The R&D Portfolio: A Concept for Allocating Science and Technology Funds

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The tremendously productive system of research and development (R&D) that the United States has enjoyed since World War II is undergoing major changes in response to the emergence of the information age, the end of the Cold War, global economic competition, pressure to reduce government budgets, and changes in the way science itself is carried out. The nation's science and technology (S&T) enterprise is having to adapt to this uncertain future and determine how to pursue research opportunities (*1*) while facing reduced or negative funding growth (*2*).

There is great concern about the future of S&T in the governmental and industrial sectors and in the university research community. Recent reports have recommended new goals, procedures for setting priorities, and criteria for allocating S&T funding (*3*–*5*). One theme is the need to treat funding as an investment rather than as a current consumption item in budgetary decision-making (*6*).

The Portfolio Approach

The investment nature of R&D and the high regard of the public for scientists and research (*7*) will help sustain government funding of S&T, but strong competing demands for federal dollars will require the S&T enterprise to restructure itself and be more active in justifying support. We propose that the concept of R&D as an investment be extended by introducing the portfolio concept from financial investment theory into decision-making on the allocation of funds for S&T. Financial investors, like R&D decision-makers, have to balance opportunity and risk to obtain the best overall return. Generally, the greater the potential return from an investment, the higher the risk involved. Most investors spread their risks by allocating their assets to a portfolio of diversified investments. The goal is a portfolio that maximizes net return at whatever level of risk the investor is willing to assume (*8*).

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Allocating S&T funding is analogous to financial investing. The success of any given research project is uncertain, no matter how carefully it is evaluated. Projects addressing fundamental questions are often the riskiest: Success, in the form of useful but unforeseen applications, may not be realized until years later. History also shows that important advances in one field sometimes come from apparently unrelated work in another field. In this situation of unpredictability, diversification is important for maximizing results. If it is not known where breakthroughs will be made or when advances will occur, then it is prudent to invest in a broad portfolio of activities (*9*).

The portfolio concept draws attention to the need to diversify not only among fields but also among other components of a successful S&T enterprise. Federal R&D policy is often judged on whether the amount of funding devoted to basic research or universities is increasing or the number of new and competing research grants is going up. A portfolio approach allows a broader perspective on what is needed to maintain a productive S&T enterprise. Because the discovery process is rarely linear, a healthy portfolio will have a mix of basic research, applied research, and development activities (*10*). Some activities will be driven by disciplinary research agendas and by interdisciplinary problems, some by federal agency missions, and some by emerging high-priority national problems. The portfolio will also balance the funding of research projects with the cost of related infrastructure and overhead activities, such as maintaining and upgrading facilities and equipment, and training future researchers.

Within a disciplinary portfolio, one would invest in all modes of research (the precise mix varying from field to field and, within fields, over time in response to changes in R&D results or national goals) to take advantage of alternative ways of attacking problems and the potential interaction and feedback that may result from multiple approaches. This approach moves beyond the traditional paradigm that "big science" and "little science" are in conflict rather than complementary.

The portfolio approach is especially important when funding is flat or shrinking. It recognizes the importance of investing in fundamental and long-term research because that will produce the new knowledge that will underlie future economic growth. Normal behavior when resources are tight or circumstances are uncertain is to play it safe by funding sure bets. Today, for example, peer reviewers tend to fund projects that promise incremental but safe results rather than truly innovative but risky ideas; industry is shifting to more applied, short-range research. The balanced portfolio directed to the future must ensure that an adequate proportion is devoted to fundamental research at each level (including the national level, taking into account the contributions of industry, universities, foundations, and other nongovernmental sources of support for basic research).

An explicit portfolio approach to funding S&T will also help ensure diversity among research performers and funding agencies. Such diversity stimulates creative research ideas and increases the chances that they will find support. The R&D programs can choose from a panoply of performers, including in-house laboratories, universities, industrial laboratories, or privately operated national laboratories and R&D centers. At the same time, if an agency finds a research proposal to be too risky, the researcher can take it to other agencies or private funders for consideration.

The portfolio concept can also justify continued or even expanded federal R&D support within a flat or shrinking discretionary budget. Portfolio theory has implications beyond allocating funding within the R&D budget. It permits a dual strategy. A strong case can be made for investing an adequate share of the federal budget in R&D in competition with the many other legitimate demands on the beleaguered federal budget. Simultaneously, the S&T enterprise can be restructured through the portfolio approach to meet the new conditions, drop obsolete activities and programs, and pass over lower-priority activities to fund the most important ones.

Implementing the Portfolio Concept

The portfolio approach does not require radical changes in the structure or procedures of decision-making on R&D budgets. In the government, for example, R&D budgeting is decentralized and builds from the bottom (where knowledge about opportunities and needs is highest) up. The process already incorporates the

diversity of approaches that has given the U.S. S&T enterprise its strength. It also includes broad input from researchers, who serve on advisory committees and in government R&D leadership positions, and from the reports and testimony of science and engineering organizations.

The current R&D budgeting process tends to focus on incremental changes from year to year. That approach works well when budgets are growing because new research opportunities can be funded without reallocation of resources from existing programs; however, it perpetuates a set of programs and institutions aimed at problems of the past. In a period of decremental budgeting, there will be strong pressures to distribute cuts proportionally across all programs rather than cut lower-priority activities in order to fund research agendas for the future.

The portfolio approach involves more a change in viewpoint or perspective than in process. It tries to build on the traditional ad hoc incrementalism of the budget process by expanding the scope of factors taken explicitly into account at each level of decision-making. It calls for decision-makers to (i) look beyond next year's budget request; (ii) ensure diversity by providing adequate investment in each field (and, within fields, funding multiple approaches to research problems and multiple modes, in terms of individual investigators, small groups, research centers, and large-scale facility-based groups); (iii) achieve an appropriate mix of all components of a successful research program (projects, facilities, equipment, and people); (iv) consider what other sectors are funding, including industry; (v) consider cooperative efforts with researchers in other countries; and (vi) see that each program, no matter how focused on a practical goal, invests adequately in long-term fundamental research.

