

## **Accountability and Public Value in Publicly Funded Science**

Testimony before the U.S. House of Representatives  
Committee on Science, Space, and Technology  
Subcommittee on Research and Technology  
November 13, 2013

Hearing on: Keeping America FIRST: Federal Investments in Research, Science, and Technology  
at NSF, NIST, OSTP and Interagency STEM Programs

Daniel Sarewitz  
Co-Director, Consortium for Science, Policy, and Outcomes  
Professor of Science and Society, Arizona State University

Mr. Chairman, Members of the Committee, thank you for inviting me to testify today. My name is Daniel Sarewitz, and I am co-founder and co-director of the Consortium for Science, Policy and Outcomes at Arizona State University, as well as Professor of Science and Society at ASU. My formal academic training is in geosciences, but for almost 25 years I have worked in science and technology policy, first as a AAAS Congressional Science Fellow and then a staffer on this Committee, working for Chairman George E. Brown, Jr., and more recently as an academic, for the past nine years at ASU. I'm always very pleased to return to the place that launched me on a new and incredibly interesting and exciting career and intellectual journey, and honored that you have asked for my input to the Committee's deliberations on how the Nation can make the best possible investments in science and technology.

In the context of this hearing I also want to make clear that I have been extraordinarily privileged during my career in having received generous research funding from the National Science Foundation, mostly from the Directorate for Social, Behavioral, and Economic Sciences, but as well from the Geosciences and Engineering directorates. I have also served as a peer reviewer of many NSF proposals, and as a review panel member for a number of NSF programs. So my perspective reflects not only my research on science policy, but also my direct involvement in many aspects of the enterprise, from the Hill to the university.

### **Introduction: A Problem Long in the Making**

The cover story of the October 19<sup>th</sup> issue of *The Economist* is entitled "How Science Goes Wrong." The article takes the lid off a problem that has been simmering for decades. The basic question is whether the scientific community itself can assure the quality of the research results that it produces. We have all been taught to believe that accountability in science is indeed an internal matter—that peer review, competition, the scientific method, an insistence that results be reproducible and replicable, and an overall culture of skeptical inquiry and devotion to objectivity was all that was necessary to ensure that the products of science were of the highest quality, greatest reliability, and most value to society. Moreover, those who insist that accountability in science is automatically delivered through the culture and practice of science

itself have also commonly asserted that efforts by outsiders, however well-meaning, to improve accountability will actually make things worse by substituting the politics of the outside world for the self-correcting essence of the scientific world.

*The Economist* article provides a valuable summary of troubling evidence, across many fields of research, that the internal mechanisms of scientific accountability are insufficient and are to some extent failing. Such evidence includes failed efforts to reproduce the results of research published in high-prestige, peer-reviewed journals; increasing rates of retraction of published results; major areas of research that yield little of public value but continue to attract significant public resources; and even what amount to experiments that show that the peer review process is unable to distinguish scientifically valid papers from those that are worthless. While much of this is familiar to those of us who work in the field of science policy, the fact that it made the cover of *The Economist*, a pragmatic, centrist international magazine that is strongly supportive of robust national investments in research and development, makes clear that the issue can no longer be dodged.

Two observations about this problem need to be emphasized from the outset. The first is that scientific misconduct—that is, the intentional manipulation or fabrication of scientific results—is not the major cause of the problems of scientific accountability and reliability covered in *The Economist* article. To a much greater extent the problem seems to be traceable to numerous sources of systemic positive bias (that is, bias toward results that confirm the ideas being researched) in the research system, a problem I'll come back to. The second observation is that the solution to the reliability problem cannot lie wholly with the science enterprise itself. *The Economist* article, and leaders in the scientific community, have appropriately emphasized the need for improved training in statistics and experimental design, better mechanisms of peer review, changes in publication policies that allow an increased focus on negative findings, and so on, in order to address systemic problems of reliability, reproducibility, and positive bias in science. But the underlying causes of the problem lie with the institutions and cultures of science, and thus will not likely be solved without incentives for change that come from outside of the science enterprise itself.

