A Rationale for Funding Large "Proof of Concept" Proposals

_cc by-nc-sa_ Robert E. Wright 2010

Government-funded research is justified to the extent that its benefits outweigh its costs. In other words, the value of increased public or private productivity, decreased pollution, and so forth should exceed the value of grants awarded and the cost of their administration at least on average over some reasonable time interval. While estimates are necessarily imprecise, most observers believe that since their founding the National Science Foundation (NSF) and the National Institutes of Health (NIH) have improved taxpayers' lives on net. That naturally leads to the question of how government-funded research programs could be expanded without diminishing their positive impact through the funding of increasingly unimportant research projects.

Ideally, government should fund research projects that private investors do not find sufficiently remunerative to support. Basic scientific research with no clear commercial application is a prime example, one that is well-understood by taxpayers and fairly well-funded by the U.S. and other governments. The NSF's Directorate for Social, Behavioral & Economic Sciences (SBE), for example, has done an excellent job of funding basic research in economic and financial history.

Large gaps persist, however, between government and private funding of some types of social scientific research, particularly in the area of economic policy. During the course of their research, writing, and teaching, many scholars experience epiphanies about how their empirical findings, causal insights, or theoretical perspectives could positively impact the real world through the creation of better policies or markets. Most such radical new ideas are deeply interdisciplinary and cannot be tested using traditional scholarly methodologies such as literature reviews, statistics, models, or experiments. Often, the ideas lay fallow and even forgotten in the researchers' minds or remain inert on the pages of journal articles or books because few possess both the incentive and the wherewithal to attempt implementation.

What is needed in such instances is an entity willing and able to fund a large-scale proof of concept proposal sufficient to demonstrate the feasibility of the business plan or policy reformation. Some ideas could be proven (or not, as the case may be) in a few months with a few million dollars but others may take decades and hundreds of millions if not billions of dollars of funding to adequately assess. Although some NGOs fund "Big Question" research projects employing traditional scholarly methodologies (e.g. John Templeton Foundation, http://www.templeton.org/what-we-fund/funding-priorities), they do not as a rule fund large-scale proof of concept proposals due to the large opportunity costs involved. Even corporations and other business ventures will steer clear if the sums needed are too large, the project's chance of success is too uncertain, or if its outcome cannot be monetized (e.g. if it cannot be copyrighted, patented, or otherwise infused with an intellectual property right).

The only remaining source of funding is the government. Although some taxpayers and politicians will blanche at the notion of funding large, highly uncertain projects, others will laud the government's foresight. Considerable care will have to be taken to ensure that only quality, high-impact projects receive government funding but the NSF already has proper procedures in place and considerable expertise screening proposals. It might also be prudent to limit, as the NSF does with its current programs, the PI's remuneration to his or her opportunity costs plus whatever laurels society sees fit to bestow (prizes, professorships, directorships, etc.). The most compelling defense of the expenditures, however, may be that it is more costly *not* to fund these sorts of "big think" ideas than to do so, especially if the monies are disbursed to American citizens on U.S. soil, and so forth.

Shiller (2003) is a case in point. Well before the most recent mortgage crisis struck, Shiller realized that insurance buffers against declining home prices could "help prevent some individuals from falling into economic hardship for their rest of their lives. And they could help prevent unnecessary disruptions of neighborhoods and collapse of cities" (120). If Shiller (and like-minded scholars such as Allan Weiss, William Goetzmann, and Barry Nalebuff) could have raised sufficient funding to establish the commercial viability of "home value" insurance, private companies may have entered the new market with enough force to stop the housing bubble and consequent mortgage meltdown and financial crisis. (Presumably "home value" insurance premiums would have risen as house prices outstripped fundamental values, sending a powerful signal that prices were rising too quickly in some markets.) At the very least, "home value" insurance would have been widely enough available to help home sales to rebound more quickly after the panic. (To this day, many potential buyers remain reluctant to purchase fearing further erosion in the economy and house prices.) As matters stand now, a handful of companies like EquityLock Financial offer "home value" insurance but few real estate professionals know about or understand the product. In short, a research expenditure of billions could have saved the nation trillions of dollars.

Similarly, Wright (2010) made numerous policy suggestions that reviewers found "insightful" but lacking "resonance in today's political climate" (Zachary Karabell, *L.A. Times*, 27 August 2010). For example, he describes a new insurance product – a combination life and health insurance policy -- that might better align the interests of insurers, doctors, and patients.

The policy would pay larger death benefits to policyholders who used fewer healthcare dollars, thereby encouraging patients to shop around for the best valued service and discouraging them from seeking expensive end-of-life treatments. Combined policies would also presumably reduce adverse selection by enticing younger, healthier people to sign up and minimize malingering and other forms of moral hazard by reducing the death benefit. Given the size of the healthcare sector, a few billions spent on big picture proof of concept research today could return many billions or even trillions later. In addition to funding a large-scale trial of the combination life-health policy or other innovative "personal security" products, the NSF could help to cut through the regulatory red-tape that currently renders many insurance-related innovations cost prohibitive.

In fact, the NSF-SBE might seek the authority to partially, temporarily, and/or locally suspend various types of regulations to facilitate the sort of policy experimentation advocated by Greenstone (2009). Economists and economic policymakers will never be able to conduct controlled, double blind experiments on a regulator basis but they would not have to limit themselves to natural experiments (or heroic econometric assumptions) if they had some power to consciously alter policy environments. Instead of allowing the fate of public policy reform proposals to rest in the hands of politicians and political ideologues at think tanks, the U.S. government could turn them over to scientific researchers funded by the NSF, a politically independent entity analogous to the Federal Reserve.

In the spirit of Greenstone, however, the SBE's large "proof of concept" program should be thoroughly tested with a few smaller cases before being implemented on a broad scale. By starting slow and small and learning from its mistakes, the SBE could soon earn for itself a much larger budget, (even) more professional respect, and the taxpayer's (sometimes grudging) gratitude.

References

Greenstone, Michael, "Toward a Culture of Persistent Regulatory Experimentation," in David Moss and John Cisternino, eds. *New Perspectives on Regulation* (Cambridge, Mass.: The Tobin Project, 2009).

Shiller, Robert J. *The New Financial Order: Risk in the 21st Century* (Princeton, N.J: Princeton University Press, 2003).

Wright, Robert E. *Fubarnomics: A Lighthearted, Serious Look at America's Economic Ills* (Buffalo, N.Y.: Prometheus, 2010).