Technology, R&D, and the Economy

Bruce L. R. Smith
and
Claude E. Barfield
Editors

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THE BROOKINGS INSTITUTION
and
AMERICAN ENTERPRISE INSTITUTE
Washington, D.C.

THE BROOKINGS INSTITUTION
1775 Massachusetts Avenue, N.W., Washington, D.C. 20036

and

AMERICAN ENTERPRISE INSTITUTE FOR
PUBLIC POLICY RESEARCH
1150 17th Street, N.W., Washington, D.C. 20036

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Preface

THIS VOLUME presents the papers and commentary emerging from a conference undertaken jointly by the Brookings Institution and American Enterprise Institute for Public Policy Research and held in October 1994 under the sponsorship of the National Science Foundation. The purpose of the conference was to analyze the contributions of research to the economy and to society. The authors have substantially revised their original papers in the light of the conference discussions.

The aims of the project were to assess the role of R&D in the economy, to identify promising new areas of research and analytical approaches, and to contribute to the public debate in a broad sense as the nation seeks to define a new framework for its R&D policies in the post–cold war era.

Since our two research institutions have been engaged in studies of R&D policy for several decades, the collaboration seemed a happy convergence of intellectual challenge and institutional tradition. The contributors brought analytical rigor to the endeavor. We thank them for their gracious approach to the hard work required of the project. Our sponsors from the National Science Foundation were unfailingly considerate and helpful while we struggled to bring the project to conclusion. We are especially grateful to Kenneth Brown of the National Science Foundation for his encouragement and wise advice throughout the effort.

Many staff members from AEI and Brookings assisted at various stages of the project, and we regret that we cannot acknowledge them all. A few must be singled out for special thanks, however. Isabel Ferguson and Michele Van Gilder skillfully organized the conference and handled the complex logistics. Michael Voll served as research assistant and verifier, devoted long hours to reformatting footnotes and complex tabular and graphical material, and generally assisted in preparing the manuscript.

Marjorie Crow provided typing services. Colleen McGuiness edited the manuscript with great care, Carlotta Ribar proofread it, and Julia Petrakis prepared the index.

The individual authors are, of course, responsible for the views and opinions expressed in the papers and comments, which should not be ascribed to the National Science Foundation or to the trustees, officers, or staff members of the Brookings Institution or American Enterprise Institute for Public Policy Research.

BRUCE L. R. SMITH
CLAUDE E. BARFIELD

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CHAPTER 1

Contributions of Research and Technical Advance to the Economy

Bruce L. R. Smith and Claude E. Barfield

IN 1972, when the National Science Foundation (NSF) sponsored a colloquium on research and development (R&D) and economic growth and productivity, the field was in its infancy. Since then, a great deal of additional research has yielded paradoxical results: Knowledge of the linkage between R&D and growth has advanced significantly, but awareness of the complexity of the issues and of the remaining gaps has increased. Further, the need for more communication between scholars and policymakers to improve the quality and relevance of the research is more evident than ever.

During the past three decades, analysts have become convinced that innovation is a continuous, disorderly, and complex process, not a discrete event or series of linear events. The solution to a basic scientific puzzle or the invention of a new product in a laboratory makes no direct economic contribution. Innovation includes not only basic and applied research but also product development, manufacturing, marketing, distribution, servicing, and later product adaptation and upgrading. Classification schemes that describe the innovation process as a straight-line progression inevitably fail to capture its essential messiness and serendipitous nature. The process contains numerous interactions among all stages, leaps ahead, feedback loops, and sudden and unexpected lacunae. For instance, in the development stage, problems may arise that reveal gaps in fundamental scientific knowledge. Chance and seemingly unrelated breakthroughs in basic science may, however, open up new avenues for technological applications. This will likely apply, for example, to superconductivity—the end products may range from computing to energy storage and transportation. Finally, users of the new technology may call for substantial redesign of the initial product. The importance of redesign and modification illustrates a significant, and often overlooked, truth about innovation: At any given time, most R&D involves product improvement and technological refinement, not the creation of new knowledge or revolutionary new products.

Although certain factors are central to technical advance in virtually every sector, important differences among sectors affect the nature and the source of technical change in individual areas. For example, in aircraft and telecommunications, the end products are complex systems composed of many subsystems and components. Technological advances may stem from improvements in individual components or from dramatic system-level redesign; in either case they result from the work of upstream component or materials producers and systems engineers. In some systems technologies, users may be especially important (for example, aircraft and the airline industry). The chemical and pharmaceutical industries, however, manifest a strikingly different model. Innovation in these industries is characterized by the introduction of new products, and much less of the incremental upgrading characteristic of systems technologies is evident. Input suppliers play little role in technical advance, but users may play a large role through the discovery of new applications for a chemical or medical compound.