Adopting the portfolio perspective will not be easy. At a time when funding will be very tight, it calls for (i) peer reviewers to be willing to recommend innovative but risky proposals; (ii) program managers to approve an adequate share of innovative proposals, fund individual proposals sufficiently rather than squeeze them to fund more proposals, and invest in longer-term activities such as equipment and facility upgrades that are easy to defer; (iii) agency heads to protect research missions, not existing personnel and facilities; (iv) decision-makers in the White House to take action when the bottom-up decision-making process is not balanced from a national point of view; and (v) Congress to take a more integrated view of federal S&T and its adequacy in the overall national budget portfolio.

Can it be done? Some field-of-science surveys conducted for federal agencies by the National Research Council have approximated a portfolio review. A good example is the recent survey of astronomy research (*11*). The survey committee examined all aspects of its discipline and gave some infrastructure needs priority over some research opportunities. Its success was based on extensive consultation within the relevant scientific disciplines, strong leadership, and a willingness of participants to agree on a clear ordering of priorities in which research opportunities and all other future needs (instrumentation upgrades and training, for example) were placed in a single framework.

Historically, there has been a reluctance to make cross-field judgments because there is no purely scientific algorithm for comparing fields. Public officials, however, routinely have to make decisions about R&D programs that affect the distribution of funds among fields. The portfolio approach provides a good way to consider the appropriate mix of fields in any programs or agencies. The portfolio perspective, which includes broad input from technical experts, could better inform decision-making at various levels of the budget process by highlighting certain issues: how programmatic decisions would affect fields, whether there is sufficient diversity of investment among fields, and whether emerging or rapidly changing fields are identified and supported appropriately.

The Will to Implement Change

The most difficult challenge is finding the collective will to set priorities in our decentralized science enterprise. Some scientists argue that the S&T community should not set priorities or identify potential budget reductions because doing so would create easy targets for budget cutters. If other interest groups are not volunteering reductions in their funding and if R&D is demonstrably in the national interest, why should the S&T community aid in cutting their own funds? In a rational political world, there would be potent advocates in public life for R&D as there are for other programs, but the numbers supportive of R&D are thinning as a result of retirements and recent elections. Reductions will be made in federal R&D appropriations under any conceivable scenario now under political discussion. The S&T community will be better off helping to guide the future direction of R&D rather than being on the sidelines, leaving the future to the politics of decremental budgeting.

The S&T leaders of World War II and the years immediately following took the lead in creating an enduring federal S&T system. A similar determination—a will—to adapt the U.S. S&T system to the future must be the watchword today, and it must be led by the scientific community. Only in that way can the strengths of the U.S. S&T system be maintained. Unless the process of priority setting begins, and is done wisely and well, the full potential of S&T contributions to the work of the next century will not be realized.

REFERENCES AND NOTES

1. For a review of current fundamental research questions, see C. Vest, "Report of the President for the Academic Year 1994–1995" [online], Massachusetts Institute of Technology. Available at http://web.mit.edu/president/communications/rpt94-95.html
2. The federal R&D appropriation for fiscal year (FY) 1997 is 4.0% higher than that for FY 1996, but after 3 years of flat R&D budgets, the FY 1997 total is still less than that for FY 1994 after inflation (by 2.0%) [AAAS, *FY 1997 Appropriations for R&D* [online], 6 November 1996 (*12*)]. The Administration and Congress project cuts in federal support of civilian R&D of 19 and nearly 23%, respectively, in real terms by FY 2002 [AAAS, *Summary Table: Projected Federal Spending on Nondefense R&D, FY 1995–2002* [online], 12 August 1996 (*12*)]. Those outyear figures are not final and are subject to revision, but the trend toward slower if not negative budget growth is evident.
3. National Academy of Sciences, National Academy of Engineering, Institute of Medicine, National Research Council, *Science, Technology, and the Federal Government: National Goals for a New Era* (National Academy Press, Washington, DC, 1993).
4. ———, *Allocating Federal Funds for Science and Technology* (National Academy Press, Washington, DC, 1995).
5. Council on Competitiveness, *Endless Frontier, Limited Resources* (Council on Competitiveness, Washington, DC, 1996).
6. Most studies of the economic impacts of R&D spending find substantial rates of return, especially from fundamental research [National Science Board, *NSB 96-21* (1996), chap. 8].
7. *Ibid.*, chap. 7.
8. As one analysis [P. L. Bernstein, *Capital Ideas: The Improbable Origins of Modern Wall Street* (Free Press, New York, 1992), p. 44] of the original (1952) article on portfolio theory explains, the portfolio approach formally reconciles two traditional adages for investing: "Nothing ventured, nothing gained," and "Don't put all your eggs in one basket."
9. R. E. Gomory, in *AAAS Science and Technology Policy Yearbook: 1995*, A. H. Teich, S. D. Nelson, C. McEnaney, Eds. (American Association for the Advancement of Science, Washington, DC, 1995), pp. 5–17.
10. As used here, federal R&D refers to investment in the creation of new knowledge and technologies, which has been called the federal S&T budget (*4*).
11. National Research Council, *The Decade of Discovery in Astronomy and Astrophysics* (National Academy Press, Washington, DC, 1991); J. N. Bahcall, *Science* **251**, 1412 (1991).
12. Available online at http://www.aaas.org/spp/dspp/rd/rdwwwpg.htm.

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