

# INVESTING IN HIGH-RISK, HIGH-REWARD RESEARCH

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## HEARING

BEFORE THE  
SUBCOMMITTEE ON RESEARCH AND  
SCIENCE EDUCATION  
COMMITTEE ON SCIENCE AND  
TECHNOLOGY  
HOUSE OF REPRESENTATIVES  
ONE HUNDRED ELEVENTH CONGRESS

FIRST SESSION

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OCTOBER 8, 2009

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**Serial No. 111-55**

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**INVESTING IN HIGH-RISK, HIGH-REWARD  
RESEARCH**

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**THURSDAY, OCTOBER 8, 2009**

HOUSE OF REPRESENTATIVES,  
SUBCOMMITTEE ON RESEARCH AND SCIENCE EDUCATION,  
COMMITTEE ON SCIENCE AND TECHNOLOGY,  
*Washington, DC.*

The Subcommittee met, pursuant to call, at 1:07 p.m., in Room 2318 of the Rayburn House Office Building, Hon. Daniel Lipinski [Chairman of the Subcommittee] presiding.

BART GORDON, TENNESSEE  
CHAIRMAN

RALPH M. HALL, TEXAS  
RANKING MEMBER

U.S. HOUSE OF REPRESENTATIVES  
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SUITE 2321 RAYBURN HOUSE OFFICE BUILDING  
WASHINGTON, DC 20515-6301  
(202) 225-6375  
<http://science.house.gov>

Hearing on:

***Investing in High-Risk, High-Reward Research***

***Thursday, October 8, 2009  
1:00 p.m. – 3:00 p.m.  
2318 Rayburn House Office Building***

**Witness List**

***Dr. Neal F. Lane***

*Malcolm Gillis University Professor and Senior Fellow, James A. Baker  
III Institute for Public Policy, Rice University*

***Dr. James P. Collins***

*Assistant Director for Biological Sciences, National Science Foundation*

***Dr. Richard D. McCullough***

*Professor of Chemistry and Vice President of Research, Carnegie Mellon  
University*

***Dr. Gerald M. Rubin***

*Vice President and Director, Janelia Farm Research Campus, Howard  
Hughes Medical Institute*

HEARING CHARTER

**SUBCOMMITTEE ON RESEARCH AND SCIENCE  
EDUCATION  
COMMITTEE ON SCIENCE AND TECHNOLOGY  
U.S. HOUSE OF REPRESENTATIVES**

**Investing in High-Risk,  
High-Reward Research**

THURSDAY, OCTOBER 8, 2009  
1:00 P.M.–3:00 P.M.  
2318 RAYBURN HOUSE OFFICE BUILDING

**1. Purpose**

The purpose of this hearing is to examine mechanisms for funding high-risk, potentially high-reward research, and the appropriate role of the Federal Government in supporting such research.

**2. Witnesses:**

- **Dr. James P. Collins**, Assistant Director for Biological Sciences, National Science Foundation.
- **Dr. Neal F. Lane**, Malcolm Gillis University Professor and Senior Fellow, James A. Baker III Institute for Public Policy, Rice University. Dr. Lane was a member of the American Academy of Arts & Sciences committee that published the report, *ARISE: Advancing Research in Science and Engineering*.
- **Dr. Richard D. McCullough**, Professor of Chemistry and Vice President of Research, Carnegie Mellon University.
- **Dr. Gerald M. Rubin**, Vice President and Director, Janelia Farm Research Campus, Howard Hughes Medical Institute.

**3. Overarching Questions:**

- What is high-risk, high-payoff research? How does it differ from the research traditionally funded by federal science agencies? What metrics should be used to evaluate the success of any approach to funding high-risk research?
- Relative to the total funding for basic science and engineering research from all sources, is the current level of support for high-risk research appropriate? If funding for high-risk research were to be increased as recommended in several recent reports, what should be the responsibility of the Federal Government in achieving that increase, and how does that responsibility differ from that of private sector research organizations and funding sources as well as research universities?
- How can federal science agencies such as the National Science Foundation (NSF) increase their support for high-risk research? In particular, what are the pros and cons of establishing targeted programs or set-asides for high-risk research versus changing how proposals are reviewed and selected across an agency's research portfolio? What are the biggest challenges or risks associated with each of these approaches?

**4. Background**

*What is high-risk, high-reward research?*

The terms 'high-risk, high-reward' (or 'high-risk, high-payoff') and 'transformative' research are often used interchangeably. The National Science Board has proposed the following definition for transformative research:

*Transformative research is defined as research driven by ideas that have the potential to radically change our understanding of an important existing scientific or engineering concept or leading to the creation of a new paradigm or field of science or engineering. Such research is also characterized by its challenge to current understanding or its pathway to new frontiers.*

The Board, mindful of NSF's unique role in funding basic research across the disciplines, says nothing in its definition about research leading to new technologies or solutions to societal challenges. Federal mission agencies, on the other hand, use a mission inspired definition for high-risk, high-reward research, or some comparable term. For example, a few years ago NIH created the Pioneer Awards for this purpose.

*The term "pioneering" is used to describe highly innovative approaches that have the potential to produce an unusually high impact on a broad area of biomedical or behavioral research.*

A handful of philanthropic organizations also invest in high-risk research. One such organization, the Keck Foundation, makes a distinction between "high-risk" and "transformative" as follows:

*"High-risk" comprises a number of factors, including questions that push the edge of the field, present unconventional approaches to intractable problems, or challenge the prevailing paradigm. "Transformative" may mean creation of a new field of research, development of new instrumentation enabling observations not previously possible, or discovery of knowledge that challenges prevailing perspectives.*

What is common to all definitions of high-risk, high-reward, or transformative (or pioneering) research is a tolerance for failure that departs from the overwhelming tendency, within the federal system at least, to fund research for which there is already a proof of concept or preliminary data, and for which the likelihood of achieving the stated aims is pretty high. In other words, scientists and engineers are not encouraged by the current federal funding system to propose their wildest (but scientifically sound) ideas; rather, they believe their only chance at getting funded is to propose something that they already know will work.<sup>1</sup> The resulting incremental advances in science and engineering are a necessary, but not sufficient element of the science and technology enterprise. In many if not most cases, great breakthroughs and paradigm shifts in S&T were the result of scientists and engineers stumbling upon some unexpected result or suddenly imagining some new application and then having the funding and/or flexibility to alter their research plans accordingly.

#### *The call for a greater federal role in funding high-risk research*

In 2006, the National Academies Committee on Prospering in the Global Economy of the 21st Century released the report, *Rising Above the Gathering Storm*, that became both the impetus and intellectual foundation for the 2007 *America COMPETES Act*. In addition to the many recommendations regarding K–12 STEM education, funding for basic research in the non biomedical sciences, and creation of an ARPA–E that were implemented as part of the COMPETES Act, the Academies Committee recommended that at least eight percent of the budgets of federal research agencies should be set aside for discretionary funding managed by technical program managers in those agencies to catalyze high-risk, high-payoff research. They provided no further details on how that might be done and chose eight percent because it was a compromise between committee members who thought five percent was sufficient and those who argued for 10 percent.

In 2004, the National Science Board convened a task force on transformative research to make recommendations on how the National Science Foundation (NSF) could encourage more funding of high-risk, potentially high-reward research. In the resulting 2007 report,<sup>2</sup> the Board recommended that NSF develop a distinct, Foundation-wide Transformative Research Initiative "distinguishable by its potential impact on prevailing paradigms and by the potential to create new fields of science, to develop new technologies, and to open new frontiers." Beyond defining transformative research and stating that the NSF Director's leadership is essential its success, the Board did not go into any details on how such an initiative should be

<sup>1</sup> One historically successful federal model for funding high-risk research is DARPA, credited with funding early development of the Internet, not to mention countless advanced military technologies. In 2007, the S&T Committee applied the DARPA model to the Department of Energy (DOE) by creating ARPA–E. ARPA–E invests in technologies that will promise true transformations in how we use or produce energy—what DOE describes on their web site as high-risk, high-payoff *concepts*. While there may be elements of DARPA and ARPA–E that are broadly applicable to all models for funding high-risk research, the ARPA model is driven by a need for mission-specific technologies, making it inappropriate for replication in basic science agencies.

<sup>2</sup> <http://www.nsf.gov/pubs/2007/nsb0732/nsb0732.pdf>



carried out, nor did it recommend a specific percentage of the NSF budget for investment in transformative research.

Perhaps in recognition of the absence of details in these reports, the American Academy of Arts and Sciences launched a new study in 2007 to develop specific recommendations for how federal agencies, universities and private foundations can encourage more high-risk, high-reward research, even absent significant growth in overall research budgets. The Academy assembled a distinguished committee of Nobel Laureates, (former) agency and National Lab directors, university presidents, private research organization directors and other notables for this purpose. The Committee also addressed support for early-career faculty, which shares some challenges in common with support for high-risk research. The resulting report, *Advancing Research in Science and Engineering: Investing in Early-Career Scientists and High-Risk, High-Reward Research (ARISE)*,<sup>3</sup> was completed in 2008.

#### *Role of Charitable Organizations and Universities*

According to NSF, non-federal, non-business entities provided \$23 billion in funding for R&D in the United States in FY 2006, out of a total of \$340 billion from all sources. This “other” category is pretty broad, including state and local governments, nonprofit organizations (e.g., charitable foundations), and universities. Funding for academic R&D in FY 2006 totaled \$48 billion.<sup>4</sup> Institutional (university) funds accounted for \$9.1 billion, or 19 percent of that total. A different category of “other” sources of funds for academic R&D, including nonprofit organizations and gifts from private individuals, accounted for \$3.2 billion, or seven percent of all academic R&D in FY 2006.<sup>5</sup>

There are many charitable foundations of varying size that fund what they consider to be high-risk, high-reward research, sometimes at universities and sometimes in their own, privately run research labs. The Keck Foundation, for example, funds academic research projects across all disciplines that might not be funded otherwise. Keck’s evaluation criteria are: 1) is this idea scientifically sound?; 2) if anyone can pull it off, can this particular individual/team?; and 3) does this individual/team have the tools at their disposal to carry out this research? In other words, Keck takes a chance on people with strong track records and access to first class research facilities. The Howard Hughes Medical Institute (HHMI) similarly takes a chance on the reputation of individual scientists, but HHMI investigators become HHMI employees, freeing them from the constant pursuit of federal support, or they join HHMI’s own world class research campus, severing ties with their home institutions altogether. Some foundations make lump-sum grants to universities and rely on the leadership within the university to run an internal competition for the best ideas.

Institutions also support their own faculty, in particular by providing start-up funds to newly recruited faculty. In the case of young investigators just starting out, the new faculty need money to build their labs and gather preliminary data before they can apply for federal funding with a reasonable chance of success. But universities may also offer generous packages to well established scientists recruited from other universities. In general, institutional funding may provide more flexibility for faculty wanting to pursue high-risk ideas than do standard federal research grants.

#### *Challenges and Approaches to Investing in High-Risk Research*

There is little doubt that flat research budgets and low proposal success rates across agencies such as NSF and NIH have contributed to more conservative funding decisions on the part of peer review panels. When budgets are constrained and success rates low, a single critical review by a peer may be sufficient to scuttle a proposal. Human nature surely plays a role as well. As an expert in the same field as the applicant, the critical reviewer may have his or her own career invested in the paradigm being challenged by the applicant. The peer-review system is, on balance, strong, functional and successful, but it is not perfect.

In general, there are two approaches to funding more high-risk research, described in detail in the *ARISE* report: creation of targeted programs or grant mechanisms, or systemic reform of the current peer-review process.

In the case of targeted programs or grant mechanisms, the agencies, or Congress, must decide how much of the total research dollars to set aside for this purpose.

<sup>3</sup><http://www.amacad.org/AriseFolder/>

<sup>4</sup>Institutional funds encompass: 1) institutionally financed organized research expenditures, and 2) unreimbursed indirect costs and related sponsored research.

<sup>5</sup>From 2008 Science and Engineering Indicators: <http://www.nsf.gov/statistics/seind08/?org=NSF>

The National Science Foundation has such a mechanism already, one that they have had in place for a number of years. It was called Small Grants for Exploratory Research (SGER) and just this year (partially to satisfy a requirement in the COMPETES Act) was split into two programs: Exploratory Grants for Early Research (EAGER), and RAPID grants for urgent response research, typically after a natural disaster.

EAGER grants are reviewed only internally at NSF and may be up to \$300,000 and for up to two years in duration. Program officers were allowed to use up to five percent of their program budget for the former SGER awards. In FY 2008, a total of 389 SGER grants were awarded across all directorates, accounting for only 0.6 percent of NSF research obligations.<sup>6</sup> The directorate that made the most use of SGER grants was Computer and Information Sciences and Engineering (CISE), at 1.9 percent. Similarly, NIH has its Pioneer Awards, but they account for about 0.01 percent of NIH's total budget and have a dismal success rate that discourages many potential applicants.

The *ARISE* Committee also makes a number of recommendations for strengthening the entire system to support more high-risk research, from changing the make-up of review panels to altering the charge to those panels. Finally, the *ARISE* Committee recommends greater investment in agency program officers to strengthen program leadership and facilitate the injection of new ideas into agency and community deliberations.

In the FY 2010 budget request, NSF announced a new Foundation-wide transformative research initiative in which each research division will set aside a minimum of \$2 million (\$92 million Foundation-wide) to explore methodologies that help support transformative research.

#### *Metrics for Success*

The *ARISE* Committee also took on the question of how to measure the success of any new policy or program to support high-risk research. They recommended evaluating programs in two phases. The first phase involves determining whether the new program or policy was successful in attracting high-risk research proposals and in funding proposals that would normally be rejected under the traditional peer-review system. The second phase should occur no sooner than 10 years after the initiation, according to the Committee, and would involve evaluation of scientific outcomes.

Evaluating the effectiveness or impact of any basic research program is a difficult, perhaps impossible task, thereby making them easy targets during the zero-sum game appropriations battles. Policies or programs for high-risk research, therefore, could face even greater uncertainty in the federal budget process. For that reason, some argue that charitable organizations and universities are better positioned to ensure long-term support for high-risk research.

### **5. Questions for Witnesses**

*James Collins, NSF*

1. Please describe the National Science Foundation's (NSF) proposed transformative research initiative. What definition is NSF using for 'potentially transformative research?' What guidance has been provided to research divisions regarding implementation of this initiative and how was that guidance developed? To what extent does this initiative entail targeted programs and grant mechanisms versus modifying the standard grant review process across the Foundation? To what extent does it overlap with initiatives to support young investigators? How will NSF evaluate the impact of its transformative research initiative?
2. How in particular is your directorate, Biological Sciences, planning to implement and evaluate the transformative research initiative?
3. What is the role of the program officer in identifying and funding potentially transformative research? What guidance is provided to program officers regarding their role? To what extent does that guidance vary across disciplines/divisions? What has been the impact of flat agency operations budgets on program officers' ability to identify and support potentially transformative research proposals?

<sup>6</sup>For a directorate by directorate breakdown, see Appendix 8 of the NSB's 2008 Merit Review Report: <http://www.nsf.gov/nsb/publications/2009/nsb0943-merit-review-2008.pdf>

4. Is there a unique role for NSF versus the university and the private sector in investing in potentially transformative research? How can NSF's models for support of potentially transformative research complement or facilitate university as well as private sector, including philanthropic support for such research?

*Neal Lane, Rice University*

1. What were the key findings and recommendations in the 2008 American Academy of Arts and Sciences report, *“Advancing Research in Science and Engineering (ARISE): Investing in Early-Career Scientists and High-Risk, High-Reward Research.”* In particular, what were the key findings and recommendations with respect to support for high-risk, high-reward research, especially in non-biomedical disciplines?
2. What are the pros and cons of establishing targeted programs or set-asides for high-risk research versus changing how proposals are reviewed and selected across a federal science agency? What are the biggest challenges or risks associated with each of these approaches? What metrics should be used to evaluate the success of any approach to funding high-risk research?
3. What are the appropriate roles and responsibilities of the various funders, including the federal science agencies, the private sector and universities themselves, in supporting high-risk research? How can federal investments in high-risk research be used to leverage private sector and university investments, and vice-versa?

*Richard McCullough, Carnegie Mellon University*

1. What percentage of science and engineering research funding at your institution comes from the Federal Government? The private sector? The university itself? How do the proposal selection methods and criteria vary across the funding sources?
2. Which of the funding sources described previously provides the most flexibility to your faculty to pursue high-risk, high-reward (or ‘transformative’) research? Do all of your science and engineering faculty have equal access to those sources (or types of sources) of funding given meritorious proposals?
3. Given the total funding for academic science and engineering research from all sources, is the ratio of funding for high-risk research appropriate? If the ratio were to be increased as recommended in several recent reports, what should be the responsibility of the Federal Government in achieving that increase, and how does that responsibility differ from that of the university itself and the private sector?
4. Do you have any specific recommendations for how federal science agencies such as the National Science Foundation could increase their support for high-risk research? In particular, what are the pros and cons of establishing targeted programs or set-asides for high-risk research versus changing how proposals are reviewed and selected across a federal science agency? What are the biggest challenges or risks associated with each of these approaches? What metrics should be used to evaluate the success of any approach to funding high-risk research?

*Gerald Rubin, Howard Hughes Medical Institute*

1. What is Howard Hughes Medical Institute’s model for funding high-risk, high-payoff research? What are the benefits of this model? What are the challenges? Is this a model that could or should be duplicated by federal funding agencies or federally funded research and development centers such as the Department of Energy National Labs or the National Institutes of Health?
2. Given the total funding for basic science and engineering research from all sources, is the ratio of funding for high-risk research appropriate? If the ratio were to be increased as recommended in several recent reports, what should be the responsibility of the Federal Government in achieving that increase, and how does that responsibility differ from that of private sector research organizations and funding sources such as HHMI?
3. Do you have any specific recommendations for how federal science agencies such as the National Science Foundation could increase their support for high-risk research? In particular, what are the pros and cons of establishing

targeted programs or set-asides for high-risk research versus changing how proposals are reviewed and selected across a federal science agency? What are the biggest challenges or risks associated with each of these approaches? What metrics should be used to evaluate the success of any approach to funding high-risk research?

Chairman LIPINSKI. Good afternoon and welcome to this Research and Science Education Subcommittee hearing on high-risk, high-reward research.

Before I start, it is important to make clear that high-risk, high-reward research is also known by many other names including high-risk, high-payoff, transformative, pioneering, and even high-risk, transformative research. There is neither a distinct definition for each of those terms nor a common definition for all of them. We chose high-risk, high-reward because it is the term used by the *ARISE* Committee, that is, the Advancing Research in Science and Engineering Committee whose report we will be discussing today.

Three years ago in the now famous *Rising Above the Gathering Storm* report, a distinguished National Academies committee recommended that each Federal research agency set aside eight percent of its budget for high-risk, high-payoff research. Not long after that, the National Science Board recommended that the National Science Foundation establish a transformative research initiative.

Both of those reports reflected a growing consensus in the research community that the peer-review system has become too conservative in its funding decisions and that even the brightest and most creative scientists are not bothering to submit more ambitious proposals. But both reports were also short on details. That same year we were working on the *America COMPETES Act*, a bill that essentially took every recommendation of the *Gathering Storm* report within the Science and Technology Committee's jurisdiction and translated it into law—that is, every recommendation except the one that set aside eight percent at every research agency for high-risk research. We all agreed there was an unmet need, and the Senate even made a commendable attempt to implement that recommendation in their bill, but during conference we all agreed to put off implementing this recommendation until we could better answer these questions. First, what exactly is high-risk research? Second, why eight percent? Third, why a set-aside as opposed to reforming the peer-review system? Fourth, does this really make sense for every federal research agency? And fifth, does this make sense for Federal agencies at all?

As we look ahead to our 2010 reauthorization of the *America COMPETES Act* and how we can address high-risk research in that bill, we turn to the distinguished panelists before us today and to their many expert colleagues in the community to help us answer these questions.

My colleagues and I up here on the dais also have a political challenge. Whatever your choice of words or definitions, high-risk research means more failures in the short-term, and funding for failures is not easy to justify in an era of ballooning deficits. It is hard enough to secure sustainable funding increases for basic research, and it is all too easy to cut science in appropriations battles. As the Energy and Water Appropriations Subcommittee Chair once said in response to concerns about cuts to the DOE Office of Science, floods kill people. So often times, funding does not seem to be at the highest end of priorities.

Therefore, I worry even more about the risks of creating a discretionary pot of funding that a priori assumes a large failure rate. I say that to remind us all of the political context that surrounds

our discussions this afternoon, and I certainly welcome any thoughts Dr. Lane may have on that topic given his many years of experience in Washington.

I want to thank all the witnesses for being here today, and I look forward to your testimony.

The Chair now recognizes Dr. Ehlers for an opening statement. [The prepared statement of Chairman Lipinski follows:]

PREPARED STATEMENT OF CHAIRMAN DANIEL LIPINSKI

Good afternoon and welcome to this Research and Science Education Subcommittee hearing on high-risk, high-reward research. Before I start, it is important to make clear that 'high-risk, high-reward' research is also known by many other names including 'high-risk, high-payoff,' 'transformative,' 'pioneering,' and even 'high-risk, transformative' research. There is neither a distinct definition for each of those terms nor a common definition for all of them. We chose 'high-risk, high-reward' because it is the term used by the *ARISE* Committee—that is, the Advancing Research in Science and Engineering Committee—whose report we will be discussing today.

Three years ago in the now famous *Rising Above the Gathering Storm* report, a distinguished National Academies committee recommended that each federal research agency set aside eight percent of its budget for 'high-risk, high-payoff' (*their term of choice*) research. Not long after that, the National Science Board recommended that the National Science Foundation establish a 'transformative' research initiative.

Both of those reports reflected a growing consensus in the research community that the peer-review system has become too conservative in its funding decisions and that even the brightest and most creative scientists and engineers are not bothering to submit more ambitious proposals. But both reports were also short on details. That same year we were working on the *America COMPETES Act*, a bill that essentially took every recommendation of the *Gathering Storm* report within the Science and Technology Committee's jurisdiction and translated it into law. That is, every recommendation except the one to set aside eight percent at every research agency for high-risk research. We all agreed there was an unmet need, and the Senate even made a commendable attempt to implement that recommendation in their bill, but during conference we all agreed to put off implementing this recommendation until we could better answer these questions:

1. What exactly is high-risk research?
2. Why eight percent?
3. Why a set-aside as opposed to reforming the peer-review system?
4. Does this really make sense for every federal research agency?
5. Does this make sense for federal agencies at all?

As we look ahead to our 2010 reauthorization of the *America COMPETES Act* and how we can address high-risk research in that bill, we turn to the distinguished panelists before us today and to their many expert colleagues in the community to help us answer these questions.

My colleagues and I up here on the dais also have a political challenge. Whatever your choice of words or definitions, high-risk research means more failures in the short-term, and "funding for failures" is not easy to justify in an era of ballooning deficits. It is hard enough to secure sustainable funding increases for basic research, and it is all-too-easy to cut science in appropriations battles. [As the Energy and Water Appropriations Subcommittee Chair once said in response to concerns about cuts to the DOE Office of Science, "floods kill people."]

Therefore, I worry even more about the risks of creating a discretionary pot of funding that a priori assumes a large failure rate. I say that to remind us all of the political context that surrounds our discussions this afternoon, and I certainly welcome any thoughts Dr. Lane may have on that topic given his many years of experience in Washington.

I thank the witnesses for being here this afternoon and I look forward to your testimony.

Mr. EHLERS. Thank you, Mr. Chairman, and I agree with your assessment of the situation and your viewpoint of it. Excellent panel that we have today.

We will be hearing from a panel of experts testifying on the Federal Government funding, both risky and potentially rewarding research, and I will get back to that in just a few minutes. Tight agency research budgets and intense competition have established an environment of cautious, incremental research proposals from many scientists seeking Federal support. Ironically, many of the greatest challenges faced by our nation in health care, energy and national security may not be addressed in a timely manner because there are limited opportunities for promising ideas to be heard and funded simply because they are outside of the box.

