. (1987) asked 650 high-level R&D managers from 130 different lines of business to answer questions about technological change in the line of business in which they operated. The respondents were asked to serve as expert observers of their line of business rather than as representatives of their firms, and were asked to report central tendencies in the form of a series of Likert scale items that ranged from one to seven. The researchers constructed line-of-business mean scores for each item on the basis of the average responses of the respondents from each line of business. I use these line-of-business mean scores to measure several dimensions of technology regimes, as described below. To map the Yale measures to SIC codes, I used the SIC code concordance developed by Levin et al (1987). When SIC codes overlapped with more than one industry in Levin et al (1987), the measures were averaged across those industries.

PREDICTOR VARIABLES

Age of the Technical Field. I measure the age of the technical field as the number of years since the three-digit patent class was established by the United States Patent and Trademark Office.

Appropriability. I measure the strength of patents as a mechanism to appropriate the benefits of innovation, using the Yale measure "patents to prevent competitors from duplicating the process" (under the heading "in this line of business, how effective is each of the following means of capturing and protecting the competitive advantages of new or improved production processes?"). This item is measured on a Likert scale which ranged from one to seven, with one equal to "not at all effective" and seven equal to "very effective."

Complementary Assets. Teece (1986) explains that specialized manufacturing capabilities are an important complementary asset. Therefore, I measured the importance of complementary assets in manufacturing by calculating the value-added from manufacture as a percentage of total value-added in the industry, using data obtained from the Census of Manufacturers.

Market Segmentation. I measure the importance of dominant design in an industry by using the Yale measure "designing products for specific market segments" (under the heading "to what extent have the following technological activities been engaged in consistently and repeatedly in this line of business?"). This item is measured on a Likert scale which ranged from one to seven, with one equal to "of no importance in this line of business" and seven equal to "very important in this line of business."

Dominant Design. I measure the importance of dominant design in an industry by using the Yale measure "moving toward a standardized or dominant product design" (under the heading "to what extent have the following technological activities been engaged in consistently and repeatedly in this line of business?"). This item is measured on a Likert scale which ranged from one to seven, with one equal to "of no importance in this line of business" and seven equal to "very important in this line of business".

Tacitness of knowledge. I measure tacitness by using the Yale measure "learn details through informal conversations with employees of the innovating firm" (under the heading "How effective is each of the following mean by which firms in this line of business may acquire technical knowledge of new or improved production processes developed by a competitor?"). This item is measured on a Likert scale which ranged from one to seven, with one equal to "not at all effective" and seven equal to "very effective."

Observability-in-use. I measure observability-in-use by using the Yale measure "acquire the product and reverse engineer it" (under the heading "How effective is each of the following means by which firms in this line of business may acquire technical knowledge of new or improved production processes developed by a competitor?"). This item is measured on a Likert scale which ranged from one to seven, with one equal to "not at all effective" and seven equal to "very effective."

Independence of R&D. I measure independence of research and development by using the Yale measure "undertake independent R&D" (under the heading "How effective is each of the following mean by which firms in this line of business may acquire technical knowledge of new or improved production processes developed by a competitor?"). This item is measured on a Likert scale which ranged from one to seven, with one equal to "not at all effective" and seven equal to "very effective."

Radicalness. To measure the radicalness of patents, I count the number of different three-digit patent classes in which previous patents cited by the given patent are found. The assignment of a patent to a particular patent class represents the assessment of the U.S. Patent and Trademark Office (USPTO) that the patent belongs in a particular technical field. Because patents belong to technical classes and because they cite previous patents, the citation to patents in particular technical fields represents the USPTO's assessment that a particular invention builds upon (cites) knowledge in that technical field. I argue that patents that cite other patents in fewer three-digit technological fields are more incremental (i.e., less radical) than patents that cite patents in more three-digit technological fields, because they draw on a narrower technological paradigm or set of paradigms.

CONTROL VARIABLES

Previous researchers have provided a variety of arguments for why different industry attributes should influence firm formation rates. I control for these alternative explanations to show that the eight hypothesized arguments about technology life cycles, appropriability conditions, and the nature of knowledge accumulation influence the propensity of entrepreneurs to found firms, over and above those provided by other theoretical frameworks.

Market Size. I measure market size as the dollar value of assets in the industry, using data from the Census of Manufacturers. A small market, it is argued, should have less firm formation than a large market because innovative activity typically involves a high fixed cost which can be amortized at a lower per-unit cost in a larger market (Giroski, 1995).

Capital Availability. I measure capital availability as the amount of venture capital funding in the industry, using data obtained from Securities Data Corporation's venture capital database. Prior research has shown that entrepreneurship is less likely to take the form of new firms when capital market imperfections make it difficult for independent entrepreneurs to secure financing (Cohen and Levin, 1989).

Firm Size. I measured the average size of firms in the industry as the dollar value of assets in the industry divided by the number of firms, using data from the Census of Manufacturers. High levels of average firm size discourage entrepreneurs from creating firms because they raise the cost of entry (Audretsch, 1995).

Concentration. I measured industry concentration as the market share of the four largest companies in the industry, using data from the Census of Manufacturers. Highly concentrated industries should discourage people from creating new firms, because concentration enhances the power of incumbents to attack new entrants and their ability to collude (Giroski, 1995).

Research and Development Expenditures. I control for R&D intensity in an industry as research and development expenditures as a percentage of the value-added of industry shipments, using the research and development expenditures obtained from Science and Engineering Indicators and the Census of Manufactures. Galbraith (1956) and Scherer (1980) argue that innovation should be undertaken by large firms in more research and development–intensive industries because large firms can achieve greater economies of scope in R&D. In addition, complementarities between R&D and other activities such as distribution are said to provide an advantage to larger firms in R&D intensive industries (Cohen and Levin, 1989). It is also argued that, since R&D is inherently uncertain, the diversified firm has an advantage in R&D because it has more market opportunities in which to exploit new knowledge (Nelson, 1959).

Technical Classes. Using dummy variables for drugs, mechanical inventions, electrical inventions, and chemical inventions, I control for the general technical field of the invention. For the purpose of analysis, the base case is drug inventions. I control for the technical field because the rate and mode of invention commercialization varies substantially by technology (Scherer, 1980).

Time. Using dummy variables for the year in which the patent was applied for (except 1996), I control for time because changes in federal law and MIT policy have changed the incentives for economic actors to start companies to exploit inventions (Henderson, et al., 1998).

Conclusions

This article examined the effect of technology cycles, appropriability conditions, and the nature of knowledge accumulation on the probability that new technology would be commercialized through the creation of new firms. By exploiting data on the population of MIT inventions over the 1980–96 period, and controlling for several other dimensions of industry, the time period, and the type of technology, I show that the eight specified dimensions of technology regimes influence the probability that an invention will be commercialized through new firm formation.

The results support all eight hypotheses. Consistent with hypothesis 1, the older the technology class, the lower the likelihood that a new firm will be founded to commercialize the invention. Consistent with hypothesis 2, when a invention is radical, the more that an industry tends toward a dominant design, the lower the likelihood that a new firm will be founded to commercialize the invention. Consistent with hypothesis 3, when a invention is radical, the more that an industry tends toward market segmentation, the greater the likelihood that a new firm will be founded to commercialize the invention. Consistent with hypothesis 4, the stronger the competitive advantage provided by patents in the industry, the greater the likelihood that a new firm will be founded to commercialize the invention. Consistent with hypothesis 5, the more value-added that is provided by manufacturing in the industry, the lower the likelihood that a new firm will be founded to commercialize the invention . Consistent with hypothesis 6, the more tacit is industry knowledge, the lower the likelihood that a new firm will be founded to commercialize the invention. Consistent with hypothesis 7, the more observable-in-use is industry knowledge, the greater the likelihood that a new firm will be founded to commercialize the invention. Consistent with hypothesis 8, the more independent is R&D in the industry, the greater the likelihood that a new firm will be founded to commercialize the invention.

Implications

This study shows how the propensity of entrepreneurs to establish new firms to commercialize technological discoveries varies across industries. This result is important to people who would like to establish new technology companies. The ease with which entrepreneurs will be able to assemble resources and succeed at the task of firm formation depends on the technological regime of the industry which they seek to enter. In general, entrepreneurs will be more likely to be successful in establishing new firms if they enter technical regimes characterized by Schumpeterian patterns of creative destruction.

The results also provide important implications for the management of risk by established firms. Managers often focus on the risks of commercializing new technology, but their failure to commercialize technology and efforts to maintain status quo are also risky. The failure to develop a particular technology when others do so can result in worse outcomes for a firm than the investment in a failed technology commercialization effort. Although managers of established firms have developed fairly good mechanisms for ensuring that they will invest in a new technology if their existing competitors do so, the new technology might be commercialized by a new firm, potentially blindsiding established firms.