Let me emphasize there is nothing new about this general problem, and about this tension between external and internal accountability. Like any community, the scientific enterprise would like to be left alone to govern its own affairs. Over the past five decades or so, issues relating to scientific accountability have emerged around such questions as: informed consent of human research subjects; patenting and technology transfer; ethical treatment of primate research subjects; research on alternative and complementary medicine; the use of stem cells in research; the gender composition of clinical trials; the risks of emerging technologies; and the broader social impacts of basic science. In these cases and more, pressure from outside the scientific community for new mechanisms of accountability have been resisted by that community on the basis of its claim that political interference would only weaken science. But in each case the tension and the resulting negotiations have led to changes that have managed to protect the prerogatives of science while also helping to meet science's obligations to a democratic society.

It is, therefore, not only appropriate but necessary to explore ways to improve scientific accountability to society through improved governance of the science enterprise. In this light, I want to compliment the Members of the Committee for beginning to confront the difficult problem of improving accountability in publicly funded science through the provisions in Title 1 of the discussion draft of the Frontiers in Innovation, Research, Science, and Technology (FIRST) Act of 2013.

With this brief, context-setting introduction, let me respond to the questions that the Committee has asked me to address.

**Question 1. What are your concerns regarding the existing grant approval process within the National Science Foundation? How does this proposed bill address policy concerns regarding the accountability of the research grant process at the NSF?**

NSF takes seriously its responsibility to subject its proposals to a rigorous peer review process and to select projects for funding based on merit as judged by expert peers. The process can likely be improved, in some ways that I'll suggest, but at the same, peer review is not the only, and is likely not the most effective, intervention point for improving the accountability of publicly funded science to the public, or for improving the potential value of NSF-funded research to society.

Most of us hold in our head an idealized view of science that corresponds to something like Albert Einstein figuring out the relationships between time and space while sitting at his desk in the Swiss patent office. There's the scientist, there's nature, and nothing in between but the scientist's brain and the methods of scientific inquiry. But most science isn't like that at all. Most scientists are like soldiers, laboring in the trenches as part of a much larger effort to accomplish a much bigger goal. Most individual projects can do no more than take another step or two towards understanding some larger problem, train a few more young scientists, demonstrate, usually not for the first time, the utility of some particular tool or method, and so on. Science is largely an incremental business, and major advances rarely come from any one particular project. Nor is any single project ever likely to make a discernible impact on societal outcomes. We want our scientists, and our soldiers, to act with integrity, to do their job well, and help advance our nation's interests as a cumulative consequence of their work. But whether scientists are working on the right projects, in the institutional settings that are most likely to lead to new knowledge that will become valuable for society, is much more a function of the character of those institutional settings than of things that any individual scientist is likely to achieve or able to control.

Title 1, Sec. 104: The grant approval process at NSF mostly reflects judgments that are made by peer reviewers. This is the soldier's point of view, not the field commander's. My point here is a bit complex, but in brief the value of the peer review process for society is only as good as the priorities and institutions within which it is working. I am very sympathetic with the Committee's effort to encourage a greater degree of accountability at NSF for ensuring that NSF

uses public moneys for science that is of good quality, and for the public good. I also think that Title 1, Section 104, on “Greater Accountability in Federal Funding For Research,” is clear in its intent to allow the peer review process to do its work, while adding another level accountability over peer review, which in principle is totally appropriate.

But I have concerns that the provisions of Title 1, Section 104, won’t appreciably advance the goals that the Committee seeks. For one thing, as I’ve suggested, the individual project is probably not the most effective point to intervene in the grant system if accountability for broadly advancing science and the national interest is the goal. For another, the list of eight criteria that would be used to determine if a particular grant is worthy of federal support is both very general and broadly inclusive. It’s hard to imagine that any competently conceived and written proposal that made it through peer review couldn’t also pass that second gauntlet unscathed. My guess is that the scientific community might therefore be concerned that decisions made at this next level would likely be subject to a political filter, rather than a scientific one. I would raise a different concern: I think, as written, this provision could actually act against the Committee’s aims by adding a meaningless level of rubber stamping to the grant approval process.