Though the National Science Board, the National Academies, the American Academy and others have identified the need to address transformative research in our basic Federal research portfolio, it is necessary to examine how to best facilitate the introduction and proliferation of this type of research. Transformative discoveries have emerged from federally funded research in spite of a lack of dedicated programs for this purpose. Learning how we might adapt our Federal funding system to elicit more ground-breaking discoveries is a worthy goal, I look forward to hearing the insights on this topic from our witnesses today. I also thank the Chairman for instigating this particular hearing, and I think it is badly needed.

One other dimension I would like to give to this, and I addressed a panel of manufacturers yesterday, and we discussed research. They were concerned about the “Valley of Death” and so forth. And I pointed out to them that they should be supporting basic research for the simple reason that they are dependent on it, and they don’t have the resources to do basic research because of the high-risk factor. We may be able to borrow money to make the world’s next best widget, but it is very difficult to borrow money when you say I don’t know what I am going to find, but I think if I do research, I will probably find something good and I will probably be able to make some money and I will probably be able to pay back the loan. But you don’t get a loan that way, and that is the point I was simply trying to make.

They should be lobbying us to do the appropriate basic research at the Federal level because the Federal Government has to take the high-risk opportunities. We can afford to miss a major discovery now and then, but we should be trying constantly to really bridge the gap and get rid of the “Valley of Death.” Do the basic research where we are taking risks where we won’t even know what the result might be, but if we do 100 such projects, we are likely to hit pay dirt in anywhere from two to ten which will more than pay for all the research.

And one of my favorite examples which I give to lay people is the laser, which is today ubiquitous, truly ubiquitous. And yet, it was started by some research, some in Russia, some in the U.S. Charlie Townes is a good friend of mine, did some very good research indicated and had some good success. He built a laser and then went ahead—I don’t know how much federal money he got, but I would guess given the value of the dollar at that time, it was probably not much more than a million dollars, 10 million at the very most. Now, you add together what the laser industry revenues are today, and it is, multi-billions of dollars. Not only that, it is, as I said, a ubiquitous device which is extremely helpful in many areas of life,

many areas of manufacturing, and we just take it for granted. Without Charlie Townes and some federal money, we might not have discovered it for another 10 or 20 years. That time difference alone and the revenue that was generated during that 10 to 20 years is more than enough to cover the NSF budget for a number of years.

So I think it is self-evident that the Federal Government has a very serious responsibility in supporting fundamental basic research because that is the springboard for innovation, creativity, and that is the springboard in turn for manufacturing and economic growth.

I yield back.

[The prepared statement of Mr. Ehlers follows:]

PREPARED STATEMENT OF REPRESENTATIVE VERNON J. EHLERS

Today our subcommittee will hear from a panel of experts testifying on how the Federal Government should fund both risky and potentially rewarding research. Tight agency research budgets and intense competition have established an environment of cautious, incremental research proposals from many scientists seeking federal support. Ironically, many of the greatest challenges faced by our nation in health care, energy and national security may not be addressed in a timely manner because there are limited opportunities for promising ideas to be heard and funded simply because they are "outside of the box."

Though the National Science Board, the National Academies, the American Academy and others have identified the need to address transformative research in our federal basic research portfolio, it is necessary to examine how to best facilitate the introduction and proliferation of this type of research. Transformative discoveries have emerged from federally funded research in spite of a lack of dedicated programs for this purpose. Learning how we might adapt our federal funding system to elicit more ground-breaking discoveries is a worthy goal, and I look forward to hearing the insights on this topic from our witnesses today.

Chairman LIPINSKI. Thank you, Dr. Ehlers. As always, I appreciate your perspective that you bring here.

Now, if there are any Members who wish to submit additional opening statements, your statement will be added to the record at this point.

At this time I would like to introduce our witnesses. First, Dr. Neal Lane who is the Malcolm Gillis University Professor at Rice University. He also holds appointments as Senior Fellow of the James A. Baker III Institute for Public Policy. Dr. Lane was a member of the American Academy of Arts and Sciences Committee that published the report, *ARISE: Advancing Research in Science and Engineering*.

Dr. James Collins is the Assistant Director for Biological Sciences at the National Science Foundation.

Dr. Richard McCullough is a Professor of Chemistry and Vice President of Research at Carnegie Mellon University.

And finally, Dr. Gerald M. Rubin is Vice President of the Howard Hughes Medical Institute and Director of the Janelia Farm Research Campus.

Okay. How do you pronounce that?

Dr. RUBIN. The way you did.

Chairman LIPINSKI. Very good. I should have stopped myself right there then.

As our witnesses should know, you will each have five minutes for your spoken testimony. Your written testimony will be included in the record for the hearing. When you all have completed your



testimony, we will begin with questions. Each Member will have five minutes to question the panel. We will start with Dr. Lane, so Dr. Lane?

**STATEMENT OF DR. NEAL F. LANE, MALCOLM GILLIS UNIVERSITY PROFESSOR AND SENIOR FELLOW, JAMES A. BAKER III INSTITUTE FOR PUBLIC POLICY, RICE UNIVERSITY**

Dr. LANE. Thank you very much, Chairman Lipinski and Ranking Member Ehlers, Members of the Committee. I greatly appreciate the opportunity to testify today on behalf of the American Academy of Arts and Sciences on High-Risk, High-Rewards Research. I commend the Subcommittee for holding a hearing on this topic, which I believe is vital to the future progress of American science and technology and innovation.

Last year, as you noted, Mr. Chairman, the American Academy released the report called *ARISE* which stands for Advancing Research in Science and Engineering, and my comments today are drawn largely from the findings and recommendations of that report.

I was privileged to serve on the *ARISE* Committee, which was chaired by Nobel laureate and former Howard Hughes Medical Institute President Thomas Cech.

Many studies have focused on the need to increase the level of funding for science and technology research, and I am not here to argue against that proposition. But the Academy Committee took on a different question. Regardless of the levels of overall Federal funding, what are the things that all stakeholders—government, universities, foundations—must do to ensure the most efficient and effective use of those Federal funds?

In considering this question, the Committee identified two issues that we felt were central to the vitality of America's research enterprise. One was the support of early-career investigators, and the second was the need to encourage high-risk, high-reward, sometimes called transformative, research. The two issues are, of course, related, since many fresh, new ideas come from researchers who are in the early stages of their career.

The *ARISE* report recommended several policy actions that we believe will strengthen the opportunities for early career investigators, and those recommendations are detailed in my written statement.

With regard to high-risk, high-reward research, our Committee concluded that across virtually all government agencies and departments that fund science and engineering research, short-term, low-risk and measurable results tend to dominate the funding decisions. While the current system does a very good job of identifying meritorious proposals using peer review, some potentially path-breaking research is not being funded because it just looks too risky. So there is a lot of truth to the often-heard advice, "Don't put it in your grant proposal unless you know it will work."

Some agencies recognize the problem and are taking steps to address it. Indeed, NSF and its National Science Board highlighted the issue some time ago and were encouraged by the statements of the Director, Arden Bement, and the inclusion in the President's

2010 NSF budget request of \$92 million specifically to foster transformative research.

We urge this Subcommittee and the Congress to build on these commitments by the Administration, and specifically, the Academy's *ARISE* report recommends that NSF and the other agencies that support science and engineering research take the following steps. First to establish and strengthen policies, programs and targeted funding mechanisms designed to foster potentially transformative research. Two, provide high-risk, high-reward research programs with sufficient support to allow funding of a significant portion of applicants. Three, establish appropriate metrics with which to evaluate the success of targeted research programs. Four, adopt funding mechanisms and policies that nurture transformative research in all award programs, not just those targeted at high-risk, high-reward, research. Five, strengthen application and review processes. High-risk research proposals face an even greater challenge in a stressed peer-review system that is not equipped to appreciate them. Six, strengthen investments in the career development of agency program officers who are indispensable to the vitality and productivity of the entire research enterprise.

Let me just comment on the last recommendation, supporting program officers at the agencies. As a former NSF Director—and actually, back in the late '70s, I was NSF Physics Division Director as a rotator—I experienced firsthand the commitment, the quality, the dedication of these program officers, and I have seen their workload increase and their support decrease. They must have the resources to be in touch with their communities. They must be viewed as leaders in the science and engineering research community, make the site visits that are necessary so that not only are they staying current in their field, but the field itself recognizes and has credibility in their decision making.

A key recommendation of the *ARISE* report was the creation of targeted grant programs, specifically aimed at high-risk, high-reward research. There are several advantages to creation of targeted grant programs and some challenges which we could go into in the question session. The Committee also considered institutions other than Federal agencies that are vital to the enterprise, particularly universities and foundations, and has offered recommendations helpful to them.

Final point, the Academy is going to have a look now at a second phase of this project with the impact of these modes of Federal funding on the universities themselves. What is it doing about the curriculum? What is it doing to the nature of the faculty culture? Are there some lessons to be learned from that? If the Subcommittee has interest in that issue, has concerns that you would like us to address, we would be pleased to hear from you.

Thank you, Mr. Chairman.

[The prepared statement of Dr. Lane follows:]

PREPARED STATEMENT OF NEAL F. LANE

Chairman Lipinski, Ranking Member Ehlers, and Members of the Committee: I am Neal Lane, the Malcolm Gillis University Professor at Rice University. I also hold appointments as a Senior Fellow of the James A. Baker III Institute for Public Policy, where I am engaged in matters of science and technology policy, and in the

Department of Physics and Astronomy. Prior to returning to Rice University, I served in the Federal Government during the Clinton Administration as Assistant to the President for Science and Technology and Director of the White House Office of Science and Technology Policy, from August 1998 to January 2001, and as Director of the National Science Foundation (NSF) and member (*ex officio*) of the National Science Board, from October 1993 to August 1998.

I am also proud to be a Fellow of the American Academy of Arts and Sciences and to serve on its Council. I co-chair with Charles Vest the Advisory Committee for the American Academy's Initiative for Science, Engineering, and Technology. Last year, as part of the Initiative, the Academy released a report, *ARISE: Advancing Research In Science and Engineering*. I am pleased to appear today on behalf of the American Academy to discuss the findings of the *ARISE* report as they apply to the issue of federal funding for high-risk, high-reward research.

The American Academy of Arts & Sciences was founded in 1780 by John Adams and other scholar-patriots to encourage dialogue among leaders of science, the arts, business and public affairs. Today, the Academy is an independent policy research institute, engaged in the study of complex problems vital to our nation's future. Through its projects and studies, and publications like the *ARISE* report, the Academy pursues practical policy responses to pressing national and global problems. On behalf of the Academy I wish to thank the Subcommittee for inviting me to summarize briefly this report's timely findings and recommendations. They are, we believe, vital to the future progress and prosperity of the Nation.

I would also like to acknowledge the distinguished Fellows of the Academy who served on the committee that developed the *ARISE* report. The group was chaired by Nobel laureate and former Howard Hughes Medical Institute President Thomas Cech. Committee members included some of the Nation's preeminent scientists and policy leaders from government, academia, and industry. In particular, I want to mention University of Maryland President C. D. (Dan) Mote, Jr. Before the hearing date was changed, President Mote rearranged his schedule in order to testify before the Subcommittee on behalf of the Academy, an indication of his strong commitment to the issues raised in the *ARISE* report. He was a valuable member of the committee and is a leader on competitiveness and science and technology research issues at his own university and nationally.

Many studies have focused on the need to increase the level of federal funding for science and technology research in order to sustain America's competitive advantage. The Academy committee that generated the *ARISE* report began its deliberations with a different question: Regardless of the levels of overall federal research funding, what are the things that all stakeholders—government, universities and foundations—must do to ensure the most efficient and effective use of those federal research funds?

In considering this question, the committee identified two issues central to the vitality of America's research enterprise: 1) the support of early-career investigators; and 2) the encouragement of high-risk, high-reward research.

### **Early-Career Faculty**

Before turning to the Committee's interest in high-risk, high-reward research, permit me to briefly summarize key points from the *ARISE* report related to new tenure-track faculty, those most talented individuals who will lead our science and technology enterprise into the future. The two issues are, of course, related since many of the most novel ideas come from early-career researchers.

In recent years, many early-career faculty have faced greater obstacles in launching and sustaining their careers than their senior colleagues. Many, probably most, early-career investigators spend excessive amounts of time constantly preparing and submitting multiple grant proposals for awards, and when they succeed, new awards often are inadequate in size and too short in duration. New researchers must sustain an intense pursuit of funding, diverting time from their research and teaching during the formative years of their research and teaching careers.

Data from the National Science Foundation and the National Institutes of Health confirm worrisome trends, shared, we suspect, across all fields of physical sciences and engineering. In general, early-career investigators must compete harder, succeed less often, and start careers later than did older, established investigators, most of whom also confront intense competition for limited resources.

While NSF and NIH have helpful trend data on early-career faculty, most mission agencies lack comparable data and analyses; they do not track demographic data on their applicant and investigator populations. The enterprise as a whole lacks an analytical capability to produce a systemic view across all agencies and fields of research.

Early-career investigators typically have had to wait too long to receive their first grant. The average age of first-time NIH awardees has risen steadily and in 2007 stood at 42.6. In many cases, tenure-track faculty will be facing an up-or-out tenure decision before they have received their first competitive grant and had time to demonstrate their research capability. In such cases, the university loses a promising faculty member and the investment it has made with a start-up package.

Of new investigators who applied for NIH awards in 2007, 20.6 percent succeed compared with 23.8 percent of established researchers, according to data reported by the Institutes.

In 1980, about 33 percent of NIH individual investigator awards went to first-time investigators; by 2006 less than 25 percent of awards went to early-career investigators.

One-half of new NSF investigators never again receive NSF funding after their initial awards.

Meanwhile, NSF and NIH data confirm that the investigator population across the sciences and engineering is graying even as non-tenure track ranks continue to grow.

In light of these trends, high frustration levels and low morale felt by many new tenure-track researchers are being communicated to promising undergraduate and graduate students as they make their own career decisions. Discouraging our brightest students from pursuing research careers is an ineffective strategy for assuring our nation's science and technological leadership in the future.

Despite these worrisome findings, there is some good news. The Obama Administration, and NSF and NIH in particular, recognize the importance of these issues and are taking steps to address them. There is evidence that mission agencies are also becoming aware of the particular challenges facing early-career investigators. But more must be done.

Recognizing that the Subcommittee's jurisdiction does not extend to all of the Federal Government's science and technology research-funding agencies, the American Academy encourages this subcommittee and the Congress to support initiatives designed to strengthen incentives and opportunities for early-career investigators. Specifically we ask you to:

1. Monitor closely actions taken to address the needs of early-career researchers across the sciences and engineering disciplines;
2. Encourage all agencies to establish targeted programs for early-career faculty;
3. Encourage all agencies to establish new research programs only if they have sufficient fiscal support to fund a reasonable percentage of applicants. Grant programs that fund a very small percentage of applications are inefficient uses of money, time, and effort;
4. Encourage agencies to give special attention to proposals of early-career investigators during competitive merit review and to adopt career-stage-appropriate expectations for grant funding;
5. Encourage agencies to create seed funding programs for early-career investigators to enable them to explore new ideas for which no results have yet been achieved;
6. Encourage agencies to remove barriers affecting those who serve their families as primary caregivers, for example, by providing grant extensions or other appropriate support mechanisms, and, finally;
7. Encourage agencies to collect and analyze demographic data on applicants and principal investigators government-wide and in a uniform format to establish a comprehensive federal database on how agencies support research. The current nonstandardized tracking among funding agencies hinders efforts to analyze funding trends. Since NSF has an excellent track record of collecting and analyzing data relevant to the future of the Nation's science, engineering and technology enterprise, its example could be helpful to other agencies that do not have such a tradition.

### **High-Risk, High-Reward Research**

Turning now to high-risk, high-reward research, the *ARISE* report highlights several important themes that I believe merit consideration by the Subcommittee.

Most research scientists and engineers achieve their goals by persistent, step-by-step work built on the discoveries and advances of others. This is, and must remain, the vital foundation of our research enterprise. Important breakthroughs do result from incremental research.

Science also progresses from bold innovation in methods, instruments, and computer software. Curiosity-based or intuition-based boldness can require even greater leaps into the unpredictable unknown. Most such efforts will fail, but the few pioneers who are successful can profoundly influence the direction of science by challenging accepted paradigms. Such research can generate deep changes in concepts, create new subfields of science or bring together different fields to make discoveries and advances that would otherwise be impossible. This research can also allow the entire community to extend its reach by creating revolutionary technologies, new products, new markets and industries and high quality jobs. Thus, high-risk, high-reward research is needed to maintain the U.S. position of leadership in science and technology and to ensure the Nation's future economic competitiveness. The *ARISE* report cites several examples of such transformative payoffs, including the transistor, quantum mechanics, and angiogenesis. The report recommends that every agency set aside a certain portion of its research budget for high-risk research.

For most of its history, the NSF has received far more proposals that have been judged by the competitive peer-review system to merit funding than the agency has sufficient funds to award. It is up to the program officers to make the final judgments as to which proposals receive awards and the large majority that do not. Other research agencies are in a similar position. When funds are this tight, all components of the system—researchers writing the proposals, experts reviewing the proposals, and program officers making the final decisions—naturally tend to become more risk averse. They tend to give highest priority to projects likely produce incremental success in the near-term. Short-term, low-risk and measurable results dominate competitive review and program management systems and decisions. The *ARISE* Committee summed it up in these words:

“As the resulting constant hunt for dollars fosters conservative thinking, it also impedes the pace of research. The thought, ‘Don’t put it in your grant proposal unless you know it will work,’ too often guides senior and junior faculty alike as they compete in an intense national grant-writing mill.”

It is important to emphasize that the system continues to fund excellent research, that it does help prepare the next generation of scientists and engineers, and that virtually all proposed research projects are challenging and are judged to advance scientific and technical understanding. But, some potentially path-breaking research is not being funded because it just looks too risky.

The American Academy and the *ARISE* Committee are encouraged by several promising recent developments designed to counter the prevailing incentive system.

In 2007, Congress created the Department of Energy (DOE) ARPA-E program as part of the *America COMPETES Act*. ARPA-E is modeled after DARPA with the goal of enhancing the economic and energy security of the United States through research into transformative energy technologies. DOE is currently evaluating the first round of applications, and successful proposals will be funded by the *American Recovery and Reinvestment Act (ARRA)*.

Similarly, this year will see the first grants awarded under the NIH Transformative R01 program (TR01), a targeted high-risk, high-reward initiative designed as a result of strategic planning to fund ground-breaking research opportunities. The proposed FY 2010 budget expands funding for this program to \$70 million, double the 2009 funding level.

The economic stimulus program enacted by Congress will support promising high-risk research at other agencies as well. NSF Director Arden Bement has pledged to give increased priority to new principal investigators and high-risk, high-return research in allocating ARRA funds. Building on the momentum provided by stimulus funding, the proposed NSF FY 2010 budget sets aside \$92 million specifically to foster transformative research.

The Academy commends the Congress, the National Science Board and NSF for their early recognition of the need to nurture high-risk research and their recent actions to address this need. The Foundation has taken important first steps to expand opportunities for new and established researchers alike to pursue high-risk opportunities. For example, NSF program officers now have the flexibility to award up to two years of funding for potentially transformative research through the EAGER program (EARly-concept Grants for Exploratory Research). This mechanism should be used more frequently across the NSF grant programs and at other funding agencies as well. Clearly, each agency must stand behind the program officers making these difficult decisions, since many of the truly bold, high-risk ideas will not bear fruit. If the agencies' expectations are too high, the entire effort will fail.

President Obama's Innovation Strategy aims to restore American leadership in fundamental research. In outlining this strategy in a September 21st speech in Troy, New York, the President stressed the importance of valuing and promoting

“the risk takers who have always been at the center of our success” and pledged “more support for high-risk, high-return research, for multi-disciplinary research, and for scientists and engineers at the beginning of their careers.”

Looking to the future, the Obama Administration has emphasized the need to build on these commitments to encourage potentially transformative research. In an August 4 memorandum from Office of Science and Technology Policy Director John Holdren and Office of Management and Budget Director Peter Orszag, executive departments and agencies were asked to prioritize high-risk, high-reward research in preparing FY 2011 budget requests, stating “Agencies should pursue transformational solutions to the Nation’s practical challenges, and budget submissions should therefore explain how agencies will provide support for long-term, visionary thinkers proposing high-risk, high-payoff research.” The directive also asked agencies to create metrics to evaluate the success of programs designed to promote high-risk research.

To these ends, the Academy respectfully asks this subcommittee and the Congress to encourage all of the science and engineering research agencies to:

1. Establish and strengthen policies, programs, and targeted funding mechanisms designed to foster potentially transformative research:
  - Applications should be relatively short and focused on the qualifications of the researcher, an explanation of the potentially transformative nature of the research, and an explanation of why the researcher believes the proposed approach could succeed.
  - The proposal and the review process should place a premium on innovation.
  - Fast-track seed money to evaluate a novel idea should be made available.
  - Agencies should be open to providing longer funding periods for those proposals that require it.
  - A possible model for sustained funding is the NSF Industry/University Cooperative Research Centers program—an initial five-year grant that, if moving forward appropriately, can be renewed for an additional five-year period at a reduced level of funding.

Because federal research agencies are highly diverse in their missions, needs, and programs, funding mechanisms that support potentially transformative research will and should vary across departments and agencies. Such diversity is a national asset and the foundation of the research enterprise. Therefore a final recommendation is:

- Convene interagency meetings to share information on how departments and agencies design, organize, implement, and evaluate their investments in potentially transformative research.
2. Nurture high-risk, high-reward research programs that have a critical mass.
  3. Establish metrics with which to evaluate the success of targeted research programs:
    - Short-term metrics: Are proposals of higher quality compared to those submitted to standard grant programs? Does the funding rate discourage future applicants?
    - Long-term metrics: Wait ten years to evaluate scientific outcomes—fruits of transformative research are not apparent in the short-term.
  4. Adopt funding mechanisms and policies that nurture transformative research in all award programs, not just those targeted at high-risk, high-reward research:
    - Charge reviewers to identify new ideas, innovation, and creativity. Consider alternative ways to select and mentor reviewers.
    - Give program administrators in all agencies the flexibility to provide extra resources or time to research unexpected but promising developments, potentially using the NSF EAGER grants as a model.
    - Recognize in grant-reporting requirements the value of fortuitous findings not related to the main objective of the research proposal.
    - For grant renewals or new grants on the same topic, restrict the number of submitted publications and require a self-assessment of each cited publication’s impact.

5. Strengthen application and review processes. High-risk research proposals face even greater challenges in a stressed peer-review system not equipped to appreciate them:
  - Require recipients of multiple grants from an agency to serve as reviewers.
  - Achieve greater continuity in reviewers.
  - Require applicants to address the following question about their proposed research: “If this works, what long-term scientific difference will it make?” Evaluate proposals based on this criterion.
  - Establish interdisciplinary review panels to consider high-risk research proposals across programs and fields.
  - Evaluate renewals for first awards for high-risk, high-reward research on the basis of project execution and potential scientific impact, not on deliverables. Resist fine-grain assessments of whether a project “worked”; expect some hypotheses to fail.
6. Strengthen investments in the career development of agency program officers who are indispensable to the vitality and productivity of the entire research enterprise. They should be encouraged and expected to engage with the professional communities they fund. This requires an adequate administrative budget, which should not come at the expense of the research budget:
  - Program officers should be leaders not only within their agencies but within their external scientific communities as well.
  - Program officers should be able, indeed encouraged, to attend professional meetings and to visit institutions and laboratories funded by programs for which they are responsible.
  - Many university faculty members serve as temporary program officers at NSF, or “rotators,” while on leave from their university. They provide essential service and leadership for NSF’s research programs. Consideration should be given to providing this flexibility to other agencies as well.

As a former Director of the National Science Foundation, I wish to affirm and commend the dedication and the quality of its program officers. They are the core of the NSF. The encouragement and support they receive directly determines how well NSF performs its important work. They must be able to travel to professional meetings, make site visits to universities, and in other ways become more active and visible leaders in their fields. Just as the program officers need to stay current on the latest developments in science and engineering research, the research community needs to know and respect these professionals, who have such large responsibilities for the quality of U.S. science and engineering. I urge Congress, through its oversight and appropriations roles, to provide the resources the NSF requests for support of the agency’s staff.