This study provides evidence that several characteristics of technology regimes influence the likelihood that new enterprises will challenge existing firms. Technology regimes conducive to new firm formation are also ones in which managers of existing firms must pay attention to the threat of competition from firms not yet in existence. The result of this study suggest that the technological regime framework provides a useful tool for identifying the conditions under which new firms are likely to emerge as competitors of established firms.

The results of this study also have several implications for public policy. First, the study suggests intellectual property policy should be assessed at the industry level. Klevorick et al. (1995) explained that intellectual property policies that are beneficial to entrepreneurial activity in one industry may be detrimental in another industry. In some industries, the locus of innovative activity lies with new firms; in others, it lies with established firms. This study shows that variation among industries in the effectiveness of intellectual property influences firm formation rates. By demonstrating that firm formation is more likely under some appropriability conditions than under others, this study suggests that intellectual property policies that are supportive of entrepreneurship in one industry may be hostile to entrepreneurship in another.

Second, the results suggest that government policies toward monopoly should be examined at the industry level. The rate at which independent entrepreneurs enter an industry influences the degree of competition in that industry (Caves, 1998). If the likelihood that entrepreneurs will form firms to commercialize new technologies varies across different technology regimes, then the tendency toward monopoly will vary by industry. Consequently, government policy toward monopoly should take the nature of the technology regime and its implications for firm formation into consideration.

Third, the results suggest that government policy toward income distribution will be influenced by technology regimes. New firm creation is one of the major mechanisms through which significant wealth is amassed by people in a capitalist society. If entrepreneurs are more likely to start firms in some technological regimes than in others, then the distribution of wealth generated by technological change will be different under different technological regimes. In industries characterized by regimes of creative accumulation, the wealth generated by technological developments will be distributed to the shareholders of established organizations. In contrast, in industries characterized by regimes of creative destruction, the wealth generated by technological developments will be distributed to independent entrepreneurs and their investors. This argument suggests that policies toward wealth distribution need to consider the nature of technological change across industries.

Fourth, the results suggest that the policies that government adopts to encourage the commercialization of technology should be different in different industries. The different institutional forms that innovation takes in different industries means that the government policies that will best help new firms overcome problems of technical risk might be different from those appropriate to help established firms to overcome similar problems. In industries in which new firms are an important institutional form for commercializing innovation, the government can fill an important gap in preparing technology for private sector investment by preparing university research for commercialization or through government investment in early stage ventures. Policy makers should pay careful attention to the needs of new firms in overcoming the obstacles to the commercialization of new technology in these industries, because the government will generate the greatest social returns to innovation by helping new firms reduce technical inventions to practice in these industries. In industries in which people commercialize new technology by founding new firms, more technology would be commercialized, and greater social benefits of innovation would be achieved if enterprising individuals faced fewer obstacles to found new companies.

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It's Not Just the Money: The Role of ATP Proposal Evaluation and Awards in Leveraging Private Support by Providing Independent Validation of Projects[*]

Jonathan Tucker

Jonathan Tucker is a graduate research assistant at the Institute of Public Policy, George Mason University.

The Advanced Technology Program (ATP) provides two important resources in the effort to promote private sector support of high-risk projects. The first resource, obviously, is money: ATP awards play an important role in helping firms hedge against the financial risks of undertaking advanced technology projects. Another source of support that ATP provides, however, is the validation of proposed projects that may serve to promote trust and cooperation. That is, an ATP award indicates to potential supporters (e.g., company management, venture capitalists) that a proposed project is a good technical bet. It is this latter support that is explored in this paper: the role that validation provided by ATP plays in the decisions of private actors to support advanced technology projects, and more specifically, the role of ATP validation in the process of risk management by prospective supporters. In other words, how does an ATP award (or a favorable evaluation by ATP) affect the perceived risk of a project? (I assume that the effect of ATP validation is limited to the technical aspects of a project; private-sector supporters are likely to consider themselves the best judge of a project's market prospects.)

ATP as a source of validation

An ATP award or evaluation may be seen to provide validation only if the relevant private decisionmakers believe that the evaluation is based on independent and expert judgment. ATP seeks to meet both criteria.

ATP awards are made following an evaluation process that encompasses both technical and business considerations. To gather the relevant expertise in business and technical matters, ATP makes extensive use of external reviewers in addition to its considerable in-house staff of scientists, engineers, and economists. To ensure independence, reviewers must certify that they have no conflict of interest.

In a recently released study, Jean Powell provides evidence that ATP awards help persuade private decisionmakers to provide support for projects.[88] Powell draws on company responses in Business Progress Reports, which ATP awardees must file on an annual basis. In the Reports, firms are asked how the ATP award affected the credibility of the project with stakeholders including management, investors, customers, and suppliers. With regard to each of these stakeholders, at least a quarter of awardees, both small and large companies responded that the ATP award increased credibility. [89] Excluding the responses from large

[*] This paper has benefited greatly from the comments and encouragement of Christopher Hill, Franco Furger, and Christopher Tucker. All errors are my own.

[88] Jean Powell, "Business Planning and Progress of Small Firms Engaged in Technology Development through the Advanced Technology Program," NISTIR 6375, October 1999.

[89] In this analysis, awardees are divided into small (less than 500 employees) or large (at least 500 employees).

firms with regard to investors, the proportion is over half. With regard to investors, Powell finds that 75 percent of small firms reported increased credibility with investors. Only 24 percent of large firms reported increased credibility with investors, but this may well reflect, as Powell suggests, the tendency of larger firms to rely on internal funds rather than outside investors. With regard to management, she finds that more than four out of five firms report increased credibility (85 percent of larger firms and 81 percent of smaller firms).

Powell's study provides important evidence that ATP funding provides validation to awardees. However, it may be that even if a proposal does not receive an award, the fact that the proposers were favorably evaluated may be taken by prospective supporters as a good reason for backing the project or, at least, to support another attempt to get funding.

Possible scenarios

Assuming that potential supporters of projects believe that the process by which ATP selects winning proposals is independent and well-informed, how does an ATP award affect the decision of potential supporters to back a project? Powell's study tells us that awardees believe that ATP awards have increased their credibility with stakeholders. However, it does not speak to how ATP awards might affect the judgment of prospective private-sector supporters. To address this question it is useful to imagine several scenarios involving different decisionmakers and concerns.

The first is the case of a project pushed by a "champion." The impetus behind a project may come from a person within the firm who believes in the technology and takes it upon himself or herself to push for the technology. However, that person usually does not have the authority within the firm to fund the project. Company management must be persuaded to provide support for the project.

Management may not have the independent technical capability to evaluate the prospects of the technology, if the champion is the firm's own technical expert on the subject. Company management may feel unable to judge whether the champion's enthusiasm is based on a valid technical assessment; as a result, management may perceive the project as too risky. Insofar as ATP is seen to provide an independent expert evaluation, an ATP award may provide management with some assurance that the project is technically sound.

The prospect as well as the fact of an ATP award may be factors in building management support. There is some anecdotal evidence that ATP awards help company personnel sell a project to management. However, when a project proposal is evaluated, ATP staff look for whether the proposed project has the support of company management to begin with. The desire for external validation of a project may lead management to sign on to a project proposal, while winning the award may bolster existing management support.

In addition, there is the issue of what role is played by external validation and what role is played by money, especially in the case of getting the management support needed to submit and defend a proposal to ATP. Presumably, before agreeing to participate in the proposal process, management must see at least some promise in the project. If management thinks the project is a loser, even the prospect of money from ATP is not enough, because the company would have to match that money.

The second possible scenario involves a firm seeking to build support in a prospective partner. This case may suggest a similar problem of persuading management of a project's validity, except that here it is another firm's management that must be "sold" on the project.

The third scenario involves a start-up firm seeking venture capital. A firm may seek support to commercialize a technology after it has already developed the technology with funds from an ATP award. Here, an ATP award has not been necessary to encourage support for initial technology development, as in the first scenario; this suggests technical risk may not be as serious a concern. Here, again, support is sought from decisionmakers external to the firm. However, the same problems may arise in persuading a venture capitalist to support a project as in persuading company management: the project may fall beyond the range of the venture capitalist’s expertise, which limits its ability to evaluate the project. ATP validation may overcome this barrier.

Assuming the technology falls within the scope of a venture capitalist's expertise and experience, and technical risk is not as much of an issue, it may still be difficult for a firm to distinguish itself among the many other applicants. Venture capitalists must decide among many proposals. In this context, an ATP award may serve to set a project apart from its competition. The successful completion of an ATP project provides the applicants with a track record, which helps establish credibility.

The case of a firm seeking a strategic partner provides a variant on the previous scenario, with similar issues of credibility.

Ideas for further research

To explore the effects of external validation by the ATP process in the decisionmaking of private supporters, completed projects could be studied to investigate the role played by ATP in providing validation, and how such validation affected the perceived risk of projects and the decision to support them. Decisionmakers within firms and venture capitalists could be interviewed on how risk is evaluated and how ATP evaluations and awards fit into that evaluation.