Yet I do think there are questions that could be asked at the project level that would help NSF and Congress achieve their mutual and hopefully commensurate goals of improved accountability. Let me suggest two.

First, a post-review accountability process could ensure that peer review panels have given “full consideration,” as required in NSF’s proposal guidelines, to both of NSF’s review criteria: “intellectual merit” and “broader impacts.” This level of accountability recognizes that the advance of basic science and the pursuit of particular desired impacts are often strongly interrelated. For example, a grant proposal in sustainable chemistry might promise that a new class of chemicals being researched would be valuable to industry because it would allow firms to reduce their exposure to expensive regulation and litigation through a cleaner production process. But if the researchers proposing this work do not have strong connections to companies that might eventually benefit from such advances, then it would be difficult for them to ascertain if their research actually would be useful for industry, and implausible that they would be able to identify effective lines of technology transfer.

Second, a post-review accountability process could focus on identifying and reducing hype in the proposal process. The super-competitive environment for getting federal grants strongly incentivizes hyping the potential for any proposed project to yield results that are important, ground-breaking, “transformational,” and so on. Hype is encouraged by universities looking to promote the research accomplishments of their faculty, but also by the promises of NSF and, it must be acknowledged, by the expectations of Congress. Hype is invited in research proposals both in claims of scientific importance, and in claims about “broader impacts” of a project to achieve social goals beyond its intrinsic scientific merit. Hype not only serves to inflate expectations about what a project might accomplish, but it also likely contributes to the bias problem discussed in the *Economist* article, by incentivizing researchers to look for positive

results even when the evidence for them is weak or even absent. So another function that a post-review accountability process could serve is to identify over-hyped proposals. This should be done rigorously, by analyzing the specific claims and promises of intellectual merit and broader impacts that are made in the proposal, and assessing their plausibility on the basis of a broader understanding of the state of the field being researched, as well as the technical and institutional capabilities available to the grant applicant.

Post-peer-review accountability could therefore focus on assuring that grants give full consideration to both NSF review criteria, and that they are not over-hyped. Implementing such a process would likely improve the peer review process itself in three ways. First, it would encourage applicants and reviewers alike to take seriously the “broader impacts” criterion and its integral relation to the “intellectual merit” criterion. Second, it would encourage peer reviewers and review panels to be skeptical about hype, and incentivize grant applicants to be more realistic in explaining the value of their work. But most importantly, it would require NSF to embrace an expanded definition of the types of expertise that needed to be involved in peer review processes, and a better balance among various types of expertise involved in the review process. For example, to continue with the hypothetical case of sustainable chemicals research, reviewers with expertise in the relevant industrial processes, in the regulatory regime, in the business models of the affected sector of industry, and in university-industry collaborations might all be directly relevant to assessing the merits of the proposed research—in addition to the academic experts in the specific field of chemistry that would constitute the standard peer review group. Such an extended peer review community could be a valuable source of enhanced accountability.

**Question 2. What approaches or strategies might NSF pursue, during these tight fiscal times, to prioritize research which supports innovation and competitiveness?**

Let’s begin by establishing something upon which I think we can all agree: The National Science Foundation is a remarkably effective federal agency that has not only done an excellent job, overall, in carrying out its mission over the decades since its creation in 1950, but has taken on an increasingly complex set of activities in support of that mission, and in doing so has often been admirably innovative and open in seeking to meet the evolving science needs of a changing nation and world. In this context, a major problem in terms of improving NSF’s focus on innovation and competitiveness is the complexity of the agency’s mission. The goals of advancing knowledge and the science base, supporting graduate education and training through research practice, providing research infrastructure, supporting STEM education and public understanding of science, and advancing basic knowledge to support particular outcomes such as competitiveness, are in many ways distinct, and in some ways contradictory. This may lead to confusion, the biggest source of which is the widely held but false belief that “basic” science must be divorced from any consideration of application.