The Committee will note that the *ARISE* report recommends both the creation of targeted grant programs specifically aimed at high-risk, high-reward research and the promotion of such research within all existing funding programs. There are several advantages to the creation of targeted grant programs, and a few attendant challenges. High-risk, high-reward research involves unique objectives, time-frames and evaluation metrics, and targeted programs permit these research proposals to be evaluated separately from standard proposals. It may also be faster and easier to implement a new targeted program than to re-tool standard funding processes to accommodate the particular needs of high-risk, high-reward proposals.

Challenges associated with targeted funding programs include the potential for extremely low funding rates that could discourage future applicants. A further challenge is that funding agencies must be prepared to follow unexpected research directions arising from high-risk, high-reward research. Finally, in evaluating the merits of high-risk, high-reward research programs, it must be kept in mind that the fruits of transformative research are often not apparent for at least ten years. Near-term evaluation of these programs must be based on different metrics, for example, whether the quality of proposals differs from those received through standard grant programs.

The *ARISE* Committee was also concerned with the role of other institutions, particularly universities, in supporting high-risk research. Institutions of higher education—especially medical schools—have tended to enlarge their faculty in times of expanding federal investment by shifting the salary burden to faculty. For the federal funding agencies, this salary support reduces the number of projects that can

be funded. For the faculty member, this requirement fosters conservative, risk-averse thinking as the path to sustained funding. When funding tightens, faculty, especially early-career faculty, after years of training often simply leave the field.

Two final *ARISE* recommendations directly address the role of universities in supporting early-career scientists and high-risk, high-reward research. These recommendations aim to mitigate concerns over the effects that boom and bust funding cycles have on tenure, training, and capital investment on campuses:

1. Universities should accept greater institutional responsibility for the salaries of faculty members.
2. In building new facilities and programs, universities should shoulder a larger share of the financial cost.

Thus, university resources are needed to buffer the scientific enterprise from the ups and downs of federal funding. If funding campaigns for construction were expected to assume some significant portion of the research expenses, it would lead universities to limit excessive building programs based on unrealistic expectations about the expansion of the research enterprise. Some universities are now beginning to recognize the wisdom of setting aside money from building campaigns for research and equipment. Universities could go even further and underwrite the creation and maintenance of centers specifically devoted to potentially transformative research. In times of economic downturn and shrinking endowments, the government and universities should consider ways to provide general support for science and engineering research that protect against disruptive boom and bust funding cycles.

The Academy is initiating a second phase of *ARISE* to study how the distribution of federal funds affects the administration, faculty, students, and the academic mission of the university. The Academy would be grateful to this subcommittee for its input as we develop this phase of the *ARISE* study.

I look forward to your questions about all aspects of the *ARISE* report. Thank you, once again, for this opportunity.

#### BIOGRAPHY FOR NEAL F. LANE

Dr. Neal Lane is the Malcolm Gillis University Professor at Rice University in Houston, Texas. He also holds appointments as Senior Fellow of the James A. Baker III Institute for Public Policy, where he is engaged in matters of science and technology policy, and in the Department of Physics and Astronomy.

Prior to returning to Rice University, Dr. Lane served in the Federal Government during the Clinton Administration as Assistant to the President for Science and Technology and Director of the White House Office of Science and Technology Policy, from August 1998 to January 2001, and as Director of the National Science Foundation (NSF) and member (*ex officio*) of the National Science Board, from October 1993 to August 1998.

Before becoming the NSF Director, Dr. Lane was Provost and Professor of Physics at Rice University in Houston, Texas, a position he had held since 1986. He first came to Rice in 1966, when he joined the Department of Physics as an assistant professor. In 1972, he became Professor of Physics and Space Physics and Astronomy. He left Rice from mid-1984 to 1986 to serve as Chancellor of the University of Colorado at Colorado Springs. In addition, from 1979 to 1980, while on leave from Rice, he worked at the NSF as Director of the Division of Physics.

Widely regarded as a distinguished scientist and educator, Dr. Lane's many writings and presentations include topics in theoretical atomic and molecular physics and science and technology policy. Early in his career he received the W. Alton Jones Graduate Fellowship and held an NSF Doctoral Fellowship (University of Oklahoma), an NSF Post-Doctoral Fellowship (while in residence at Queen's University, Belfast, Northern Ireland) and an Alfred P. Sloan Foundation Fellowship (at Rice University and on research leave at Oxford University). He earned Phi Beta Kappa honors in 1960 and was inducted into Sigma Xi National Research Society in 1964, serving as its national president in 1993. He served as Visiting Fellow at the Joint Institute for Laboratory Astrophysics in 1965-66 and 1975-76. While a Professor at Rice, he was two-time recipient of the University's George R. Brown Prize for Superior Teaching.

Through his work with scientific and professional organizations and his participation on review and advisory committees for federal and State agencies, Dr. Lane has contributed to public service throughout his career. He is a fellow of the American Physical Society, the American Academy of Arts and Sciences (member of its governing council), the American Association for Advancement of Science, the Associa-



tion for Women in Science and a member of the American Association of Physics Teachers. He serves on several boards and advisory committees.

Dr. Lane has received numerous prizes, awards, including the AAAS Philip Hauge Abelson Award, AAAS William D. Carey Award, American Society of Mechanical Engineers President's Award, American Chemical Society Public Service Award, American Astronomical Society/American Mathematical Society/American Physical Society Public Service Award, NASA Distinguished Service Award, Council of Science Societies Presidents Support of Science Award, Distinguished Alumni Award of the University of Oklahoma, and over a dozen honorary degrees. In 2009, Dr. Lane received the National Academy of Sciences Public Welfare Medal, the American Institute of Physics K.T. Compton Medal for Leadership in Physics, and the Association of Rice Alumni Gold Medal for service to Rice University.

Born in Oklahoma City in 1938, Dr. Lane earned his B.S., M.S., and Ph.D. (1964) degrees in physics from the University of Oklahoma. His thesis advisor was Chun C. Lin (currently at the University of Wisconsin–Madison). He is married to Joni Sue (Williams) Lane and has two children, Christy Saydjari and John Lane, and four grandchildren, Allia and Alex Saydjari, and Matthew and Jessica Lane.

Chairman LIPINSKI. Thank you, Dr. Lane. The Chair now recognizes Dr. Collins.

**STATEMENT OF DR. JAMES P. COLLINS, ASSISTANT DIRECTOR, DIRECTORATE FOR BIOLOGICAL SCIENCES, NATIONAL SCIENCE FOUNDATION**

Dr. COLLINS. Congressman Lipinski, Congressman Ehlers and Committee Members, thank you for the opportunity to testify for the Research and Science Education Subcommittee.

My name is James P. Collins. I have served as Assistant Director for Biological Sciences at the National Science Foundation. I am on leave from Arizona State University where I am Virginia M. Ullman Professor in the School of Life Sciences, and in this capacity I maintain a research lab. So I understand this intersection, this deep and important intersection between basic research and the university environment and the sort of policy issues that are at work here in terms of the Federal Government.

Today we will discuss high-risk, high-reward research in the context of transformative science at the National Science Foundation. The U.S. National Science Foundation is first and foremost an innovation agency. NSF has a long history of success in supporting research with far-reaching impacts on the U.S. economy and the well-being of all Americans. Since 1950, this success has relied on, first, the close partnership with America's colleges and universities; second, on a merit-review system based in the scientific community; and third, on a continuously refreshed cadre of program officers who are stewards of the Nation's investment in basic scientific research and education.

Unlike industry, whose typically shorter-term goals and proprietary results are aimed at the marketplace, NSF investments are both short- and long-term, and most importantly, its results are public. It is sometimes mistakenly assumed that research investments that are scientifically successful in the short run can produce similar short-term economic gains, and that this outcome is the only valid measure of success. In fact, the transformative impacts of the knowledge and technologies that result from successful scientific investments on subsequent scientific research, the economy, and society are often realized only many years later.

Research in the history, philosophy and social studies of science teaches us that attempts to predict the individual ideas or projects

that will be transformative are imprecise at best. The process of scientific discovery is a community endeavor. This endeavor takes time and is by design cumulative, skeptical and critical of new results. Transformative discoveries happen because of these qualities.

NSF seeks to advance the transformative science by encouraging high-risk, high-reward research in the context of the structures, programs, and policies of an innovation agency. Transformative science is supported by institutions designed to foster such research, and NSF is just such an institution.

NSF's merit-review process, which is based in the scientific community, is a form of what is now called "crowd sourcing." It uses the collaborative wisdom of the crowd to identify the best research. NSF merit review, done by convening groups of experts, creates a special role for NSF in the evolution of the values in the American scientific community. As we discuss transformative research and investments in risky but potentially high-reward research with NSF panels, researchers incorporate these ideas in their evaluations and promote them in their own scientific venues.

Establishing and sustaining interactions among NSF reviewers, program officers, applicants and awardees has shaped the culture of American science and is at the heart of the process of discovery in U.S. science. NSF program officers are stewards of the Nation's investment in research and science education. In addition to merit review, they manage awards, they mentor post-doctoral fellows and early career scientists, they facilitate national and international connections within and across fields and engage in outreach to promote broader participation in education for knowledge economy.

But as the research enterprise accelerates and becomes interdisciplinary, the demands of proposal and award management are becoming overwhelming. Time for just thinking about a problem, interacting with researchers, and imagining creative new ways to find and fund the best research is decreasing. As NSF experiments with new methods of review and funding directed at enabling transformative science, program officers will experience even greater demands on their time and attention in order to manage these innovative processes.

Like other Federal funding agencies, NSF seeks to describe itself in terms of its research awards. However, in searching for more meaningful assessments, NSF is exploring new methods and measures—to understand the transformative contributions of new scientific knowledge to economic and social outcomes, to inform future investments, and to convey this information to policy-makers and the public.

For nearly 60 years, NSF has been forward-looking in its management of the Nation's scientific enterprise. Our challenge for the future is to sustain a culture of creativity and innovation that pervades NSF and guides our decisions. NSF must continue to innovate, even in the midst of excellence.

Once again, Mr. Chairman, thank you for giving me the opportunity to testify on this important subject. I would be pleased to answer any questions that you have.

[The prepared statement of Dr. Collins follows:]

PREPARED STATEMENT OF JAMES P. COLLINS

**INVESTING IN HIGH-RISK, HIGH-REWARD RESEARCH**

Chairman Lipinski, Ranking Member Ehlers, and distinguished Members of the Subcommittee on Research and Science Education, thank you for inviting me to participate in this hearing on *“Investing in High-Risk, High-Reward Research.”*

Mr. Chairman, as you know, the U.S. National Science Foundation (NSF) is first and foremost an innovation agency that has a long history of success in supporting research with far-reaching impacts on the U.S. economy and the well-being of Americans. Since 1950 this success has relied on a close partnership with America’s colleges and universities, which are the principal locus of the research NSF funds. NSF research grants are made for the short- or long-term and its results are public, unlike industry which usually has shorter-term goals aimed at the market place and proprietary results. An NSF hallmark is its continuing effort to advance transformative science by encouraging high-risk/high-reward research in the context of the structures, programs, and policies needed to function as innovation agency.

Scientific discovery is a social process, a community endeavor that takes time, and is by design cumulative, skeptical, and critical of new results. Transformative discoveries happen because of these qualities (not in spite of them). Moving from an “aha moment” to value creation in a knowledge economy is a complex process involving interactions among people, social structures, and institutional practices and cultures. Research in history, philosophy, and social studies of science teaches us that attempts to predict which individual ideas or projects that are likely to be “transformative” are challenging and imprecise at best.

The challenge for agencies like NSF that fund research done by other organizations is to create and sustain a culture of innovation in which the flow of information among its members creates an institutional culture and framework that stimulates, reinforces, and rewards creativity, and pervades the agency and guides its decision-making process.

**Creating and sustaining innovation**

NSF’s decisions are based on the advice of its constituents though merit review, which is a form of what is now called “crowd sourcing” or a way to leverage group collaboration toward the goal of identifying the best research. Most merit review at NSF is done by convening groups of scientific experts, which creates a special institutional role for NSF in the evolution of values in the American scientific community: in this case valuing the importance of potentially transformative ideas and investment in potentially high-reward research that has risks.

As we share and discuss transformative science with reviewers, panelists, and advisory committees, they incorporate that idea in their own evaluations and promote it in other scientific venues. Interactions among NSF reviewers, program officers, applicants for research and education funding and awardees have shaped and are shaping the culture of American science. Establishing and sustaining this three-way relationship is a signal contribution of NSF and at the heart of the process of discovery in U.S. science.

The recent Netflix million-dollar prize competition is a compelling example of the successful use of crowd sourcing for technological discovery while also contributing to a culture of innovation. Netflix offered \$1 million to anyone who could improve their algorithm for matching movies with customers. The incentive was hugely successful. Of the many creative submissions, two proposed the same promising and highly transformative approach. These two submissions were 20 minutes apart so that under the rules of the contest, the first submission won. However, as described in a recent *New York Times* (September 22, 2009) report by Steve Lohr:

“ . . . the scientists and engineers on the second-place team, and the employers who gave many of them the time and freedom to compete in the contest, were hardly despairing.

Arnab Gupta, Chief Executive of Opera Solutions . . . took a small group of his leading researchers off other work for two years. ‘We’ve already had a \$10 million dollar payoff from what we’ve learned,’ Mr. Gupta said. ‘So for us, the \$1 million dollar prize was secondary, almost trivial.’”

By any measure, the outcomes of NSF’s investments in frontier research in science, engineering, and science education are impressive. NSF’s tradition of merit review that enables new ideas to be tested and funded has served the Nation well. The hallmarks of NSF merit review are:

- Review criteria that identify those ideas that will make a difference both in terms of intellectual merit and broader impacts;

- A selection process that combines evaluation by independent expert merit reviewers with the professional scientific experience and judgment of NSF program officers;
- Management of the merit review process by a combination of permanent program officers, who provide institutional memory and experience, and visiting scientist program officers who contribute recent research expertise.

In the May 12, 2008 issue of *The New Yorker*, James Surowiecki (The open secret of success; [http://www.newyorker.com/talk/financial/2008/05/12/080512ta\\_talk\\_surowiecki](http://www.newyorker.com/talk/financial/2008/05/12/080512ta_talk_surowiecki)) writes about innovation at the Toyota car corporation, which has two elements. First, Toyota turns principles, such as eliminate waste, have parts arrive when needed, fix problems as soon as they arise, into practice better than its competitors. And second, Toyota defines “innovation as an incremental process, in which the goal is not to make huge, sudden leaps but, rather, to make things better on a daily basis . . . Instead of trying to throw long touchdown passes, as it were, Toyota moves down the field by means of short and steady gains.” This leads Surowiecki to conclude: “And so it [this process] rejects the idea that innovation is the province of an elect few; instead it’s taken to be an everyday task for which everyone is responsible.” Said differently, innovation succeeds in practice when it is “institutionalized,” when it is central to the institution’s culture, and when the institution itself is structured to create and sustain innovative thinking.

Multiple lines of evidence support the conclusion that discovering the very best science to fund is a social process. The results are context dependent, which means that is crucial to create and sustain an institutional culture that is open to transformative ideas since hoped for discoveries are often resisted because ideas are premature. Discoveries are prized because they are often challenged and tough to achieve. Path breaking is hard work, and the decision to follow someone down a new road is not always the obvious thing to do. Making that decision requires experience and often wisdom.

NSF’s Program Officers are at the center of this decision-making process; they are the keystone of the agency’s culture of innovation.

### **The NSF Program Officer’s role in fostering transformative research**

NSF relies on the expertise and experience of its permanent and visiting scientist program officers for funding recommendations. After reading proposals, listening to visiting panel reviewers and gleaning advice from external referees it is the program officer who recommends action on a proposal. It is her or his responsibility to integrate all of the information and make a final recommendation based on an understanding of all of the sources. For this reason program officers play a central role in identifying potentially transformative research.

Stewardship and scholarship responsibilities of program officers go beyond merit review responsibilities. These science administrators look for the extraordinary in the proposals they review to create an award portfolio of emerging ideas and outcomes. Beyond the ideas in proposals, new areas for support emerge from a broad and constant set of interactions with the scientific community. As stewards of the Nation’s investment in research and science education they determine enabling levels and durations of funding, mentor postdoctoral fellows and early career scientists, facilitate national and international connections within and across fields, and engage in outreach to promote broader participation and the education of a new generation of scientists as well as the general workforce.

A culture of creativity at NSF is encouraged by regular exercises in which program officers identify and present exciting and emerging areas for future investment within and across directorates. “Blue Sky projects” not limited by disciplinary boundaries are encouraged. Such exercises help program officers to incorporate risky, transformative, and/or interdisciplinary research and education projects as essential parts of their award portfolios.

As NSF experiments with and develops new methods of review and funding directed at enabling transformative science, program officers will experience even greater demands on their time and attention in order to manage these innovative processes and their anticipated additional workload. The Subcommittee asked for an assessment of “the impact of flat agency operations budgets on Program Officers’ ability to identify and support potentially transformative research proposals.” As the research enterprise accelerates and becomes more interdisciplinary, the day to day obligations of proposal and award process management are significantly increasing. Time needed for “just thinking” about a problem, interacting with researchers, and imagining creative new ways to find the best research to fund is decreasing. Fostering program officer creativity requires investment of time and money. Sufficient

personnel and infrastructure support, as requested in the President's 2010 Budget, is needed to ensure that NSF remains a 21st century innovation agency.

### **Supporting institutional creativity through practices and policies**

Identifying proposals during the review process that will produce transformative results before the research is conducted and before the scientific community can assimilate the findings is challenging and imprecise at best. However, the Foundation can and does identify proposals that contain potentially transformative research ideas or concepts, and as discussed already is shaping the institution in ways that facilitate the identification of transformative research. Specifically, NSF has:

- Modified the intellectual merit review criterion to include potentially transformative concepts;
- Established an operational definition of transformative research;
- Provided training to new program officers on the importance of supporting potentially transformative as part of a balanced awards portfolio.

*Modifying the Intellectual Merit Review criterion.* As a result of discussions with the National Science Board and within NSF, a simple but important addition to the NSF Intellectual Merit review criterion was adopted to emphasize to the scientific community and to NSF staff members the importance of potentially transformative research. On September 24, 2007, NSF's Director issued Important Notice No. 130 on transformative research; important notices are sent to presidents of universities and colleges and heads of other NSF awardee organizations. The notice stated that effective October 1, 2007, the NSF Grant Proposal Guide, as well as new funding opportunities issued after that date, would incorporate the following revised Intellectual Merit Criterion—the new wording is underlined:

#### **What is the intellectual merit of the proposed activity?**

*How important is the proposed activity to advancing knowledge and understanding within its own field or across different fields? How well qualified is the proposer (individual or team) to conduct the project? (If appropriate, the reviewer will comment on the quality of prior work.) To what extent does the proposed activity suggest and explore creative, original, or potentially transformative concepts? How well conceived and organized is the proposed activity? Is there sufficient access to resources?*

All proposals received after January 5, 2008, have been reviewed using this revised criterion. Program officers instruct reviewers to pay special attention to those proposals that may include potentially transformative research.

*Defining potentially transformative research.* The National Science Board (NSB) defined transformative research as “research driven by ideas that have the potential to radically change our understanding of an important existing scientific or engineering concept or leading to the creation of a new paradigm or field of science or engineering. Such research also is characterized by its challenge to current understanding or its pathway to new frontiers.” To make the NSB definition operational within the context of NSF's funding programs, the NSF uses the following definition, which builds on the NSB definition with explanatory text and examples:

*Transformative research involves ideas, discoveries, or tools that radically change our understanding of an important existing scientific or engineering concept or educational practice or leads to the creation of a new paradigm or field of science, engineering, or education. Such research challenges current understanding or provides pathways to new frontiers.*

*Transformative research results often do not fit within established models or theories and may initially be unexpected or difficult to interpret; their transformative nature and utility might not be recognized until years later. Characteristics of transformative research are that it:*

- a. *Challenges conventional wisdom,*
- b. *Leads to unexpected insights that enable new techniques or methodologies, or*
- c. *Redefines the boundaries of science, engineering, or education.*

NSF Senior Managers, such as Division Directors, discuss concerns about the conservative aspects of peer review with every panel in order to raise consciousness about the importance of risk-taking and creativity in research. Panels are asked to flag high-risk/high-reward/transformative research.

*Training new program officers.* To ensure that program officers understand NSF's commitment to supporting high-risk/high-reward/transformational research, the Foundation developed a training presentation for new program officers. Senior NSF staff members are advisors at each training session. New program officers receive the May, 2007, NSB Report, "*Enhancing Support of Transformational Research at the National Science Foundation*," the Foundation's guiding principles in support of transformational research; the Foundation's working definition of transformational research, including examples; and a set of Frequently Asked Questions (and answers) related to potentially transformational research. Finally, NSF's Annual Report to Employees in 2007 provided guidance to all NSF staff members about the critical importance of identifying and supporting potentially transformational research.

While we cannot predict which research investments will invariably produce transformational results, we can create institutional structures and cultures, such as those discussed already, that provide a context for recognizing and supporting projects that have the greatest chance of leading to fundamentally new discoveries. Collectively, these institutional mechanisms constitute the process of discovery for potentially transformational research. But if NSF is to be America's premier "innovation agency," the institution itself must always be looking for novel mechanisms to discover the best research to fund. Here are some ways NSF is exploring this exciting frontier.

#### **New approaches for identifying potentially transformational research**

NSF is experimenting with novel mechanisms for developing, reviewing, and funding exploratory and especially creative research. All are new ways to foster NSF's process of discovery.

In January, 2009, NSF announced a new foundation-wide funding mechanism modeled on the Small Grants for Exploratory Research Program. EAGER (Early-concept Grants for Exploratory Research) awards support the initial stages of untested, but potentially transformational research ideas or approaches. The work may be considered especially "high-risk—high-payoff" in the sense that it involves radically different approaches, applies new expertise, or engages novel disciplinary or interdisciplinary perspectives.

Appendix I has a summary of seven targeted NSF programs that support potentially transformational research.

At the Subcommittee's request, activities in the Directorate for Biological Sciences will be reviewed to illustrate how NSF has many features of an innovation agency, and is actively developing structures, programs, and policies needed to function as such an institution.

Biology research and education today increasingly differ from how they were done 10, even five years ago. Frontiers are often at disciplinary "edges:" the intersection of biology and computer and information sciences, engineering, geosciences, mathematics, physical sciences, and social sciences. To the extent that it ever did, biology no longer stops at disciplinary margins, but is reflected in interdisciplinary areas such as bioengineering, biogeochemistry, biomathematics, chemical biology, and evolutionary psychology. The Directorate for Biological Sciences is responding to this reality through:

- Joint CAREER panels involving the Directorate for Biological Sciences and the Directorate for Math and Physical Sciences, which have for six years successfully reviewed proposals from young investigators that integrate innovative research and education at the interface of biology and physics.
- A shared program officer between the Directorate for Biological Sciences and the Directorate for Math and Physical Sciences who is charged with identifying and reviewing proposals in the emerging interdisciplinary area of chemical biology. The success of this activity led us to expand this model with the Geosciences Directorate.
- An Integrated Global Systems Science activity will bring together program officers and professional science support staff members from the Directorate for Biological Sciences and the Directorate for Geosciences in an effort to identify and support the best interdisciplinary research needed to address the global challenges we face as a planet.
- The recently released report "Transitions and Tipping Points in Complex Environmental Systems" from NSF's Advisory Committee for Environmental Research and Education warns that "The global footprint of humans is such that we are stressing natural and social systems beyond their capacities. We must address these complex environmental challenges, and mitigate global-scale environmental change—or accept likely all-pervasive disruptions." This chal-

allenge requires both interdisciplinary research at the interface of natural and human systems and improved environmental literacy that will enable policy-makers both in the U.S. and around the globe to make the informed decisions that will enable us to live sustainably on Earth. A three-year-old Memorandum of Understanding among the Directorates for Biological Sciences, Geosciences, and Social, Behavioral and Economic Sciences to establish Coupled Natural and Human Systems (CNH) as an ongoing cross-directorate program is a successful example of cross-directorate thinking put into action.

- The Directorate for Biological Sciences is exploring the idea of “Fellowships at the Interface,” which will provide training and experience at the interface of biology and other scientific disciplines and education. Consideration also is being given to expanding this program (with an additional investment) to include experience for mid-career scientists at the interface of biology and education.

