

A Skeptical View of the National Science Foundation's Role in Economic Research

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We can imagine a plausible case for government support of science based on traditional economic reasons of externalities and public goods. Yet when it comes to government support of grants from the National Science Foundation (NSF) for economic research, our sense is that many economists avoid critical questions, skimp on analysis, and move straight to advocacy.

In this essay, we take a more skeptical attitude toward the efforts of the National Science Foundation to subsidize economic research. We offer two main sets of arguments. First, a key question is not whether NSF funding is justified relative to *laissez-faire*, but rather, what is the marginal value of NSF funding given already existing government and nongovernment support for economic research? Second, we consider whether NSF funding might more productively be shifted in various directions that remain within the legal and traditional purview of the NSF. Such alternative focuses might include data availability, prizes rather than grants, broader dissemination of economic insights, and more. Given these critiques, we suggest some possible ways in which the pattern of NSF funding, and the arguments for such funding, might be improved.

Although our discussion here will be phrased in terms of grants for economic research from the National Science Foundation, similar arguments would apply to grants in support of economic research from the National Institutes of Health (which

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plays a significant and growing role in funding health care economics and studies of public health performed by economists) and from other agencies. Those interested in a good overview of the economics of science might begin with Diamond (2008).

Evaluating NSF Funding on the Margin

The grants given to economists by the National Science Foundation should be viewed in the context of the portfolio of extensive government support for economic research. About 80 percent of the academic economics sector, measured by the number of students, is accounted for by state universities. Charitable donations to universities and colleges, along with research centers and other nonprofits, are tax-deductible. The government produces a number of datasets widely used in economic research and makes them freely available, including those from the Bureau of Economic Analysis, the Census Bureau, the Bureau of Labor Statistics, Bureau of Justice, the Department of Education, the Federal Reserve, and others. The government also hires economists directly.

Moreover, universities and colleges provide strong incentives for economists and other academics to turn their research into a public good through publication. Consider the mantra of “publish or perish”—what better private incentives could one ask for? Citations to published research are strong predictors of salary for individual professors as well as the prestige of the departments where professors are employed (Ellison 2013; Hilmer, Ransom, and Hilmer 2015). Economists at research universities, in particular, are given high salaries, low teaching loads, plenty of nonstructured time, and access to a highly skilled and motivated labor force at low cost in the form of graduate students. The subsidy that society provides to economists is large, especially once we consider the opportunity cost of highly skilled labor. The obvious question, although one which many economists are reluctant to ask, is how much subsidization of economic research is enough?

A common strategy in defending NSF grants to economists is to point to research that can be linked to substantial real-world policy improvement. But private incentives are strong not only to publish but also to produce research with real-world implications. For example, the promotional NSF (2000) book, *America's Investments in the Future* lauds NSF funding of auction theory and, in particular, the work of Paul Milgrom. Milgrom's work has indeed been spectacular, but the implicit argument that incentives were lacking for work on auction theory seems incorrect. Indeed, few areas in economics have been as privately remunerative as auction theory. As noted on Milgrom's webpage (<http://www.milgrom.net/business-activities>, accessed April 2, 2016):

Milgrom has advised bidders in radio spectrum auctions, power auctions, and bankruptcy auctions. One advisee, Comcast and its consortium, SpectrumCo, followed the advice of Milgrom's team in FCC Auction 66 to achieve the most exceptional performance in US spectrum auction history. SpectrumCo saved

nearly \$1.2 billion on its spectrum license purchases compared to the prices paid by other large bidders—such as T-Mobile and Verizon—for comparable spectrum acquired at the same time in the same auction.

Advice on auctions is a highly valuable private good. Hal Varian, Peter Crampton, Preston McAfee, and Susan Athey are just a few of the other notable economists and auction theorists who have in recent years moved to important roles in the private sector. Lest we be misunderstood, we applaud and celebrate such activity. Our point is that the existing public and private incentives for at least some kinds of research are quite strong, and it seems potentially misleading to conclude that, in the absence of NSF grants, private incentives for such work are lacking.