These interviews should be guided by an attempt to understand how company decisionmakers manage risk more generally. External validation is just one resource in the process of risk management. Unfortunately, although the technology management literature is filled with prescriptive schemes for how investments ought to be made, there is little empirical literature which speaks to how investment decisions are made. In their chapter, Chesbrough and Rosenbloom draw on the precious little available work in a discussion of the role of business models in biasing the R&D investment decisions made by companies.

More specifically, there is no work that speaks to the role of external validation in private decisions to make technology investments. Thus, original research in this area is warranted.

ATP project evaluations and awards are just one source of external validation. Therefore, in looking at the role of ATP evaluations and awards in validating proposed projects, research should also examine alternative sources of validation available to companies and venture capitalists, such as outside consultants, reputation, or the track record of the proposal team, and how and when these sources are used in evaluating proposed projects. Identifying alternative sources of information used in validation and how these sources are used in evaluating proposals permits a better assessment of the relative importance of ATP in validating a project, and provides a way of assessing the claims of decisionmakers about the role of ATP in a particular project.

It would also be useful to contact companies that did not receive an ATP award but did well in the competition to see how they have fared in securing support. As suggested earlier, even though a firm does not win an ATP award a good performance in the evaluation process could impress prospective supporters.

Appendix A: Workshop Agendas

June 21-22, 1999: Practitioners' Workshop

June 21

6:30 PM **Dinner**

Rosalie Ruegg (Chief Economist, ATP)

A brief introduction to ATP and the reasons for this study.

Dr. Ruegg's remarks will be followed by a discussion moderated by Lewis Branscomb (Aetna Professor of Public Policy and Corporate Management, emeritus, Kennedy School of Government).

June 22 **Morning Program**

The three morning sessions will explore the different viewpoints of the technical project innovators, the business executives to whom they are accountable and the financiers who risk their money. We anticipate the groups' views on technical risk will differ, as do their responsibilities. An interesting question is the effectiveness of communication between the three groups about technical risk.

8:00 AM **Breakfast and registration.**

9:00 **Technologists' Panel**: The panel will discuss cases in which projects (successful or failed) that entailed an unusual level of technical risk were undertaken, and how those risks were assessed and managed. To what extent did technical managers share their concerns about technical risk with business executives or investors? How do the technologists manage the likelihood of failure?

Lewis Branscomb (Kennedy School of Government, Harvard), moderator
Howard Frank (Dean, Robert H. Smith School of Business, Univ. of Maryland)
David Lewis (Vice President, General Manager of Chemical Products, Lord Corp.)
Mark Myers (Senior Vice President, Xerox Research and Technology, Xerox Corp.)

10:00 **Break**

10:10 **Business Executives' Panel**: How do technical elements of business risk differently influence business decisions, depending on the size of the firm, the technical knowledge of business executives, the nature and source of the financing? How are technical failures defined? How are they managed?

Ken Morse (Managing Director, MIT Entrepreneurship Center), moderator
Larry Jarrett (Vice President, OrganoSilicones R&D, Witco Corp.)
Steve Kent (Chief Scientist, BBN Systems and Technologies)
James McGroddy (Ret. IBM Sr. VP Research; Chairman, Integrated Surgical Systems)

11:10 **Financiers' Panel**: Whether a CFO, a venture investor, or an angel, what part does technical risk play in the decision to invest? How can investors determine whether a technical idea is sufficiently mature to have a good chance of success? How are technical failures defined from the investor's perspective? Would partial funding of the reduction-to-practice research by government be an attractive way to mitigate the risk?

Josh Lerner (Associate Professor, Harvard Business School), moderator
Rick Burnes (Charles River Ventures)
Mark Chalek (Director, Office of Corporate Research, BID Medical Center)
Robert Charpie (Chairman, Ampersand)
David Morgenthaler (Founding Partner, Morgenthaler Ventures)

12:15 PM **Lunch**

Seating will be assigned, with tables having a mix of technical managers, business executives, and investors. Discussion will be aimed at comparing and rationalizing the different perspectives of the three groups.

1:45 **Afternoon Session**

The afternoon will be devoted to two cases that have been researched and analyzed at HBS and MIT. These cases will be discussed by the principals involved. The written cases will be distributed in advance to all participants in the workshop. Mike Roberts of HBS will chair these two sessions. We will explore the role of technical risk in the decisions made.

Presentation and discussion of the Advanced Inhalation Research case

Mike Roberts (Senior Lecturer, Harvard Business School), moderator
David Edwards (President, Advanced Inhalation Research)
Robert Langer (MIT/Advanced Inhalation Research)
Terry McGuire (General Partner, Polaris Venture Partners)

2:45 **Presentation and discussion of the Trexel case**

Mike Roberts (Senior Lecturer, Harvard Business School), moderator
Alex D'Arbeloff (Chairman, MIT Corporation)
David Bernstein (President and CEO, Trexel)

3:45 **Summary:** Discussion of lessons learned moderated by Lewis Branscomb.

September 16, 1999: Analytic Workshop

8:00 - 9:00	Continental breakfast served outside Bell Hall.
9:00 - 10:00	*Session I: Distinguishing Technical Risk, Product Specifications and Business Risk: the Scherer R&D Expenditure Model and its Alternatives.* Chair*: Lewis Branscomb (JFK School, Harvard University)* *Panel*: George Hartmann (Xerox Corp.), David Lewis (Lord Corp.), F. M. Scherer (JFK School, Harvard University).
10:15- 11:45	Session II: New Firms: Technology, Funding and the Changing Roles of Venture Capital and Universities. *Chair*: Mike Roberts (Harvard Business School) *Paper 1*: Josh Lerner (Harvard Business School), "When Bureaucrats Meet Entrepreneurs: The Design of Effective 'Public Venture Capital' Programs" *Paper 2*: Scott Shane (University of Maryland Business School) "Technology Regime and New Firm Formation." *Panel*: Mark Chalek (Beth Israel Deaconess Medical Center), David Morgenthaler (Morgenthaler Ventures), John Preston (Quantum Energy Technologies).
11:45 - 1:00	Lunch
1:00 - 2:30	Session III: The Relationships between Technical Innovators, Investors and Managers: Institutional Differences among New, Small, Medium and Large Firms. *Chair*: F. M. Scherer *Paper 1*: Henry Chesbrough and Richard Rosenbloom (Harvard Business School), "The Dual Edged Role of the Business Model in Leveraging Corporate Technology Investments" *Paper 2*: James McGroddy (Integrated Surgical Systems, former IBM CTO) "Raising Mice in the Elephants' Cage"

	Panel: Marco Iansiti (Harvard Business School), Larry Jarrett (Witco Corp)
2:45 - 4:00	Session IV: Future Trends in Public and Private Promotion of Research-based Innovation *Chair*: Ken Morse *Paper 1*: Christopher Hill and Jon Tucker (George Mason University), "The Varied Role of Technical Uncertainty in Company Decision-making: A Consideration of Several Completed ATP Projects" *Paper 2*: Mary Good (Venture Capital Investors, LLC and University of Arkansas), "Will Industry Fund the Science and Early Technology Base for the 21st Century?" *Panel*: David Ragone (Ampersand), George Hartmann (Xerox Corp.)

Appendix B: Participant Biographies

David Bernstein

PRESIDENT AND CEO, TREXEL

postal: 45 Sixth St. Woburn, MA 01801
email: david@trexel.com
tel: 781 932-0202 x 239
fax: 781 932-3324

Mr. Bernstein, who has held several executive positions in finance, general management, sales, and marketing, focuses on the commercialization and marketing of advanced technologies. He spent 9 years with Teradyne, a leading manufacturer of electronics testing equipment, where, as Vice President of Sales and Support, he built a global, 250-person organization to sell and service $150 million of capital equipment annually. Also while at Teradyne, Mr. Bernstein negotiated a $250-million OEM agreement with the General Electric Company. As a Vice President of Thermedics Detection, a Thermo Electron Company, Mr. Bernstein built and managed a worldwide business that sold operationally critical equipment to the Coca Cola and Pepsi Cola companies and established a worldwide support organization to service it. Also while with Thermedics Detection, he negotiated successful OEM and licensing relationships with leading European bottling-equipment companies. Mr. Bernstein received a B.A. from Harvard College and an M.B.A. from Harvard University.

Lewis Branscomb

AETNA PROFESSOR OF PUBLIC POLICY AND CORPORATE MANAGEMENT, EMERITUS, KENNEDY SCHOOL OF GOVERNMENT, HARVARD

postal: Kennedy School of Government (L-331B), 79 J. F. Kennedy St. Cambridge, MA 02138
email: lewis_branscomb@harvard.edu
tel: 617 495-1853
fax: 617 495-5776

Dr. Lewis M. Branscomb is the Aetna Professor of Public Policy and Corporate Management emeritus and Emeritus Director of the Science, Technology and Public Policy Program in the Belfer Center for Science and International Affairs at Harvard University's Kennedy School of Government. He is Principal Investigator of the Harvard Information Infrastructure Project and other projects in Technology Policy in the Center.