An important 1994 article by the economists Nathan Rosenberg and Richard Nelson documents that the vital contribution of American universities to industrial advance since the second half of the nineteenth century has been through the creation of basic scientific knowledge relevant

to industrial needs. As the authors are careful to note: “A widely accepted definition of basic research has come to focus on the absence of a concern with practical applications rather than the search for a fundamental understanding of natural phenomena. This is unfortunate, indeed bizarre.”<sup>1</sup>The authors go on to explain, “It is a gross misconception to think that if research is ‘basic’ this means the work is not motivated by or funded because of its promise to deal with a class of practical problems.”<sup>2</sup>

A good illustration of this important distinction is the discovery, in 1948, of the transistor effect, which helped to launch the information and communications revolution that has created the world that we live in today. It is well known that this phenomenon was first identified, through research that was undeniably basic, at ATT’s famous Bell Laboratories. As a research institution, Bell Labs was consciously designed to mix “scientific curiosity, technological utility, and corporate goals” in advancing innovation for telecommunications. Yet at the same time as the Bell group was making their famous discovery, another group of academic physicists, at Purdue University, was working on a similar problem of semiconductivity. Historians of technology have speculated that, had the Purdue group “been looking for a solid-state amplifier, instead of exploring general physical phenomena, [they] would have invented the transistor.”<sup>3</sup>

Now I want to clearly explain that I am not suggesting that NSF should be supporting scientists who are directly in the business of developing industrial products. But keep in mind that the work done at Bell Labs, and the work done at Purdue, were both “basic science,” it’s just that at Bell there was an awareness of the larger context in which the new knowledge might be valuable, and direct linkages to other parts of the innovation process that could make use of that new knowledge. This awareness of contexts, and these linkages to the world beyond the laboratory walls, are attributes that can and sometimes do exist at universities. Not only that, it was only *after* the discovery of the transistor effect that the basic science field of semiconductor physics really blossomed at major research universities—precisely because the potential value of transistors for innovation required rapid advances in basic knowledge—a point that illustrates how basic academic science itself can benefit from links to the industrial context.

The larger point here is that if the committee is interested in improving NSF’s capacity to contribute to innovation and competitiveness through advances in basic science, one way to do this is to focus on creating mechanisms that enhance communication and exchange between academic researchers and those involved in actual innovation processes. This will often mean collaboration with industrial firms, but also with state and local governments and non-profit

---

<sup>1</sup> Rosenberg, N, and Nelson, R., 1994, American universities and technical advance in industry, *Research Policy* 23, p. 332.

<sup>2</sup> Ibid, p. 340.

<sup>3</sup> Misa, Thomas J., 1985, Military Needs, Commercial Realities, and the Development of the Transistor, 1948-1958, in: M.R. Smith, ed. *Military Enterprise and Technological Change: Perspectives on the American Experience* (Cambridge, MA: MIT Press), p. 257.

organizations that need new scientific and technological capabilities to solve problems. Indeed, NSF has, through its Engineering Research Centers, Materials Research Science and Engineering Centers, and Nanoscale Science and Engineering Centers, sought to do that in selected domains. Arguments that such interactions will inevitably limit the imagination of scientists, and thus limit as well the potential contributions of science to industrial advance, are groundless. The goal is not to shackle academic science to an industrial agenda, but to accelerate the learning that can take place between researchers at universities and in industry. As the case of transistors shows, such collaboration can in fact lead to the explosion of new fields of academic basic research.