More generally, the classic science-to-technology paradigm suggests that basic research leads to applications. History, however, is full of examples in which the process is reversed and private applications lead to basic research (Kealey 1996). In economics, for example, Koopmans developed some of his key ideas in operations research and resource allocation when working for the British Merchant Shipping Mission in Washington. Advances in finance have often been driven by the demands of the finance industry (Derman 2004). Amazingly, the Vickrey–Clarke–Groves auction mechanism was rediscovered by engineers at Google when they were looking for ways to raise revenue efficiently in sponsored search auctions (Varian and Harris 2014). Instead of being subsidized to work in the ivory tower, economists might contribute more to the public good by working at least part of the time directly in the private or government sector—perhaps looking at consumption functions at the Federal Reserve, or at supply and demand coordination at Uber, or on market design at the New York Stock Exchange—and then returning to academia with a heightened sense of which research questions are most useful to pursue.

Crowding Out and Crowding In

If a substantial quantity of economic research is currently being provided by the combination of educational, nonprofit, and for-profit institutions, then additional funding has a lower marginal value. A federal program to fund mosquito control is harder to justify if state and local programs already exist. Furthermore, government funding for economic research may also crowd out other sources of funding. Crowding out will tend to raise the cost of additional public funding because any given increase in net funding will require higher gross funding with the attendant deadweight loss of taxation and also additional administrative costs, including the overhead charged by universities to the National Science Foundation (Noll and Rogerson 1997).

In the broader literature, Hungerman (2005) finds that government provision of welfare crowds out private charity on the order of 25 cents to the dollar. For the National Endowment for the Arts, Dokko (2009) finds crowding out effects of up to 60 cents to the dollar. Such crowding-out does not preclude public provision, but given the welfare costs of taxation—which average perhaps 30 percent of the expenditure (Ballard, Shoven, Whalley 1985; Bohanon, Horowitz, McClure 2014)—it does reduce the desirable level of public provision.

Table 1

Number of NSF Grants in Economics by Organization

<i>Organization</i>	<i>Number of grants</i>	<i>Percentage of total</i>	<i>Cumulative percentage</i>
National Bureau of Economic Research	212	15.4	15.4
New York University	60	4.3	19.7
Stanford University	56	4.1	23.8
Northwestern University	49	3.5	27.3
Columbia University	48	3.5	30.8
Yale University	48	3.5	34.3
Duke University	46	3.3	37.6
Princeton University	41	3.0	40.6
University of California—Berkeley	40	2.9	43.5
University of Wisconsin—Madison	39	2.8	46.3
University of Pennsylvania	38	2.7	49.0
Harvard University	36	2.6	51.6

Note: NSF grants in field of economics (code 1320) since 2005. From <http://www.nsf.gov/awardsearch/advancedSearch.jsp>.

Crowding out may vary by the type of public good produced, and we do not know of any specific estimates for NSF economics funding. However, we do know that the NSF allocates most of its funding to high-prestige economists doing mainstream research at wealthy institutions and schools. Over 50 percent of NSF grants since 2005, for example, have gone to just 11 universities and the National Bureau of Economic Research as indicated in Table 1. The NBER itself distributes funding towards top researchers and universities, especially Harvard and MIT. Wachtel (2000) provides an earlier analysis showing NSF grants flow primarily to a small number of prominent institutions, while Feinberg and Price (2004) discuss the role of social capital and connections in the NSF funding process.

If Harvard, MIT, and Stanford do not feel that it is worth paying faculty or providing research support for a certain economics project, should American taxpayers necessarily have a different opinion and feel compelled to fill the gap? And if prominent and well-endowed academic institutions do feel that it is worth paying faculty and providing research support, why should the NSF risk crowding out such support? Those schools have large, already subsidized endowments and also a strong track record in picking research winners.

