Making Choices: Do We Have a Policy for Allocation of R&D Funds?

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In his address to the 2007 Science & Technology Policy Forum, Jack Marburger, President George W. Bush's Science Advisor, observed that "the science posture of a nation expresses itself in the myriad activities of its scientists and engineers, students and technicians – activities that may or may not sum to a coherent or effective whole. No law of nature or of politics guarantees that this real-life science *posture* will reflect a sensible science *policy*."[1] Whether what emerges from the ongoing interplay of bottom-up program plans, federal advisory committee reports, National Research Council studies, research community advocacy and the top-down PCAST and NSTC reports, OSTP/OMB priorities memos, Secretarial initiatives, authorization bills, or appropriation report language represent a reasoned science policy or not, it is, as Dan Sarewitz claims, "not only axiomatic but also true that federal science policy is largely played out as federal science budget policy."[2]

The American Academy of Arts & Sciences' 2014 report, *Restoring the Foundation: The Vital Role of Research in Preserving the American Dream*, lays out the paradigmatic argument for growing research funding: [1] The predominant driver of GDP growth over the past half-century has been scientific and technological advancement; [2] Virtually every new technological product is traceable to a research

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1 Marburger III, J. H. (2015). *Science Policy Up Close*. Harvard University Press, p.182.

2 Sarewitz, D. (2007). "Does science policy matter?" *Issues in Science and Technology*, 23(4), 31.

discovery, often one pursued with no application in mind; [3] Research is the lifeblood of a high-tech economy and plays a critical role in the economic and personal well-being of most citizens; [4] America is permitting its highly successful system to atrophy; [5] There is a deficit between what America is investing and what it should be investing to remain competitive, not only in research but in innovation and job creation; and [6] It is important when making allocation decisions to distinguish between spending for present consumption and spending for investment.[3,4] Even if these assertions were verifiably true, they are at least not damaging if you are the staffer attempting to raise the topline allocation to research programs and not at all helpful in allocating the hard-won marginal dollar despite the vigor and frequency with which they are made.

This esteemed group correctly diagnoses the fact that "few mechanisms currently exist at the federal level to enable policy-makers and the research community to set long-term priorities in science and engineering research,"[5] And yet, they make no recommendation for setting long-term priorities, only that research should be a priority over other spending needs. The group calls for setting a real growth rate target of at least 4 percent in the federal investment in basic research, implementing budgeting process reforms (multiyear appropriations, strategic capital budgets, and 5-year budgeting), reforming the administrative demands placed on grant recipients, and strengthening cooperation between government, industry, universities and national labs – actions for which the transactional hurdle is high and the benefit for the government is uncertain.

When it comes to making choices within subfields, we have well developed methods in wide-spread use – the decadal survey in astronomy or long-range plans in high energy physics are some of the clearest

[3] Augustine, N. R., & Lane, N. F. (2014). *Restoring the Foundation: The Vital Role of Research in Preserving the American Dream*. In American Academy of Arts and Sciences. pp. 13-18.

[4] As an aside, research advocacy over the decades has mastered a narrative of complexity and serendipity that reinforce this message to the political bodies: "Hands Off!" How can one set priorities if not even the experts could have predicted that Einstein's Theory of General Relativity would essential for keeping Global Positioning System satellites from drifting out of synchronization or that the World Wide Web would have been developed by a particle physicist at CERN? The rhetorical devices of science funding, e.g., a supreme confidence that funding is in a perpetual state of decline, the fragility of the research enterprise, willful political naiveté on the part of the researchers, hopeless incomprehension by the public, and hostility by political bodies also merit serious critique.

[5] Augustine, N. R., p. 8.

examples, yet the research community has never been comfortable with a policy for choosing between dissimilar research opportunities. We have short list of rules of thumb that memorialize these détentes: The NIH budget should be split 90% external and 10% internal performers or defense R&D's share should be approximately half of the federal portfolio. As trite as these may appear, the absence of a widely accepted truce can subject research programs to the perennial clash between competing visions of society's future. No better examples can be found than the energy technology programs where one analysis found a one in three chance that these programs would receive a funding change (increase or decrease) greater than 27% in any given year.[6] The only thing you know for certain under such a scenario is that effective technology development is all but impossible.