Dr. Branscomb was graduated from Duke University in 1945, summa cum laude, and was awarded the Ph.D. degree in physics by Harvard University in 1949. In addition to Harvard he has held teaching positions at University of Maryland and the University of Colorado. He is a former President of the American Physical Society and of Sigma Xi, the Scientific Research Society.

A research physicist at the U.S. National Bureau of Standards (now the National Institute of Standards and Technology) from 1951 to 1969, he was Director of NBS from 1969 to 1972. President Johnson named him to the President's Science Advisory Committee in 1964, and he chaired the subcommittee on Space Science and Technology during Project Apollo. In 1972 Dr. Branscomb was named Vice President and Chief Scientist of IBM Corporation and to its Management Committee, serving until his retirement from IBM in 1986. While at IBM, he was appointed by President Carter to the National Science Board and in 1980 was elected chairman, serving until May 1984. In 1987 he was appointed a Director of the Massachusetts Centers of Excellence Corporation by Governor Dukakis of Massachusetts, and in 1991 to the Governor's Council on Economic Growth and Technology by Governor

Weld. He is recipient of the Arthur Bueche Prize of the National Academy of Engineering and the 1998 Okawa Prize <http://www.csk.co.jp/tof/fdne070.html> in information science and telecommunications. Among his other presidential appointments, Branscomb was appointed to President Johnson's Science Advisory Committee, and by President Reagan to the National Commission on Productivity.

He is a member of the National Academy of Engineering, the National Academy of Sciences and a member of the Academy's Council, and the National Academy of Public Administration. In 1993 he was elected a Foreign Associate of the Engineering Academy of Japan.

Richard M., Burnes

CHARLES RIVER VENTURES

postal: 1000 Winter St., Suite 3300 Waltham, MA 02154
email: rick@crv.com
tel: 781 487-7060
fax: 781 487-7065

Rick has been a venture capitalist since 1965, nearly his entire professional life. He was a co-founder of Charles River Ventures in 1970 and has played a major role in the firm's development into one of the nation's most successful venture funds. In recent years, he has focused on investments in the fields of communications and information services.

Cascade Communications (NASDAQ: CSCC), Chipcom Corporation (acquired by 3COM), Epoch Systems (acquired by EMC), Abacus Direct (NASDAQ: ABDR), Summa Four (NASDAQ:SUMA), Concord Communications (NASDAQ: CCRD), Prominet (acquired by Lucent), Aptis (acquired by Nortel) are among the successful investments he has led on behalf of Charles River. More recently, Rick is responsible for investments in AirSpan and Sonus, and holds Board seats at Concord Communications, OMNIA and SpeechWorks.

Apart from venture capital, Rick is a Trustee of Boston's nationally recognized Computer Museum. He is a past Chairman of the Board of The Middlesex School, and a major fund raiser for that institution. Rick holds an AB degree in history from Harvard College and an MBA degree from Boston University. He and his wife Nonnie have three children. Deep-water sailing is a passion of Rick's; in fact, he and his family once sailed across the Atlantic, via Iceland, in the family's 50 foot yawl Adele.

Mark Chalek

DIRECTOR, Office *OF CORPORATE RESEARCH, BETH ISRAEL DEACONESS MEDICAL CENTER*

postal: 330 Brookline Ave. Boston, MA 02215
email: mchalek@caregroup.harvard.edu
tel: 617 632-8559
fax: 617 632-7196

Mark Chalek is the Director of Corporate Research at Beth Israel Deaconess Medical Center (BIDMC), a major teaching hospital affiliated with Harvard Medical School. He is responsible for intellectual property management and corporate relations including research agreements and technology transfer. His twenty-five years of health care entrepreneurial experience includes the founding of the Boston Health Careers Academy, the Boston Biotechnology Innovation Center, and major roles in a number of technology-based startup companies. He was a Vice President for Business Development at Massachusetts Biotechnology Research Institute (MBRI), and the Executive Director of the Boston Area Health Education Center (BAHEC). From 1990-1993, Chalek was Program Director for the City of Boston's Economic

Development and Industrial Corporation, where he was responsible for supporting biomedical industry growth in the City. At BIDMC, Chalek played a key role in the development and adoption of a technology transfer policy which included guidelines for the Medical Center and its employees in taking equity in startup companies. Over the past year, BIDMC has "spun out" several new biotechnology and information technology companies, including Consensus Pharmaceuticals, Convergence Pharmaceuticals, and a new internet-based health care information company.

Robert Charpie

CHAIRMAN, AMPERSAND

postal: 55 William St., suite 240 Wellesley, MA 02481
tel: 781 239-0700 X 101
fax: 781 239-0824

Dr. Robert A. Charpie is Chairman of Ampersand Ventures.

He is a graduate of Carnegie Institute of Technology, where he received his B.S. with honors in 1948, his M.S. in 1949, and his D.Sc. in Theoretical Physics in 1950.

Following graduation he joined Union Carbide Corporation on the staff of Oak Ridge National Laboratory as a physicist. He was appointed Assistant Director of ORNL in 1955 and Director of the Reactor Division in 1958 and, in 1961, moved to Union Carbide's New York Office as Manager of Advanced Developments. In 1963 he became General Manager, Development Department, and in 1964 was appointed Director of Technology. In 1966 he was named President of the Electronics Division of Union Carbide. He became President of Bell & Howell in March 1968 and served in this capacity until joining Cabot Corporation as President and Chief Executive Officer in May 1969. He became Chairman of Cabot Corporation in February 1986 and served until retiring in September 1988.

Dr. Charpie is a Fellow of the American Physical Society, the American Nuclear Society, the American Academy of Arts and Sciences, and the New York Academy of Sciences. He is also a member of the National Academy of Engineering; a Director of Champion International Corporation. He is a Trustee of Carnegie Mellon University and a retired Trustee of Massachusetts Institute of Technology. He holds honorary doctorates from Denison University, Adlerson-Broaddus College, Marietta College and Boston College.

Henry Chesbrough

ASSISTANT PROFESSOR OF BUSINESS ADMINISTRATION, HARVARD BUSINESS SCHOOL

postal: Morgan Hall T61, Harvard Business School Boston, MA 02163
email: hchesbrough@hbs.edu
tel: 617 495-5037
fax: 617 496-4072

Henry Chesbrough is an assistant professor of business administration, and the Class of 1961 Fellow. He holds a joint appointment in the Technology and Operations Management (TOM) and Entrepreneurial Management (EM) areas at the Harvard Business School. He received his Ph.D. in Business Administration from the University of California-Berkeley in May of 1997, in the area of Business and Public Policy. He was a recipient of the Robert Noyce memorial fellowship from the Intel Foundation. He also holds an MBA from Stanford University, where he was a Arjay Miller Scholar. He holds a BA from Yale University in Economics (with an Engineering minor), where he graduated summa cum laude, and was elected to Phi Beta Kappa.

Professor Chesbrough has consulted with leading personal computer hardware, software and information service companies in both the US and Japan on issues of technology and innovation management. Prior to embarking on an academic career, he spent ten years in various product planning and strategic marketing positions in Silicon Valley companies. He worked for seven of those years at Quantum Corporation, a leading hard disk drive manufacturer and a Fortune 500 company. He was Vice President of Marketing and Business Development for an entrepreneurial subsidiary of Quantum, Plus Development Corporation. Previously, he was an Associate Consultant at Bain and Company, in the Boston office.

He lives outside of Boston with his wife, Katherine, and their two daughters. They enjoy hiking in the mountains, skiing, and traveling.

Alexander V. D'Arbeloff

CHAIRMAN, MIT CORPORATION

postal: 77 Massachusetts Ave., Room 5-205 Cambridge, MA 02139
email: alexdarb@mit.edu
tel: 617 253-6700
fax: 617 253-0271

Alex d'Arbeloff, a member of the MIT Corporation since 1989, was named Chairman of the MIT Corporation on July 1, 1997. He has served on MIT's Corporation Development Committee and on visiting committees for the Departments of Economics, Electrical Engineering and Computer Science, and Mechanical Engineering. In addition, Mr. d'Arbeloff has taught classes at the Sloan School of Management, and developed and teaches a course on management and entrepreneurship for graduate students in mechanical engineering. He received the SB in Management from the Massachusetts Institute of Technology in 1949.

Mr. d'Arbeloff is Chairman of Teradyne, Inc., a leading manufacturer of automatic test equipment and interconnection systems for the electronics and telecommunications industries. He cofounded Teradyne in 1960 and served as vice president (1960-1971), president and chief executive officer (1971-1997), and chairman (1977-present). Under his presidency, Teradyne's annual sales increased from $13 million to over a billion dollars in 1995 and again in 1996. Teradyne is now the world's largest producer of automatic test equipment.

Mr. d'Arbeloff also serves as a director of several private companies. He is a director and past chairman of the Massachusetts High Technology Council, and a trustee of the Massachusetts General Hospital and the New England Conservatory.