In addition to crowding out, there is also the possibility of crowding in, which often occurs in science and the arts (Heutel 2014; Cowen 2010). Certification from a centralized governmental authority helps the grant recipient to raise money from other sources. Some of these additional funds may be “new money,” but it may also lead to redistribution of the pool of funds. There is also the possibility that crowding in will increase inequality, as even more support is funneled to NSF recipients while other economists or other scientists receive less. In theory, this outcome could be better or worse for encouraging the quality of economic research, but overall economists tend to be relatively critical of “winner-take-all” markets because of increases

in inequality, losses in diversity, and increased incentives for rent-seeking (Cook and Frank 1995). In this regard, the long-run funding impact of NSF grants may not be entirely positive.

NSF Fellowships for Graduate Students

The NSF has a longstanding program of supporting graduate students in economics (Freeman, Chang, and Chiang 2005), currently about 30 per year. We have two reservations about this program. First, a preponderance of the fellowships go to graduate students who choose the top-rated schools. Those schools already receive tax subsidies for their sizeable endowments, and already support highly talented graduate students. It is not clear that further public subsidy is warranted. Second, those individuals are usually extremely talented, but we don't know which allocation of their talent would produce the highest social return. If these individuals did not become economists, they might enter other sciences, or business, or the tech world, or perhaps run innovative nonprofits. In which of these areas are the external benefits from creativity the greatest? We do not pretend to know the answer and so the proper assessment of these grants should be agnostic. Citing the quality of the supported individuals chosen only raises the stakes, rather than settling the issue.

Other Open Questions: Opportunity Cost and Elasticity

It seems plausible that NSF Economics funding has a higher marginal value than *some* government programs. We are reasonably confident that NSF funding for economics is a better idea than, say, ethanol subsidies. But defenses of a government program that compare it only to apparently inferior investments are just special pleading. Even if we believe that NSF funding more than "pays for itself," at the relevant margin, the alternative may be other programs which "pay for themselves" even more. For example, is the NSF Economics Program a better investment than, say, speeding the approval processes at the Food and Drug Administration, hiring additional good economists to work at the US Treasury, or funding research into communicable diseases? The answers are far from obvious. It would be quite remarkable if NSF funding for economics were the number one activity at the margin for government funds.

Some of the proposals to reduce NSF funding of economics and other social sciences would explicitly reallocate the funds to other branches of science, so the question of opportunity cost is pertinent. Given the existence of other non-NSF support for economic research, is spending on economic research of higher value than the average NSF expenditure?

A related question is to consider the elasticity of supply for quality economic research, with respect to wages or payment. Over the last few decades, many in the economics profession have concluded that tax cuts for high earners, at current tax rates, have relatively small effects on labor supply; for example, that argument is often cited as one reason why the Bush tax cuts passed into law in 2001 yielded disappointing economic results. Yet when it comes to NSF grants, there is often the implicit presumption that the elasticity of supply for economic research with respect

to additional government grants is relatively large. We are agnostic on the elasticity question, but without understanding this issue, it is difficult to evaluate NSF bang for the buck, and thus economists should not be so confident about the efficacy of this funding.¹

Are NSF Grants the Best Method of Government Support for Economic Science?

Public goods theory tells us that the National Science Foundation should support activities that are especially hard to support through traditional university, philanthropic, and private-sector sources. This insight suggests a simple test: to the extent that the NSF allocates funds to genuine public goods as opposed to subsidies on the margin, we ought to see a large difference in the kinds of projects the NSF supports compared to what the “market” sector supports. But what stands out from lists of prominent NSF grants (like the one provided by Moffitt in this symposium) is how similar they look to lists of “good” research produced by today’s status quo. If we take public goods theory seriously, what areas of economics should be supported?

