

**University of Notre Dame**

**"Knowledge for What?"**

**Obstacles & Opportunities for Reinvigorating the  
Social Purpose of the University"**

**Meeting**

**June 4, 2019**

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**MR. DESCH:** Good morning and welcome to the full day of our workshop "Knowledge for What: Obstacles and Opportunities for Reinvigorating the Social Purpose of the University." Again, I'm Mike Desch. I'm a professor of political science at the University of Notre Dame in South Bend and also the director of the Notre Dame International Security Center. You may wonder why an international security center is sponsoring a conference, so I'm going to say a few words about that in a moment or two to put that in context.

Before I do so, I wanted to say some words of thanks. First of all, to our hosts, our the staff of the Notre Dame Center in Washington, D.C. Mora Polacheli (phonetic) who I don't see. She was here last night, and I guess she and Isabel Kabases (phonetic) is the real brains behind the operation, were up late reconfiguring the room. But Isabel and Mora, thank you very much for your gracious hospitality here. Also Kathie Olsen and Mora McClear (phonetic) are colleagues of mine from Notre Dame, but they are primarily based here in Washington D.C. and do among many other things federal relations for the University of Notre Dame. And Kathy and Mora were instrumental in helping me come up with some of the folks on the various panels and also identifying people to invite for the workshop, so it's great to work with Kathy again and also Boris Newt (phonetic) of Notre Dame. It's a great first kick off of what I hope will be many joint operations down the road.

Finally, our benefactor, you may see The Carnegie Institution across the street. We have a representative of Andrew Carnegie in the person of Steve

Del Rosso who has supported the work that I've done in the area of Bridging the Gap for a number of years. I think Steve thinks too many groups at this point. But you may notice that there are a number of other people on the program and in the audience like James Steinberg, Jim Goldgeier, Frank Gavin -- let's see. Anybody else? Oh, Bruce Jentleson -- who have also been the beneficiaries of the largess of the Carnegie Corporation. Steve, if I could ask you to take a minute or two and tell us about Carnegie's equity in Bridging the Gap and then I'll say how this particular event fits into that.

**MR. DEL ROSSO:** Do you want me to stop there?

**MR. DESCH:** Yes, please.

**MR. DEL ROSSO:** Well, in Mike Desch form as I was walking in, he said, are you going to give some remarks, right? And then I noticed I had an email at 11:31 last night after my bedtime. Saying a few words tomorrow? So sure, I'll just vamp. I'm so successive to the fact that there are a number of people -- you mention Steve Van Evera as well, right -- who are here that have been part of our Bridging the Gap initiative that's been going on for more than 15 years and every one of them occurred through the shop corner clichés that I've gotten a response like this so I'll spare you talking about Andrew Carnegie's belief in the power of ideas and how we believe that big policies are formed by good ideas and we look to the academy. I'll spare you all that. I'll even spare you the great Stanley Hoffmann's line about how foundations are the dumbwaiters in the kitchens of power in academic salons. I'll skip all that. Okay.

**UNIDENTIFIED MALE:** Thank you.

**MR. DEL ROSSO:** So, yeah, buddy. So yes, we've had this interesting initiative I could say for a decade and a half under a governmental foreign service officer who stumbled into a PhD later in life. I saw how the sausage was made in my misspent youth in government, and through both my doctoral work and my time in the foundation world, I vicariously lived the lives of many an academic and heard all the tales of grow and challenge within the guild as Mike calls it. So we've been trying to chip away at some of the disincentives within the academy in hiring, tenure, and promotion. Bruce can regale you during his presentation later today on that.