Mr. d'Arbeloff and his wife, Brit, also an MIT alum, reside in Brookline, Massachusetts.

David Edwards

PRESIDENT, ADVANCED INHALATION RESEARCH INC.

postal: 840 Memorial Drive Cambridge, MA 02139
email: david@airpharm.com
tel: 617 354-6400
fax: 617 354-6444

Dr. Edwards is the co-founder and President of Advanced Inhalation Research, Inc. (AIR). Prior to founding AIR in 1997, Dr. Edwards taught at Penn State University, MIT, and the Technion (Israel). He has published widely in the field of drug delivery and has co-authored two textbooks in the area of applied mathematics (Interfacial Transport Processes and Rheology, 1991, and Macrotransport Processes, 1993). He is the youngest scientist to have received the Ebert Prize of the American Association of Pharmaceutical Scientists three times (1995, 1996, 1999).

Howard Frank

DEAN, ROBERT H. SMITH SCHOOL OF BUSINESS, UNIVERSITY OF MARYLAND

postal: 2416 Van Munching Hall College Park, MD 20742-1815
email: Hfrank@rhsmith.umd.edu
tel: 301 405-2308
fax: 301 314-9120

Howard Frank is Dean of the Robert H. Smith School of Business of the University of Maryland and also professor of Management Sciences at the Smith School. As dean, he is responsible for the school's academic and outreach programs including the undergraduate, MBA, MS and Ph.D. programs, the school's institutional development and its centers for entrepreneurship, executive education, global knowledge, information and supply chain management.

Previously, Dr. Frank was Director of the Defense Advanced Research Project Agency's Information Technology Office where he managed a $300 million annual budget aimed at advancing the frontiers of information technology. Dean Frank was responsible for DARPA's research in advanced computing, communications, software, language systems and human computer interaction. He administered over $1 billion of research contracts with the nation's leading university and industrial researchers. Dean Frank was awarded the Distinguished Service Medal by the Secretary of Defense (the Defense Department's highest civilian honor) for his contributions during his four years at DARPA.

Earlier, he was founder, Chairman and CEO of Network Management Inc., President and CEO of Contel Information Systems (a subsidiary of Contel), President, CEO and founder of the Network Analysis Corporation, a visiting consultant within the Executive Office of the President of the United States in charge of its network analysis activities, and an Associate Professor at the University of California, Berkeley. He is also a Senior Fellow at the Wharton School's SEI Center for Advanced Studies in Management and has served as an Adjunct Professor of Decision Sciences at the Wharton School.

Dr. Frank is a widely recognized as a world-class information technology expert whose accomplishments include fundamental contributions to the development of the Internet. He is also a seasoned information industry executive with over 20 years of senior line management experience as well as experience in the venture capital and mergers and acquisitions fields. He is a member of the board of directors of Intek Global Corporation and has been a member of the board Network General Corporation, Contel Corporation and six other telecommunications and computer companies.

Dr. Frank has been a member of six editorial boards, has been a featured speaker at hundreds of business and professional meetings, has authored over 190 articles and chapters in books. He is a Fellow of the Institute of Electrical and Electronic Engineers and a recipient of its 1999 Eric Sumner Award. He received his MS and Ph.D. from Northwestern University and his BSEE from the University of Miami (Florida).

Mary Good

VENTURE CAPITAL INVESTORS

postal: 400 W. Capital, suite 1845 Little Rock, AR 72201
email: venture@aristotle.net
tel: 501 372-5900

fax: 501 372-8181

Dr. Mary L. Good is the Donaghey University Professor at the University of Arkansas Little Rock and serves as Interim Dean for the College of Information Science and Systems Engineering, She is a managing member for Venture Capital Investors, LLC, a group of Arkansas business leaders who expect to foster economic growth in the area through the opportunistic support of technology-based enterprises. Dr. Good also presently serves on the Board of Biogen, a successful biotech company in Cambridge, Massachusetts; IDEXX Laboratories of Westbrook, Maine; the Lockheed Martin Energy Research Corporation Board of Oak Ridge, Tennessee. She is president-elect of the AAAS, American Association for the Advancement of Science and serves on the Board of Directors of Whatman, plc of Maidstone, England, UK.

Previously, she was the Under Secretary for Technology for the Technology Administration in the Department of Commerce, a Presidential appointment, approved by the U.S. Senate. The Technology Administration is comprised of the National Institute of Standards and Technology, the National Technical Information Service, and the Office of Technology Policy. The Technology Administration is the focal point in the Federal government for working in partnership with U.S. industry to improve its productivity, technology and innovation in order to compete more effectively in global markets. In addition to her role as Under Secretary for Technology, Dr. Good chaired the National Science and Technology Council's Committee on Technological Innovation, and coordinated the Clinton Administration's Partnership for a New Generation Vehicle ("Clean Car") effort.

Dr. Good was senior vice-president of technology at AlliedSignal Inc., where she was responsible for the centralized research and technology organizations, with facilities in Morristown, NJ; Buffalo, NY; and Des Plaines, IL. She was a member of the Management Committee and was responsible for technology transfer and commercialization support for new technology-based activities. This position followed assignments as President of AlliedSignal's Engineered Materials Research Center, Director of the UOP Research Center, and President of the Signal Research Center. Dr. Good's accomplishments in industrial research management are the achievements of a second career; she moved to an industrial position after more than 25 years of teaching and research in the Louisiana State University system. Before joining AlliedSignal, she was professor of chemistry at the University of New Orleans and professor of materials science at Louisiana State University, where she achieved the University's highest professional rank, Boyd Professor.

Dr. Good was appointed to the National Science Board by President Carter in 1980 and again by President Reagan in 1986. She was the Chairman of the Board from 1988 until 1991, when she received an appointment from President Bush to become a member of the President's Council of Advisors on Science and Technology.

Dr. Good holds a B.S. from University of Central Arkansas (Chemistry), and an M.S. and Ph.D. from the University of Arkansas (Inorganic Chemistry).

George Hartmann

CORPORATE RESEARCH AND TECHNOLOGY, XEROX CORP.

postal: 800 Phillips Rd., Mail Stop 0114-20D Webster, NY 14580
email: ghartmann@crt.xerox.com
tel: 716 422-6448
fax: 716 422-6039

George Hartmann is a principal in the Strategy and Innovation group, concerned with technology strategies for Xerox Corporate Research and Technology. He is located in the Wilson Center for Technology, Webster, New York. During 29 years with Xerox, he has contributed to research on novel marking systems, managed groups dealing with development of xerographic technology for new products, and contributed to technical planning. He holds B.S. and Ph.D. degrees in physics from MIT.

Marco Iansiti

ASSOCIATE PROFESSOR OF BUSINESS ADMINISTRATION, HARVARD BUSINESS SCHOOL

postal: Morgan Hall T69, Harvard Business School Boston, MA 02163
email: miansiti@hbs.edu
tel: 617 495-6643
fax: 617 496-4072

Originally a physicist, Marco Iansiti switched to the Business School in 1989. He teaches a second-year MBA course, Starting New Ventures, and is the chair of an Executive Education workshop for general managers, Leading Product Development. He has taught the required first-year MBA course, Technology & Operations Management, and several other executive programs including the Advanced Management Program, and the Program for Management Development.

His research has focused on technology and product development, seeking to answer 'why some people end up being twice as fast and three times as productive as other people.' He conducted a worldwide study of the methods and practices of technology development in the microelectronics and computer industries, comparing 13 American, Japanese, and European companies. The findings were published recently in Harvard Business Review. His most recent book, "Technology Integration: Making Critical Choices in a Dynamic World," was published by Harvard Business School Press in 1997. He is currently involved in two research projects. The first focuses on innovation in software development, investigating the performance of most major organizations, ranging from dominant players like Microsoft to rapidly growing start-ups like NetDynamics. His second project is aimed at understanding the parameters of effective new venture design. As a physicist, Professor Iansiti studied microelectronics circuits—of about 40 atoms in width. He came to HBS "because I thought this would be a really interesting combination of solving real, practical problems that affect companies and staying in academia where I could do research."

He received A.B. and Ph.D. degrees in Physics from Harvard. He does consulting work for several large corporations in a variety of industries, ranging from medical products to financial instruments, and from computers to telecommunications.

Larry Jarrett

Larry Jarrett is a Director of the Industrial Research Institute (IRI). Until recently he chaired the IRI’s "Research on Research" Committee, which has about a dozen active research projects studying the innovation and portfolio management processes. Over the years, he has helped to build decision processes for several new business development activities. Some years ago, he spent a year on a sophisticated Monte Carlo simulation of potential project outcomes to support a major project decision.

Larry has a B.S. in Chemical Engineering from West Virginia University and a Ph.D. from Ohio State; he serves on departmental advisory boards at both universities. He has held a wide variety of R&D positions during his career with Union Carbide Corporation, OSi Specialties, Inc., and Witco Corporation. Over the years, his research teams have received key awards for innovation, including the R&D 100 award from Research and Development magazine and the Kirkpatrick Award from Chemical Engineering magazine. He lives in Connecticut with his wife Linda.

