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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%, with 13% coming from private sources and 5% 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 funding 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% over the last four years at Carnegie Mellon and NIH hit rates have dropped 18% 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 2 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 of 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 collaborate 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 **1** $66,000 EAGER grant from the NSF and **0** NIH Director's Pioneer Awards, **0** New Innovator Awards, and **0** 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 2 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% 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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