The problem isn't that any of the American Academy's recommendations are unwarranted, it's that they don't address the annual challenge OMB faces setting priorities in the *ex ante* allocation of budgetary resources. One fundamental challenge is to understand how much research is enough. The American Academy's suggested metric, "the number of capable researchers whose work is adequately funded,"[7] is politically non-viable. Moreover, we've run that experiment with the policy to double the NIH budget, and the outcome was in fact the opposite of what was advertised. In a system that assumed perpetually growing R&D funds allocated through a system of unfettered competition, large funding increases led to falling success rates for proposals, excess conservatism in what was funded, and a Malthusian oversupply of biomedical researchers.[8] As proof of the aversion to making choices, the year-to-year percentage change in the individual budgets of the 21 current NIH institutes have moved in lockstep 63% time over the 10 years since the 1998-2003 doubling period.[9]

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6 Narayanamurti, V., Anadon, L. D., & Sagar, A. D. (2009). *Institutions for energy innovation: a transformational challenge*. Energy Technology Innovation Policy research Group, Belfer Center for Science and International Affairs, Harvard Kennedy School.

7 Augustine, N. R., p. 17.

8 Alberts, B., Kirschner, M. W., Tilghman, S., & Varmus, H. (2014). Rescuing US biomedical research from its systemic flaws. *Proceedings of the National Academy of Sciences*, *111*(16), 5773-5777.

9 NIH Almanac, appropriations tables. [http://www.nih.gov/about-nih/what-we-do/nih-almanac/appropriations-section-1]. The number is 78% if you allow year-to-year differences no greater than ±0.1% during the 1998-2003 period. By comparison, only 27% of the budgets for the institutes moved in a similar lock step fashion during the 1998-2003 doubling period, a number which is high compared to the programs in almost any other research agency.

While the United States remains atop the list of the world's R&D-performing nations, our share of total global R&D has declined from 37% to 30% over 2001-2011 period.[10] With the arrival of an era where the U.S. finds itself a parity player rather than the dominant global R&D figure, the science posture we get as a result of fierce, decentralized competition for the marginal dollar may not be our best strategy. We have to be more sophisticated in identifying research areas where the Nation must have a leadership position and those where parity or a posture of careful watching developments elsewhere while maintaining a capacity to respond when necessary is acceptable.

Prioritization Framework

As a counterpoint to the advocacy-based competition for the marginal R&D dollar and to stimulate discussion, I propose the following framework for the prioritization of federal R&D funds. The first claim on federal R&D funds should be programs that produce policy-relevant research.[11] The next priority should be federal R&D programs that produce economically-relevant "public good" knowledge that would otherwise be underinvested in by private-sector actors. The final claim on federal R&D funds should be pure, discovery-oriented science, which is not meant to imply zero funding.

Public Sector Knowledge. This framework begins with a belief that government has a duty to make decisions based upon the best possible information, and so research programs that inform the exercise of federal powers should be given a higher priority for funding than research that is less directly related to governmental action. Within this category of public sector research, programs that inform the exercise of coercive federal powers should rank more highly than programs that inform the management of public assets, such as management of federal lands, or the provision of government services, regardless of whether those services are delivered at federal, state or local levels of government. National security-

[10] National Science Board. 2014. *Science and Engineering Indicators 2014*. Arlington VA: National Science Foundation (NSB 14-01), p.4-18.

[11] Frameworks for prioritization have been proposed previously, most notably Press, F. (1988). "The dilemma of the golden age." *Science, Technology, and Human Values*, 224-231. NAS President Press prioritized [1] preserving the human resource base and the research pipeline, [2] research addressing national crises, [3] extraordinary scientific breakthroughs, [4] large projects, and finally [5] the prerogatives of the political system.

related R&D is the most obvious type of funding in federal powers category, but so too is the science that informs decision-making by the modern regulatory state, whether those are environmental, economic or other regulatory regimes.