About 18 months ago Malcolm Gladwell argued in an article in *The New Yorker* that ideas are easy to come by; implementing them is hard. Ideas, Gladwell argued, are not precious, but everywhere. He concluded, therefore, “maybe the extraordinary process that we thought necessary for invention—genius, obsession, serendipity, epiphany—wasn’t necessary at all.” The trick, he felt, was getting together a group of thoughtful, creative people all thinking about how to solve a problem: (“In the Air;” [http://www.newyorker.com/reporting/2008/05/12/080512fa\\_fact\\_gladwell/?yrail](http://www.newyorker.com/reporting/2008/05/12/080512fa_fact_gladwell/?yrail)).

The Directorate for Biological Sciences is using three methods to take advantage of this line of reasoning.

- The “Sandpit” is an experiment in real time, interactive peer review to explore novel solutions to existing problems or indentify new areas of research. The Directorate for Biological Sciences, with participation and support from the Directorates for Math and Physical Sciences, Engineering, Social, Behavioral and Economic Sciences, and Computer and Information Sciences and Engineering, sponsored its first sandpit in the area of synthetic biology in conjunction with the United Kingdom’s Engineering and Physical Sciences Research Council (EPSRC) in April, 2009. This sandpit produced five interdisciplinary, multi-investigator projects with support from NSF and EPSRC.
- The Directorates for Biological Sciences, Engineering, and Social, Behavioral and Economic Sciences also funded an EAGER proposal that focuses on developing a “prediction market” for synthetic biology. A prediction market is a social networking method used to predict the most likely outcome of an event like a presidential election or next quarter’s sales for a business. The principal investigator for this award will use the method to assess where the most creative research investments can be made to advance the area of synthetic biology.
- Synthesis Centers promote the process of collecting and connecting disparate data, concepts, or theories to generate new knowledge or understanding. Beyond its necessity for innovation in basic science, synthesis increasingly contributes to novel and effective solutions for pressing problems, and to the emergence of new ideas or fields of inquiry that would not otherwise exist. Biology Directorate-funded synthesis Centers in conjunction with other NSF Directorates and federal agencies emphasize interdisciplinary research and education in critical areas of the biological, computer, and social sciences. Current centers include: the National Center for Ecological Analysis and Synthesis, the National Evolutionary Synthesis Center, the National Institute for Mathematical and Biological Sciences, and the iPlant Collaborative. These centers advance our understanding by interdisciplinary activities as well as by “getting together a group of thoughtful, creative people all thinking about how to solve a problem.”

Modern cyberinfrastructure can greatly facilitate these ways of identifying the likely places for a commitment to supporting high-risk/high-reward/transformativ research. The social networking manifest in models like crowd sourcing or prediction markets is based on arguments that there is great value in a collective effort focused on uncovering the best sort of research to fund—the so-called “wisdom of the crowd” argument. However, as noted already, NSF’s merit review system is at its root a wisdom-of-the-crowd model. The new extensions of this fundamental model rely on modern computer and information sciences to integrate tens, hundreds, or even, as in the case of the Netflix Prize also discussed earlier, thousands of researchers focused on solving a common problem. These sorts of social networking models are potentially, in an analogy with Clayton Christian’s *The Innovator’s Di-*

*lemma*, a “disruptive technology” when it comes to discovery related to research and education.

But every presumptive innovation carries with it an implicit challenge: How would one know that a novel idea, invention, or method really made a difference? How can we assess any effort at creativity?

### The assessment challenge

NSF tends to describe itself in terms of its awards, just as other federal basic research funding agencies. One form of assessment, then, is a review of the narrative summarizing the kinds of research the agency funds.

NSF tracks research outcomes in the form of highlights, which are short descriptions of research and educational outcomes composed by program officers using material provided by principal investigators. Just as for research proposals, merit review can be applied to institutions, and NSF also uses this method. NSF relies on the judgment of external experts to maintain high standards of program management, to provide advice for continuous improvement of NSF performance, and to ensure openness to the research and education community served by the Foundation.

Every NSF program is evaluated by a Committee of Visitors (CoV) every three years. Each CoV submits a detailed report to the appropriate NSF Advisory Committee, which itself is composed of members drawn from the communities NSF supports. All CoV reports are available (<http://www.nsf.gov/od/oia/activities/cov/covs.jsp>). CoV reviews provide NSF with assessments of the quality and integrity of program operations and program-level technical and managerial matters pertaining to proposal decisions. Each CoV comments on how the results generated by awardees contribute to NSF’s mission and strategic outcome goals, including an assessment of the division/program’s investments in high-risk/high-reward/transformational research projects.

The Advisory Committee for GPRA (*Government Performance and Results Act*) Performance Assessment (AC/GPA) is charged with determining whether NSF has demonstrated “significant achievement” under its strategic outcome goals. This Foundation-wide Advisory Committee has 22 members from outside of NSF drawn from academia, industry, and government. AC/GPA reports to NSF’s Director. In its annual evaluation, the committee focuses on program highlights, reports from CoVs, and issues such as transformational research, broadening participation, and societal benefit. The most recent report notes:

*It is the unanimous judgment of the 2008 Advisory Committee for GPRA Performance Assessment (AC/GPA) that the National Science Foundation successfully met its performance objectives by demonstrating significant achievement for each of the following three long-term, qualitative, strategic outcome goals in its 2006–2011 Strategic Plan:*

- *DISCOVERY: Fostering research that will advance the frontiers of knowledge, emphasizing areas of greatest opportunity and potential benefit and establishing the Nation as a global leader in fundamental and transformation science and engineering.*
- *LEARNING: Cultivating a world-class, broadly inclusive science and engineering workforce, and expand the scientific literacy of all citizens.*
- *RESEARCH INFRASTRUCTURE: Building the Nation’s research capability through critical investments in advanced instrumentation, facilities, cyberinfrastructure and experimental tools.*

However, the AC/GPA also took issue with the practice of evaluating NSF’s performance using only highlights because they were limiting in several ways:

- Highlights are annually scoped and cannot address long-term outcomes or societal impacts.
- Highlights are written about individual awards or projects, not fields or communities. The relevance of an individual project or result cannot be understood in isolation.
- Highlights do not capture “people outcomes,” which are central to NSF’s vision.
- Highlights are anecdotal, both in subject matter and in the non-systematic nature of their collection.

At any given time, these assessment mechanisms provide a contemporary case history of how research results from NSF awards relate to the agency’s mission and strategic goals. However, the longer-term “transformational” impacts of the knowledge and technologies that result from these successful scientific investments—on subse-



quent scientific research, the economy, and society—are often realized years later. For funding agencies like NSF, identifying proposals during the review process that will produce transformative results before the research is conducted and before the scientific community can assimilate the findings is challenging at best.

Mistakenly, it is sometimes assumed that research discoveries can be quickly brought to market and this rate can serve as an assessment metric. But it is intrinsic to the research enterprise that investments that are scientifically successful in the short-term cannot guarantee similar short-term economic gains. Dr. Julia Lane of NSF noted recently that:

“. . . [A] focus on economic value alone may also understate the true returns of investments in science. Indeed, one strand of research is attempting to develop a public value mapping of outcomes: outcomes that are public, non-substitutable, and oriented to future generations and that capture dimensions such as competitiveness, equity, safety, security, infrastructure and environment.” [Assessing the Impact of Science Funding. 2009. *Science* 324, 1273–1275]

The 2008 AC/GPA recommended that NSF “consider ways to convey the long view of NSF investments in science and engineering” and “track future outcomes from people trained and supported by the Foundation.” However, the absence of computer information systems designed to manage information rather than to simply process reviews, awards, or reports is a serious impediment to understanding how NSF awards connect to leading edge science and long-term outcomes. What is needed is a program information management system that connects the agency’s award portfolios with one another, with other federal research agencies, with the scientific community, and with the public. Such a system would enable a reciprocal interaction (another form of crowd sourcing) among all of these elements.

The NSTC’s Science of Science Policy Interagency Group has identified this lack as a major issue in its recent Roadmap ([http://www.ostp.gov/galleries/NSTC%20Reports/39924\\_PDF%20Proof.pdf](http://www.ostp.gov/galleries/NSTC%20Reports/39924_PDF%20Proof.pdf)). In particular, there are currently no data infrastructure that identifies the universe of individuals funded by federal science agencies (PIs, co-PIs, graduate and undergraduate students, lab technicians, science administrators, etc.) and that systematically couples science funding with the outcomes generated by those individuals. In searching for prototypes for the development of more meaningful assessment methods, NSF has begun to look within—to the Directorate for Social, Behavioral and Economic Sciences (SBE), where Program Officers and a research community think about these things—for the methods and measures needed to understand the transformative contributions of new scientific knowledge to economic and social outcomes, to inform future investments, and to convey this information in a manner that is understandable to policy-makers and the public. SBE has programs such as Science of Science and Innovation Policy (SciSIP) and Science, Technology and Society (STS) that are funding work on next generation science assessment. Also, SBE’s Science Resource Statistics Division, the Nation’s resource for science statistics, is dedicated to continual improvement through ongoing workshops and consultations.

### **The role of NSF, universities, and the private sector in supporting potentially transformative research**

As noted earlier, NSF has a long history of success in supporting research with far-reaching impacts on the U.S. economy and the well-being of Americans. Since 1950 this success has relied on a close integration with America’s colleges and universities, which are the principal locus of the research NSF funds; unlike other federal agencies, NSF has no intramural labs or research staff. Significantly, NSF research grants are made for the short- or long-term, and results are not classified, but readily published in the open literature. In contrast, industry usually has shorter-term goals aimed at the market place, and results are often proprietary and therefore not readily shared.

In the October, 2008, issue of *Computerworld*, Gary Anthes wrote: “By most measures, the U.S. is in a *decade-long decline in global technological competitiveness*. The reasons are many and complex, but central among them is the country’s retreat from long-term basic research in science and technology, coupled with a surge in R&D by countries such as China.” He went on to note that “the kind of pure research that led to the invention of the transistor and the Internet has steadily declined as companies bow to the pressure for quarterly and annual results.” He emphasized how many companies now support development, as opposed to the kind of basic research done at colleges and universities with NSF support. And there is also an increasing trend on industry’s part to take even the basic research that it does offshore. Thomas Friedman recently noted to his dismay that “America’s premier solar equipment maker, Applied Materials, is about to open the world’s largest pri-

vately funded research facility—in Xian, China.” (The New Sputnik. *New York Times*, Sunday, September 27, 2009: p. wk 12.)

If federal agencies such as NSF were to adopt shorter-term perspectives exclusively as a way to meet new national needs, we risk an eventual intellectual and technological vacuum. Anthes feels this is already happening: “The refocus from long-term research to shorter-term development in industry—and Bell Labs is by no means the only example—has been mirrored by a similar trend among the Washington agencies that fund science and technology, such as the Departments of Defense and Energy, the *National Institutes of Health* and the National Science Foundation. Federal funding for R&D has not declined overall—it has, in fact, increased. But since the early 1990s, funding has been more and more focused on the short-term needs of government.” He reports no evidence in support of this claim, but the point deserves reflection.

The U.S. must continue to support transformative research with potential long-term benefits. In a science and technology-based world that will underlie knowledge-based economies to divert our focus from the frontier is to disadvantage us in many ways. Sometimes it just takes unfettered time to make discoveries at the leading edges of knowledge: it is just this freedom that is the essential quality of the R&D that NSF as an innovation agency supports in partnership with America’s institutions of higher learning. The NSF activities in Appendix II are examples of the productive intersection between basic research supported by a federal agency and the private sector and universities.

For nearly 60 years NSF has been forward looking in terms of how the agency manages the scientific enterprise. Merit review fosters the “process of discovery,” that is the means by which researchers can identify and answer leading/transformativ/grand challenge questions. At the heart of the task of being a manager or administrator of the scientific enterprise there should be an abiding interest in the best ways to identify leading/transformativ/grand challenge research opportunities. As new modes of science management emerge, especially those facilitated by modern information management systems, science administrators at the frontier will increasingly experiment with these new methods.

Mr. Chairman, as I noted at the start of my testimony, NSF has many features of an innovation agency, and these features will continue to evolve in ways that will ensure NSF’s place as first and foremost an innovation agency dedicated to funding the world’s best research and education.

I appreciate the opportunity to appear before the Subcommittee to speak to you on this important topic. I would be pleased to answer any questions that you may have.

## Appendix I

### NSF-targeted activities supporting potentially transformative research

In the Directorate for Engineering (ENG), the **Office of Emerging Frontiers of Research and Innovation (EFRI)** was conceived specifically to support high-risk, high-reward research. Beginning with its first awards in 2007, EFRI has funded investigations in areas where new concepts, new collaborations, and new approaches are essential to address grand engineering challenges or national needs. For example, EFRI researchers are investigating the topic of autonomously reconfigurable systems, which can respond to even unanticipated changes of circumstance. Teams are conducting unprecedented research to forge a theoretical framework for embedding autonomous reconfigurability into any type of complex system, including air traffic, wireless communication networks, and urban transportation networks. One team is creating a group of robots that can sense variables in their surroundings and self-assemble into a structure best suited for that particular environment. Engineering this new capability into human-made systems could transform infrastructure reliability and disaster response.

Since its inception, the **Engineering Research Center (ERC)** program has supported high-risk, transformative research and the development of the Nation’s leaders in innovation. The 2009 solicitation focuses explicitly on new mechanisms to link discovery to technological innovation in order to concurrently advance technologies and produce engineers who can lead U.S. innovation in a globally competitive economy. Two examples of transformative results from ERC-supported research include the portable defibrillator and early warning systems for tornadoes and other low-ground storm systems.

In the Directorate for Computer & Information Science & Engineering (CISE), the focus in 2010 for transformative research will include the **Expeditions in Computing** Program. Expeditions are large multi-disciplinary awards targeted to compelling, transformative research agendas that promise disruptive innovations in computing and information science and engineering. Funded at levels of up to \$10M, Expeditions represent some of the largest single investments currently made by CISE.

The NSF-wide **Cyber-enabled Discovery and Innovation (CDI)** program is another example of NSF's support for potentially transformative research. CDI recognizes that "computational thinking" (i.e., computational methods, concepts, models, algorithms and tools) will transform how all science and engineering will be conducted in the 21st Century. Computational abstractions, as much as high-speed computers and high-bandwidth networks will enable scientists and engineers to make new discoveries by changing the very questions they can ask. Above and beyond the usual NSF requirements, CDI uniquely requires that research projects advance two or more disciplines as well as innovations in or innovative uses of computational thinking.

The NSF Office of Cyberinfrastructure (OCI) will focus investments on the **Strategic Technologies for Cyberinfrastructure (STCI)** Program whose primary purpose is to support work leading to the development and/or demonstration of innovative cyberinfrastructure services for science and engineering research and education that fill gaps left by more targeted funding opportunities. In addition, STCI considers highly innovative cyberinfrastructure education, outreach and training proposals that lie outside the scope of targeted solicitations.

The **Directorate for Social, Behavioral and Economic Sciences (SBE)** is working to catalyze transformative science in three major ways. First, its largest funding opportunities are for multi-disciplinary research projects, thus encouraging the transformations that are possible when disciplinary silos are shattered. Second, SBE has alerted its scientists that it is interested in funding complexity science projects. Complexity science lies at the edge of normal science and is especially promising terrain for transformative insights. Third, SBE is working with its communities to identify and create major infrastructure—particularly new databases and new tools for assembling, analyzing and managing data—that will enable next generation analyses of social, behavioral and economic phenomena. SBE has chosen to do all this by integrating these transformative mechanisms into its regular standing scientific programs rather than by creating separate activities. This is because they want to ensure that the appreciation and norms for reviewing and supporting potentially transformative science are visible to and integrated into the entire community, rather than separated from normal scientific review and discussion.

The NSF **Plant Genome Research Program (PGRP)** within the Directorate for Biological Sciences (BIO) began in February 1998 as part of the National Plant Genome Initiative (NPGI), which is managed across federal agencies by an Interagency Working Group on Plant Genomes. The long-term goal of the NPGI is to develop and apply basic plant genome knowledge to a comprehensive understanding of economically important plants and plant processes. Connecting basic research to plant performance in the field accelerates basic discovery and innovation, which enables improved management of agriculture, natural resources, and the environment. To date the PGRP has contributed to the genome sequences and tools for studying both model and crop plants, including *Arabidopsis*, maize (corn), soybean, potato, tomato and Medicago. Training and outreach is built into all PGRP projects. PGRP-supported tools such as Targeted Induced Local lesions IN Genomes (TILLING) are now used in research and commercial settings for a wide range of plants and animals. TILLING technology has led to a spin-off company that is now part of Arcadia Biosciences. Since agricultural challenges do not stop at national borders, the PGRP, in coordination with USDA and USAID, expanded its efforts in 2004 to include Developing Country Collaborations for Plant Genome Research. In 2009, the NSF in partnership with the Bill & Melinda Gates Foundation (BMGF) established a new program called **Basic Research to Enable Agricultural Development (BREAD)**. With equal support from NSF and BMGF (a total of \$48 million over five years), BREAD will fund basic research to develop innovative solutions to the agricultural problems faced by small farmers in developing countries. This exciting new partnership will enable NSF to leverage basic research advances made through the NPGI with BMGF funding for implementation to international partners. The Plant Genome Research Program has developed tools and resources that not only have transformed our understanding of plant structure and function, but that now are enabling us to tackle pressing needs for new plant-based materials, new energy sources, and plants that adapt to environmental stresses resulting from a changing climate.

## Appendix II

### Examples of NSF activities at the intersection of federally funded basic research and the private sector and universities

NSF-funded Centers are designed from the outset with built-in flexibility so that investigators can pursue innovative ideas within the context of a defined program of research. Examples are legion, and include the Mosaic web browser developed at NSF's National Center for Supercomputing Applications at the University of Illinois. NSF's creation of two Centers for the Environmental Implications of Nanotechnology (CEIN) in 2008 exemplify innovative networks that are connected to other research organizations, industry, and government agencies to strengthen our nation's commitment to understanding the potential environmental hazards of nanomaterials and to provide basic information leading to the safe environmentally responsible design of future nanomaterials.

The **Industry/University Cooperative Research Centers (I/UCRC)** program develops long-term partnerships among industry, academe, and government. Each I/UCRC contributes to the Nation's research infrastructure, enhances the intellectual capacity of the STEM workforce by integrating research with education, and encourages and fosters international cooperation and collaborative projects. For example, the NSF Industry/University Collaborative Research Center (I/UCRC) known as the Berkeley Sensor and Actuator Center conducts industry-relevant, interdisciplinary research on micro- and nano-scale sensors, moving mechanical elements, microfluidics, materials, and processes that take advantage of progress made in integrated-circuit, bio, and polymer technologies. This I/UCRC has developed and demonstrated a hand-held device that allows verified diagnostic assays for several infectious diseases currently presenting significant threats to public health, including dengue, malaria, and HIV. The device uses a dramatically simplified testing protocol that makes it suitable for use by moderately-trained personnel in a point-of-care or home setting. The center has also created many spin-off ventures including companies in the areas of wireless sensor networks for intelligent buildings; MEMS mirror arrays for adaptive optics; and optical flow sensors for industrial, commercial, and medical applications.

The objective of the NSF **Small Business Innovation Research (SBIR)** program is to increase the incentive and opportunity for small firms to undertake cutting-edge research that would have a high potential economic payoff if successful. For example, in 1985, Andrew Viterbi and six colleagues formed "QUALITY COMMUNICATIONS." In 1987-1988 NSF SBIR provided \$265,000 (Phase I 8660104 and Phase II 8801254) for single chip implementation of the Viterbi decoder algorithm. Qualcomm introduced CDMA (code division multiple access) which replaced TDMA (time division multiple access) as a cellular communications standard in 1989. This advance led to high-speed data transmission via wireless and satellite. Now the \$78B company holds more than 10,100 U.S. patents, licensed to more than 165 companies. Another example—Machine Intelligence Corp. was supported by SBIR Phase I and Phase II awards to develop desktop computer software that could alphabetize words, a feat that previously had been accomplished only on supercomputers. When Machine Intelligence went bankrupt, principal investigator Gary Hendrix founded Symantec and continued the project. The line of research resulted in the first personal computer software that understood English, marketed as "Q&A Software." Q&A quickly became an extremely successful commercial product and remains a widespread commercial application of natural language processing. Symantec research supported by NSF SBIR eventually led to six other commercial products and contributed to 20 others. Now, Symantec is a leading anti-virus and PC-utilities Software Company valued at \$12B with more than 17,500 employees worldwide.

NSF launched the **Integrative Graduate Education and Traineeship Program (IGERT)** in 1997 to encourage innovative models for graduate education at colleges and universities across the Nation that would catalyze a cultural change in graduate education—for students, faculty and institutions. IGERT was designed to challenge narrow disciplinary structures, to facilitate greater diversity in student participation and preparation, and to contribute to the development of a diverse, globally-engaged science and engineering workforce. The result has been a cadre of imaginative and creative young researchers. For example, an NSF-funded IGERT award to the Scripps Institute of Oceanography (NSF #0333444) supported a doc-

toral student who successfully modeled the extinction of the Caribbean monk seal and demonstrated the magnitude of the impact of over-fishing on Caribbean coral reefs. This research developed improved ecological models, which may influence environmental policy and ultimately lead to the preservation of species and ecosystems for future generations.

#### BIOGRAPHY FOR JAMES P. COLLINS

Dr. James Collins received his B.S. from Manhattan College in 1969 and his Ph.D. from the University of Michigan in 1975. He then moved to Arizona State University where he is currently Virginia M. Ullman Professor of Natural History and the Environment in the School of Life Sciences. From 1989 to 2002 he was Chairman of the Zoology, then Biology Department. At the National Science Foundation (NSF), Dr. Collins was Director of the Population Biology and Physiological Ecology program from 1985 to 1986, and Assistant Director for Biological Sciences from 2005 to 2009. NSF is the U.S. Government's only agency dedicated to supporting basic research and education in all fields of science and engineering at all levels. Collins oversaw a research and education portfolio that spanned molecular and cellular biosciences to global change as well as biological infrastructure. He coordinated collaborations between NSF and other federal agencies through the President's National Science and Technology Council where he chaired the Biotechnology Subcommittee and co-chaired the Interagency Working Group on Plant Genomics. He was also NSF's liaison to NIH.

Dr. Collins's research has centered on the causes of intraspecific variation. Amphibians are model organisms for field and laboratory studies of the ecological and evolutionary forces shaping this variation and its affect on population dynamics. A recent research focus is host-pathogen biology and its relationship to population dynamics and species extinctions. The role of pathogens in the global decline of amphibians is the model system for this research.

The intellectual and institutional factors that have shaped Ecology's development as a science are also a focus of Dr. Collins's research, as is the emerging research area of ecological ethics. Federal, State, and private institutions have supported his research.

Dr. Collins teaches graduate and undergraduate courses in ecology, evolutionary biology, statistics, introductory biology, evolutionary ecology, and professional values in science; he has directed 33 graduate students to completion of doctoral or Masters degrees. Collins was Founding Director of ASU's Undergraduate Biology Enrichment Program, and served as Co-Director of ASU's Undergraduate Mentoring in Environmental Biology and Minority Access to Research Careers programs.

Honors include the Pettingill Lecture in Natural History at the University of Michigan Biological Station; the Thomas Hall Lecture at Washington University, St. Louis; and serving as Kaeser Visiting Scholar at the University of Wisconsin-Madison. ASU's College of Liberal Arts and Sciences awarded him its Distinguished Faculty Award. He is a Fellow of the American Association for the Advancement of Science and a Fellow of the Association for Women in Science.

Dr. Collins has served on the editorial board of *Ecology and Ecological Monographs* as well as *Evolution*. He is the author of over 100 peer reviewed papers and book chapters, co-editor of three special journal issues, and co-author with Dr. Martha Crump of *Extinction in Our Times. Global Amphibian Decline* (Oxford University Press, 2009).

Chairman LIPINSKI. Thank you, Dr. Collins. Dr. McCullough.

#### **STATEMENT OF DR. RICHARD D. MCCULLOUGH, VICE PRESIDENT FOR RESEARCH; PROFESSOR OF CHEMISTRY, CARNEGIE MELLON UNIVERSITY**

Dr. MCCULLOUGH. Thank you very much, Chairman Lipinski, Ranking Member Congressman Ehlers, Members of the Committee, ladies and gentlemen. It is a distinct honor to be before you today testifying about investing in high-risk, high-reward research.