Replication

The NSF could support replication studies on a significant scale. A significant fraction of economic research does not easily replicate (Dewald, Thursby, Anderson 1986; Chang and Li 2015; Duvendack, Palmer-Jones, and Reed 2015; but also, Camerer et al. 2016, who offer a more positive outlook for experimental economics). Replication and reproducibility studies are true public goods that are not rewarded highly by most top journals or by the tenure process at research universities. Consider Zimmermann’s (2015) plea for a replication journal:

There is very little replication of research in economics, particularly compared with other sciences. This paper argues that there is a dire need for studies that replicate research, that their scarcity is due to poor or negative rewards for replicators, and that this could be improved with a journal that exclusively publishes replication studies. I then discuss how such a journal could be organized, in particular in the face of some negative rewards some replication studies may elicit.

Instead of pointing to the prestigious economists whose research they have funded, perhaps the NSF might point to the prestigious research that has been convincingly replicated—or not replicated.

¹Arora and Gambardella (2005) find that NSF grants have only a slight positive impact on the marginal productivity of well-known researchers, though a higher impact on lesser-known researchers. However, their dataset is from 1985–1990, and we do not consider this to be a decisive result for today.

Datasets

The NSF should encourage the availability of useful, publicly available datasets especially in areas where data is not yet collected in a sufficient manner. Such data are a public good across the entire community of researchers, and collecting data seems to be an underprovided activity (for example, Belter 2014 finds a high value for public datasets in science). Furthermore, in the tenure and promotion process, creating a new and important dataset is not strongly rewarded at most schools, as typical standards for promotion and tenure emphasize the publication of new research in top journals. That is all the more reason why government science funding should pay more attention to the creation or opening up of useful datasets.²

One example of an NSF success is the funding supplied to the Panel Study of Income Dynamics. Thousands of articles have sprung from this dataset and influenced discourse on income mobility, taxes, demography, and many other areas of direct policy relevance. The NSF should be proud of its support here, but the next step is to consider whether more funds should go to comparable enterprises. Looking through NSF grants, we do not see that creating or opening up datasets has been a priority, much less a dominant form of expenditure. The NSF does require (without enforcement) that NSF-supported economics researchers should make their data available to the public. That is a good idea, but still quite different from funding datasets themselves.

Furthermore, the NSF does not always have to *create* new datasets. It could also play a role in improving current datasets or increasing the availability of data. Many current databases have proprietary status, to varying degrees. Universities may buy data licenses for their own researchers, but they are less willing to pay to open up the data more generally. The NSF could buy access rights or do so in a selective manner with a license for qualified researchers. Of course, that would mean more money sent to the private companies that own such datasets and less money sent to high-prestige economists, but that is one reason why the NSF should consider such a move.

We have also noticed a trend for more work to be done using administrative datasets, which have the troubling property that they are often difficult and expensive for most researchers to access for replication or original research. We do applaud the work of the National Science Foundation to expand the number of Research Data Centers, which are secure Census Bureau facilities at locations around the country where external researchers who fulfill certain requirements are given access to confidential microdata. More could be done, however, especially as this is an issue of a growing importance (for some of the issues, see Card, Chetty, Feldstein, and Saez 2010; Mervis 2014).

² Of course, the National Science Foundation is not the only institution that could encourage researchers who produce public goods. Perhaps there should have been more consideration of a Nobel Prize for Irving Kravis, Robert Summers, and Alan Heston, the creators of the Penn World Tables? Or how about a Nobel Prize for Stephen Davis, John Haltiwanger, Ron Jarmin, and Javier Miranda for their work in developing the Business Dynamics Statistics database?

Support for Projects with High Fixed Costs

It is a well-known proposition from industrial organization that markets may underinvest in product variety when fixed costs are high. In the current context, the implication is that the NSF should focus more on funding research areas with relatively high fixed costs, including high capital costs (all forms of research involve some fixed intellectual costs). More concretely, this argument suggests that stronger candidates for support would include expensive or lengthy randomized control trials, costly field experiments, and forms of experimental economics that require costly lab investments.³

Conversely, an emphasis on supporting research with high fixed costs would imply less support and perhaps no support at all for economic theory. Pencil and paper and even computers simply aren't very costly, and furthermore economic theory seems to have made bigger breakthroughs before the 1990s than it has made since then (Hamermesh 2013). An emphasis on research with high fixed costs also implies a lower level of support for empirical economics based on readily available datasets where the regressions are run on a personal computer. Again, that kind of research just doesn't cost very much money, and it is already being funded by research universities through their high salaries and low teaching loads.

