

**Responses to
Questions for the Record
of
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**Questions posed following the:
United States House of Representatives
Subcommittee on Research and Science Education
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Oversight of the Need for Federal Investments and Priorities for Funding.”*
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Introduction

Chairman Brooks and distinguished Members of the Committee, it has been an honor to testify before you to discuss the important topic of federal funding in the Social, Behavioral, and Economic (SBE) sciences.

Following the meeting of the subcommittee on June 2nd, I was asked to respond on the record to the additional questions listed below.

1. NSF is essentially the only federal agency that historically does not receive earmarks. It prides itself on the merit-review process which, while not perfect, is currently the best we have. Given its imperfections and the reality that some less than stellar grants are funded in ALL scientific disciplines, how would you recommend that it be improved?

In responding to this question, I would like to highlight four guiding principles:

1. Peer review, not political review
2. Maintain cross-disciplinary reviews
3. Take a portfolio approach
4. Limit misunderstandings through researcher communication

1. Peer review, not political review

Let me start by echoing strongly the sentiment in the question that the NSF's current merit-based review process, "while not perfect, is currently the best we have." As I mentioned in my earlier written testimony, when thinking about peer review one is reminded of Winston Churchill's famous quote, "Democracy is the worst form of government, except all the others that have been tried."

Like members of Congress, scientists are committed to democracy. The NSF's process is already deeply participatory—with literally tens of thousands of scientists providing reviews of grant proposals for the agency. Like any democracy, the output of the voting process doesn't always please everyone, but there is no viable option to democracy.

The alternative—that is, the political review of science—is chilling, whether we consider it as a replacement or as a supplement to peer review.

Political review is big government

Moving from peer review to political review is chilling first because it represents the worst kind of big government intervention to have politicians and bureaucrats attempt to dictate from the top what constitutes good science. Specific research projects should be judged on their merits only by people qualified to understand them thoroughly.

Otherwise, we run the risk of a politicized process fueled by ignorance of the underlying science. This is not merely speculation—as I discussed during my earlier testimony, in 2007 a member of Congress proposed rescinding my NSF grant *at the same time* that the Army took the initiative to declare this research valuable for the warfighter. We do not want a political review process that runs the risk of judging books by their cover—however well-intentioned the process.

Political review makes us lose the very most talented and early-stage scientists

A second chilling aspect is that the political review of research would cause us to lose the most talented and creative scientific minds. The very most brilliant scientists choose to be paid literally a fraction of what they could earn in the private sector—and they make this unusual choice for their love of intellectual freedom. Robbing them of this freedom through a politicized scientific process would hurt the U.S. economy. The most creative and curious scientific minds are particularly chilled by big government constraints on their work, and they are the most likely to respond to the loss of freedom by 'voting with their feet'—that is, taking higher-paying jobs on Wall Street and elsewhere. Losing these scientists is a loss for society. It is the best people who typically have the best options outside of academic employment. Political review also sends a particularly chilling message to early-stage scientists, who are young enough to have their career alternatives salient and open to them. However, these early-career scientists are often the sources of the most innovative ideas, which are needed to move science forward.

It is worth noting, in case anyone believes that rescinding one or two grants will affect only one or two research teams, that the people who leave sciences may not be the ones directly affected by political review. Instead, those departing may 'see the writing on the wall' and merely speculate that the same treatment could happen to them.

Political review would unravel the cost-effective volunteer review system

A third source of chill would be for the peer review process itself. It is worth noting—although participants of in-person panels at the NSF are paid an honorarium for their time—that tens of thousands of scientists around the world contribute review reports without being paid. To emphasize, it is strictly on a volunteer basis that scientists each offer several hours of their time nearly whenever asked. At least in my own case, out of appreciation for the NSF, upon receiving an

invitation I typically drop whatever else I am working on to spend about half a day volunteering as a reviewer. Even with conservative assumptions about hourly rates, one can estimate that getting these peer reviews at no cost currently saves millions of dollars for the US taxpayer.