Because of the complexity of the innovation process, determining precisely the qualitative or quantitative benefits to society from individual research projects has been difficult. Although observers strongly suspect a positive correlation between investment in R&D and economic growth, the exact nature of the relationship has remained elusive. Economists have, however, made various attempts to examine the quantitative and qualitative benefits to society from investment in research. Three approaches have been common. First, economists have constructed models attempting to describe the relationship between R&D inputs and such outcomes as changes in the value added, net profits, output, or rate of growth. Various levels of aggregation have been used, from the individual firm to whole industries, regions, or national economies. Among the studies based upon econometric modeling, two types of analyses are especially noteworthy: growth accounting and individual industry modeling. Robert Solow pioneered the field of growth accounting with research on the rate of technological change in the United States during the first half of the twentieth century.[1] Solow concluded that a residual or unexplained

1. Solow (1957).

portion of U.S. economic growth stemmed from technological advances and that this residual far outweighed changes in capital or labor.

Subsequently, other economists refined and broadened Solow's analysis to constitute a second major approach. Edward Denison's work in growth accounting has had a big impact on the thinking of economists and policymakers.[2] Denison estimated that 20 percent of U.S. economic growth between 1939 and 1957 is accounted for by R&D. Edwin Mansfield attempted to measure the payoff from research through the analysis of technological change in numerous industries. In an initial set of studies, Mansfield found that, for the period 1975–85, about 10 percent of the new products in seven industries—information processing, electrical equipment, chemicals, scientific instruments, drugs, metals, and oil—could not have been developed (or would have faced substantial delays) without the use of recent academic research. He found that the mean time lag in these industries between academic research and first commercial product or process introduction was about seven years. His tentative estimate of the social rate of return to society (that is, the benefits to society beyond those to the industry itself) was 28 percent. He singled out drugs, scientific instruments, and information processing as industries for which academic research had been particularly important. Mansfield's chapter in this book summarizes the highlights of this past research and adds new data and analysis from current research.

A third approach to estimating the payoff from R&D consists of detailed case studies tracing the history of individual products or processes to their research origins. The Department of Defense's (DOD) Project Hindsight (1966) and the National Science Foundation's Technology in Retrospect and Critical Events in Science (TRACES) project (1968) are examples of this type of analysis. Project Hindsight explored the origins of specific defense weapons systems and concluded on the basis of a cost-benefit analysis that the DOD investment in R&D in the period 1946–62 resulted in a high payoff for the department in fulfilling its mission. But the biggest short-run payoff came from applied research and advanced development work supported by the Department of Defense. The weapons projects drew only indirectly on the common pool of basic research that was widely available.

The TRACES project ascertained the evolution of five innovations (magnetic ferrites, oral contraceptives, NCRs, the electron microscope,

2. Denison (1974, 1979, 1985).

and matrix isolation) and described the institution's program and R&D activities that produced the successful outcomes. The TRACES project demonstrated the importance of the basic research knowledge base for each technological advance and showed that a relatively quick (ten years) average payoff for the research occurred. The TRACES project, however, produced a warning on the difficulty of determining the scientific roots of any technological advance, not least because of the serendipitous nature of the entire innovation process. This cautionary note is worth recalling in the context of the present debate over the role of basic research in U.S. competitiveness.

The current state of research is thus marked by both progress and continuing intellectual puzzles and gaps in knowledge. In the general context of greatly increased public interest in the relationship between technology and economic advance, it seemed timely to undertake a major review of the subject. Independently, the Brookings Institution and the American Enterprise Institute for Public Policy Research (AEI) discussed with the National Science Foundation the idea of an anniversary conference to revisit the major themes of R&D policy and the economy discussed at the 1972 NSF colloquium. The NSF suggested that Brookings and AEI might fruitfully combine forces in such a venture. We happily agreed and submitted a joint proposal to the NSF for the conference.

The Aims of the Conference

A first goal of the conference was to assess the current state of knowledge and to note the advances made in the intervening years since the initial colloquium. In particular, we wanted to assess the more recent contributions given the heightened awareness of the complexity of the R&D process, the globalization of technology and of financial markets, and the increased international competition in many high-technology sectors. What were the implications of the new findings for public policy and for corporate competitive strategies? But we were also interested in the broader implications of the contributions of research in areas beyond economics, such as education, health, environment, and quality of life. A further aim of the conference was to identify the priorities for additional research on the most important questions.