Thus, if the Committee wishes to encourage NSF to more effectively support innovation and competitiveness in specific, and the linking of basic science to societal advance more generally, it should focus on encouraging communication, interchange, and understanding among university scientists and the potential users of new fundamental knowledge in industry and other sectors. Again, turning to Rosenberg and Nelson, if university scientists are to conduct basic research that has strong potential to contribute to innovation, there “must be close communication and interaction between those who do research, and those who are responsible for product and process design and development . . .”<sup>4</sup> These links cannot be achieved by advisory committees or tech transfer offices or annual meetings, but require meaningful and ongoing interactions via shared positions, personnel exchanges, jointly supported students, joint project review teams, and so on.

This approach raises interesting opportunities for the Committee and NSF to rethink the problem of accountability. Because the effective linking of fundamental scientific advance to innovation and competitiveness requires a focus on appropriate institutional arrangements, accountability cannot be assessed merely in conventional terms of “scientific excellence.” Rather, it must be sought in the quality and persistence of relationships between academic scientists and their collaborators beyond the university walls. This means assessing projects and programs not merely in terms of promised “intellectual merit” and “broader impacts” but also through evidence of linkages to and engagement with firms and other organizations and sectors that are prospective users of the science being done. I want to emphasize that such linkages also provide a direct and important check on research quality, because firms and other knowledge-using organizations will not only demand, but may be in a position to test, the reliability and relevance of the scientific knowledge that they are getting from academic collaborators. Indeed, some of the most powerful recent evidence of systemic unreliability in biomedical research has come from biotechnology firms that have been unable to replicate the results of academic research projects.

For this Committee there is a question of appropriate expectations here as well. The impacts of fundamental scientific advance on innovation and competitiveness are typically gradual and not highly predictable. Hinging research accountability on promises to achieve economic results would therefore be a mistake, and can mostly be recognized as hype. But hinging

---

<sup>4</sup> Rosenberg and Nelson, Op. cit. p. 346.

accountability on the demonstrated ability of projects and programs to forge persistent and meaningful linkages between NSF-funded researchers and collaborators in industry and other sectors would be an entirely appropriate lens for assessing the capacity of NSF's basic academic research activities to support innovation and competitiveness.

### **Question 3. Why is it important that the United States address predictable, sustainable future science funding?**

Warnings about unsustainable expansion of the post-World War II American science enterprise date back to the early 1960s. In this light it is, or at least should be, uncontroversial to note that the academic scientific community has been utterly unable to control its own growth, and thus its demand for public funding, despite a widespread awareness that continued expansion of the science enterprise must eventually outstrip the capacity of the federal government to sustain that growth.

The causes of this expansion are well documented.<sup>5</sup> In part they are simply a function of population dynamics: professors in our research universities train new Ph.D. scientists at a much faster rate than the academic research enterprise can absorb them or than the research funding system can support them, and have been doing so for the past fifty years. At the same time, more and more universities have come to recognize that fielding prestigious scientific research programs across multiple fields is part of the formula for success for the modern university. It is basically impossible for a university to attract good students or good faculty, or to do well in the national rankings, or even, to an extent, to get big alumni gifts, without a robust scientific research capacity.

Voices in the scientific community have of course periodically proclaimed that a funding crisis was occurring or was about to occur, and that the prospects of the nation were thus in jeopardy and could only be addressed through additional federal spending on science. Meanwhile, federal support for research has grown robustly in real terms (for example, from FY 1976 – FY 2013 total federal spending for research, defense and non-defense, rose from \$25.8 billion to \$63.3 billion in constant 2013 dollars<sup>6</sup>). Nonetheless it was never plausible that budgetary growth would keep up with demand for funding, given, as just one example, that the number of science and engineering Ph.D.'s produced annually has risen from about 5600 per year in the late 1950s to about 27,000 per year today.<sup>7</sup> This tension between has come to a head in the current funding climate.

I should emphasize at this point that the problem here is not that science has not been given adequate priority across the portfolio of national needs. Federal R&D funding as a proportion of the total non-defense discretionary budget has stood at a remarkably stable 10% or so for more

---

<sup>5</sup> For an excellent recent discussion of the problem and some possible solutions, see Howard, D. and F. Laird, 2013, *The New Normal in Funding University Science, Issues in Science and Technology*, (Fall), pp. 71-76.