This volunteer system works because scientists believe that they are serving the democratic process. Their time feels well spent for having a voice in the future of the field. But this volunteer system would be jeopardized if scientists believed that their voices could be just as easily overturned by politicians and bureaucrats. As a metaphor, I used to spend Thanksgiving and Christmas mornings as a volunteer to cook and serve food at a homeless shelter—and imagine what it would have been like if someone showed up every now and then to toss out some of the food. Presumably, people would have stopped volunteering.

No one minds being given guidelines for their reviews, and ideas about these guidelines are discussed below in the response to Question 2. In the metaphor of the homeless shelter, no one would have minded being told that we needed more side dishes or less salt. However, once the criteria are described clearly and these criteria are followed in good faith, after that it is not possible to discard volunteers' hard work without undermining the volunteer system.

2. Maintain cross-disciplinary reviews

In limiting the amount of “less than stellar” grants being funded, it is important to maintain the NSF's current commitment to including reviewers across multiple scientific disciplines.

As a democracy, peer review is best when it includes perspectives from different walks-of-life. If research is proposed that is seen as important only by small groups of people, then such work will presumably be reviewed less favorably by scientists outside of those cliques.

This diversity of perspectives could be further enhanced by making additional peer review invitations to academically qualified individuals outside of university settings. This includes people holding Ph.D.s in the relevant field who are now employed in industry, non-profit, educational, and government settings. This could be accomplished readily with two caveats. First, when including professionals from applied settings, an emphasis must be made that they are still reviewing basic research—not applied research. Second, even while seeking diverse representation, it is crucial to ensure that each reviewer has the proper research-based academic qualifications. To allow anyone with less than a doctoral degree to provide peer reviews would be like allowing Canadians to vote in the US elections.

3. Take a portfolio approach

I want to take a stand and say that we need to take seriously the metaphor that the NSF holds a ‘portfolio’ of research investments. It is simply not possible to avoid entirely “the reality that some less than stellar grants are funded in ALL scientific disciplines”. Successful investors in the stock market know that you need to take some risks in order to earn a good return.

As I discussed during my earlier testimony, we do not have the ability to look into a crystal ball and predict with certainty where each project will lead. This information becomes revealed over many

years. It is tempting to play ‘Monday morning quarterback,’ and to imagine that we could have known in advance the return on investments. However, we all know that this does not work for the stock market, and neither could it work for the NSF.

How do equity investors account for the reality that some stocks are less than stellar? They fine-tune their selection process with a sense of acceptance that there is some unpredictability. Investing is inherently speculative. If you do *not* want to speculate—and prefer, instead, safe investments—then you need to stick with bonds or other instruments that have far lower rates of return. But successful investors do not look back at their portfolios while ruminating and punishing themselves for not anticipating the demise of some stocks and the promise of others. The efficiency of the markets is such that—if all of this were predictable—then the returns to investors would be low. The returns are high precisely because we do *not* know in advance exactly which investments will succeed.

So what do successful equity investors do to account for this inherent risk? They hedge the risk by diversifying their portfolios. The flip side of having some “less than stellar” grants is having some grants that succeed beyond all imagination. Perhaps there are some people who had the foresight to know that NSF grants for teaching machines how to recognize the thickness of lines would lead to the invention of barcodes, or others who knew that NSF grants for digital libraries would lead to the founding of Google. But, more often, science proceeds with the likes of Pasteur studying spoiled milk and Mendel studying garden peas. As with the stock market or a hedge fund, one expects the portfolio as a whole to yield good returns because these runaway successes more than make up for the flops.

Taking seriously the notion that the NSF’s investments are like a portfolio, we will not dwell as much on the lack of a crystal ball to predict the success of individual grants. Imperfect predictability is woven into the process. We need to follow the same good practices that stockbrokers use in managing long-term portfolios by casting a wide net, diversifying our investments, and encouraging risk rather than punishing it.

4. Limit misunderstandings through researcher communication

Surely it must be true—as implied by the question—that there are at least some “less than stellar grants” funded in all of the sciences. However, we also know that many examples of the public trivialization of government-funded research are instead a matter of misunderstanding.