Dissemination of Economics Research

Another possible task for the NSF is to encourage broader dissemination of economic research. Steps along these lines might include subsidizing open access journals and also spreading educational resources, including disseminating knowledge about good teaching and communications techniques, and encouraging economists to do more outreach to policymakers.

In recent years, the NSF has supported some teaching and access activities, such as a recent project on teaching economics in community colleges. The researchers surveyed community college economics faculty and organized meetings to address the problem of the isolation of community college instruction from professional standards. Many part-time faculty in community colleges do not even have a graduate degree in economics, and so they are not always well-informed about what they teach. Maier and Chi (2016) survey this project and offer a generally favorable assessment. (This project, like the NSF support for economics graduate students, is actually funded through the educational branch of the NSF rather than the economics section.) Still, in percentage terms this kind of project constitutes only a small amount of what the NSF does in economics.

If we ask ourselves which economics activity is undersupplied as a public good in today's profession, a lot of indicators point in the direction of good teaching rather than quality research. Many poor economic policy decisions stem from a basic neglect of straightforward economic concepts that do not rest on any particularly

³ In the interests of full disclosure, we should note that our own department, George Mason University, has received some National Science Foundation grants for its work in experimental economics, which does require a costly lab. We have not ourselves been the recipients of such funds.

partisan view: for example, farm subsidies are undesirable economic policy for straightforward reasons; free trade is (usually) good for reasons that have been well understood for over two centuries; rent-seeking problems were analyzed persuasively by Adam Smith; the basic arguments against price controls have been known for over a century; the Fed should not tighten money if a recession is approaching; and (most) tax cuts do not automatically pay for themselves but rather require offsetting expenditure cuts over some time horizon. To be sure, not all questions of economic policy are as simple as those listed here. If the federal government is considering an extension of the Earned Income Tax Credit, it might want some precise estimates of costs and elasticities, of the kind that would require sophisticated research, which could be done by economists inside of government.

Again, our theme is that economists should be willing to face tradeoffs when thinking about NSF Economics funding. One possible tradeoff is that dissemination and outreach regarding well-accepted basic economic insights may be a more valuable public good than the support of marginal cutting-edge research.

Another public good the NSF might fund is simply a study of which of its previous expenditures on economics have had the greatest marginal value-added. Based on conversations with NSF staff in economics, we are not able to identify such a study. One proposal would be to fund such a study and then follow many or all of its recommendations; after all, the NSF presumably believes it is capable of generating useful research results with practical implications.

High Risk, High Gain, and Far Out Basic Research

It's not surprising that the NSF funds mainstream projects similar to what is already being funded because the NSF chooses which projects to fund by committee peer review. Committee peer-review will gravitate towards funding that reviewers think is valuable and high quality. Almost inevitably, giving high-prestige economists a leading role in deciding on NSF grants means funding research that is relatively close, in intellectual terms, to what already is well accepted in the profession. While this procedure may seem self-evidently correct to most high-prestige economists, it seems peculiar to believe that the best mechanism for allocating public goods should be dominated by the preferences of suppliers. Moreover, it seems more likely than not that supporting mainstream research leads to inefficient allocation.

A common argument for government funding of science, originating with Arrow (1966), is that the private sector will underfund some high-risk projects because the private rate of risk aversion and loss aversion is too high relative to the socially optimal rate. The Defense Advanced Research Projects Agency (DARPA), for example, doesn't subsidize private sector research but instead creates small teams to take on "high-risk," "high-gain," "far out" basic research (to use terms that have been prominent in the agency's mission back to its earliest days, see Hafner and Lyon 1996, p. 22). DARPA is widely considered to be the most successful government research funding agency.