I am the Vice President at Carnegie Mellon, as has been noted, and I am also an active researcher. I am still a funded researcher, and I have a lab of many graduate students. I also have taken NSF funding to generate innovation in my lab which has turned into a company that employs 70 people. So I understand high-risk re-

search from the beginning all the way through the cycle, so I think I can speak with it in a special way.

Today I really want to give you a report from the front lines, in the trenches, perspective on high-risk, high-reward research, and I want to sort of let you know that when a researcher comes up with an idea, that researcher is faced with how to fund the idea. Often they see it as a high-risk proposal, but they don't necessarily understand whether it is a high-reward or transformative idea. So often they turn to very few sources they have to fund this idea. One needs to get preliminary results in order to go to the standard funding agencies like NSF or NIH to get your proposal funded, but there are no funds to fund the graduate students or post-docs who actually do the work, so that you can show that you have preliminary results, so that you can actually have these proposals funded. Whereas there are programs at the NSF and NIH to fund these programs, and NIH has actually done quite a good job recently of funding these high-risk proposals, except there is very little money to go after, and whether that is real or perceived, it is mostly at least perceived. At our university, we only have one such high-risk, high-reward proposal that has been funded in the whole entire university, and that is at the NSF and it is for \$66,000.

So most people go to the standard proposal mechanism to get their research funded. So it is the chicken and the egg problem in that they don't have the funds to get started, and so they have to find funds either within their existing grants or within the university or sometimes with foundations to be able to get these projects going so that they can get the reward of a grant at the NSF or the NIH, so that they can move this project forward.

We have many examples which I have included in my testimony, and I won't walk through all of those today. But some of the most striking examples of high-risk, high-reward research are where a computer scientist, who is an expert in data mining, comes together with a person who does brain imaging, and the two of them can combine high-output brain imaging scans and data mining together to be able to tell what people are thinking. Now, you may be scared of that in some respect, but it is really important for brain research from an injury, for our soldiers, and things like this that we can understand how people can heal. This proposal wasn't funded two times by the NSF because all the panel people said that it was way too high-risk. It hadn't been proven. And I have many other examples like that.

So the system itself has some failings. It is not the program officers that are at fault. They are just faced with a very difficult challenge of lots of proposals that they have to fund, including often determining whether someone is going to get tenure or not or whether they are going to remove the funding from an established researcher.

So I have a few recommendations that I will make to you today. One is, I think high-risk, high-reward research has to be funded. I think it has to be a set-aside so there is not competition within the agencies to direct those funds into normal proposals, which I think are low-risk and not necessarily innovative.

I think there needs to be a process, guidance by reviewers with language written in these high-risk programs that says what they

expect these proposals to actually be, that maybe preliminary results are not needed. We can't depend completely on the program officers because often these areas are so interdisciplinary and so broad that it is impossible to expect one person to be able to make that decision alone. So I think special panels with special guidance are important.

I would recommend funding a milestone proof-of-concept where you can apply for high-risk, high-reward research where you get \$100,000, \$200,000 to work on that project, and then if you show you can get proof of concept, then you can get further payments down the line so that these turn into not just high-risk with no reward but high-risk so they can be put into the system in a normal way.

Another recommendation that I would make is that there should be a mechanism for basic research that becomes transformative. Often great discoveries are made that we don't know are going to happen until they happen. But when they become transformative, there ought to be mechanisms for accelerator funds from these agencies that a person can go to and say, I have made this amazing discovery. Please help me take this and make it transformative. And right now there are no mechanisms to do that except to work off the back of these proposals.

So thank you very much for the opportunity to tell you what is going on in the front lines.

[The prepared statement of Dr. McCullough follows:]

PREPARED STATEMENT OF RICHARD D. MCCULLOUGH

Chairman Lipinski, Ranking Member Congressman Ehlers, Members of the Committee, and ladies and gentlemen. It is a distinct honor to testify before the Committee on *Investing in High-Risk/High-Reward Research*.

My name is Rick McCullough and I am the Vice President for Research and a Professor of Chemistry at Carnegie Mellon University. In addition to my administrative job, I remain active in doing research. I am also a co-founder of Plextronics, Inc., a Pittsburgh-based, high-tech start-up company with over 70 employees that produces printable, green solar technologies and printable inks for lighting and display applications. So I have had a variety of experiences with high-risk/high-reward research.

Today, I want to give you a "frontline/in the trenches" perspective on high-risk/high-reward research. As you know, there are a number of excellent reports on high-risk/high-reward or transformative research providing an enormous amount of motivating background information. These include: the 2007 National Academy Report, *"Rising Above the Gathering Storm: Energizing and Employing America for a Brighter Economic Future,"* the 2007 National Science Board Report, *"Enhancing Support of Transformative Research at the NSF,"* and the more recent 2009 American Academy of Arts and Sciences (AAAS) Report, *"ARISE: Advancing Research in Science and Engineering."*

The United States' leadership in science and technology is at risk. This is particularly troublesome when one considers how vital innovation is to the US economy and our ability to be competitive as a nation. While increased resources for basic research are absolutely vital to our ability to remain leaders in science and technology, it is also important to consider if the process for obtaining funds for high-risk/high-reward research is broken. Consider what happens when a researcher has a new idea. First, this will require funding to pursue the research needed to test that idea. The faculty member can pursue basic research funding or a high-risk/high-reward funding.

Where would a faculty member turn for research funding? Like most Tier 1 research universities, Carnegie Mellon receives most of its research funding from the Federal Government. Carnegie Mellon's percentage of federal science and engineering funding is around 82 percent, with 13 percent coming from private sources and five percent coming from the university. So a faculty member generally thinks of federal agencies such as the NIH, NSF, DOE, NASA, or the DOD as sources of fund-

ing for their new ideas. However, the researcher is faced with an extremely competitive grant climate and must maximize the odds of receiving funding for the project. What faculty members know or feel is that hit rates on NSF proposals have dropped 13 percent over the last four years at Carnegie Mellon and NIH hit rates have dropped 18 percent over the last three years. Great progress has been made by Congress to increase research funding and we are most grateful, however there is a lag to realize this new funding. To maximize the probability of getting your grant funded (in a regular program or one of the very small high-risk programs), one of the most important factors is the ability to demonstrate proof of concept and/or present preliminary results that show the feasibility of the proposed approach. In order to get preliminary results, the faculty must either have funded graduate students or postdoctoral researchers that actually perform the work. Faculty members can sometimes find overlap between the high-risk research idea and projects funded by other grants. However, if the idea is truly transformational, then probability of success in obtaining funding is a problem. That is, you need results to get funded and you need funding to get results. I would be shocked if the NIH or the NSF had programs where the idea is truly new and is high-risk/high-reward, if that proposal would be funded without preliminary results. I could be wrong, but I assure you that the number of high-risk funding opportunities without preliminary results is diminutive.

Nevertheless, the NIH is working hard to create new programs such as the NIH Director's Pioneer Award, New Innovator Award, and the Transformative RO1, all of which accounted for the awarding of \$348M to 115 grantees. This is a tremendous start. However, when a faculty member or a brand new researcher is setting out on a new strategic area of research he or she may find it difficult to obtain the rare (18 in 2009) Director's Award. I hope for an increase in the number of pioneers for the future. I recommend that Congress explore directing additional funding toward Pioneer Awards that stimulate high-risk research projects.

If you go to the NSF, the situation is worse. In my opinion, the system is broken. The NSF has had the Small Grants for Exploratory Research (SGER) program that evolved to the Early-concept Grants for Exploratory Research (EAGER). These grants began, as I recall, as one-time \$50K grants that were rarely funded. I can tell you about a grant that I submitted with two other top researchers that would create a completely new way to make plastic superconductors that was not funded; it was probably too risky and we did not have proof of concept. Nevertheless, the program has expanded where \$2M/division has been allocated for transformative research. This is a start, but I believe that the system of evaluation and funding of high-risk/high-reward research at the NSF needs to be improved. My colleagues at Carnegie Mellon have related to me that it is often easier to get resources for high-risk research by getting preliminary results at a very slow pace and then using the normal grant mechanisms to fund transformational research. This is the way I look for funding for high-risk research as well. From the perspective of these faculty members, high-risk/high-reward research funding is virtually unavailable from traditional federal sources.

Reading the National Science Board's 2007 report entitled "*Enhancing Support of Transformative Research at the NSF*," one can find that many of the needed improvements to the program are recommended in that report. I find that report echoes many of the recommendations I would make to you today.

For example, I agree with the NSB report that our first challenge is clearly defining transformative or high-risk/high-reward research and how to distinguish it from the definition of basic research. It is important to note two caveats to defining high-risk/high-reward research: 1. scientists and engineers are often not that good at marketing and sales and many will rarely think of their ideas initially as high-reward or transformative and 2. many scientific discoveries occur in basic science and are even accidental and then become transformative.

In addition, in the EAGER program at NSF leaves funding of high-risk/high-reward proposals to program directors. This presents multiple challenges in the evaluation process, such as: 1. program officers often do not have the expertise to determine what is high-risk/high-reward research; 2. program officers do not often have the expertise to judge the proposals which can be broad and highly interdisciplinary in scope; and 3. the monies that are set-asides are usually at the discretion of the program officers who are faced with the pressure of not having enough resources to fund highly rated proposals. For example, a program officer who is faced with funding a mid-career scientific leader, or funding the last attempt by a junior faculty member who is up for tenure, would find it extremely difficult to divert funds for high-risk/high-reward research. In addition, highly interdisciplinary research that is seeking high-risk research funding will find itself in one discipline with a program officer from that one discipline. In theory, such program officers can col-



laborate to fund the proposal across disciplines by going to other program officers and asking if they are interested in jointly funding the proposal. However, collective funding across divisions is probably a difficult process. This is not to be critical of the NSF program managers. They have a very difficult task because the reality is that they do not have enough resources to fund all the great proposals that they receive and they face ever-changing reporting requirements and short-term accountability.

Consequently, high-risk/high-reward proposal programs are not viable options in cases such as these. As an example, Carnegie Mellon has **one** \$66,000 EAGER grant from the NSF and **zero** NIH Director's Pioneer Awards, **zero** New Innovator Awards, and **zero** Transformative RO1 grants.

Alternatively, a researcher might hope to get funding for a high-risk/high-reward proposal via the normal NSF or NIH process; however these proposals are not a good fit for that process either. Typical panels that review the basic research proposals clearly do not reward high-risk/high-reward proposals with funding. Panels generally (not all) reward incremental research where preliminary results are absolutely critical to funding. Panels are often the "white blood cells" of high-risk/high-reward research, since these proposals are easy targets and the *reason* for elimination from competition. As one advisory board member to one of the divisions of the NSF said, the system is set up to reward low-risk research. One program manager's response was, if he is expected to report in one year how this research has contributed to our country, how can he take a chance on high-risk research? I will give you multiple anecdotes on proposals in the regular process that get killed for being high-risk/high-reward proposals.

I do believe that one solution might be to create special panels led by hand-picked committee chairs that would review proposals for their *potential* as transformational or high-reward. New guidance by the NSF could instruct special panels and/or outside reviewers that preliminary results are not necessary so that researchers (new and old) moving into new areas of high-risk research can have a chance at funding. I would also suggest a system where seed funding can be provided and, after proof of success, additional funds can be released. For example, funding might be provided for two years and with success of converting the high-risk research into proof of concept results, an additional release of funds could occur.

Faculty members can also turn to foundations for the support of high-risk/high-reward research. Examples where Carnegie Mellon has had success in this regard would include the Keck Foundation, the Heinz Endowments, the R.K. Mellon Foundation, the Gordon and Betty Moore Foundation, the John D. and Catherine T. MacArthur Foundation, and the Doris Duke Foundation. However, the opportunities for funding from these foundations are highly limited to a few faculty members within the university. In the same vein, private support is limited to a few selected centers or individuals. An example would be private support for programs such as the Ray and Stephanie Lane Center for Computational Biology.

In addition, one strange aspect to high-risk/high-reward research is that many great discoveries are accidental. As the late Carnegie Mellon Nobel Prize winner Herb Simon used to say, to do world-class research, one should look for surprises and explain them. This is how the material  $C_{60}$  was discovered. The late Nobel Prize winner, Rick Smalley of Rice was shooting high powered lasers at graphite and off came buckyballs or  $C_{60}$ . It was later found that when  $C_{60}$  is combined with certain conducting polymers (that we discovered), one can make an ink that can be printed to form a plastic solar cell that absorbs light from the sun and makes energy. The transformational discovery of  $C_{60}$  may end up transforming energy production by making solar incredibly inexpensive.

### Examples of High-Risk/High-Reward Projects at Carnegie Mellon

#### *Reading Minds with Computers*

In the early 2000's two of our top professors (one in psychology and one in computer science) wrote two NSF proposals to seek funding for research that applies machine learning to fMRI (functional magnetic resonance imaging) brain image analysis. The idea is that using high-speed/data mining of brain scans, it might be possible to understand human thoughts. The use in medical brain research and therapy such as the treatment of traumatic brain injury, as only one example, would be profound. The first proposal received weak reviews and was not funded. The reviews said that while the impact of the proposed work would be very high, the techniques were unproven and the work was too high-risk. A year or so later, a second proposal was submitted, this time with compelling preliminary results showing that the researchers could train machine learning programs to decode various cognitive states of a person from their brain image data (e.g., whether they were reading a

sentence or viewing a picture). Again, the reviews said this was unproven technology and the proposed research was too high-risk, in comparison with other proposals. It was headed for a rejection, but a wise NSF program manager used his discretion to bump it up into the barely fundable category, and the NSF provided small grant so that we could start the work. The Provost's office at Carnegie Mellon provide funds when the NSF funds ran out and eventually we were able to get some funding from the Keck Foundation. This work has been a huge success and has been featured recently on *60 Minutes*. The one of the success stories of a high-risk/high-reward project.

*Using the Power of Ubiquitous Sensors and Computers as Safety Sensors*

We have a team of top professors in Civil Engineering and Electrical and Computer Engineering that have created hardware sensors and software that can be used anywhere at anytime to monitor buildings, roads, bridges, water infrastructure, etc. This group recently submitted a proposal, whose reviews were generally quite complimentary, and described by many of the reviewers as a clear example of a high-risk, high-reward endeavor. However, they were also criticized for not presenting sufficient results to back up the proposed approach as being feasible. My office at Carnegie Mellon is currently funding the project and supporting one student. However, the project is at risk of not continuing.

*Using Free Human Work on the Internet to Digitize Books*

We have a project by an award winning computer science professor that proposes to use computer programs to digitize books. When people open accounts on gmail, Yahoo, etc., or buy tickets on-line they have to translate a distorted word to be able to open said account or buy tickets. These distorted words called CAPTCHAs prevent computers from opening the accounts, because computers cannot read the distorted words. However, humans can translate the distorted words with ease. It turns out that distorted words are a problem when books are digitized. A person makes a copy of the book and at the edge, some of the words are distorted and therefore cannot be read by a computer. The professor's idea was to use the same distorted words from book digitization as *the* words that need to be translated for book digitization. Therefore, free human work to translate the distorted words to open accounts gets sent back and help to digitize books. The NSF declined to fund this work. The work was funded internally and led to ReCAPTCHA and a spin-out company from Carnegie Mellon that was recently sold to Google.

*Others*

We had a project that uses machines to interpret biomedical research data and the computer can teach itself what to look for in cancer diagnostics. We have proven that machines can do this work better than humans can. This project was funded by the Scaife Foundation, then Keck, and by private sources, but was always reviewed by the NIH as high-risk/high-reward research and was never initially funded. Another similar project uses high-power computer science to attack massive data sets related to cancer diagnostics. The professor told me that he wrote a proposal to the NSF that was funded and is funding the high-risk project at a 10 percent level from that grant. His initial grant focusing on this approach was rejected as being too high-risk.

Our work at the university in Green Chemistry has had a very difficult time securing federal funding. One of our professors has created revolutionary new catalysts that activate non-toxic hydrogen peroxide to create systems that, in a green way, can be used to clean up toxic rivers, bleach pulp in the paper bleaching process, allow very little water to be used in laundry wash cycles, etc. He has not been able to secure NSF funding.

We have multiple areas of futuristic research at Carnegie Mellon, such as Claytronics (the ability to make programmable matter) that have struggled mightily to receive any funding. These are just a few examples of high-risk/high-reward research just at Carnegie Mellon, so you can imagine what high-risk/high-reward research that is being not (and not funded) at other top universities.

In closing, I want to again express appreciation for the support Congress has shown in restoring growth to federal research funding. In combination with the innovation funding provided in the *American Recovery and Reinvestment Act*, this support reflects the critical role that American higher education must play in restoring economic competitiveness and growth. The comments I have shared with you today reflect my belief that this full potential can only be realized by recognizing the critical importance of supporting high-risk/high-reward research. I believe that actions to increase support for those programs that do fund high-risk research and efforts

to infuse a focus on breakthrough research into existing program review processes can bring the full return we must realize from this renewed investment in American research.

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#### BIOGRAPHY FOR RICHARD D. MCCULLOUGH

Richard McCullough was appointed Vice President for Research at Carnegie Mellon University in July 2007. In this new senior leadership position, McCullough will nurture interdisciplinary research initiatives and oversee sponsored research, technology commercialization and a number of cross-college research centers. Prior to this position he served as the Dean of the Mellon College of Science at Carnegie Mellon University. He came to Carnegie Mellon in 1990 as an Assistant Professor and quickly rose through the tenure ranks, being promoted to Associate Professor in 1995 and Professor in 1998. In 1998 he assumed the role of Department Head of Chemistry. He was appointed Dean of the Mellon College of Science in 2001.

McCullough is internationally known as the world's expert in the area of printable electronics and is well known for his discovery of regioregular polythiophenes—a material that led to plastic solar cells and plastic transistors. His research focuses on the design and understanding of the structure-property relationships in conducting materials and nanoelectronics.

In addition to his position at Carnegie Mellon, McCullough is also the chief scientist and founder of Plextronics, Inc., the world leader in developing active layer technology for printed electronics devices, such as organic light-emitting diode displays, polymer solar cells and plastic circuitry. Since its inception in 2002, the Pittsburgh Company has grown to more than 70 employees and received numerous honors, among them being named the 2008 Going Green Top 100 Company and a *Wall Street Journal* Technology Award runner-up.

He was a postdoctoral fellow at Columbia University and holds a Ph.D. from the Johns Hopkins University and a B.S. from the University of Texas at Dallas.

Chairman LIPINSKI. Thank you, Dr. McCullough. Dr. Rubin.

#### STATEMENT OF DR. GERALD M. RUBIN, VICE PRESIDENT AND DIRECTOR, JANELIA FARM RESEARCH CAMPUS, HOWARD HUGHES MEDICAL INSTITUTE

Dr. RUBIN. Thank you for the opportunity to speak before you today. As a science philanthrope whose explicit goal is the discovery of new knowledge, the Howard Hughes Medical Institute, or HHMI, seeks to use its investment of intellectual and financial capital to see growth and change to foster fresh thinking.

HHMI's biomedical research philosophy can be summarized in three words: people, not projects. By appointing scientists as HHMI investigators rather than awarding research grants, the Institute provides long-term flexible funding that enables its researchers to pursue their scientific interests wherever they lead. These two flagship research programs, the HHMI Investigator Program, currently employs 346 researchers selected through rigorous national competitions who direct institute laboratories on the campuses of 72 universities and other research organizations throughout the United States.

HHMI scientists include mathematicians, physicists, engineers, physicians, chemists and classically trained molecular and cellular biologists.

The success of HHMI's people-not-projects philosophy can be seen in the high productivity and breakthrough insights generated by HHMI investigators. For example, HHMI investigators have been awarded Nobel Prizes in nine of the past 11 years, and 14 investigators have all received the Nobel Prize. Just earlier this week, HHMI investigator Jack Szostack was awarded the Nobel Prize in physiology or medicine, and HHMI researcher Thomas Steitz was awarded the Nobel Prize in chemistry.

With freedom and flexibility come high expectations for intellectual output. HHMI demands creativity and innovation. Investigators are expected to work at the frontiers of their chosen field, to ask fundamental questions, and to take risks.

Although the Institute already had the highly successful HHMI investigator program, the scientific leadership continue to explore new ways to support the research of some of this nation's most creative scientists. These discussions led to the establishment of HHMI's Janelia Farm Research Campus in Northern Virginia, which opened in 2006.

The blueprint for Janelia Farm grew out of an acknowledgment by HHMI leadership that while most biomedical problems are handled well in the university setting, there are some that are better addressed in a place where small groups of researchers with different skills can work together without the barriers typically encountered at a university.

I have described Janelia Farm more fully in my written testimony, but in the interest of time, I would like to conclude by giving my personal perspective on Federal support for innovative research.

The central question I have been asked to address is what is the best mechanism that Federal funding agencies can use to support high-risk, high-reward research? With regard to funding, my own personal belief, backed up by HHMI's nearly 30-year experiment, is that in the long run, high-reward research comes from focusing on people, not projects. In today's funding environment, researchers are compelled to define and advance the goals, methods, and likely outcomes of the research project in a detailed grant application. While this funding model is appropriate for some types of biomedical research, it has two major limitations. First, proposals for higher risk projects, even those that may have enormous impacts if successful, have traditionally fared poorly. Second, the ability to move quickly to take advantage of unforeseen targets of opportunity is fairly constrained. How can a scientist capitalize on a flash of insight if he or she must first write a grant proposal and then wait a year, even if the grant application is successful for the funding to test the idea? Federal funding agencies need to do a better job of providing research support under terms that permit rapid changes in research direction, encourage taking on challenging research problems, even if the chance of short-term success is low.

I think these changes could bring more innovation per dollar spent. In 2003 I was asked to join a task force convened by Dr. Elias Zerhouni, NIH Director at that time. The group was charged

with recommending new ways to find high-risk, high-impact research. The primary recommendation of our panel was to establish a new set of awards to researchers based on their track record of innovation. In fact, the journal *Science* covered our panel's recommendations in a news story headlined, "NIH to Award People, Not Projects." That headline nicely summed up our recommendations, but in practice, the NIH came up short in carrying out this initiative.

Take the NIH Director's Pioneer Awards, for example, which aimed specifically at stimulating highly innovative research and supporting promising new investigators. Our task force recommended that the NIH award 10 percent of its R01 grants, which would equate to roughly 700 grants per year, on a people-not-projects basis. In 2004, the first year the awards were made, the NIH selected only nine Pioneer Award recipients from among approximately 1,000 nominations.

It is somewhat more encouraging to see that this year the NIH has awarded a total of 115 grants for high-risk, high-reward research through its Pioneer Awards, New Innovator Awards, and the NIH Director's Transformative R01 Awards. The total number of these awards, however, still falls far short of our 2003 recommendation.

Even with these new awards, the NIH is still heavily weighted toward project-oriented research with 98 percent of grants going to projects. As I stated earlier, I strongly believe that giving money to scientists of exceptional and demonstrated creativity and allowing them to follow their instincts is a better way to promote innovation. In my opinion, even a modest shift in the Federal research portfolio, going from perhaps 98 percent to 90 percent project-oriented could make a big difference in producing innovative and potentially transformative research results.

I would like to end with a quotation from the Nobel Prize winner, Max Perutz, who directed the Medical Research Council Laboratory of Molecular Biology in England for more than 20 years. "Creativity in science, as in the arts, cannot be organized. It arises spontaneously from individual talent. Well-run laboratories can foster it, but hierarchical organization, inflexible, bureaucratic rules, and mounds of futile paperwork can kill it. Discoveries cannot be planned; they pop up, like Puck, in unexpected corners."

Thank you, Mr. Chairman.

[The prepared statement of Dr. Rubin follows:]

PREPARED STATEMENT OF GERALD M. RUBIN

Mr. Chairman and Members of the Committee:

Thank you for the opportunity to speak before you today. I am Gerald Rubin, a Vice President at the Howard Hughes Medical Institute (HHMI) and Director of the Janelia Farm Research Campus in Ashburn, Virginia. I am honored to testify before the committee as it begins to examine the mechanisms for funding high-risk, high-reward research, and the appropriate role of the Federal Government in supporting such research in the United States.