Again, to speak again from my own experience, my NSF-funded research was singled out for ridicule by a member of Congress *at the same time* that the Army declared this research valuable for the warfighter. Likewise, and as I also mentioned in the earlier testimony, the Coburn Report provided many examples of misjudging books by their covers. Scientists typically break down large problems into lots of smaller problems and, viewed out of context, these smaller problems may seem like poor uses of our nation’s scarce resources. This appears to be what happened with the two entries in the Coburn Report that received the most press coverage: shrimp running on a treadmill and robots folding laundry.

Further, the Coburn Report relied on many news articles written about NSF-funded research projects, rather than information directly from the researchers.

If my hypothesis is true—namely, that much of the concern about “less than stellar” grants comes from miscommunication rather than poor science—then one way to increase the apparent value of scientific research is by communicating effectively about the work and its importance to the US taxpayer.

Along these lines, the NSF might experiment with enhancing the mechanisms by which researchers communicate directly with the public. The question could be posed to each federally-funded investigator: Given the scarcity of taxpayer dollars, why is your work an important use of this money?

Currently, after grants are approved, each researcher revises a one-page abstract that is published on the NSF’s website. Other features could be added. Perhaps one could be a brief video made cooperatively with the investigator, something like 5-10 minutes responding to the key question, “Why should the U.S. taxpayer fund this research?” A second feature could be a set of links updated over time pointing to relevant sources of information that highlight what is valuable about the work. These could include blogs, journal articles, and other resources accessible via the Internet, using language that the interested public could readily understand.

Providing researchers systematically with the opportunity to make their best case to the public should prevent other people from making this case—and trivializing it—without them. We could have a good faith request that any politician who questions the value of a grant at least to watch the brief video before making a public critique. This would allow the scientist proactively to provide input into the debate. Certainly, reasonable people can still disagree about the value of a research project, but at least the disagreement would be informed and could proceed on the merits.

If we created resources like this, I firmly believe that well-intentioned politicians would change dramatically their estimates of the amount of government-funded research that can be called “waste.”

2. In your testimony, you state that "Agencies like the NSF are in the best position to prioritize federal funding for SBE research..." Besides highlighting "transformative" research, how else can NSF prioritize research? Are there other elements that you would suggest focusing on to guide prioritization?

In responding to this question, I would like to highlight one guiding principle: **Set universal criteria for impact and value, but do not weaken science by setting specifics.**

Let me start by emphasizing what we should *not* be doing: as discussed above, we should not attempt to dictate from the top what constitutes good science. The history of science suggests that one cannot predict in advance exactly what agenda scientists should follow—any more than the planned economies of the former Communist block could have resulted in flourishing business sectors.

In the sciences, curious minds make connections that can seem ridiculous to everyone else—until these ideas are proven brilliant, often much later. After all, outside of the scientific community, who

would have told Pasteur to study spoiled milk? Further, much of innovation results from mistakes, such as the mold growing on a laboratory dish that led to the discovery of penicillin.

Accordingly, we need funding for the basic sciences that allows researchers to develop fundamental principles to understand the world around us. Any attempt to use a big government approach to impose specific fields, topics, or other narrow criteria is bound to be the subject of intense political debate—and to become outdated as the issues pressing to our nation change over time. Perhaps more importantly, external constraints can ruin the innovation process, because the best minds value their intellectual freedom the most, and they use it to travel in the most creative directions.

Of course, there is also a place for topic-driven research, but to be clear that is the definition of *applied* rather than *basic* science. NSF is the government source for basic science. As such, any move towards specifying topics would represent a change to the fundamental mandate of this agency and its distinct role within the federal government. However, for applied research—which focuses on specific topics and specific problems facing the US—researchers can readily approach the private sector, foundations, and other government agencies.

So what kinds of elements are appropriate in guiding priorities at the NSF?