Unambiguous examples of research propelling policy outcomes are fairly rare. The clearest case may be the story detailed in Richard Benedick's *Ozone Diplomacy*, which documents the development of the Montreal Protocol.[12] Here, rapidly moving discoveries about the atmospheric chemistry of chlorofluorocarbons and industry's ability to develop innovative substitutes put ever tighter constraints on the political options for phase out of the most detrimental CFCs. In what may be the embodiment of the hopelessly naïve notion that science sits outside politics, easy-to-implement assessment tools that help us monitor the development of policy-relevant scientific literatures and their influence on policy-making communities could help ground the priorities and budgets of these research programs in something other than a proxy fight about the policy outcome itself. Climate change research programs are obvious and frequent subjects of this type of proxy fight. SciSP could support research to examine characteristics of policy-focused research programs, whether it is some organizational factor, particular cohorts of performers, or peculiarities of oversight structures, that serve to make one program more or less vulnerable to political attack.

It is not too difficult to find examples of areas of consistent underinvestment in research that should inform the exercise of government authority, and forensic science is but one good example. The National Research Council identified an extensive list of problems that require investment from a lack of mandatory standardization, certification, and accreditation for forensic scientists and medical examiners to problems relating to the interpretation of forensic evidence and need for research to establish limits and measures of performance of forensic techniques.[13] As the report notes, "Forensic science research is not well supported, and there is no unified strategy for developing a forensic science research plan across

12 Benedick, R. E. (1991). *Ozone diplomacy: New Directions in Safeguarding the Planet*. Harvard University Press.
13 National Research Council. (2009). *Strengthening forensic science in the United States: A path forward*.

federal agencies. Relative to other areas of science, the forensic disciplines have extremely limited opportunities for research funding." This neglect of a field of science so critical to the fair operation of our criminal justice system provides SciSP an opportunity to examine why science advocates often express little more than passing interest in the science-based functions of agencies. Is the rhetoric of science policy so steeped in the language of economic influence and technology transfer that public sector science is inherently less interesting? Is a different examining framework needed for programs where there is no extramural research funding to fight for or no regulatory political battle to protect against?

Public Goods Knowledge. Next in precedence for a claim on federal R&D funds should be the economically-relevant basic scientific and technological research where firms are prone to underinvest because they cannot adequately capture all the benefits of their research investment or effectively exclude other firms from benefiting from it. In these areas of research, the United States should invest competitively against our economic rivals. While the United States remains atop the list of the world's R&D-performing nations, our share of total global R&D has declined from 37% to 30% over 2001-2011 period.[14] Since total global R&D continues to grow as a result of the growing knowledge-intensiveness of the economic competition among the world's nations, we will likely need to be better informed about how major economic competitors are investing their R&D portfolios so that we have the earliest signal possible about the relative position of portfolios we deem economically important.

Tools to measure the relative position and performance of agency programs and the speed with which they respond to challenges from international competitors are required. Since programs supporting economically-relevant research, e.g., materials science, are more likely to be found in multiple agencies, those tools should allow program-to-program comparisons as well as international comparisons. It would also be helpful to understand why research agencies don't appear to use some of the existing inexpensive,

[14] National Science Board. 2014. *Science and Engineering Indicators 2014*. Arlington VA: National Science Foundation (NSB 14-01), p.4-18.

easy-to-implement tools, such as the NRC's international benchmarking approach, to inform their portfolio management?[15]

We also need more scholarly study of whether federal research programs can effectively coordinate private sector R&D and how. Established in 1987, SEMATECH may have been the most coordinated effort by the US government to catalyze an R&D strategy for a major industrial sector.[16] While such direct public subsidies of an industrial R&D consortium may no longer be politically acceptable, what aspects of SEMATECH that were effective could inspire mechanisms for today's challenges?

***Discovery-oriented Knowledge*.** The final claim on federal R&D funds should discovery-oriented science. These are fields such as particle physics, cosmology, manned space flight or fusion, where the scientific or technical relevance of the program itself to current concerns are weak at best or non-existent. In these areas of research, the United States has invested cooperatively with other nations and should continue to do so, but we have no reliable framework for assessing whether leadership, parity or abandonment of any particular discovery-oriented field of research matters.

We may invest in a technological dead-end or trade away one of these "non-relevant" fields absent any awareness of what we are likely to lose. As an example, does it matter to the U.S.'s or the E.U.'s long-term scientific or economic strength whether one builds the next linear collider, the ITER fusion reactor, or a human mission to Mars? Or is one "big science" field as good as the next, and the choice should be driven by whichever program offers the least expensive incremental advance?