Steve Kent

CHIEF SCIENTIST, BBN SYSTEMS AND TECHNOLOGIES

postal: BBN Technologies 10 Fawcett St., 2nd Floor Cambridge, MA 02138
email: kent@bbn.com
tel: 617 873-3988
fax: 617 873-4086

In his role as Chief Scientist, Dr. Kent provides oversees information security activities within BBN Technology, and works with government and commercial clients, consulting on system security architecture issues. In this capacity he has acted as system architect in the design and development of several network security systems for the Department of Defense and served as principal investigator on a number of network security R&D projects for almost 20 years. As Director of the SPC, Dr. Kent monitors all security related aspects of the service offerings of GTE Internetworking Services. He reports to the President of GTE Internetworking and coordinates with engineering, operations, and marketing to ensure the security quality of offerings. As CTO for CyberTrust Solutions, Dr. Kent provides strategic direction for this certification authority business, reporting to the General Manager of CyberTrust.

Over the last 20 years, Dr. Kent's R&D activities have included the design and development of user authentication and access control systems, network layer encryption and access control systems, secure transport layer protocols secure e-mail technology, multi-level secure (X.500) directory systems, public-key certification authority systems, and key recovery (key escrow) systems. His most recent work focuses on public-key certification infrastructures for government and commercial applications, security for Internet routing, and security for mobile computing.

The author of two book chapters and numerous technical papers on network security, Dr. Kent has served as a referee, panelist and session chair for a number of conferences. Since 1977 he has lectured on the topic of network security on behalf of government agencies, universities, and private companies throughout the United States, Europe, Australia, and the Far East. Dr. Kent received the B.S. degree in mathematics from Loyola University of New Orleans, and the S.M., E.E., and Ph.D. degrees in computer science from the Massachusetts Institute of Technology. He is a Fellow of the ACM, a member of the Internet Society and of Sigma Xi.

Robert Langer

GERMESHAUSEN PROFESSOR OF CHEMICAL& BIOMEDICAL ENGINEERING, MIT

postal: E25-342, Cambridge, MA 02139
email: rlanger@mit.edu
tel: 617 253-3107
fax: 617 254-7091

Professor Langer has been a member of the MIT faculty since 1978. He received the BS degree from Cornell University in 1970 and the ScD in chemical engineering from MIT in 1974.

The only active member of all three US National Academies—sciences, engineering and medicine—Professor Langer's groundbreaking research in polymers dispelled the belief that only some sizes of molecules could be slowly delivered. His discoveries led to the first approaches to the slow release of ionic drugs, peptides and other large molecules such as proteins and DNA.

As a biomedical engineer whose major focus is biomaterials, Professor Langer specializes in controlled drug delivery and tissue engineering. His groundbreaking research in the development of new systems for controlled delivery of pharmaceuticals, specifically his work with polymers, has led to a variety of novel drug delivery systems, including a treatment for brain cancer developed with Dr. Henry Brem of Johns Hopkins University Medical School. This is the first FDA-approved treatment for brain cancer in 20 years and the first polymer-based treatment to deliver chemotherapy directly to the tumor site.

A pioneer in the field of tissue engineering, Professor Langer discovered, with surgeon Jay Vacanti, that synthetic polymers could be seeded with mammalian cells to produce replacement tissue or organs. These discoveries formed a basis for creating new tissues such as artificial skin for burn victims, or cartilage and other tissue for patients suffering from tissue loss or organ failure. Tissue loss and organ failure cost the nation more than $500 billion in health care costs in 1997.

Professor Langer's research has been applied in areas including vaccines, diagnostics, innovative waste disposal technologies, novel therapeutics and tissue repair. In 1997, sales of advanced drug delivery systems in the United States were approximately $14 billion.

In the mid-1970s, Professor Langer began his research into polymers. His numerous breakthroughs have earned him more than 60 national and international awards and honors. He is the only engineer to receive the Gairdner Foundation International Award (49 previous winners subsequently won a Nobel Prize) for discoveries that led to the development of slow drug-release systems, as well as the William Walker Award from the American Institute of Chemical Engineers and the Wiley Medal from the US Food and Drug Administration.

Professor Langer has been a member of the MIT faculty since 1978. He holds more than 320 patents, has edited more than 12 books and has published over 550 articles. He received the BS degree from Cornell University in 1970 and the ScD in chemical engineering from MIT in 1974.

Josh Lerner

ASSOCIATE PROFESSOR OF BUSINESS ADMINISTRATION, HARVARD BUSINESS SCHOOL

postal: Morgan Hall, Room 395 Boston, MA 02163
email: jlerner@hbs.edu
tel: 617 495-6065
fax: 617 496-7357

Josh Lerner is an Associate Professor at Harvard Business School, with a joint appointment in the Finance and the Entrepreneurial Management Units. He graduated from Yale College with a Special Divisional Major which combined physics with the history of technology. He worked for several years on issues concerning technological innovation and public policy, at the Brookings Institution, for a public-private task force in Chicago, and on Capitol Hill. He then undertook his graduate study at Harvard's Economics Department. His research focuses on the structure of venture capital organizations, and their role in transforming scientific discoveries into commercial products. Much of his research focuses on the structure of venture capital organizations, and their role in transforming scientific discoveries into commercial products. (Much of his research is collected in The Venture Capital Cycle, forthcoming from MIT Press.) He also examines the impact of intellectual property protection, particularly patents, on the competitive strategies of firms in high-technology industries. He is a Faculty Research Fellow in the National Bureau of Economic Research's Corporate Finance and Productivity Programs.

David Lewis

VICE PRESIDENT, GENERAL MANAGER OF CHEMICAL PRODUCTS, LORD CORP.

postal: Box 8012 / 111 Lord Dr. Cary, NC 27512-8012
email: david_lewis@lord.com
tel: 919 468-5979x6236
fax: 919 469-5777

David L. Lewis is a Vice President of Lord Corporation and General Manager of the Chemical Products Division. Lord is a major supplier of specialty adhesives and coatings and is a manufacturer of vibration and motion control devices for aerospace and general industrial.

In 1988, Dave Lewis joined Lord Corporation as Director of Corporate Research at the Thomas Lord Research Center in Cary, North Carolina. In 1991, he was named Vice President, Corporate Research. In January, 1993, Dave was appointed as Vice President & General Manager of the Chemical Products Division. Prior to joining Lord, Dave was President of Amspec Chemical (1985-88), a specialty and fine chemicals manufacturer. Prior positions held at Amspec Chemical included Vice President of Operations (1984-85) and Director of Technology (1983-84).

Dave also held the position of Technical Director, Industrial Chemicals (1980-83) for Harshaw Chemical, a subsidiary of Gulf Oil, where his responsibilities included Divisional R&D, Technical Service, and pilot plant operations.

Prior to joining Harshaw, Dave was employed by Diamond Shamrock (1974-80) in both the Electrochemicals Division and Corporate R&D.

Dave received his B.S. in Chemistry from RPI (1970) and his Ph.D. in Inorganic Chemistry from the University of North Carolina-Chapel Hill (1973). He spent one year as a Post Doctoral Fellow with S. J. Lippard at Columbia University (1974).

Jim McGroddy

RET. IBM SR. VICE PRESIDENT RESEARCH; CHAIRMAN, MIQS INC.

postal: 200 Business Park Drive, Suite 307 Armonk, NY 10504
email: mcgroddy@advanced.org
tel: 914 765-1130
fax: 914 765-1131

Jim McGroddy retired from IBM as a Senior Vice President at the end of 1996, after leading its research laboratories from 1989 to 1995. He is currently an advisor to several government agencies, serves on a number of National Research Council panels, and spends time as an advisor and a visitor at a number of universities. He also serves as Chairman of the Board of Integrated Surgical Systems, a public company which is bringing robotic technology to the operating room. He is heavily involved in the restructuring of the local health care system in Westchester County.

McGroddy originally joined IBM in its Research Division in 1965 after receiving a Ph.D. in Physics from the University of Maryland. He earned his B.S. in Physics from St. Joseph's University in Philadelphia in 1958. In his first years at IBM Research he focused on research in solid state physics and electronic devices, and as a result of achievements in these areas was named a Fellow of both the Institute of Electrical and Electronic Engineers and of the American Physical Society. In the 1970-71 academic year he was a Visiting Professor of Physics at the Danish Technical University. Returning to IBM, he served in a number of management positions in research, development and manufacturing before returning to head the Research Division in 1989. He is a member of the U.S. National Academy of Engineering, and serves as Chairman of the Visiting Committee on Advanced Technology at

NIST. He is Chairman of MIQS Inc.; a Director of Paxar, Inc.; Chairman of the Board of Directors of Phelps Memorial Hospital Center; a Director of the HealthStar Hospital Network; a Trustee of the Guglielmo Marconi Foundation; and a Trustee of his alma mater, St. Joseph's University in Philadelphia.