As discussed in my earlier testimony, I strongly support the recent attempts by the NSF to focus on research that is transformative. Along these lines, there are a number of criteria that often indicate when work is particularly likely to be transformative—and examples of these are detailed below. Focusing on research that has at least some of these criteria is likely to improve innovation in basic science and to increase the return-on-investment to the American taxpayer:

- **Encourage interdisciplinary research**
Creativity often happens when path-breakers cross over the boundaries of traditional academic fields. They bring existing solutions from one area to solve the problems faced by another area. (For example, Edison's early light bulbs kept falling out of their sockets until a lab assistant 'borrowed' the solution of screw-top caps from the makers of kerosene.)
- **Encourage early-stage researchers**
Creativity is enhanced with fresh perspectives from people with new ideas and new energy. They are less influenced by habits about how things have always been done.
- **'Spread the wealth'**
Take seriously the idea that NSF investments are like a portfolio—a winning strategy for high returns is to hedge one's bets by investing in many different ideas. You never know for certain where the next big discovery will begin. One strategy for casting a wider net is to increase the number of smaller grants, and to reduce the turnaround time to review small grants.
- **Encourage risk-taking**
Again, portfolio managers seek out risky ideas, with the logic of 'high-risk-high-return'. One way to take calculated risks is to provide riskier ventures with smaller grants, or with grants that are administered in stages pending milestones.

- **Look for cross-disciplinary impact**

The pattern of scholarly citations can indicate when researcher's ideas are influential across fields and subfields—regardless of whether the research team itself draws from multiple disciplines. This criterion could be applied most easily to senior researchers.

- **Encourage adversarial collaboration**

When researchers disagree with each other, an innovative solution that has emerged is for them to conduct a study together. Typically a mutually respected third-party acts as an arbitrator. Resolving key scientific debates can help to direct future efforts in the most productive directions.

Note that what distinguishes these kinds of helpful criteria from the dangerous specifics is that they are universally applicable across topics of study.

Even while emphasizing transformative science, the NSF still needs room to fund normal science that fills the gaps in our understanding of important research questions. Again, using the metaphor that grants funded by the NSF are an investment portfolio, every well-balanced portfolio needs to have some secure investments, such as bonds or money market accounts.

In closing, let me emphasize that government funding of the basic social sciences will always be necessary for a healthy engine of innovation.

I have faith that the members of Congress who are closest to these issues have the sophistication to seek counsel from those individuals most informed about the topics.

Notably, during the subcommittee meeting on June 2nd, one of my co-panelists made a number of statements that reflected a simple lack of education about scientific research. Ms. Furchtgott-Roth presented a great deal of misinformation about the resources currently available to support basic science. For example, researchers cannot sell their articles to academic journals, because these non-profit enterprises—unlike popular magazines—do not actually pay authors for their work. More important and damaging was the misinformation that there are numerous private corporations and foundations standing ready to fund basic research in the social sciences if the government exits this domain. Corporations will fund only the research that is close to commercialization—which eliminates most basic research. Further, corporations typically withhold from the public the results of their privately-funded research and, thus, this work would not be available to other scientists. And foundations will typically fund only the research that is related to a specific problem of immediate concern—which, again, eliminates most basic research.

The same co-panelist also pointed out that many of the most important early advances in the social sciences were made without government funding, such as Adam Smith's work in economics—implying that today's government funding is not necessary. However, what Ms. Furchtgott-Roth left out is that historically many of these advances without government funding came from researchers who were supported financially by religious institutions (e.g., residence in monasteries) or by independent family wealth. It is hard to imagine that members of Congress would want advances in the social sciences to come primarily from researchers funded by these two sources.

In spite of these remarks from my co-panelist, which indicated a lack of general understanding of the topic matter, it seems that their related conclusions were emphasized in the Subcommittee's official summary of our hearing posted at the website below:

<http://science.house.gov/press-release/witnesses-assess-value-taxpayer-federal-spending-social-sciences>

I can only assume that this emphasis was made inadvertently by staff and members of Congress who were unaware that the conclusions relied on misinformation presented during the hearing. Again, I have faith that members of Congress will rely on informed subject matter experts when weighing such important policy issues as the NSF's funding of basic social science research.

Thank you again for this opportunity to serve.