To accomplish these aims, a broad group of researchers, policymakers, educators, and corporate leaders was convened to review the research findings and to discuss the full range of issues. The need to examine the nation's R&D strategy from a broad perspective appeared both timely and compelling. For most of the postwar period, the nation's R&D effort has been focused on national security, space, energy, and health. Five years after the fall of the Berlin Wall in 1989, policymakers have not fully assessed the implications of the changed national security environment for the size and the nature of the U.S. scientific and technology establishment. The defense R&D budget has been reduced only modestly even though troop levels have declined substantially (and probably will decline even further to levels below the 1.6 million base force announced by the Bush administration or the bottom-up projections of the Clinton administration). Basic changes in the defense R&D system thus appear likely, especially as smaller troop levels put more pressure on the military to maintain a high state of readiness.

New goals have surfaced on the public policy agenda, including the desire to deregulate the economy, combat crime, reduce health care costs, and lower the budget deficit. These objectives may call for redirections in the country's R&D investment as some technology-intensive missions decline in importance. The Clinton administration came to office in January 1993 with pledges to reorient the balance of the national effort between civilian and military research, to augment the resources devoted to various National Institute of Standards and Technology (NIST) programs, to promote "dual use" technology through the Defense Department's Technology Reinvestment Program (TPR), to complete the information superhighway, and in general to link technical activities and research goals more closely to broad social goals. Meanwhile, in Congress, the then-chairman of the Senate Committee on Commerce, Space and Science, Barbara Mikulski, D-Md., was pushing hard for a 60 percent numerical target for strategic research in the total R&D effort of the National Science Foundation. The nation's continuing fiscal crisis made more likely further pressures for budget cuts in the area of discretionary spending, potentially affecting R&D budgets and forcing agencies to rationalize their research strategies. The Government Performance and Results Act of 1993 (P.L. 103-62) required all agencies, including research-supporting agencies, to submit measurable goals and performance criteria. In short, for a host of reasons we felt that the time had come to revisit the subject of research priorities, strategies, and contributions to social and economic goals.

Major Themes

Each chapter in this book begins with the implicit or explicit assumption that the current research system is heavily needs-driven and not only curiosity-driven. The relevant concerns are not only how to turn the nation's technical enterprise toward more concrete and practical ends, as the problem is depicted in some of the public debate, but also to judge the contributions of a large and heavily targeted national research investment, to assess the differences in social and private rates of return from research, and to throw light on the most effective institutional roles for the various types of research performers as well as the appropriate mix of public and private support for research. An overall national strategy will almost surely involve some mixture of public and private support for research, but the level and the specific thrust of that support remain at issue. How much to articulate central priorities rather than to follow a more decentralized strategy of setting social goals and linking technical effort to those goals is a related issue.

Harvey Brooks traces the tensions between research autonomy and social direction back to the 1930s debate in Great Britain between Michael Polanyi and J. D. Bernal. Polanyi stressed the need for scientific autonomy and self-governance if research were to contribute most creatively to society, while Bernal foresaw greater need for the large-scale mobilization of research to achieve explicitly formulated social goals. Brooks views the tensions between the two approaches as healthy and unavoidable. Brooks notes that the usual distinction between basic and applied research mixes two separate issues: the matter of top-down versus bottom-up research management and the roles of generalists versus specialists in the choice of research goals. Various combinations are possible that can blend central direction with specialized expertise in the choice of research projects and in efforts to solve social problems. He devotes attention to the educational role of scientists and sees the need for emphasizing greater diffusion of knowledge instead of only knowledge generation in scientific effort and in federal research policies.

Richard R. Nelson and Paul M. Romer do not find cause for alarm in a more explicit orientation of the universities toward the fulfillment of social need. They are uncomfortable with the tendency of some scientists to insist almost as a matter of principle on the nonutility of their research. Nelson and Romer point out that some of the most interesting scientific discoveries and the most significant applications have come from work of a problem-solving character that was neither wholly curiosity-driven nor wholly needs-driven. Echoing a theme from Donald E. Stokes, they argue that scientific work in Pasteur's quadrant is a highly appropriate and common model.[3] Louis Pasteur was a scientist intensely interested in fundamental scientific concepts but whose work was heavily influenced by practical problems arising in medicine and in the industries of his day. Nelson and Romer caution that a strategy pushing too strongly or exclusively for either targeted strategic research or investigator-driven effort would be mistaken.