Married to Sheree Wen, he is the father of four daughters and one son. The family resides in Briarcliff Manor, NY.

Terry McGuire

GENERAL PARTNER, POLARIS VENTURE PARTNERS
postal: 1000 Winter St., Suite 3350 Waltham, MA 02451
email: tmcguire@polarisventures.com
tel: 781 290-0770
fax: 781 290-0880

Terry McGuire, based in Boston, is a founder and General Partner of Polaris as well as a General Partner of funds managed by Burr, Egan, Deleage & Co. and Beta Partners.

Terry specializes in medical technology and life sciences companies. He previously served or currently serves on the Boards of Directors at Accordant Health Services, Acusphere, Advanced Inhalation Research, Aspect Medical Systems, deCODE genetics, Inspire Pharmaceuticals, Microbia, and Paradigm Genetics. Terry is the first and only venture capitalist to be elected to the Boards of Directors at both the Massachusetts Biotechnology Council (the organization of Massachusetts biotech and pharmaceutical companies) and MassMedic (the organization of Massachusetts medical device manufacturers). Terry holds an MBA from Harvard Business School, an MS in Engineering from Dartmouth College, and a BS in Physics and Economics from Hobart College. Terry was a Thomas J. Watson Fellow in 1978.

David Morgenthaler

FOUNDING PARTNER, MORGENTHALER VENTURES
postal: Terminal Tower, 50 Public Sq., Suite 2700 Cleveland, OH 44113
email: dmorgenthaler@morgenthaler.com
tel: 216 416-7500
fax: 216 416-7501

David Morgenthaler founded Morgenthaler Ventures in 1968. Over more than thirty years he has built a national reputation for venture capital industry leadership. Between 1977 and 1979 he served as the President and Chairman of the National Venture Capital Association. He recently became the first recipient of NVCA's Lifetime Achievement Award.

Dave is Chairman of Ribozyme Pharmaceuticals and has been a director of many companies, ranging in size from startup to more mature, publicly-traded corporations. Morgenthaler has raised over $600 million, and has funded more than 130 companies (e.g. Apple, Atria, Aptis, Illustra, Microchip Technology, MotherNature.com, Premisys, Software House and Vical). The firm invests in health care, information technology and services, and industrial technology. From 1957 to 1968 he was President of Foseco, Inc., a venture capital-financed manufacturer of specialty chemicals. Previously, he was an entrepreneurial manager with several growth companies.

He is a graduate of Massachusetts Institute of Technology (B.S. and M.S. in Mechanical Engineering).

Ken Morse

MANAGING DIRECTOR, MIT ENTREPRENEURSHIP CENTER

postal: 70 Memorial Drive, Room E51-355 Cambridge, MA 02142
email: kenmorse@mit.edu
tel: 617 253-8653
fax: 617 253-8633

Morse studies the international sales and marketing challenges faced by fast-growing high-tech firms. He has played a key role in launching several MIT-related high-tech startups, including 3Com Corporation, Aspen Technology, Inc., a biotechnology firm, and an expert systems company. He teaches the Entrepreneurship Laboratory course in which engineering, science, and MBA students work in teams on important projects in startup ventures. From 1972 to 1980 he lived in China as founder and president of a trading/consulting firm. From 1992 to 1996 he lived in Brussels as Managing Director of an enterprise software company.

Mark Myers

SENIOR VICE PRESIDENT, XEROX RESEARCH AND TECHNOLOGY, XEROX CORP.

postal: P.O. Box 1600 Stamford, CT 06904
email: mmyers@crt.xerox.com
tel: 203 968-3759
fax: 203 968-3942

Mark B. Myers is senior vice president of Xerox research and technology at the Xerox Corporation in Stamford, Connecticut.

He directs the company's worldwide research, advanced development, technical architecture and corporate engineering. His responsibilities include the corporate research centers in Palo Alto, California, Webster, New York, Mississauga, Ontario, Canada, Cambridge, United Kingdom, and Grenoble, France. Myers has oversight for the 1300 people and $240M investment of the research centers.

Myers reports to Richard Thomas the CEO of Xerox. He is a member of the senior management team that sets the strategic direction and boundaries for the $20B Xerox annual operating and strategic planning process. Myers serves as the co-chair of the R&D Executive Committee guiding the $1.2B RD&E investment for the corporation.

Dr. Myers' technical and management interests involve digital imaging systems and the creation of new technical and business enterprises involving emerging areas of technology. He has been a thought leader in the development of new models for innovation systems in industry and their relationships to universities and government. Myers has broad experience in international R&D including U.S., Canada, Europe, and Japan. He is engaged with select study panels with interests in science and engineering education and government and economic policy.

Mark Myers is a member of the National Research Council's Board on Science, Technology and Economic Policy and the National Academy of Engineering's Task Force on Engineering Education in the U. S. and Japan. He currently co-chairs with Richard Levin, President, Yale University the NRC Science, Technology and Economic Policy (STEP) Board's study of the U.S. Intellectual Property System. He is a member of the Brookings Institution's Study of Intangible Assets with respect to R&D Policy.

Myers serves on the Board of Directors of SDL, Inc. and ScanSoft, Inc. He serves on advisory boards at the engineering school at Cornell and the materials science program at Penn State. He is a member of the Telecommunications Center Advisory Board at Stanford as well as the Laser Energetics Laboratory Board of the University of Rochester. He is chair of the Board of Trustees serving Earlham College and the Earlham School of Religion in Richmond, Indiana and Conner Prairie Museum near Indianapolis, Indiana. He formerly served on the boards of Xerox Canada, Inc., the American Electronics Association, EDUCOM, and the Ontario Research Foundation as well as advisory boards at colleges of engineering at Illinois, Delaware, Rochester and the Center for Imaging Science at RIT.

In 1960, Myers earned a bachelor of arts degree from Earlham College, Richmond, Indiana and in 1964, a doctorate in materials science from Pennsylvania State University.

John Preston

CEO, QUANTUM ENERGY TECHNOLOGIES

postal: 238 Main Street, suite 324 Cambridge, MA 02141
email: preston@mit.edu
tel: 617 497-4831 / 4803

Preston focuses on intellectual property, entrepreneurship, and venture capital. He has founded, directed, and invested in several high-tech companies, and is on the board of directors of Clean Harbors, Quantum Energy Technology, Medical Foods, UCR Inc., and Technology Development Corp. He is owner of Quantum Catalytics. He is also a contributor to *Thinking Ecologically: The Next Generation of Environmental Policy* (Yale University Press, 1997), which outlines an environmental policy for the United States and other nations.

David Ragone

AMPERSAND VENTURE MANAGEMENT CORP./MIT

postal: 55 Williams St. Wellesley, MA 02181
email: ragone@mit.edu
tel: 781 239-0700 X 143
fax: 781-239-0854

David V. Ragone, partner in Ampersand Ventures, began his association with the firm as a member of its Technical Advisory board in 1984. He joined the firm on a more active basis when it spun out of Paine Webber in 1988, and is involved in the full range of Ampersand's activities, with a particular emphasis on the technology/market evaluation of potential investments.

During his extensive academic career he has served as President of Case Western Reserve University, Dean of Engineering at the University of Michigan, Dean of the Thayer School of Engineering at Dartmouth, ALCOA Professor of Metallurgy at the Carnegie-Mellon University. As Senior Lecturer in MIT's Department of Materials Science and Engineering he taught courses in Thermodynamics and Kinetics for the last eleven years and wrote texts for two of them.

Dr. Ragone's broad commercial experience has been a complement to his academic background. He has been Assistant Director of the Hopkins Laboratory at General Atomic in which capacity his research centered on the commercialization of high temperature, gas-cooled reactors as well as on traction batteries. He participated as a co-founder of two venture-backed enterprises in the Boston area, and has served as a Director of more than a dozen public and private companies. Recent directorships have included: Augar, B.F. Goodrich, Cleveland Cliffs, McLough Steel, Sprague, SIFCO, and the Cabot Corporation. He also participated as a member of the Technical Advisory Boards at Volvo, Gulf and Celanese.

His experience with Government and non-profit agencies includes membership in the National Science Board, and in the Department of Commerce Technical Advisory Board. He is currently a Director of the MITRE Corporation and the Henry Luce Foundation.