Consider whether the economics profession would have benefited from support for a broader range of research in the lead-up to the Great Recession.

Prior to the Great Recession, a number of mainstream economists argued that the economy was undergoing a Great Moderation (Clarida, Gali, and Gertler 2000). Since then, there has of course been a dramatic rethinking of what had been considered settled wisdom. Paul Krugman, one of the world's most recognized economists and a prominent gadfly, says that the past 30 years of macroeconomic research was "spectacularly useless at best, and positively harmful at worst" (as quoted in the *Economist* 2009). More measured reevaluations have occurred under the auspices of Olivier Blanchard and the IMF who held substantial conferences in 2011, 2013, and again in 2015 on the theme of "Rethinking Macro Policy" (Blanchard, Dell'Ariccia, and Mauro 2010, 2013, see also the conference webpage: <http://www.imf.org/external/np/seminars/eng/2015/macro3>). One does not have to agree with the post-Keynesians, econo-physicists, or the Austrians to see an argument for broadening the NSF portfolio of grants beyond the usual mainstream contributors (as the NSF has occasionally done sometimes in the mathematics section rather than the economics section). Indeed, the fact that one does not agree with radically different approaches is a good case for funding them. Perhaps the NSF should offer greater support for heterodox economics research for the same reason government funding may be necessary to preserve crop varieties against the risks of monoculture.

The focus of NSF funding on low-risk, mainstream projects is by no means restricted to economics. Biochemist and Nobel Laureate Roger Kornberg lamented in 2007 (as quoted in Lee 2007, referring to funding from the National Institutes of Health) that "the funding decisions are ultraconservative. If the work that you propose to do isn't virtually certain of success, then it won't be funded. And of course, the kind of work that we would most like to see take place, which is groundbreaking and innovative, lies at the other extreme." Ironically, better bibliometric citation measures may make this problem worse, given that Wang, Veugelers, and Stephan (2016) find that novel papers are more likely to be published in journals with lower impact factors. The conservatism of the committee review system suggests that we should use a mix of funding mechanisms to increase the variety of projects that are funded.

Innovation Prizes, Not Grants

Incentives for academic research can be provided through prizes as well as grants (Tabarrok 2011; Williams 2012). The NSF focuses on grants, but arguably a greater share of government support of science should take the form of prizes for achievement of a pre-specified goal or task. Prizes impose greater risk on the scientists. But on other side, the government does not have to decide in advance of production of research who deserves an award, and it is often easier to evaluate excellence after an achievement is completed rather than before the research starts. Furthermore, the government pays out if and only if the valuable end is actually achieved. In recent years, many government agencies including NASA, Health and Human Services, the Environmental Protection Agency, the Department of Agriculture, and the Department of Homeland Security have offered prizes to stimulate research.

DARPA is famous for offering prizes and challenges, including the DARPA Grand Challenge for autonomous vehicles. When the first challenge was held in 2004, the best team travelled just seven miles on a 150 mile course. Nevertheless, the Challenge helped to stimulate deep and surprising innovations that led to today's autonomous vehicles. Or consider how much research was stimulated by Robert Axelrod's iterated prisoner's dilemma competition and the surprising win of the tit-for-tat strategy (Axelrod 1984). The Center for Disease Control recently sponsored a prize to find models to successfully predict the timing, peak, and intensity of the flu using demographic, economic, and social-media data (Office of Science and Technology Policy 2015)—so there is precedent for an agency to offer the types of prizes that might be useful in economics. For example, NSF-sponsored challenges might include better forecasting of recessions and predictions of laboratory and field behavior.

As a simple example, the NSF could sponsor a competition for the best question to add to the Current Population Survey, whereby the NSF would pay the US Bureau of the Census for the winner's question to be added. Kleiner and Krueger (2013) suggest that testing, developing, and editing a new question would cost \$50,000 in the first year and less in subsequent years.