My testimony will cover three broad areas: HHMI's approach to biomedical research; HHMI's motivation for creating a new kind of research center at Janelia Farm; and a summary statement that reflects my perspective on how the Federal Government could improve its support of high-risk, high-reward research.

### **The Howard Hughes Medical Institute Invests in People, Not Projects**

Nearly 25 years ago, as the HHMI Trustees prepared to sell the Hughes Aircraft Company to General Motors Corp., in order to establish the first permanent endowment for the Howard Hughes Medical Institute, *The New York Times* issued an emphatic challenge to the leadership of the newly reorganized entity. In an editorial that was published on June 15, 1985, the newspaper urged the Institute to avoid the temptation to plug gaps in federal spending and instead to “be more venture-some and fund high-risk research, and by methods as different as possible from the Government’s.”

As a science philanthropy whose explicit goal is the discovery of new knowledge, HHMI seeks to use its investments of intellectual and financial capital to seed growth and change, to foster fresh thinking.

HHMI’s biomedical research philosophy can be summarized in three words: people, not projects. By appointing scientists as HHMI investigators—rather than awarding research grants—the Institute provides long-term, flexible funding that enables its researchers to pursue their scientific interests wherever they lead.

The Institute takes the “long view,” preferring to nurture the creativity and intellectual daring of scientists who are willing to set aside conventional wisdom or the “easy” question for a fundamental problem that may take many years to solve. Among the distinguishing characteristics of HHMI’s scientists are qualities such as creativity, a high tolerance for risk-taking, and a commitment to discovery, productivity, and perseverance.

HHMI’s unique research model is an imaginative and powerful alternative to project-based research support or funding biomedical research through grants. The Institute’s flagship research program, the HHMI Investigator Program, currently employs 346 researchers who direct Institute laboratories on the campuses of 72 universities and other research organizations throughout the United States. HHMI scientists represent a wide range of biomedical research disciplines—from chemistry, neuroscience, and bioinformatics to structural biology, immunology, and clinical genetics. They include mathematicians, physicists, engineers, physicians, chemists, and classically trained molecular and cellular biologists.

The success of HHMI’s “people, not projects” philosophy can be seen in the high productivity and breakthrough insights generated by HHMI investigators. In recent years, HHMI researchers have made many major research advances, including:

- Identifying a new drug that is now approved by the FDA to treat patients whose chronic myeloid leukemia failed to respond to standard treatment with Gleevec
- New microscopes and imaging techniques that let researchers visualize cells and proteins with unprecedented resolution
- A non-invasive test for genetic mutations associated with colon cancer
- Gene microarrays and “protein chips,” enabling researchers to simultaneously measure the function of thousands of genes or proteins.

HHMI investigators have been awarded Nobel Prizes in eight of the last 10 years, and 12 investigators overall have received the Nobel Prize. Currently, there are 131 HHMI investigators who are members of the National Academy of Sciences. Election to the Academy—one of the highest honors a scientist can receive—is based on distinguished and continuing achievement in original research. HHMI investigators presently compose about six percent of the Academy’s 2,100 current members (this does not include foreign associates).

Since the early 1990s, investigators have been selected through rigorous national competitions. The Institute solicits applications directly from scientists at medical schools and other research institutions in the United States, with the aim of identifying those who have the potential to make significant contributions to science. HHMI employs an open application process to ensure that it is selecting its researchers from a broad and deep pool of scientific talent.

After they have been selected, HHMI investigators continue to be based at their home institutions, typically leading a research group of 10–25 students, postdoctoral associates and technicians, but they become Institute employees and are supported by HHMI field staff throughout the country.

With freedom and flexibility come high expectations for intellectual output. HHMI demands creativity and innovation. Investigators are expected to work at the frontiers of their chosen field, to ask fundamental questions, and to take risks. HHMI prizes impact over publication volume in its merit-based renewal of investigator appointments and recognizes that some areas of research will proceed more slowly than others.

In reviewing its scientists, HHMI expects not only that its investigators be talented and productive scientists, but also that they demonstrate some combination of the following attributes to an extent that clearly distinguishes them from other highly competent researchers in their field:

- They identify and pursue significant biological questions in a rigorous and deep manner.
- They push their chosen research field into new areas of inquiry, being consistently at its forefront.
- They develop new tools and methods that enable creative experimental approaches to biological questions, bringing to bear, when necessary, concepts or techniques from other disciplines.
- They forge links between basic biology and medicine.
- They demonstrate great promise of future original and innovative contributions.

HHMI's annual research budget, though substantial, is dwarfed by the Nation's investment in research through the National Institutes of Health and the National Science Foundation. Yet in holding fast to a distinctive model for supporting scientific research, HHMI uniquely serves science, creating a culture of inquiry that encourages the free and unfettered pursuit of knowledge.

#### **Examples of HHMI's Approach to Science**

HHMI scientists work avidly and passionately toward tomorrow's discoveries. Sometimes inventing wholly new areas of study, HHMI researchers are pioneers in such areas as neuroscience, genomics, and computational biology. The examples below are just a few of many that illustrate HHMI's approach to science.

#### **Richard Axel and Linda Buck**

The olfactory mechanics that make possible the exquisite ability to discern smells from the most subtle to the blatant have been the subject of study for HHMI investigators Richard Axel and Linda B. Buck for much of their research careers. Axel and Buck, who joined HHMI in 1984 and 1994, respectively, were awarded the 2004 Nobel Prize in Physiology or Medicine for their discoveries of "odorant receptors and the organization of the olfactory system."

The process of smelling an odor begins with odorant receptors that are located on the surface of nerve cells inside the nose. Researchers now know that when an odorant receptor detects an odor molecule, it triggers a nerve signal that travels to a way station in the brain called the olfactory bulb. Signals from the olfactory bulb, in turn, travel to the brain's olfactory cortex. Information from the olfactory cortex is then sent to many regions of the brain, ultimately leading to the perceptions of odors and their emotional and physiological effects.

The trail to the Nobel began many years earlier as an attempt to understand how the brain creates an internal representation of the external sensory world. Little was known about the mechanics of smell before Axel and Buck published their seminal discovery of odorant receptors.

In 1991, Axel and Buck (who was working on her second postdoctoral fellowship in Axel's lab), were three years into their search for odorant receptors. Approaching the problem with her training in immunology, Buck had been trying to identify rearranged genes in the mammalian nervous system. She was intrigued by the possibility that gene rearrangement or gene conversion might be involved in the generation of a varied set of odorant receptors or regulate their expression, as with antigen receptors in the immune system. Buck became obsessed with finding the odorant receptors and stayed on in Axel's lab to look for them.

Buck and Axel, who is at Columbia University, initially adopted an "unbiased approach" with regard to the structure of odorant receptors, choosing to focus on two assumptions: that the receptor proteins would be selectively expressed by olfactory sensory neurons and, given the structural diversity of odorants, there would be a family of related, but varied, odorant receptors that would be encoded by a family of related genes.

Their efforts produced nothing at first. The tide turned when, using scattered evidence from other labs, Buck decided to narrow her search to G protein-coupled receptors (GPCRs), many of which were known to be involved in cell signaling. Making use of the recently developed gene amplification technology called PCR, or polymerase chain reaction, Buck decided to conduct an exhaustive search for GPCRs in the olfactory epithelium by taking a novel approach.

Further analysis of the PCR products narrowed the search to one candidate. Buck cloned this PCR product, sequenced five of the clones, and found precisely what she had been looking for. When Buck finally found the genes in 1991, she could not believe her search was over. Furthermore, none of the genes she found had ever been seen before. They were all different, but all related to each other.

### **Roderick MacKinnon**

Roderick MacKinnon of The Rockefeller University joined the Howard Hughes Medical Institute in 1997 as a self-taught structural biologist. Already an accomplished scientist, MacKinnon considered his HHMI appointment a special opportunity to take an entirely new research direction in order to further his work.

Prior to coming to Rockefeller, MacKinnon was a successful scientist at Harvard Medical School, where he ran a laboratory that studied ion channels, tiny doughnut-shaped pores that penetrate the membrane that surrounds living cells. They permit ions—charged atoms of potassium, sodium, chloride, and calcium—to flow across cell membranes, thereby generating electrical signals. Ion channels are fundamental to health and to the normal function of the human body; their impulses create the sparks of the brain and nervous system, allowing us to walk, talk, fall in love, and, for example, cast a fishing line with accuracy.

Building on decades of clever observations by their predecessors, MacKinnon and others had been inching toward a deeper understanding of how the pores performed their feats of exquisite discrimination among ions and responsiveness to minute changes in their environment—enabling the cell membrane to suddenly become permeable, but only to highly specific types of ions.

But though the genes behind the channel proteins had been cloned, which gave scientists new traction on the problem, channel aficionados were still struggling.

Trained as a physician, MacKinnon decided to teach himself the rudiments of x-ray crystallography because he wanted to find a way to solve a specific problem: defining the structure and mechanism of the channel that controls the flow of potassium into the cell. To devote himself to this pursuit, he moved his laboratory from Harvard to Rockefeller University, where he was named an HHMI investigator shortly after joining the faculty. His creativity, ability to approach his research from a new perspective, and single-minded pursuit of a significant scientific problem exemplify many of the attributes HHMI seeks in its investigators.

In April 1998, the journal *Science* published two elegant articles by MacKinnon. In the first article, he defined the “inverted teepee” structure of the potassium channel in a strain of bacteria and in the second he confirmed that the human potassium channel was structurally similar. MacKinnon continues to generate new insights that illuminate the structure and function of ion channels. These insights are critical to understanding new approaches for treating human diseases as varied as hypertension and epilepsy. Like many other HHMI investigators, MacKinnon has focused on fundamental biological questions that have significant implications for the understanding and treatment of human disease.

Five years after those *Science* articles were published, MacKinnon received the ultimate vindication of his out-of-the-box creativity and persistence in the face of high-risk: He shared the 2003 Nobel Prize for Chemistry with Johns Hopkins researcher Peter C. Agre who discovered water channels in cells.

### **Huda Zoghbi**

Using some of the most advanced techniques in genetics and cell biology, HHMI investigator Huda Zoghbi and her collaborators unraveled the genetic underpinnings of a number of devastating neurological disorders, including Rett syndrome and spinocerebellar ataxia type 1. Their discoveries may one day lead to better methods for treating these diseases and provide new ways of thinking about more common neurological disorders, including autism, mental retardation, and Parkinson’s disease.

Zoghbi’s interest in Rett syndrome began long before she established her own research laboratory at Baylor College of Medicine. While in the second year of medical residency, Zoghbi encountered a very puzzling patient. The girl had been a perfectly healthy child, playing and singing and otherwise acting like a typical toddler. Around the age of two, she stopped making eye contact, shied away from social interactions, ceased to communicate, and started obsessively wringing her hands. The girl made a huge impression on Zoghbi, who set out to determine what could have caused this sudden neurological deterioration.

Sixteen years after she saw that first patient, Zoghbi and her collaborators identified *MECP2*, the gene responsible for Rett syndrome. Children afflicted with this rare neurodevelopmental disorder develop normally for about six to 18 months and



then start to regress, losing the ability to speak, walk, and use their hands to hold, lift, or even point at things. *MECP2*, it turns out, encodes a protein whose activity is critical for the normal functioning of mature neurons in the brain; it is produced when nerve cells are forming connections as a child interacts with the world. The disease occurs primarily in females, because boys who inherit an inactive form of *MECP2*—which lies on the X chromosome—usually die shortly after birth. Girls survive because, with two X chromosomes, they stand a good chance of inheriting a healthy copy of the gene.

For the first 15 years of her career, Zoghbi spent 20 percent of her time seeing patients with childhood neurological disorders. Driven by a desire to improve the clinical outcome of her patients, she became convinced that more basic research was needed.

Zoghbi and her colleagues have also identified the mutation responsible for spinocerebellar ataxia type 1 (SCA1), a neurodegenerative disorder that renders its victims unable to walk or talk clearly, or eventually to even swallow or breathe. The culprit is a sort of genetic stutter that increases the size of the SCA1 gene. The normal gene harbors a stretch of nucleotides in which the sequence CAG is repeated about 30 times. In individuals with the disease, the tract expands to include 40 to 100 iterations. As a result, the product of the mutant gene—a protein called ataxin-1—grows large and sticky, forming clumps throughout the cell. These ataxin-1 aggregates overwhelm the molecular machinery that cells use to recycle damaged proteins and eventually disable the neurons involved in controlling movement. Using mice and flies that produce the mutant protein, Zoghbi is now searching for compounds that enhance the clearance of ataxin-1 tangles. Such drugs could slow the progression of the disease or prevent it altogether.

#### **Creating a New Scientific Culture at Janelia Farm**

Although the Institute already had the highly successful HHMI Investigator Program, the scientific leadership continued to explore new ways to support the research of some of this nation's most creative scientists. The genesis of the Janelia Farm Research Campus occurred in 1999 in a series of informal conversations at HHMI about ways to expand the boundaries of biomedical research.

The blueprint for Janelia Farm grew out of an acknowledgment by HHMI leadership that while most biomedical problems are handled well in a university setting, there are some that are better addressed in a place where small groups of researchers with different skills can work together without the barriers typically encountered at a university. Development of new tools to facilitate biological discovery, for example, can require diverse expertise. But at universities, scientists from different fields are often compartmentalized, and demands placed on researchers by their departments may restrict collaboration outside those walls. To avoid these constraints, HHMI decided to bring together researchers from disparate disciplines in a free-standing campus.

Scientists at the Janelia Farm Research Campus, which opened in 2006, are working in two synergistic areas: discovering the basic rules and mechanisms of the brain's information-processing system, and developing biological and computational technologies for creating and interpreting biological images. These two areas were chosen because they are truly large, unsolved problems in biology and because there is a very good chance that they will not be solved by one laboratory or by scientists in one discipline.

In planning Janelia Farm, HHMI carefully studied the structure and scientific culture of other important research models at both academic and for-profit biomedical laboratories, including the Medical Research Council Laboratory of Molecular Biology (MRC LMB) in England and the former AT&T Bell Laboratories in the United States. The MRC LMB and AT&T's Bell Labs are generally considered to have been the most successful research institutions in biology and electronics, respectively.

Though the MRC LMB and Bell Labs were different in many ways, they did have several things in common. Both institutions kept research groups small, and principal investigators worked at the lab bench. The single sponsor provided all funding—applying for outside grants was not allowed—and good support services and infrastructure were in place. Notably, both institutions evaluated their own people rather than rely on expert opinions from outsiders. HHMI decided to incorporate these core concepts into Janelia Farm.

Researchers at Janelia Farm are freed from most of the administrative, grant writing, and teaching duties that consume time at a university. Traditional academic environments are suitable for a large proportion of research projects, but they can be too conservative and restrictive, stifling the kinds of creative, long-term projects that can lead to true breakthroughs. This is true, in part, because the reli-

ance on external funding sources forces scientists to define their research programs in advance when they apply for grants.

By setting the course of the research plan up front, scientists are restricted in their ability to pursue questions and opportunities that arise during their studies. The bulk of the scientific community is limited to projects that can be funded by peer-review committees, which tend to be very conservative. These grants have to be reviewed every three to five years, making it very difficult for people to take on high-risk, high-reward projects.

It is important to remember that we think of Janelia Farm as an experiment. We don't have all the answers. We have a working hypothesis. We formulated the hypothesis by studying previously successful research institutions and analyzing what made them successful. We may not get it exactly right at first, but we'll adapt. We will revise the hypothesis, like any good scientist would do.

Ultimately, we believe the success of our approach might be measured by a "deletion test." Twenty years from now, would the scientific landscape look substantially different if Janelia Farm's contributions were to be deleted? Of course, since Janelia Farm is only three years old, we do not know the answer yet.

### **Summary Statement and Perspective on Federal Support for Scientific Research**

The central question that I have been asked to address is what is the best mechanism that federal funding agencies can use to support high-risk, high-reward research. I have outlined HHMI's approaches, which focus on people, not projects. It is worth noting here that although there are numerous organizational cultures in which scientific research is conducted, from HHMI's perspective, no single culture has emerged as "the best."

But with regard to funding, my own personal bias, backed up by HHMI's nearly 30-year "experiment," is that in the long run, high-reward research comes from focusing on people, not projects. And I believe that federal funding agencies, such as the National Institutes of Health and the National Science Foundation, should allocate a greater portion of their research portfolios to supporting truly innovative scientists (identified as such by their track record) and not make funding decisions based on the projects those researchers propose to study.

In today's funding environment, researchers are compelled to define in advance the goals, methods and likely outcomes of their research project in a detailed grant application. While this "funding model" may be appropriate for some types of biomedical research, it has two major limitations. First, proposals for higher-risk projects—even those that may have enormous impact if successful—have traditionally fared poorly. Second, the ability to move quickly to take advantage of unforeseen targets of opportunity is severely constrained.

As I like to say, how can a scientist capitalize on a flash of insight that occurs at 3 A.M., if he or she must first write a grant proposal and then wait a year—even if their grant application is successful—for funding to test the idea? Federal funding agencies need to do a better job of providing research support under terms that permit rapid changes in research direction and encourage taking on challenging research problems, even if the chance of short-term success is low.

I think these changes will bring "more innovation per dollar spent" without adding more money into the research budgets of these agencies. In 2003, I was asked to join a task force convened by Dr. Elias Zerhouni, NIH Director at that time. The group was charged with recommending new ways to fund high-risk, high-impact research. Our panel made three main recommendations, but I will focus on just one of those: establishing a new set of awards to researchers based on their track record. In fact, the journal *Science* covered our panel's recommendations in a news story headlined, "NIH to Award People, Not Projects." That headline nicely summed up our recommendations. But in practice, the NIH came up short in carrying out this initiative.

Take the NIH Director's Pioneer Awards, for example, which were aimed specifically at stimulating highly innovative research and supporting promising new investigators. Our task force recommended that the NIH award 10 percent of its R01 grants—which would equate to roughly 700 grants—on a "people, not project" basis. In 2004, the first year the awards were made, the NIH selected only nine Pioneer Award recipients from among approximately 1,000 nominations.

It is somewhat more encouraging to see that this year NIH has awarded a total of 115 grants for high-risk, high-reward research through its Pioneer Awards, New Innovator Awards, and the NIH Director's Transformative R01 Awards. The total number of these types of awards, however, still falls far short of our 2003 recommendations.

Even with these new awards, the NIH research budget is still heavily weighted toward project-oriented research, with 98 percent of grants going to projects. As I stated earlier, I strongly believe that giving money to scientists of exceptional and demonstrated creativity is a better way to promote innovation. In my opinion, even a modest shift in the federal research funding portfolio—going from 98 percent to 90 percent project-oriented—could make a big difference in producing innovative and potentially transformative research results.

I would like to end with a quotation from the Nobel Prize winner Max Perutz, who directed the Medical Research Council Laboratory of Molecular Biology in England for more than 20 years: “. . . (C)reativity in science, as in the arts, cannot be organized. It arises spontaneously from individual talent. Well-run laboratories can foster it, but hierarchical organization, inflexible, bureaucratic rules, and mounds of futile paperwork can kill it. Discoveries cannot be planned; they pop up, like Puck, in unexpected corners.”

Thank you, Mr. Chairman. I would be pleased to answer any questions that the Committee might have.

#### BIOGRAPHY FOR GERALD M. RUBIN

A Vice President of the Howard Hughes Medical Institute (HHMI) since 2000, Gerald M. Rubin was named in 2003 the first Director of HHMI's Janelia Farm Research Campus. At Janelia, Rubin directs scientific programs designed to speed the development and application of new tools for transforming the study of biology and medicine. A 760,000 square-foot biomedical research complex in Ashburn, Virginia, which opened in the summer of 2006, Janelia will eventually accommodate a research staff more than 300. It houses laboratories and provides short-term housing for visiting researchers, along with a conference center.

Rubin served as HHMI's Vice President for biomedical research from 2000 to 2002, when he was appointed Vice President and Director of Planning for Janelia Farm. Before moving to HHMI headquarters, Rubin was an HHMI investigator at the University of California, Berkeley, where he was the John D. MacArthur Professor of Genetics in the Department of Molecular and Cellular Biology. An internationally recognized geneticist, Rubin led the publicly funded effort to sequence the fruit fly *Drosophila melanogaster* genome. In addition, his laboratory has worked to determine the function of fruit fly genes that have homology to human genes and, more recently, to develop genetic tools to help probe the structure and function of the fruit fly brain.

Rubin received his Bachelor's degree from the Massachusetts Institute of Technology and earned his Ph.D. in molecular biology from the University of Cambridge in England. He did postdoctoral work at the Stanford University School of Medicine before joining Harvard Medical School in 1977 as an assistant professor of biological chemistry. In 1980 he joined the Carnegie Institution of Washington as a staff member in the department of embryology, and three years later moved to UC-Berkeley. Rubin is a member of the National Academy of Sciences, the Institute of Medicine, and the American Academy of Arts and Sciences.

#### DISCUSSION

Chairman LIPINSKI. Thank you, Dr. Rubin. I thank all our witnesses for their testimony, and at this point, we are going to begin our first round of questions, and the Chair will recognize Mr. Tonko.

Mr. TONKO. Thank you, Mr. Chair. Dr. Lane, you are probably as familiar as anyone in this room with the annual budget process and the challenges that accompany that. In these very difficult budget times, what advice can you give us as to defending the dedication of a pot of money in those tough times when there is a high probability of failure?

Dr. LANE. Mr. Tonko, I thank you and appreciate the question. The *ARISE* report didn't answer that specific question, so I will give you my best take on it based in part on the discussions of the Academy Committee but also my own experience.

I think there is a problem with terminology here. My sense is that even the highest-risk ideas proposed to our agencies by researchers who have strong reputations and track records are not likely to fail. It is quite possible that the particular goal that was put forward in the research proposal might not pan out. But in the meantime, young people are educated, often technologies are developed if this is an experimental process.

So I think even though the *ARISE* report recommended in terms of the merit-review system focusing differently on high-risk, high-reward research from the normal grant program, I think in both cases one should look at the potential for transforming the field, a major breakthrough, and at the same time, other outcomes, other aspects of the research that are almost assured to come out. I don't think we have to tell the public and your colleagues here on the Hill that we are going to spend more money on research that has a high probability of failing. I think the research that we invest in here is likely to seed on many dimensions, even though it might not actually reach the conclusion that the researcher hopes for or create the device that the researcher wants.

An example might be the LIGO project, Laser Interferometer Gravitational Wave Observatory at Caltech. Significant funding hasn't yet seen a gravitational wave as far as I know, but out of that have come spin-off companies, new technologies, lots of educated young people. So I think we need to talk about what is a better way to articulate what we are doing here. I think there is a good story to be told, and maybe we haven't worked hard enough on doing that.

Mr. TONKO. Might there be a way for some of these projects to prove themselves before they become a target? Is there—

Dr. LANE. I think the issue is—one of the issues is one of funding. Sometimes it is rather expensive to try this idea. You can't really do it on the cheap. So we proposed seed funding, for example, to allow an investigator with a wild idea, let us say, to explore, enough data, enough experience that he can convince the peer reviewers that this has a higher probability of success. I think that is what you can do with seed funding. That is what universities could do more of if they had the resources to devote in that way.

Mr. TONKO. Dr. McCullough, you have talked of the failure of some of these proposals or at least the process that relates to these proposals at NSF. But it is colleagues from the research universities that are oftentimes the reviewers serving on these panels. Is there any sort of input or encouragement that could be provided to these colleagues because they are scoring these given situations? And you know, again, they are impacted by these tough times, economic times. Is there some sort of encouragement or training that can be provided to the colleagues?

Dr. MCCULLOUGH. That is one of the reasons I think it would be good to have a set-aside and direct instructions from the agencies that we want to fund things that are a big more high-risk, high-reward if you will, proposals with sort of specific guidelines. I think the way it happens now is high-risk, high-reward research is often sort of a check box that means that the proposal should not be funded because it has not been proven yet. There is no proof of concept for those proposals. And so I think having sort of a mechanism

by which the panels, chair people, are guided, outside reviewers are guided in a special program and not leaving it up to the unfortunate program officers who are really struggling to try to find the proposals that are very, very highly rated already.

Mr. TONKO. So in a sense it is a cultural change that we need to incorporate into the review process? Is it just perhaps requiring that a certain bid of high-risk projects be looked, at maybe taking the top threshold of those projects?