<sup>6</sup> <http://www.aaas.org/spp/rd/guihist.shtml>

<sup>7</sup> <http://www.nsf.gov/statistics/nsf06319/>

than forty years.<sup>8</sup> In other words, through good times or bad, the government has seen fit to devote approximately 10% of its discretionary resources to science. Whether this is the right number, or should be more, or less, is not objectively answerable, but the stability of the number over time does demonstrate a remarkably consistent level of commitment.

That being said, as the demand for resources has continued to outstrip growth in the amount of federal funding, competition has become increasingly fierce for federal research dollars. I suspect you are familiar with these sorts of numbers, but for example success rates for proposals to NSF and NIH have declined from around 30% on average in the 1990s to 20% or less today<sup>9</sup>—this at the same time that universities increasingly pin their own reputations on their ability to mount top-flight science programs across diverse disciplines. At the same time, promotion, tenure, and professional stature, not to mention the resources necessary to pursue one’s scientific interests, depend on getting federal grants, and NSF plays a particularly important role here as the one agency that funds basic academic science across almost all fields of endeavor. Professional advance also depends on publishing results, preferably in high-prestige journals, preferably demonstrating important discoveries that can set one apart from one’s peers and give one a competitive edge in the pursuit of still more research funds. And so on.

A key point documented in the *Economist* article, one that seems broadly accepted by the scientific community as a whole, is that the hyper-competitive nature of the academic research enterprise puts a premium on a particular kind of success in one’s research: the success of making new discoveries, of new findings that attract the attention and envy of one’s peers, that make it into university press releases, and onto the pages of the best journals. The system, that is, has become pervasively biased toward the achievement of positive results from research—even as the problems that science is dealing with become more complex, interdisciplinary, and difficult. The peer review system has limited capacity to police and control this bias: peer reviewers are every bit as busy as the scientists they are reviewing; peers may well have bought into exactly the same assumptions and biases that influence the research to begin with; and besides a reviewer can’t wade in and reproduce everything that’s reported in a paper, or address all of the interdisciplinary issues raised in a manuscript authored by a team of scientists, as so many today are. They can look for errors, for implausible assumptions or conclusions, inappropriate methods and so forth, but in the end their ability to vouch for the validity of an asserted scientific finding is limited.

Title 1, Sec. 110, 112, 113, 114, 116: The Committee’s desire to ensure that research dollars are well-spent, that research results are not misrepresented, that large facilities are openly re-competed, that peer review is not biased toward more senior researchers, and that “alternative research funding models” are explored, is laudable, yet somewhat scattershot. A key strategic goal here must be to maintain the capacity, productivity, and integrity of the research system despite the fact that federal support for science is not likely to increase significantly over the

---

<sup>8</sup> <http://www.aaas.org/spp/rd/Hist/BudgetDISC.jpg>

<sup>9</sup> Howard and Laird, op. cit.

next several years—and despite the fact that, over the longer term, no plausible amount of funding growth can slake the ever-increasing appetite for money of the academic research system as it is currently organized. And let me re-emphasize the links here between expectations and integrity. To the extent to which academic scientists are judged by how much federal money they can bring in through research grants, the resulting hypercompetitive environment in turn feeds the systemic bias toward positive findings, as investigators, peer reviewers, program managers, university administrators, and policy makers alike harbor the expectation that every project must be somehow leading directly to an important new discovery that can advance science and solve social problems.