It is reasonable to assume that current funding levels for these programs are the result of some combination of the hold their discoveries or technical feats of daring have on the public imagination, a measure of internal program momentum, the scientific tastes of an important policy actor (e.g., the President's Science Advisor), or an assessment by OMB of the minimum increment necessary to avoid a

[15] Committee on Science, Engineering, and Public Policy, *Experiments in International Benchmarking of U.S. Research Fields* (Washington, DC: National Academies Press, 2000).

[16] Irwin, D. A., & Klenow, P. J. (1996). "High-tech R&D subsidies Estimating the effects of SEMATECH." *Journal of International Economics*, *40*(3), 323-344.

distracting political confrontation with important Members of Congress. Every federal research program has its favorite example of serendipitous outcome that privileges curiosity-driven discovery over strategic direction. Particle physics touts Tim Berners-Lee's development of the worldwide web during his time at CERN. They wax eloquent about the benefits pushing the performance limits of superconducting magnetic tape for their accelerators have had in enabling the commercialization of magnetic resonance imaging systems for medical diagnostics. A more rigorous approach than totaling the count of interesting anecdotes must be possible.

We already know a lot from the work of economists about the role of technological change in economic growth, we just don't use that knowledge in any systematic way in setting budget priorities.[17] We should focus our attention on where to put the marginal dollar based on an analysis of spillover "tie breakers" in the sense of favoring programs that have the greatest *relative rate* of spillover effects. We want to be able to identify programs that consistently push commonly-used technologies (e.g., computing, communications, power electronics, magnets) earlier and harder than leading-edge industry practice or that can demonstrate unusually high rates of intellectual and human capital export (theoretical approaches, statistical methods) to other scientific and technical fields and to society at large. For favored fields, we should invest enough to maintain a measure of scientific progress – a "low boil." For fields that don't make the cut, we should scale-back, merge or terminate programs.

As an aside before closing, an interesting question will be to understand how the Giving Pledge, a commitment by the world's wealthiest individuals and families to dedicate the majority of their wealth to philanthropy – most of which will be disbursed to uses other than research, will affect federal science policy making. Effective philanthropists have always seeded risky new areas of science that, once proven, attracted much larger follow-on investment by R&D agencies. Will this philanthro-policymaking enabled by aggregate endowments as large as $600 billion by some estimates influence the agendas of

[17] For example, Nelson, R. R. (2003). On the uneven evolution of human know-how. *Research Policy*, *32*(6), 909-922.; Nelson, R. (1962). The link between science and invention: The case of the transistor. In *The rate and direction of inventive activity: Economic and social factors* (pp. 549-584). Princeton University Press; and Rosenberg, N. (1994). Path-dependent aspects of technological change. *Exploring the Black Box. Technology Economics, and History*, 1-6.

R&D agencies in ways that are materially different than what we've seen in the past?[18] Absent a framework for the prioritization of federal R&D funds, will the wealthiest individuals in our society be able to exert an outsized influenced in yet another policy arena?

SciSP could address some of the practical challenges of wielding blunt budgetary instruments by a supporting research that expands our understanding of examinable moments in the budget formulation processes and by assessing the effectiveness of frameworks for program examination [19] It is important to note that *ex ante* program examination is different than *ex poste* program evaluation, an exercise for which there exists a well-developed body of knowledge. It is my hope that the scholarly community will deliver to those of us in the practitioner community tools that will one day enable us to say that is not only axiomatic but also true that federal science budget policy is largely played out in response to federal science policy.

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[18] Rogers, R. (2011). "Why philanthro-policymaking matters." *Society*, *48*(5), 376-381.

[19] Greater detail on the practice of program examination can be found in Holland, M.J. (2013). "Key Players and the Nature of Their Interactions in U.S. STI Policy: Resource and Budgetary Allocations by the White House and Congress, Study of Innovation and Technology in China," Policy Brief 2013-8, San Diego, CA: University of California Institute on Global Conflict and Cooperation. http://escholarship.org/uc/item/5742m536