Dr. MCCULLOUGH. In my opinion, my personal opinion, yes, because if you look at the testimony that I have written, you will see examples of five or six projects that we have at the University that are just unbelievably spectacular . . . that we really struggle to try to fund them internally, through foundations, providing those seed funds. And as I was saying earlier in my spoken testimony, I think providing funds as you suggested where, as Dr. Lane suggested, we can provide seed funding so people can get the projects off the ground. And then if they don't show proof of concept, then they can't maybe get follow-on funds.

So it is less of a risk as you were indicating if we had some sort of limit in the initial funds, but then the ability to open a gate for them to bring these things and deliver on the high-reward transformative nature of the research programs.

Mr. TONKO. Mr. Chair, if I might just ask one more question. The relationship with venture capitalists—I am often told that venture capitalists will walk away from some of these high-risk situations, but is there a way, is there a threshold of involvement from the public sector that might be incorporated with a venture capitalist funding that might maximize what we could do here?

Dr. MCCULLOUGH. I think that one of the things we are doing at Carnegie Mellon—I am directing something called an innovation ecosystem, and what we are trying to do is create funds within the University so that once these projects get to the research phase, that we can find funding mechanisms to delivering—now I am using proof of concept in a different way now—but proof of concept for commercial situations. And so to create pre-commercial research and take it to the proof of concept for commercial reasons, there are funds like that that are not available, that we are trying to raise those privately, work with venture capital funds, angels, et cetera, to try to keep things from approaching the “Valley of Death” and these concepts so that we can have higher probability for these research ideas to end up creating jobs, and help us to be leaders in technology innovation. I think that is an absolutely critical thing because as a person who has a start-up company, I have lived through this, and these are the kinds of funds that we are trying to do within our University and work with other agencies and foundations to try to create.

Mr. TONKO. Do any of the other three of our witnesses have anything they want to say on that partnership that can be had with venture capitalists in the high-risk area? Yes, Dr. Collins.

Dr. COLLINS. Well, let me make two points, Mr. Tonko. First, with respect to the notion of failure of a project, I would build on Dr. Lane's remarks, and actually something that you said, in that it would be a mistake to focus on an individual project and think

about it solely as succeeding or failing, and that's the reason why the United States invests in the basic research enterprise.

For me, the United States invests in a basic research enterprise in order to sustain what is an innovation ecosystem as far as the United States is concerned. Some projects will succeed, some projects will fail. But the point is that you have individuals constantly trying to think about where the next steps are with respect to the whole process of discovery, and I think a great example of this we saw recently with the Netflix competition, which had to do with a better algorithm for picking movies as far as the Netflix Corporation was concerned. Interesting thing about the analysis of that that appeared in *The New York Times* was that there was a prize for the winner, and they were of course quite happy about that. But when the second-place individual was interviewed, that company, they said we gained as much by participating in this competition, even though we did not win, because the very fact of trying to think through these problems affected the culture of our institution.

It seems to me that is what you can do with Federal research dollars, is you can give our institutions the freedom to take those kinds of risks. That is the reason we do it. We fund institutions in order that we can do this kind of work. So your phrase was exactly right, it is a cultural issue. It is a cultural issue within our universities and within our research institutes, and it is a cultural issue within a funding agency like the National Science Foundation. You are absolutely right again in that what we do is we direct the panels, we direct the program officers, we direct our senior managers to discuss with the faculty members, with the reviewers, what it means to think about transformative research and risky research, and we ask them to take that into consideration when they look at these proposals coming in. It seems to me that is where you put your finger on the issue and fostering the combination of the culture and institutions that are open to these kinds of issues.

Mr. TONKO. Thank you.

Dr. LANE. May I just comment, Dr. Tonko, to Dr. Collins. I think it is important I think to make the point that high-risk, high-payoff research is not always just basic research. It can certainly be research that has some toward directed hoped-for goal, very much on the practical side. I mean, the transistor came because there was an effort at Bell Labs to find a replacement for a vacuum tube triode. Well, the rest is history. So there is great opportunity, I think, for agencies like the NSF to work with the private sector, largely through the partnership with the universities as it does with its Engineering Research Centers, Industry/University Cooperative Research Centers, the SBIR grants that are made directly to industry. One might have a look at those mechanisms, some of which have longer-term time horizons on them to see whether there is not an opportunity to do more to address the high-risk, high-payoff goals that we are talking about today through some of those mechanisms.

Mr. TONKO. Thank you.

Chairman LIPINSKI. Thank you, Mr. Tonko, for your questions. I know it did go on a while, but I think those were all very interesting questions, and we are getting a little off the—a little far

afield, to some extent, talking about bringing private funding, but I think it is a critical question and because this is something that I am interested in a broader sense of how we best do these public/private partnerships essentially in research and development. I would like to hear what was said there also.

So with that, I will now recognize Dr. Ehlers for five minutes or however long. He always has plenty of good questions, so I will let him go ahead.

Mr. EHLERS. I better be a little careful. My dad was a preacher, so we could be here quite a while.

Chairman LIPINSKI. Okay, not that long.

Mr. EHLERS. Not that long. Okay. I was interested in the comments about people, not projects, and I would like to pursue that with each of you. I happen personally to think that is in general a good idea because I recall in my days in university life, it wasn't too hard to pick out the really outstanding and bright young people. But I also learned that they don't necessarily make the best investigators. There is a big difference between thinking of an idea and carrying it out. And so I guess I am asking for comments from all of you because I think the concept is good. I wonder how it can be executed well in practice.

I will just give you one example that could be a problem. There are others. A junior researcher just starting out doesn't have much of a track record. He may appear to be very bright, but you are really not sure. How do you evaluate that? I happen to know one who is really quite bright and has done very well but had a terrible time getting his first NSF grant because he wasn't well-known, even to the extent that an idea he had which he had discussed with another researcher at another university, that person picked up on it and submitted the proposal and got it funded, whereas the young person who thought of the idea submitted the proposal which didn't get funded.

So I would just like an open discussion from all of you, and Dr. Rubin, I will let you start because you mentioned the people, not projects first. But how does this work out in practice?

Dr. RUBIN. I think you raised a couple of good issues here, so I would say at the beginning, I don't think you could convert the entire research enterprise to a people not projects, and I certainly wouldn't suggest that. As I said in my verbal remarks, I am talking of going from two percent to ten percent of the grants awarded, recognizing people based on their track record for innovation and success.

So this does create a problem for people who are just starting, but our experience at the Howard Hughes Medical Institute, we have no trouble identifying people who had an independent, say, faculty position at a university for four or five years. At that point it is pretty clear who the innovative, creative individuals are, and we believe that past performance is a better indicator of future success than any written proposal will ever be, and that people who have an ability and desire to be innovative and are willing to go in uncharted territory, that is a personality trait which carries over. And you can identify such people, and at least in our experience, we do much better by saying to someone, we are going to give you generous funding for your research for five years. You better

do something with it because in five years we are going to look over what you did, and if we don't like it, we are not going to give you any more money. But we are not going to tell you what to do with the money. We are betting on you as an individual, and we are going to win or lose our bet.

I would say that our experiment that we have done could make a very good case that betting on those—placing your bets in that way gives you a higher rate of success than placing your bets by reading a stack of research proposals, because in research proposals, you often reward the people who are very articulate and can write very good research proposals, rather than the people who have the good ideas or are going to be able to execute if you give them the money.

So it is just an alternative, and I think a portfolio, a diversified way of funding research is always better than putting all your eggs in one basket. I just think that we are out of balance now in the way we fund research in this country.

Mr. EHLERS. Let me just get back to you on the one issue there. I don't think this would work at the NSF which does in fact review stacks of proposals because they really—I don't think either the program officers or the panel of reviewers generally don't know the applicant that well. I take it that you, at HHMI, is that right?

Dr. RUBIN. Yeah.

Mr. EHLERS. Really get to know these people well and bet on them because you have investigated them and worked with them or talked to them enough that you are quite convinced that they really are above the pack.

Dr. RUBIN. Well, I think the way they send in applications initially to be appointed, and—they are reviewed in a way not dissimilar from typical peer review. I would say—let me give you an example just from my NSF colleagues.

NSF has an award every year called the Waterman Award which is supposed to give money to the most creative, best scientists under the age of 35 or something like this. And I am sure they have 20 or 30 very good nominees for that. I would think the NSF—a good use of the NSF budget would be to say these 20 people who are nominated for this award are all outstanding. Let us just give them each \$10 million to fund their—whatever they want to do in science for the next five years instead of reviewing a lot of little grants. I think they would get more output in research dollars. I mean, I am making these numbers off the top, but they already have a mechanism in place to do that. It would require very little extra person power to implement a policy like that.

Mr. EHLERS. Are you basically suggesting the McArthur approach?

Dr. RUBIN. Something along that ilk, I think to a certain amount. I mean, this approach has limitations, but I do think that the most creative innovative ideas come from when you give innovative, creative people some money and you don't try to tell them what they should do about it because by definition, an innovative, transformative idea—if someone can write down in their proposal and submit it to you, it is not an innovative, transformative idea, almost by definition, by my definition of those terms.



Mr. EHLERS. Okay. I want to get back to that in a minute but first Dr. McCullough, you have been smiling broadly.

Dr. MCCULLOUGH. Well, I mean, there is certainly room for these sorts of programs where HHMI and Waterman Award winners, who are the rarified group of people at the top who get funded, probably are amazing. We have a McArthur genius at our place, and he is just out of this world, you know, an idea all of the time. But you know, for those of us who went to community college and you know, peaked later in life and you know, some of the examples of colleagues at my university, one in particular I think of, who came from Poland and after he became a professor really, although it is not necessarily the rule, but is now the most-cited chemist in the world and he is often mentioned as a Nobel Prize candidate.

And I know that Dr. Rubin is not suggesting that we change the whole program to bet on horses. In the DoD world they often do this in terms of funding. They will find someone who they like and they can work with, and they will bet on that professor and they will fund that professor. And they get often locked into the system. I think that there are pitfalls with that approach, and one has to be worried about funding people.

You know, there is also the aspect of beyond the proposal. You know, you meet people at conferences, they hear you talk, there is access, and especially for young researchers, I think it is sometimes very difficult to see who is going to be the greatest innovator. So I think that there is a role. I am not disagreeing with Dr. Rubin, but I also believe that it is important for those who peak later in life to—it doesn't mean that they are not going to be innovative. I do agree with the *ARISE* report that the most creative ideas often come from brand-new faculty members who are really thinking out of the box. So I think that is an area of concern.

Mr. EHLERS. Yeah, I am still waiting for myself to reach my peak. I have to admire the creativity of some of my colleagues, particularly when they have done something wrong and they are explaining it to the press. I think that is a different sort of creativity.

Dr. COLLINS, would you like to offer a few comments from the NSF perspective?

Dr. COLLINS. I would. Thank you, Congressman. A couple of thoughts. I would agree with you that there is a difference between just being able to think up an idea and be able to carry out that idea. It suggests that at times you do want to be taking chances as far as individuals are concerned, and I will come back to your point also about young investigators, and we can tie those two things together across.

For example, the program managers at the National Science Foundation, where, as I indicated in my oral testimony and opened up in the written testimony, these individuals not only manage awards but they work and mentor post-docs, they facilitate connections, they engage in this outreach. They really do work with the individuals. We summarize that under this single word of "stewardship." It is the notion that these program officers are not only just processing paper, but they really are deeply engaged in the scientific process itself in an ongoing conversation. And this is the heart of the enterprise, and it is what we need to continue to foster in terms of something that is being challenged right now with the

terrific workload that is coming as far as NSF is concerned, and I might mention NIH as well.

So you do want to evaluate individuals and indeed that is what happens when we do take chances on young people. So for example, the career program which you know and funds on average about 450 individuals a year—this year, because of the ARRA funding that was able to go up to 700 individuals. And those then enter into a relationship with our program officers, where they work with these young investigators as well as they work with senior investigators in terms of stewardship.

Now, the senior investigators also, and young investigators can also call out their program officer and say I do have this great idea. I have this great idea at 3:00 in the morning, and we now have the eager mechanism where that individual can get up to \$300,000 for a couple years to begin to pursue that idea and get the preliminary data that is needed in order to move that idea along to a proposal.

Furthermore, program officers once again have the prerogative for a successful program or for a program that is moving along. They can call out that investigator and discuss a creativity extension where there is minimal application needed in order to continue that funding for another couple of years or three years.

So there are a variety of mechanisms that are in the hands of the program officers, and it goes back to my point earlier on having a culture of an institution that is willing to make these kinds of engagements and these kinds of investments over time, so that you can use past performance as an indicator, but you can also work with young people in order to take the risks that are needed to begin to build up the infrastructure within the country. I was taken by this this week with the Nobel Prizes. NSF molecular and cellular biology had funded two of the individuals who received Nobel Prizes this week, and they funded them early in their careers. It means that the program officers 20 years ago, 25 years ago, were prescient enough to look at that group of applicants and say, this is someone to fund. Twenty-five, thirty years later, you see the fruits of that labor and you also see it as a result of our relationship between the funding agencies in the United States, where the NSF got these individual started, and in one instance is still funding that individual. But the individual receives support from other kinds of funding agencies.

So it speaks to the need for diversity of institutions, not only diversity of approaches, that give you really breadth of support for an innovation enterprise as far as the country is concerned.

Mr. EHLERS. Okay. I think we will have to cut this off here. I have used too much time already. We may get back to you later, Neal.

Just a comment. I forget who mentioned accelerator funds. I think that is a great idea, Dr. McCullough. When you find something, and I have been in that situation where you are doing some research and you find something really great, and you want to pursue that, it is nice to have a mechanism to do that.

The other comment I want to make is I don't like the term transformative research. Obviously high-risk doesn't go too well, it doesn't survive the Proxmire Golden Fleece award requirements where, you know, you talk to the public about doing high-risk re-

search, that doesn't really mesh. But transformative doesn't mean too much to the public, either. I might suggest you come up with something like NASCAR research because the point of NASCAR is you take some very high risks in hopes of winning. That is exactly what you are doing here. So you are taking the NASCAR approach. Let us fund the stuff that we think is really going to payoff, and we know there is some risk attached to it, just as there is to driving a car at 200 miles an hour. But the rewards can be great. And the public can clearly identify with that one.

With that, I yield back. Thank you, Mr. Chairman.

Chairman LIPINSKI. Thank you. Dr. Ehlers, I am thinking about that NASCAR award and thinking about if somehow we could have crashes and other ways to somehow interest the general public while this research is being done, then maybe that could work. Colorful cars and other things like that. We have to keep people interested in the race while it is going on, not just the final.

Mr. EHLERS. Perhaps we can ask them to contribute to down payments for the first trip to a dark hole. That is a pretty safe bet actually.

Chairman LIPINSKI. The Chair now recognizes himself for five minutes, and I have a lot of questions. I am going to try to limit it a little bit, but if we can try to limit our responses a little might be good. I am glad that Dr. Ehlers went down the road of the—Dr. Rubin had talked about the people, not projects, and we talked a little bit about that. I know a couple years ago Secretary Chu, at that time at Berkeley Lab, that was one thing that he really—a couple years ago I had lunch with him out there, and the one thing that he impressed upon me that that is something that he really thought was a good way to go.

I sit here as a former political scientist, maybe I still am a political scientist, but I wonder, does this work better—no one may have any strong opinions on this or thoughts, but I sit here thinking, does this work better for some disciplines than others in some different scientific areas, disciplines? Would this not work as well, do you not see as much possibilities or does this question not really pertain? Does anyone have any thoughts on that? Dr. Lane.

Dr. LANE. I don't have any wisdom on it, I have thought. It connects a little bit with my earlier observation that I think high-risk—you already said in your opening statement, Mr. Chairman—there is still a remaining question about what is this that we are talking about, high-risk, high-reward research, and I wanted to make the point that it doesn't just have to be basic research, it can also be research that is being done with some particular outcome in mind. I think that if we go field by field, and my colleagues can help me in many of these fields, but my sense is this question is apt in all fields that I know anything about for a couple of reasons. One reason is that breakthroughs often occur as total surprises so the research that was being done maybe strikes us as somewhat routine, somewhat dull, whoever is funding it, and suddenly there is a funny blip on the screen, there is a number that is unexpected, there is some surprise that comes out of the research. And the ability to pursue those surprises, the flexibility, the freedom of the investigator to pursue such a surprise, it is extremely important. First all, it has to be a kind of person who is so curious and so driv-

en that he or she wants to do that, but then the environment, the funding, the organization, the institution has to be willing to go with that. Bell Labs is a really good example, so you know, not too long ago they had seven Nobel Prizes to their alumni. It must be ten or a dozen or something like that now. There was an environment in which funding was available. People were being bet on, and when they found surprises, they were able to pursue them, including the observation of background blackbody radiation from the origin, from the first big bang in the universe.

So I would think that, in any field that one could think of, there is an opportunity for these kind of breakthroughs. And therefore, the question is at least apt whether one is properly addressing the issue of high-risk, high-reward research.

Chairman LIPINSKI. Anyone else? Dr. Rubin.

Dr. RUBIN. Well, I agree that in any field you need a range of research projects. So in, say, biology, one of our most successful projects recently was the Human Genome Project, which was a very well-defined project with a clear goal and could be measured. I wouldn't call that—it was only high-reward, it wasn't high-risk. On the other hand, you have other projects which are higher risk, or more unpredictable is a word I would prefer. But I just want to emphasize what Dr. Lane said about flexibility in being able to alter the goals that you are working on to pursue a new—take advantage of some unexpected result. I don't think that, the way the funding agencies work now. A lot of it is just the problem of peer review, are very good at rewarding people for not pursuing what they originally said they were going to do but to take advantage of something much more interesting or important that came up as an unexpected result within that.

So anything that we can do to change the culture to support individuals having more freedom, I think would be a good thing.

Chairman LIPINSKI. Dr. Collins.

Dr. COLLINS. Mr. Chairman, I would agree with Dr. Lane. I don't think this is a discipline-limited issue, and in fact, I wouldn't even limit it to the sciences. I think there are high-risk activities in the humanities and the arts as well where individuals take chances and they take risk, and sometimes it works and sometimes it doesn't. Breakthroughs indeed do come as a result of these surprises that are present. They are inherent in the research enterprise itself. After all, that is why we call it research and we don't call it demonstration. It is something that we are involved in as a process of discovery.

Chairman LIPINSKI. One thing I would go back to. I am not sure who had made the comment. I don't know if it was Dr. McCullough. My experience, and again we are talking social sciences, political sciences, was you don't propose anything that you don't already know the answer for. And so that sort of goes counter to the research that is sort of wide. It is still possible to discover other things, but that narrows it down much, much more. And I tend to think we are not going to stay here all afternoon and the next few weeks talking here, so I am not going to open this up, but the culture issues are certainly also very important. But please don't start talking about that because we will be here forever. But was there something else you wanted to—

Dr. COLLINS. I would agree with you. I think the culture is important, and that is why on this issue of surprise and breakthroughs, program officers have the flexibility to work with the investigator at that point. That is why we give grants, we don't do contracts. And it is that flexibility that is inherent in the process as far as NSF is concerned.

Chairman LIPINSKI. One thing, shifting gears here, my colleague, Bob Inglis, and I had sponsored the H Prize. We had legislation for the H Prize. We eventually worked, got that into the energy bill that we passed a couple years ago. The idea of prizes, I just wanted to throw that out there. That is one way to avoid the political issues associated with high rate of failures. You know, you put out a prize worth something specific. The H Prize was for advances in use of hydrogen for transportation. But you also have examples of prizes promoting basic research. For example \$1 million in Millennium Prizes offered by Clay Mathematics Institute.

So what extent do you think that prizes can motivate, transform into research? Dr. McCullough.

Dr. MCCULLOUGH. I think that they do play a major role. If I can point to the DARPA Urban Grand Challenge as a situation where they asked for autonomous vehicles to drive within an urban situation. That was a challenge, and universities came together to accomplish this. Of course, I bring it up because we won at Carnegie Mellon, and there was \$1 million prize. Carnegie Mellon and private groups and companies invested in this, and much technology came out of these things to create these autonomous vehicles. There is nothing like a good competition to get people's juices flowing and actually create something and bring teams of people together that maybe normally don't work.

So I think it is a very interesting idea, and I think it certainly plays a role like many other funding mechanisms, but I think it is a very interesting one.

Chairman LIPINSKI. Anyone else?

Dr. LANE. I would just add to that, I completely agree that the prizes can be enormously stimulating and have the advantages you just described, but you sort of have to know what the goal is so you can decide who won, and if it is a high-reward research that is going to show its worth in 20 years or 15 years, then it is a somewhat different category. So I think there are several ways to stimulate people to be thinking, to be taking risks, with the possibility of great payoffs. But there are many dimensions to that issue, and a prize certainly would be a very important one.

Chairman LIPINSKI. Thank you. The Chair will now recognize—Mr. Carnahan, do you have questions? Mr. Carnahan for five minutes.

Mr. CARNAHAN. Thank you, Mr. Chairman. And again, I thank the panel. I apologize I had to come in late. I was in some other meetings, but I had a chance to look at some of the written testimony and wanted to particularly ask Dr. Collins. You cited an article by Gary Anthes who stated, "The kind of pure research that led to the invention of the transistor and the Internet has steadily declined as companies bow to the pressure for quarterly and annual results." Well, during this year's energy and water appropriations, I, along with several other colleagues, passed appropriations to

fund Secretary Chu's request for energy innovation hubs. Three were funded out of the eight requested. These hubs as you know are modeled after the research labs, involved in the Manhattan Project Labs, Lincoln Labs at MIT and AT&T/Bell Labs that developed the transistor. Each hub envisioned would embrace within these topical areas the goals of both understanding and use without erecting barriers between basic and applied research. It will seek advances in highly promising areas of energy, science, and technology and will result in many solutions being deployed into the marketplace.

Today's hearing is focused on suggestions moving forward for funding high-risk, high-reward research. Would you qualify these energy innovation hubs? It is models that other agencies should employ? And are these good models for government agencies to fund but not necessarily institutions like NSF? Dr. Collins?

Dr. COLLINS. Continuing on from really the last conversation and the last question, the real key, it seems to me as far as innovation is concerned, and especially within the structure of the Federal agencies is to have a diversity of different kinds of approaches across the different agencies. In fact, to go back to the point about competition, that in and of itself can be affected. But even within an institution, to have different ways in which one would go about this whole process of discovery, whether you are using individual applications, whether you are looking at groups, whether you are using centers at some times, the Science and Technology Centers, for example, as far as the NSF is concerned.

The thing you have to be careful about is this intersection between basic and applied. You have to know what you're going for, especially if you are going to use something like a prize, and the danger here in terms of basic research is to have a metric that is too short. So basic research sometimes takes quite a bit of time. Manhattan Project can be a pretty good example for some kinds of things. It is a pretty straightforward engineering solution that you're looking for. But if the path forward were perfectly clear, we wouldn't call it basic research. We wouldn't need to have that time that's needed in order to discover the fundamental issues that are at work in order to then come up with the application. So it's really this mix of things that is needed both within and in between institutions in order to stay ahead.

Mr. CARNAHAN. And let me ask Dr. Lane if you would comment as well.

Dr. LANE. Thank you very much, Mr. Carnahan, for the question. The first thing I should say is this particular issue was not addressed in the *ARISE* report, so I don't want to answer with the thought that I am reflecting on—however Steve Chu, or Secretary Chu, was a member of our Committee, so he probably had it in his mind at that time.

Just adding to what Dr. Collins said about diversity, the Department of Energy of course has a rich experience in not only funding university research, high-quality university research, but also laboratories, national laboratories. And the advantage of a laboratory, whether it is one of these new hubs or the existing national labs, is you have a cadre of talented people there who can fairly quickly, if needed, work together in different ways to address a major na-

tional need, like energy, for example. And I think—I don't know what Secretary Chu has specifically in mind here, but they look a little bit like little Bell Laboratories, having that ability to focus on the quality of people, qualify an innovative idea or move rapidly, not be judged too much on short-term timelines and such matters. And so that is a very thoughtful concept. Then, it seems to me, it is appropriate for the Department of Energy. It is not the way academic research works in universities. It is not so easy to quickly put together large teams of researchers around this single goal. So I think the diversity issue Dr. Collins spoke to is the right way to think about it, and I personally am very pleased to hear your support for Secretary Chu's ideas. But it was not something we addressed in the *ARISE* report.