Mike Roberts

EXECUTIVE DIRECTOR ENTREPRENEURIAL STUDIES, HARVARD BUSINESS SCHOOL

postal: Baker Library 287 Boston, MA 02163
email: mroberts@hbs.edu
tel: 617 495-3795
fax: 617 496-5305

Michael J. Roberts is the Executive Director of Entrepreneurial Studies and a Lecturer at Harvard Business School, where he teaches the Entrepreneurial Management course. Dr. Roberts is responsible for helping to coordinate research and course development efforts around entrepreneurial studies for MBA and Executive Education Programs. He is also the coordinator of activities at the new HBS California Research Center. Dr. Roberts was formerly an Assistant Professor at the school in the General Management area and taught the second-year elective course "Entrepreneurial Management." He also developed and taught the second-year elective course "Managing the Growing Enterprise." Dr. Roberts has worked in a variety of private sector industries. Prior to and during business school, he worked for McKinsey & Co. and Morgan Stanley, respectively. From 1989 to 1991, he served as Director of International Business Development for Cellular Communications, Inc. where he led a successful effort to acquire the second cellular license in Italy. He has also served as Chief Financial Officer of a start-up chain of quick service Italian restaurants, and as Vice President of Business Development for a company in the health care services field. Among the other clients of Dr. Roberts were numerous start up companies as well as AT&T, Chrysler and Ameritech. Dr. Roberts received his BA, cum laude, from Harvard College in economics in 1979. He was awarded his MBA, with high distinction, from Harvard Business School in 1983. He completed his formal studies in 1986 when he received his doctorate, as a Dean's Fellow, in Business Administration from Harvard Business School. He is the author of over 50 case studies on starting and managing entrepreneurial companies. He co-authored New Business Ventures and the Entrepreneur with Howard H. Stevenson and H. Irving Grousbeck, a text book that is used at over 100 graduate business schools. Dr. Roberts is also the author of numerous papers and articles on the challenges of managing the transition from entrepreneurial to professional management.

Richard Rosenbloom

DAVID SARNOFF PROFESSOR, EMERITUS, HARVARD BUSINESS SCHOOL

postal: Cumnock Hall 300 Boston, MA 02163
email: rrosenbloom@hbs.edu
tel: 617 495-6295
fax: 617 495-8736

Richard S. Rosenbloom is the David Sarnoff Professor of Business Administration, Emeritus, at Harvard Business School, where he taught courses on Manufacturing Management, Innovation, and Technology and Competitive Strategy from 1958 to 1997.

Professor Rosenbloom was co-editor of and contributor to Engines of Innovation: U.S. Industrial Research at the End of an Era (HBS Press 1996). Other recent writings include "Rethinking the Role of Industrial Research" (Research Technology Management) , "The Transformation of Industrial Research (Issues in Science and Technology), "Explaining the

Attacker's Advantage" (Research Policy) and "Technological Discontinuities, Organizational Capabilities, and Strategic Commitments" (Industrial and Corporate Change).

During the past 25 years he has served as a Director of nine public companies in the United States, Great Britain, and Israel, including Arrow Electronics Corporation, General Instrument Corporation, Lex Service PLC, and Elscint Limited. He has also been a consultant on R&D management and innovation to a number of industrial firms, including serving as a senior advisor to two Chief Technical Officers of Xerox Corporation.

Among his community activities, he is Treasurer of Hebrew College, Boston, and Chairman Emeritus of Harvard-Radcliffe Hillel.

Rosalie Ruegg

CHIEF ECONOMIST, ADVANCED TECHNOLOGY PROGRAM

postal: ATP, NIST, Building 101, Room A301 Gaithersburg, MD 20899
email: Rosalie.ruegg@nist.gov
fax: 301 975-4776

Rosalie Ruegg is Director of the Advanced Technology Program's (ATP) Economic Assessment Office. Since this public-private partnership program made its first investment in1990, she has led its impact assessment and advised on economic and business issues. Prior to joining the ATP, she was a senior economist at the National Institute of Standards and Technology (NIST); and prior to joining NIST, she was a financial economist for the Federal Reserve Board of Governors, college instructor, and consultant in economics. She is the author of more than fifty publications, and is a frequent speaker on topics relating to economic evaluation and technology. She is the recipient of the Department of Commerce's top two awards, the Gold Medal and Silver Medal; a Woodrow Wilson Fellow and member of Phi Beta Kappa; and a member of the Federal Senior Executive Service. She received degrees in economics from the Universities of North Carolina and Maryland, an MBA with specialty in finance from The American University, professional certification from Georgetown University, as well as extensive management and leadership training. Her professional interests center on evaluation of S&T programs, technology policy, the economics of technological change, and the business of commercializing new technologies.

Scott Shane

ASSOCIATE PROFESSOR, ROBERT H. SMITH SCHOOL OF BUSINESS, UNIVERSITY OF MARYLAND

postal: The Michael D. Dingman Center 4321 Hartwick Rd., suite 300 College Park, MD 20740
email: sashane@starpower.net
tel: 301-403-4290
fax: 301-403-4292

Prof. Scott Shane is Director of Research and Associate Professor of Entrepreneurship at the Dingman Center for Entrepreneurship at the University of Maryland. From 1997 to 1999 he was the Leghorn Career Development Assistant Professor in entrepreneurship at the Sloan School, MIT. From 1993-1996, he was assistant professor and director of the DuPree Center for Entrepreneurship and New Venture Development at Georgia Institute of Technology. The author of over 30 scholarly articles on entrepreneurship and innovation management, Dr. Shane's work has appeared in Management Science, Academy of Management Journal, Academy of Management Review, Strategic Management Journal, Decision Science, Journal of Economic Behavior and Organization, Journal of Management, Journal of Business Venturing. Journal of International Business Studies, and Entrepreneurship Theory and Practice among other journals. His research has been quoted in the Wall Street Journal, Inc.

and Entrepreneurship Magazine. Dr. Shane has consulted to numerous large and small organizations and has taught in executive education programs in Norway, Poland, New Zealand and the United States. Dr. Shane's Ph.D. in applied economics is from the Wharton School of the University of Pennsylvania, after an AB `86 from Brown and MS `88 from Georgetown University. His current research examines how entrepreneurs discover and evaluate opportunities, assemble resources, and design organizations.

F.M. Scherer

AETNA PROFESSOR OF PUBLIC POLICY AND CORPORATE MANAGEMENT, KENNEDY SCHOOL OF GOVERNMENT, HARVARD

postal: 601 Rockbourne Mills Ct. Wallingford, PA 19086
email: fm_scherer@harvard.edu
tel: 610 872-2557

Frederic M. Scherer, Aetna Professor of Public Management and Corporate Management, has focused his research on two main themes: industrial organization economics and the economics of technological change. His publications include several books: International High-Technology Competition; New Perspectives on Economic Growth and Technological Innovation; The Weapons Acquisition Process; Mergers, Sell-Offs and Economic Efficiency; Innovation and Growth; The Economics of Multi-Plant Operation; and Competition Policies for an Integrated World Economy, as well as two textbooks: Industrial Market Structure and Economic Performance; and a newer (1996) textbook, Industry, Structure, Strategy, and Public Policy. During the mid-1970s, he was director of the Federal Trade Commission's Bureau of Economics. His current research is on the economics of the music composition "business" in the 18th century.

Jon Tucker

RESEARCH ASSOCIATE, GEORGE MASON UNIVERSITY

postal: The Institute of Public Policy, MSN 3C6 Fairfax, VA 22030-4444
email: jtucker3@gmu.edu
tel: 703 993-1319
fax: 703 993-1574

About the Advanced Technology Program

The Advanced Technology Program (ATP) is a partnership between government and private industry to conduct high-risk research to develop enabling technologies that promise significant commercial payoffs and widespread benefits for the economy. The ATP provides a mechanism for industry to extend its technological reach and push the envelope beyond what it otherwise would attempt.

Promising future technologies are the domain of the ATP:

- Enabling technologies that are essential to the development of future new and substantially improved projects, processes, and services across diverse application areas;
- Technologies for which there are challenging technical issues standing in the way of success;
- Technologies whose development often involves complex “systems” problems requiring a collaborative effort by multiple organizations;
- Technologies which will go undeveloped and/or proceed too slowly to be competitive in global markets without the ATP.

The ATP funds technical research, but it does not fund product development. That is the domain of the company partners. The ATP is industry driven, and that keeps it grounded in real-world needs. For-profit companies conceive, propose, co-fund, and execute all of the projects cost-shared by the ATP.

Smaller companies working on single-firm projects pay a minimum of all the indirect costs associated with the project. Large, "Fortune-500" companies participating as a single firm pay at least 60 percent of total project costs. Joint ventures pay at least half of total project costs. Single-firm projects can last up to three years; joint ventures can last as long as five years. Companies of all sizes participate in ATP-funded projects. To date, more than half of the ATP awards have gone to individual small businesses or to joint ventures led by a small business.

Each project has specific goals, funding allocations, and completion dates established at the outset. Projects are monitored and can be terminated for cause before completion. All projects are selected in rigorous competitions which use peer-review to identify those that score highest against technical and economic criteria.

Contact the ATP for more information:

- On the World Wide Web: http://www.atp.nist.gov;
- By e-mail: atp@nist.gov;
- By phone: 1-800-ATP-FUND (1-800-287-3863);
- By writing: Advanced Technology Program, National Institute of Standards and Technology, 100 Bureau Drive, Stop 4701, Gaithersburg, MD 20899-4701.