On the contribution of technical advance to economic growth, Nelson and Romer propose a useful distinction between hardware (all the nonhuman objects used in production, including capital, goods, structures, land, and raw materials), software (knowledge or information on paper, data on a computer disk, images, drawings, blueprints, and so on), and wetware (the "wet" computer of the human brain, human capital, tacit knowledge, management practices). Their analysis draws attention to the interplay among the three types of inputs and attempts to assess the significance of each. The relative importance of the accumulation of physical and human capital in economic growth is underscored, unlike earlier estimates stressing the importance of technology. In addition, Nelson and Romer propose revisions in intellectual property laws for software as a means to spur innovative activity.

Michael J. Boskin and Lawrence J. Lau also attach importance to improvements in capital stock as a central factor in productivity growth for the G-7 nations in the postwar period. Although their analysis of the sources of U.S. economic growth finds a smaller residual effect directly attributable to R&D than earlier studies, they caution that conventional measures may underestimate the impact of R&D as it interacts with other factors and achieves synergy. Perhaps the most important finding of the Boskin and Lau study is the complementarity between R&D and human capital, human capital and tangible capital, and technology and tangible capital that produces significant interactive effects. Thus, for example, the benefits to the economy of R&D in improved microprocessors will depend on the amount of tangible capital that can make use of the faster microprocessors and on the human capital able to use the computers and the other forms of technology, such as advanced software, that are available and that enhance the capabilities of the improved systems. The current state of the econometric art creates, however, a continual need to refer to industry case studies as a kind of benchmark. Hence, Boskin

3. Stokes (forthcoming).

and Lau are brought full-circle to the conclusion that "R&D is important to economic growth, but just how important is a question economists are not yet fully able to answer."

But Bronwyn H. Hall is perhaps the most skeptical about the current state of knowledge. Arguing that in some instances analysts are unable to specify what would be an adequate test of their propositions, she discusses the refractory methodological difficulties of assessing the impact on industrial innovation and company behavior of university-based and government research. From background research to more focused R&D in the individual firm and then to the marketplace is a long chain of inferences, assumptions, and interpretative leaps. She acknowledges that overwhelming evidence exists that some types of research produce some positive externalities and benefits for society, but she insists on the need for a more satisfying rationale for appropriate levels of R&D investment and on what types of government R&D investment are most productive and necessary.

In contrast, Mansfield presents a decidedly more upbeat appraisal that partly reflects his reliance on industry case studies instead of econometric estimation of production functions. In presenting his paper, he engagingly stated his attachment to an analytical framework "where you can call each data point by its first name." The weight of the empirical evidence is clearly in favor of a high rate of return from research. High rates of return from research are more clearly evident in certain industries than in others, but across the board the social rates of return from research are substantial. Altering assumptions and imposing the most conservative methodological assumptions did not shake his overall conclusions that society benefits heavily from research investments.

Assessing private rates of return is more of a problem because technology diffuses rapidly from firm to firm, sector to sector, and nation to nation. Mansfield devotes a section to explaining how Japanese firms, through heavy investments in process technologies, have been able to make effective and rapid use of research done elsewhere. Whether a firm or country can pursue exclusively or continue to rely mainly on a follower and borrower strategy vis-à-vis technology is more doubtful, however. Investments in technical training, applications, applied research, and testing and equipment seem increasingly necessary for the absorption of technology.

Mansfield attempts to delineate the links between basic research done in universities and technical advance in industry. He finds substantial evidence that basic research has contributed to industrial innovation. Data obtained from seventy-six private companies in seven industries show that about 10 percent of new products and procedures could not have been developed—at least without great delays—in the absence of recent academic research. Factors that encourage the industries to rely on specific academic institutions include the quality and reputation of the faculty, R&D activity in the relevant field, and the convenient geographic location. Although he does not believe that any single pattern of university-industry relationship should apply to all universities, Mansfield advocates closer university-industry linkages in general and sees good reasons for those universities with traditional close ties to certain industries and firms to go farther in this direction. Mansfield identifies some key areas for needed further research. Firms within the same industry often show significant differences in the degree to which they rely on academic research in their new products and processes. Why is that? Considerable reliance also has been placed on some academic researchers as consultants instead of on formal research grants to universities, and this role of the consultant is another promising area for future research.