Mr. CARNAHAN. Any others? Comments?

Dr. MCCULLOUGH. I would just give you one ancillary effect of these innovation hubs which is really very positive. Having these very large programs, what happens is groups of people start teaming together across universities and companies and national labs and forming groups that would have never be formed in any other situation, and often there is a great benefit for these teams to be formed and trying to chase after this sort of money that's around the innovation hubs. But there are great things that come out of that because people who would not normally come together find each other and start to collaborate and find other sources of going after funds.

So these kinds of programs and the diversity of these programs are really important to get people's attention, to bring them together. So I think there's a great effect beyond what you will see just out of the program. You will see other groups that will be formed that will not be funded, and great things will come from them as well.

Mr. CARNAHAN. Thank you, Doctor. Thank you very much, Mr. Chairman.

Chairman LIPINSKI. Thank you, Mr. Carnahan. And before we close, I recognize Dr. Ehlers for closing comments.

Mr. EHLERS. Thank you, Mr. Chairman. I really want to thank you, Mr. Chairman, for an excellent panel, good cross-section of people to deal with this topic. And I have learned a great deal here, so I thank you for coming and thank you for your comments and your answers.

One other comment, when I talked about the NASCAR award, I was not suggesting, although maybe it had seemed that way to you—one thing I have become very sensitive to as a scientist in Congress and that is the scientific community should be very careful about how they say things, and I recall the time I had to dash to the floor because one of my colleagues got up and offered an amendment to cut the budget of the National Science Foundation because they were going to fund gain theory research and ATMs. And that is what was in the bill. That is what came up. And I dashed down there just to point out that gain theory was not what they thought it was. It was a very important part of theoretical physics and also the ATM that they were ridiculing, my colleagues said, the banks use ATMs. Let them pay for the research. And I said, I am sorry but ATM stands for a-synchronous transfer mode,

and we need some research on that so you can make the internet better. So NASCAR is not a bad idea. I am not really going to go to bat for it or publish it but my point is simply transformative sounds like gobbledygook. High-risk does pass through a Senator Proxmire test. So see if you can come up with a better term. And I won't patent the NASCAR returns. So thank you very much.

Chairman LIPINSKI. Thank you, Dr. Ehlers. I think it is just the high-reward part that we could work on some other names for it. I want to thank the witnesses for testifying today, and certainly this was a very interesting topic, not one that I think has a really wide appeal here. But it is critically important, and how we best do this. We know how to keep the United States at the forefront of technology. We have to be doing this research and have to be doing this research that is whatever you want to call it, that we get the high rewards. I think that sounds very critical. And as we move into early next year, this Subcommittee will be working on writing the NSF reauthorization. This is something that we will be looking closely at, and as the Full Committee works on America COMPETES through next year also, this will certainly be a part of what we are working on doing.

So again, I want to thank all the witnesses for their testimony. The record will remain open for two weeks for additional statements from the Members and for answers to any follow-up questions the Committee may ask of the witnesses. With that, the witnesses are excused, and the hearing is now adjourned.

[Whereupon, at 2:29 p.m., the Subcommittee was adjourned.]



## Appendix 1:

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ANSWERS TO POST-HEARING QUESTIONS

## ANSWERS TO POST-HEARING QUESTIONS

*Responses by Neal F. Lane, Malcolm Gillis University Professor and Senior Fellow,  
James A. Baker III Institute for Public Policy, Rice University*

**Questions submitted by Representative Vernon J. Ehlers**

*Q1. Do you have any recommendations on how to modify the peer review process—without changing all the things about it that currently work well—that would help reveal which investigators are truly attempting high-reward research? In other words, is there a better way for grant committees to “get to know” investigators?*

*A1.* This is a critically important question and I offer the following observations in response, based largely on the deliberations and recommendations of the Academy’s *ARISE* Committee. My comments focus on three primary elements of the process: the quality of outside reviewers; the criteria agencies use to evaluate potentially transformative research proposals; and the resources made available to agencies’ professional program staff. Each of these factors has a decisive impact of agencies’ ability to recognize and provide appropriate support for worthy applicants pursuing potentially transformative research.

**Quality of Outside Reviewers**

Strong reviewers are a lynchpin of a successful, high-integrity peer review system. The best possible reviewers are attracted to participate if they perceive the process to be well-managed.

The peer review systems operated by the National Science Foundation and the National Institutes of Health have long been considered to be the gold standard of competitive research award systems. However, as noted in the *ARISE* report, the rapid increase in applications to these agencies has placed serious strains on their peer review systems. As the workloads of reviewers, program officers, and staff grew, NIH’s own analysis documented an erosion of quality resulting from a loss of continuity in panel membership and rapid turnover of program officers and reviewers. Similarly, more than one-third of NSF reviewers reported “great” or “somewhat” decreased attention to each proposal.

A high-quality, well-organized review process attracts gifted reviewers. Potential reviewers will be discouraged from participating if they have reason to believe that their time will be wasted—whether from personal experience or based on the experiences of colleagues.

We recommended the following steps to strengthen the application and review processes:

- Require recipients of multiple grants from an agency to serve as reviewers.
- Achieve greater continuity in reviewers.
- Establish interdisciplinary review panels to consider high-risk research proposals across programs and fields.
- Consider alternative ways to select and mentor reviewers.
- Consider dividing applications of more senior researchers from new investigators and form separate review panels with separate quotas.

**Evaluation Criteria**

The *ARISE* Committee suggested steps for promoting the prospects of investigators pursuing high-risk, high-reward research, and also noted a number of ways in which funding agencies sometimes inadvertently discourage such research. Recognizing the inherent uncertainties and additional time required for such research, the Committee offered the following recommendations for adjustments to peer review systems specifically to foster high-risk, high-reward research:

- Applications should be relatively short and focused on the qualifications of the researcher, an explanation of the potentially transformative nature of the research, and an explanation of why the researcher believes the proposed approach could succeed.
- The proposal and the review process should place a premium on innovation and reviewers should be charged to identify new ideas, innovation, and creativity. Require applicants to address the following question about their proposed research: “If this works, what long-term scientific difference will it make?” Evaluate proposals based on this criterion.

- Agencies should not reject proposals solely on the grounds that the proposed work is “overly ambitious.”
- Fast-track seed money to evaluate a novel idea should be made available.
- Agencies should be open to providing longer funding periods for those proposals that require it.
- Recognize in grant-reporting requirements the value of fortuitous findings not related to the main objective of the research proposal and give program administrators the flexibility and expectation to provide extra resources or time to research unexpected but promising developments.
- For grant renewals or new grants on the same topic, restrict the number of submitted publications and require a self-assessment of each cited publication’s impact.
- Evaluate renewals for first awards for high-risk, high-reward research on the basis of project execution and potential scientific impact, not on deliverables. Resist fine-grain assessments of whether a project “worked”; expect some hypotheses to fail.

#### **Support for Professional Staff**

The *ARISE* Committee focused particular attention on the indispensable role of program officers in creating and maintaining the vitality and productivity of the research enterprise. Program officers manage millions of taxpayer dollars; their careers and opportunities for participation and leadership in their professional communities must be strengthened. The entire research system will greatly benefit if program officers are given greater opportunities to exercise leadership within the professional communities they fund and for whom they are responsible.

If agencies and departments improve the professional opportunities of their research program officers, several benefits will follow. Program leadership will be strengthened, and career satisfaction will be improved. New ideas will be injected into agency and community deliberations. Researchers and program managers will be challenged in creative, timely, and innovative ways. Mutual understanding and communication will be strengthened. Counterproductive misperceptions will be identified more quickly. The return on investment of taxpayer dollars will be enhanced.

Just as the program officers need to stay current on the latest developments in science and engineering research, the research community needs to know and respect these professionals, who have such large responsibilities for the quality of U.S. science and engineering.

The *ARISE* Committee recommended the following steps to strengthen the system:

- Administrative budgets should keep pace with research budgets.
- Program officers should be leaders not only within their agencies but within their external scientific communities as well.
- Program officers should be encouraged to attend professional meetings and to visit institutions and laboratories funded by programs for which they are responsible, and agencies should make the resources available for them to do so.
- Many university faculty members serve at NSF as temporary program officers, or “rotators,” while on leave from their university. They provide essential service and leadership for NSF’s research programs. This practice should be encouraged and program funds should be allocated for this purpose at other agencies as well.

Again, I deeply appreciate the opportunity to bring these issues to the attention of the Committee.

## ANSWERS TO POST-HEARING QUESTIONS

Responses by James P. Collins, Assistant Director, Directorate for Biological Sciences, National Science Foundation

**Questions submitted by Representative Vernon J. Ehlers**

*Q1. Do you have any recommendations on how to modify the peer review process—without changing all the things about it that currently work well—that would help reveal which investigators are truly attempting high-reward research? In other words, is there a better way for grant committees to “get to know” investigators?*

*A1.* In Dr. Collins’ written testimony submitted for the record, there is a section on “New approaches for identifying potentially transformative research” that addresses these questions.

Briefly, NSF is experimenting with novel mechanisms for developing, reviewing, and funding exploratory and especially creative research. All are new ways to foster NSF’s process of discovery and thus “reveal which investigators are truly attempting high-reward research.”

About 18 months ago Malcolm Gladwell argued in an article in *The New Yorker* that ideas are easy to come by; implementing them is hard. Ideas, Gladwell argued, are not precious, but everywhere. He concluded, therefore, “maybe the extraordinary process that we thought necessary for invention—genius, obsession, serendipity, epiphany—wasn’t necessary at all.” The trick, he felt, was getting together a group of thoughtful, creative people all thinking about how to solve a problem: (“In the Air;” [http://www.newyorker.com/reporting/2008/05/12/080512fa\\_fact\\_gladwell/?yrail](http://www.newyorker.com/reporting/2008/05/12/080512fa_fact_gladwell/?yrail)).

NSF’s is using three methods to take advantage of this line of reasoning.

- The “Sandpit” is an experiment in real time, interactive peer review to explore novel solutions to existing problems or identify new areas of research. The Directorate for Biological Sciences, with participation and support from the Directorates for Math and Physical Sciences, Engineering, Social, Behavioral and Economic Sciences, and Computer and Information Sciences and Engineering, sponsored its first sandpit in the area of synthetic biology in conjunction with the United Kingdom’s Engineering and Physical Sciences Research Council (EPSRC) in April, 2009. This sandpit produced five interdisciplinary, multi-investigator projects with support from NSF and EPSRC.
- The Directorates for Biological Sciences, Engineering, and Social, Behavioral and Economic Sciences also funded an EAGER proposal that focuses on developing a “prediction market” for synthetic biology. A prediction market is a social networking method used to predict the most likely outcome of an event like a presidential election or next quarter’s sales for a business. The principal investigator for this award will use the method to assess where the most creative research investments can be made to advance the area of synthetic biology.
- Synthesis Centers promote the process of collecting and connecting disparate data, concepts, or theories to generate new knowledge or understanding. Beyond its necessity for innovation in basic science, synthesis increasingly contributes to novel and effective solutions for pressing problems, and to the emergence of new ideas or fields of inquiry that would not otherwise exist. Biology Directorate-funded Synthesis Centers in conjunction with other NSF Directorates and federal agencies emphasize interdisciplinary research and education in critical areas of the biological, computer, and social sciences. Current centers include: the National Center for Ecological Analysis and Synthesis, the National Evolutionary Synthesis Center, the National Institute for Mathematical and Biological Sciences, and the iPlant Collaborative. These centers advance our understanding by interdisciplinary activities as well as by “getting together a group of thoughtful, creative people all thinking about how to solve a problem.”

Modern cyberinfrastructure can greatly facilitate these ways of identifying the likely places for a commitment to supporting high-risk/high-reward/transformative research. The social networking manifest in models like crowd sourcing or prediction markets is based on arguments that there is great value in a collective effort focused on uncovering the best sort of research to fund—the so-called “wisdom of the crowd” argument. However, as noted elsewhere in Dr. Collins’ written testimony, NSF’s merit review system is at its root a wisdom-of-the-crowd model. The new ex-

tensions of this fundamental model rely on modern computer and information sciences to integrate tens, hundreds, or even thousands of researchers focused on solving a common problem.

These sorts of social networking models are potentially, in an analogy with Clayton Christian's *The Innovator's Dilemma*, a "disruptive technology" when it comes to discovery related to research and education. In relation to the question posed by the Subcommittee, these mechanisms are ways "to modify the peer review process—without changing all the things about it that currently work well."

## ANSWERS TO POST-HEARING QUESTIONS

*Responses by Richard D. McCullough, Vice President for Research; Professor of Chemistry, Carnegie Mellon University*

**Questions submitted by Representative Vernon J. Ehlers**

*Q1. Do you have any recommendations on how to modify the peer review process—without changing all the things about it that currently work well—that would help reveal which investigators are truly attempting high-reward research? In other words, is there a better way for grant committees to “get to know” investigators?*

*A1.* The key to increasing program manager and review committee engagement with researchers is to expand the tools that facilitate quality interactions. These tools include early investigator awards that expand the exposure of young faculty members and encourage more expansive and higher-risk research activities early in careers. In addition, the expansion of seed and challenge grants that could provide \$100,000 for early stage exploratory projects in high-risk, high-reward areas could provide a platform for both stimulating faculty engagement and expanding access to program managers. These seed grants would be designed to advance concepts to a stage where they may be applicable for traditional program competitions. Further, the funding could be stage-gated with \$100K/year for two years then \$200K/year for two years if the science becomes innovation.

## ANSWERS TO POST-HEARING QUESTIONS

*Responses by Dr. Gerald M. Rubin, Vice President and Director, Janelia Farm Research Campus, Howard Hughes Medical Institute*

**Questions submitted by Representative Vernon J. Ehlers**

*Q1. Do you have any recommendations on how to modify the peer review process—without changing all the things about it that currently work well—that would help reveal which investigators are truly attempting high-reward research? In other words, is there a better way for grant committees to “get to know” investigators?*

*A1.* One way to modify the peer review process is to change the criteria for reviewing grants to place more emphasis on creativity and originality and that would be more tolerant of the chance of failure, especially in cases where the reward for success is high. As many types of research are needed to advance knowledge, it might be best to have a separate category of grants that specifically emphasized so-called high-risk/high-reward research. The NIH is trying this approach, but, in my opinion, at too small a scale. As part of the review process for the Pioneer Award, NIH does conduct personal interviews of the top applicants.





## Appendix 2:

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ADDITIONAL MATERIAL FOR THE RECORD

STATEMENT OF PROFESSOR FRANKLIN M. ORR, JR.,  
STANFORD UNIVERSITY, REPRESENTING  
THE DAVID AND LUCILE PACKARD FOUNDATION

The David and Lucile Packard Foundation appreciates the invitation to share our views on high-risk research with the Committee. This response to questions from the Committee staff is presented by Franklin M. Orr, Jr., trustee of the Foundation from 1999 to 2008, who has long been involved with the two programs discussed below.

**1. Why does the Packard Foundation fund basic research? How does, or should the Foundation's role differ from that of the Federal Government?**

David Packard was co-founder of the Hewlett-Packard Company. The success of the Hewlett-Packard Company has been built on technology, derived in large measure from research and development in university laboratories. Because the endowment of the David and Lucile Packard Foundation would not have been possible without the success of HP and the research performed by university-educated engineers and scientists employed by this company, the Foundation has a long-standing interest in strengthening both university-based research and graduate education. In 1988, the Foundation established the Packard Fellowships for Science and Engineering to allow the Nation's most promising young professors to pursue their science and engineering research with few funding restrictions and limited paperwork requirements. The goal of the program is to encourage talented young faculty to build research groups that make career-long contributions by training talented graduate students and by conducting research that will be the basis for future scientific and economic progress. The Foundation also supports the Monterey Bay Aquarium Research Institute (MBARI). These two programs receive support of approximately \$50 million per year.

MBARI's mission is to conduct advanced research and education in ocean science and technology, and to do so through the development of better instruments, systems, and methods for scientific research in the deep waters of the ocean. MBARI emphasizes the peer relationship between engineers and scientists as a basic principle of its operation. MBARI has been a leader in oceanographic science and in the development of remotely-operated vehicles (ROVs), autonomous underwater vehicles (AUVs), ocean observatories, and *in situ* chemical and biological sensors for research. Both the science and engineering have been high-risk in many respects. Some developments pioneered at MBARI have taken five or more years to field, but have enabled ground-breaking discoveries on important issues such as ocean acidification, nitrogen uptake, harmful algal blooms, and invasive species. Once proven, these new tools have been transferred from MBARI to other oceanographic institutions, NOAA labs, and commercial vendors such as Satlantic and Battelle. Such developments would be difficult, if not impossible, to undertake with traditional, short-term government grants, and would also be unattractive to industries concerned with near-term profits.

The Packard Fellowships are designed to identify 16–20 of the most promising early career faculty members, chosen from a field nominated by 50 of the Nation's leading research universities. The intent of the fellowships is not high-risk research directly. Instead, the aim is to provide some very talented and creative young scientists and engineers with the opportunity to pursue their research interests with substantial unrestricted research funds that can be used flexibly over the course of the five-year fellowship. The result of that arrangement has been a rich flow of innovative research across a very wide range of disciplines. It is often high-risk, in the opinion of the Packard Fellows, in the sense that they report that the questions they investigated often could not have been supported, at least at the outset, because these problems or approaches were not yet at the stage where they could attract federal research support. Many Fellows also report that they feel that the flexibility of the fellowship confers on them an obligation to use the funds in ways that open new areas for their research groups or develop research areas, experimental approaches, or theoretical attacks that require effort over a period that is long compared to shorter federal funding cycles. In other words, they feel that they should take advantage of the fellowship funds to do something that would be difficult or impossible to do in the context of the traditional funding mechanisms.

**2. What is the Foundation's model for funding high-risk, high-payoff research? What are the benefits of this model? What are the challenges? Is this a model that could or should be duplicated by federal funding agencies or federally funded research and development centers such as**

### **the Department of Energy National Labs or the National Institutes of Health?**

In contrast to the current federal system of reviewing research proposals, MBARI reviews the researchers. Scientists and engineers who are highly productive, show exceptional creativity, remain relevant to the Institute's strategic plan, and show good citizenship receive steady support. Formation of interdisciplinary teams that can focus on a topic for an extended period is encouraged. Over its 20 years of existence, MBARI has attracted scientists and engineers who dare to push the limits of what is possible in a culture that rewards risk taking.

In accord with David Packard's wishes, MBARI limits the fraction of its total funding that comes from federal sources. The intent of this approach is to preserve the independence of the Institute and its ability to investigate problems that are not constrained by programmatic objectives and the inevitable increases and decreases in support typical of changeable funding cycles. In addition, this approach complements the federal portfolio by allowing sustained effort on research challenges that may require extended periods of development. This is a model that cannot be transferred, as stated, to the federal research establishment, although the autonomy of the Defense Advanced Research Projects Agency may approach a similar degree of independence in the short run.

The intent of the Packard Fellowship Program is to identify and provide support for unusually creative young faculty researchers early in their careers (nominees must be in the first three years of their academic careers). The Foundation seeks to support innovative individual research that involves the Fellows, their students, and junior colleagues, rather than extensions or components of large-scale, ongoing research programs.

Fellows are selected in two stages. Each of 50 invited research universities nominates two candidates. The competition within the universities is tough enough to produce good candidates. An independent advisory panel, whose research expertise spans a wide range of scientific and engineering specialties, then reviews the applications of the 100 candidates and the recommendations they solicit from mentors and leaders in their fields. The Foundation awards up to 20 fellowships each year based on the recommendations of the review panel. The number of nominations was selected to be large enough to provide a very good pool of candidates, but small enough to make the review process manageable and the probability of success reasonable.

Again, the Foundation's process focuses on selecting individuals, rather than their research projects. Fellows are encouraged to take risks and to change their research plans in the course of their fellowships if they judge that it makes sense to do so. Given their talents and creativity, of course, it is hard to prove that the Fellows would not have pursued high-risk research absent the Packard Fellowship. Similarly, their rapid advancement and acknowledged leadership across a wide range of scientific and engineering disciplines cannot be attributed to the fellowship alone. As noted above, the Fellows report that this approach gives them a highly valued opportunity to pursue high-risk research. In effect, this approach replaces a review process based on a detailed review of specific research proposals with a process that attempts to evaluate the creative potential of the investigators. Either process inevitably has its own challenges and imperfections.

It is clear that there are good candidates in the pool of nominees each year who do not receive fellowships and that there are good young faculty at institutions not on the list of institutions invited to nominate. The amount of support available from the Foundation is small (\$14–17.5 million per year) compared to the size of the national research enterprise. NSF CAREER awards do provide support for young investigators, and there are other young investigator awards that do so as well. There is likely to be room for additional support that could be applied productively. In addition, a program that provided support for high-risk research with potential for significant breakthroughs would augment these young-investigator programs. It should be noted that a federal program that focuses on high-risk research will require modification of the traditional peer review process. Reviewers will have to be conditioned to accept the risk inherent in an attempt to support research that is risky but has high-potential rewards if it is successful. In the current review process, creative ideas for research may be rejected for funding because reviewers are not convinced (by detailed proof of concept experimental results, for example) that the investigators can achieve the goal. One way to deal with that problem would be to create a tiered program in which shorter-term projects with lower funding levels (e.g., at the amount required to support a post-doctoral fellow for long enough to do the proof of concept experiment) are considered in addition to longer-term

projects for which the pathway is established even though there are many hurdles to overcome.

- 3. Given the total funding for basic science and engineering research from all sources, is the ratio of funding for high-risk research appropriate? If the ratio were to be increased as recommended in several recent reports, what should be the responsibility of the Federal Government in achieving that increase, and how does that responsibility differ from that of private sector research organizations and funding sources such as the Packard Foundation?**

These questions are important ones that reflect choices about the portfolio of research programs and funding levels. The Foundation has not attempted to study these questions and can offer no detailed informed judgment concerning the appropriate balance. It seems reasonable to argue, however, that there should be some fraction of research funding that should support new ideas that involve risk. Our commitment to independent funding of excellent research that can attack areas that are long-term and therefore involve risk is reflected in the international reputations earned by MBARI and by a large majority of the past and present Packard Fellows. It is our intent to continue to fund those efforts as the endowment of the Foundation allows (the decline in the value of the Foundation's endowment this last year led to a reduction from 20 to 16 fellowships this year). We believe that our support of these scientists and engineers has demonstrated the value of supporting those who undertake high-risk research. Given the magnitude of the federal research enterprise compared to that funded by foundations, however, significant expansion of funding of high-risk endeavors will have to come from growth in federal support for such research.

- 4. Do you have any specific recommendations for how federal science agencies such as the National Science Foundation could increase their support for high-risk research? In particular, what are the pros and cons of establishing targeted programs or set-asides for high-risk research versus changing how proposals are reviewed and selected across a federal science agency? What are the biggest challenges or risks associated with each of these approaches? What metrics should be used to evaluate the success of any approach to funding high-risk research?**

David Packard believed in finding excellent scientists and engineers, providing resources, and then trusting the investigators to use the funding wisely. This is a thread connecting the Foundation's support of unmanned research vehicles and the science they can carry out in the ocean with our support of early career faculty members. By affording creative scientists the independence to pursue their curiosity, with an understanding that "failures" are part of the research enterprise, creative results can follow. To the extent that programs focused on high-risk studies can achieve a similar end, they serve the Nation's interest. At the same time, it should be noted that not all of the research needed by the Nation is or should be high-risk in nature, and funding should also reflect the reality that the contributions of science and engineering to human well-being arise from solid, reliable understanding of nature, won only in part through breakthrough studies.

The notion of risk implies that some fraction of the work that is done will fail to produce the originally intended result, though it may have unanticipated value that becomes apparent later. Any program that emphasizes high-risk work must tolerate redirection as the work proceeds and the possibility that some work will be unsuccessful. Because the time scales for high-risk research are likely to be long, any attempt to measure effectiveness will have to reflect that time scale. Simple metrics based on counts of papers and published and citations of them will likely not be useful in the short-term. It seems likely that the real value of such programs will be much more apparent in retrospect, when those areas that have developed rich and productive sets of ideas and results can be identified more readily. Thus, taking the long view of measuring program effectiveness will be essential.

The Foundation is grateful for the opportunity to provide its views to the Committee.