Although incentives that help steer universities and the science community toward more realistic expectations and more sustainable behavior must come mostly from the universities themselves, the NSF, and other federal R&D funders, can contribute to the creation of a more sustainable science enterprise, and this Committee can provide guidance to help them do so. What sorts of “alternative funding models” and other policies might contribute to a longer term establishment of a more sustainable science enterprise? The Committee could explore a range of approaches. Here are some possibilities:

1. Universities often try to “poach” highly productive faculty from other universities, especially those who bring with them big federal grants. This competition helps to drive up the salaries and resource demands of “star” researchers in ways that make the system increasingly costly and unsustainable. NSF could provide a disincentive to this practice by prohibiting the transfer of grants from one institution to another.
2. NSF could provide preference in funding competitions to (a) researchers whose previous work has been replicated by independent research groups; (b) researchers whose academic units assess quality of publications, rather than quantity, as a criteria for promotion and tenure; (c) researchers who can demonstrate that their research has been positively influenced by engagement with knowledge users outside of the university setting, or that results from previous projects have been applied to real-world problem solving; and so on.
3. NSF could broaden the range of academic research projects that require partnerships between universities and other entities, including firms, non-profit organizations, philanthropic foundations, museums, state and local governments, and so on. The goal of such partnerships would in part be cost-sharing, but equally important would be creating the linkages between knowledge creation and knowledge use that can increase the social value of scientific research.
4. NSF could fund “science shops” that support university faculty working directly with local and regional governments or organizations to address problems with scientific and technical underpinnings. Such programs would be relatively inexpensive ways to encourage academic scientists to engage with their local communities, and could also be a powerful laboratory for orienting science students towards career paths aimed at solving real-world problems.

5. NSF could competitively fund “red team” projects aimed at replicating (or falsifying) research results from high-priority or high-profile lines of research; they could similarly fund “sensitivity auditing” groups that would assess the scientific robustness of computer models used in a variety of fields with potential application to policy making.

6. In support of these, or any like-minded, efforts to improve the accountability of the science enterprise and the public value of the nation’s investment in science, this Committee could play a direct positive role by working with NSF to ensure that the agency has sufficient and appropriate staffing capabilities to carry out the sorts of programmatic innovations that will be necessary. In the absence of an adequate administrative capability at NSF, the default will be to revert to the standard, entrenched programmatic practices that have attracted the Committee’s attention to the need for action in the first place.

While these suggestions are made tentatively, I want to emphasize that cumulatively, a portfolio of such policies, strategically conceived and carefully implemented, might have the effect of helping to catalyze a shift in the incentive structure and culture of university science in ways that could better allow the federal government to ensure sustainable, long-term support, and improved public value for our public investment in scientific research.

As I’ve tried to emphasize in my testimony today, there are many interrelated issues at play here: the science community’s expectation that resources must always expand to keep up with demand; the fierce, often counter-productive competition resulting from such an expectation; the incentives and reward structure of the research enterprise; the links between considerations of scientific excellence and broader impact; and ultimately and most importantly, the need for improved ways to assure accountability for the delivery of research results that are scientifically reliable and societally useful. There will be no single policy intervention that can productively address all of these issues together, yet it is important to recognize that neither can they be considered or addressed separately. What I’ve tried to suggest is that there are many possible intervention points where relatively modest changes in policy or priorities might move things in the direction of stronger accountability and greater public value. Today’s hearing offers a valuable opportunity for consideration of such options.

Daniel Sarewitz is Professor of Science and Society, and co-director and co-founder of the Consortium for Science, Policy, and Outcomes (CSPO), at Arizona State University ([www.cspo.org](http://www.cspo.org)). His work focuses on revealing and improving the connections between science policy decisions, scientific research and social outcomes. His most recent book is *The Techno-Human Condition* (co-authored with Braden Allenby; MIT Press, 2011). He is editor of the magazine *Issues in Science and Technology* ([www.issues.org](http://www.issues.org)) and is also a regular columnist for the journal *Nature*. From 1989-1993 he worked on R&D policy issues for the U.S. House of Representatives Committee on Science, Space, and Technology. He received a Ph.D. in Geological Sciences from Cornell University in 1986. He directs the Washington, DC, office of CSPO, and focuses his efforts on a range of activities to increase CSPO's contribution to federal science and technology policy processes, and to improve public debate about scientific and technological issues.