Most out of step with her colleagues in some respects is Susan E. Cozzens on the subject of quality of life returns from basic research. She frames her topic in unconventional terms by focusing primarily on the process of research and on the values that guide individual researchers, not on research outcomes or traditional institutional relationships. Drawing on biomedicine as a case in point, she studies the issue of the quality of life returns from the pattern of national investments in medical education and research from a one-hundred-year and then from a fifty-year perspective. The Flexner medical reform movement early in the twentieth century produced a more centralized system of training for doctors in fewer institutions of higher quality, but they had the negative effect of leaving rural areas and certain regions of the country without access to local doctors. The gains in health and life expectancy during this period may have been produced more directly by improvements in public health and sanitation measures than by the discovery of vaccines in the new scientific medical schools. Similarly, the great expansion of research support that boosted the National Institutes of Health (NIH) after World War II produced much new knowledge but widened the gap between research and medical practice and did not enrich undergraduate curricula in the universities. Cozzens calls for a process that would incorporate a more explicit awareness and discussion of the broad goals to be accomplished by research in advance of performing the research, and she seeks a form of dialogue between the researchers and the end-users or consumers of the research before new research programs are undertaken. Researchers could then, in her view, internalize a wider set of values and guide their specific research decisions by a more socially aware and relevant metric. She does not explicitly state what criteria government funding agencies might use to evaluate research processes instead of outcomes and whether her approach would strengthen or weaken popular support for science.

The contributions to this volume and the conference discussions only touched on the underlying political climate for the support of research. Some of the attendees at the conference presumed a broad base of support existed for research and wanted to debate the advantages and disadvantages of expanding the university mission to include a more direct contribution to industry and to the economy. The tacit assumption was that universities could enlarge their mission if they were willing to take on the additional burdens of close industry ties but could otherwise simply retain their current roles and levels of research support. Other participants viewed new civilian technology missions as essential to offset inevitable reductions in defense research expeditures. Some doubted, however, whether the defense reductions would have the same impact on universities as on the government's own laboratories and the defense industry. Other participants believed that the push for strategic research that was manifested in the Mikulski proposal could be only the tip of a larger iceberg and that society seemed bound to impose stricter standards of relevance on the scientific community, whatever happened in domestic politics and whatever the level of defense spending. Most participants saw the familiar boundaries between the government laboratories, industry, and the universities as blurring, and thus believed that new opportunities as well as new vulnerabilities could emerge in the post–cold war research system. Government labs, industrial firms, and universities could become interlinked in new cooperative ways but might also find themselves in some ways more competitive than before.

The political earthquake of the 1994 midterm elections underscores the changing circumstances affecting the research system. Republicans were swept into power in the U.S. Senate and House of Representatives, controlled eight of the nine large-state governorships, and achieved parity overall in state legislative seats. The Clinton administration technology strategy to augment the nation's civil technology efforts seemed to be in jeopardy as a result of the election. The Republican House of Representatives had by summer 1995 cut back sharply the administration's budget request for the Defense Department's Technology Reinvestment Program and for the Commerce Department's Advanced Technology Program (ATP). The Senate partially restored some of the cuts.

Republican and Democratic perspectives on R&D have roughly mirrored their party differences generally. Republicans in Congress in general have strongly supported basic research but have been skeptical of federal programs to aid civil technology. They have been less interested in a "dual use" role for defense R&D and seem to prefer a return to a heavy reliance on explicit national security objectives in defense R&D support. Republicans are also more disposed toward cost-benefit and risk assessment approaches in environmental regulation than their Democratic colleagues. Moreover, Republicans, in general, have favored smaller government and tighter controls on discretionary as well as entitlement spending. Space, defense, energy, and health research might be differently affected, but to the extent that government agencies shrink in size, the research supported by the agencies will also shrink. The pressures toward performing more strategic research—as called for by Senator Mikulski—will likely be diminished by the Republican ascendancy.

The Clinton administration and the Congress are stalemated over federal spending priorities in coming years, but both sides have agreed to balance the federal budget by the year 2002, and this consensus will almost certainly result in strong downward pressure on the discretionary elements of the federal budget, most particularly on R&D. While a great deal of attention has focused on the Republican Contract with America, which, over the next seven years, according to calculations of the American Association for the Advancement of Science, would result in a 30 percent reduction in the federal civilian R&D budget, the Clinton administration's agreement also to balance the total federal budget within seven years means that it will be forced to accept reductions of the same magnitude. In the face of these realities, the scientific community seems torn between keeping a low profile or aggressively mobilizing to defend and justify itself.

The dilemma raised by Vannevar Bush in 1945 may yet become a central issue for debate in U.S. science policy.[4] Bush called for a large central research foundation to handle all aspects of federal research support and technology development. He was convinced on the basis of his wartime experience that an operating agency would not support adequately the technical mission and long-term research. Short-run pressures would inevitably force the operating department to cut back on long-term research and technology development in favor of near-term priorities. Bush wanted the whole spectrum of R&D support to remain in the

4. Smith (1990, chapter 3).