Prizes can also confer legitimacy when important ideas come from outside the mainstream. The famous "longitude prize" offered by the British government for a method by which a ship at sea could determine its longitude was won not by Isaac Newton or a member of the Royal Society but by a clockmaker, John Harrison (although Leonhard Euler did receive a runner-up award) (Sobel 1995). A heterodox approach to prediction in economics, for example, would gain greater legitimacy if it were to best mainstream approaches in a competition.

Direct Practical Experience versus Research Funding

If the government wants more of the public good of economic research, it could hire economists directly for this purpose. There is a widespread belief, often expressed in the economics and political science literature, that government relies too heavily on expensive private contractors who often pursue their own agendas, and does not use enough direct employees. When economists themselves need to produce or receive additional ideas, they typically resort to in-house production, rather than outsourcing. For instance, an economist might hire a research assistant and assign that person a specific task, or take on a co-author, but it would be unlikely for an economist to commission outside research through an arms-length relationship. In-house research tends to be more practical, focused, and applied. There is also a general perception that the quality gap between the very top economists and middle-tier economists from good schools has been closing due to the spread of high standards and technical proficiency and the globalization of economics study, among other factors. That change too would seem to militate in favor of more direct commissions of economists as government employees and less use of economists in the role of freestanding discretionary contractors.

DARPA is well-known for hiring researchers for a limited period of time to work to achieve a specific goal. Analogously, if the BEA needs a better method of adjusting price indexes for quality change, perhaps they should hire a team of researchers to work directly on this question. Yet in our experience, academic economists are not eager to compare support for academic research with direct hiring of researchers or government economists. Instead they are keener to argue that when it comes to supporting economists, both methods of supporting economic research should be expanded, which is an interesting case of economists neglecting tradeoffs.

Concluding Remarks

In considering the case for grant-based funding of economics research by the National Science Foundation, we find that a number of pertinent questions are rarely asked, let alone clearly answered. Instead, economists often put forward relatively weak arguments that they would likely dismiss if applied to government subsidies not reserved for economists.

For example, one common approach to defending NSF grants for economists is to list the prestigious individuals with whom the program has been associated. In his paper in this journal, Moffitt notes: “The program has supported every Nobel Prize winner in Economics since 1998 and almost every John Bates Clark medal winner since 1961.” (NSF economics funding started in 1960). Indeed, the list of grant recipients from NSF Economics is a literal “Who’s Who” of the top economists over the last half-century. But we don’t find the prestige of NSF recipients to be a good substitute for an estimate of the public benefits of research. Imagine a group of chefs who defended a hypothetical “National Food Foundation” on the grounds that it had provided grants to Alice Waters, Thomas Keller, Grant Achatz, and every winner of a James Beard Award since 1990. If these names are not familiar, rest assured that their published research output and training of students is very impressive. While we would not consider this information irrelevant (better to fund good chefs than bad ones), as economists we would be unimpressed by this case for government funding of chefs. Talk of how these grants brought about innovations in the culinary arts—such as *sous vide*, molecular gastronomy, and the introduction of quinoa to the American diet—would also not swing the argument. Instead, as economists, we would focus on how food markets would have operated without such grants and what else might have been done with the money.

Economics should think much harder about the marginal benefits of National Science Foundation grants for economics, and for other subjects, in the context of the many other ways in which society funds research, along with how such money should be spent and what the relevant alternatives might be. There is a good case for a significant change in NSF priorities towards replication and reproducibility of research, data access, and teaching. The extent to which NSF grants add to the sum total of economic research, or whether NSF grants are superior to having the government simply hire economists to perform specified research tasks, isn’t

obvious. But when it comes to government funding, many economists transform into special pleaders who prefer to ignore tradeoffs. This metamorphosis would not have surprised Adam Smith.

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