And we have at this point about ten grants working at different aspects of this issue, pushing against various doors. And you know the vein of existence for a foundation person is measuring impact and influence, and we're still woefully underdeveloped in that area in philanthropy I can say that, and you can quote me on that. But in this field, in particular, the changes are incremental but they're discernible. But I think that just anecdotally in the last ten years, I've seen a real change and I'd like to think that that had something to do with our modest investments. There's a cohort of younger academics now who not only are interested in policy-relevant work, however we define that, but are feeling empowered because they have other colleagues in those schools and faculty who support it. So the trend line is moving in the right direction. And though I can't definitively convince the bean counters that do some of the evaluative work in

philanthropy that there's really progress being made. To their satisfaction, I wouldn't be doing this if I didn't think it was doing something positive. So let me just say I've been to a number of these meetings over the year. I'm glad to see this mix of stalwarts of the program and a lot of new faces. And I have no doubt that I'll learn new things, and we'll keep moving the ball forward in true Notre Dame fashion. Thank you.

**MR. DESCH:** Thanks very much, Steve. I would like to think that in my own little way I've satisfied clearer quantitative metrics of progress in terms of dollars spent on, why don't you run that by your program evaluation.

**MR. DEL ROSSO:** I will. I'm going to reclaim 30 seconds because I want to give Mike -- I want to praise his book the *Cult of the Irrelevant* but also to say that if you do read it, the title may not reflect the actual text which is an incredible history of the ups and downs of the effective scholars on the policy realm. And you're quite frank about those individuals, that read the book, who had a pernicious effect on the policy realm; be careful what you wish for. So the *Cult of the Irrelevant* -- and I think I've said that Steve Van Evera was actually the person who coined that phrase. In fact, I'm pretty sure it was you. Anyway, we'll give you credit. He's going to give you a commission check from the royalties.

**MR. DESCH:** Twenty-five cents for every copy of the book sold.

**MR. DEL ROSSO:** But the fact that the subtitle should have been something like "The history of the positive and negative effects of scholars" something like that, so don't be dissuaded by the title because I would say it's a

catchy title -- thanks to Steve -- but it's really an amazing history of the slings and arrows of how scholars have worked and not worked with policy well.

**MR. DESCH:** Thank you very much, Steve. You know when you publish a book, the editors at the press have a lot of say to the title of the book, but I'm holding out hope that when the movie version comes out -- it most surely will -  
- that I think may be to (crosstalk 0:07:46.0)

**MR. DEL ROSSO:** Is David Letterman going to play you?

**MR. DESCH:** So I just want to very quickly give you a lay down of the intellectual flow for the rest of the day. And I've been accused -- I've organized a number of these workshops over the years and Steve's colleague Barry Posen at MIT accuses me of organizing workshops the way the Japanese organized the Bataan Death March; there's a lot packed into one day and guilty as charged. But the idea of this workshop was to take some of the particular issues that those of us in the Carnegie Bridging the Gap portfolio of projects have been wrestling with primarily in the area of political science and foreign policy and international security affairs. And think about it more broadly in terms of larger issues that affect the relationship between the university and not with the government but society more generally. And here we're taking a page out of the book of Jim Goldgeier and Bruce Jentleson's work with the Bridging the Gap project at American University, and one of the things that they did was sponsor a series of meetings with higher education leaders that produced some very fruitful discussions and ideas and so we're trying to build on that.

The first panel is on the challenges and opportunities of interdisciplinary and non-disciplinary programs in the modern research university, and I'll let Jim introduce the panel in just a moment. But we have, as you'll see, a very broad range of people from disciplinary backgrounds who are all in one way or another working at the interdisciplinary (inaudible) on the ivory tower side.

The second panel is what we're calling the "Demand Side" for Interdisciplinary and Non-disciplinary Research. Here we're looking at both consumers but also -- we're pleased to add at literally the 11th hour Evan Heit from the National Science Foundation, and NSF is sort of not only a consumer but also a producer of this sort of research as well.

And so the third panel is going to focus on what Steve Van Evera and I regard as a good case study of the importance of interdisciplinary research in the contemporary world which is a whole question of whether the pace of science has outrun our ethical and practical frameworks for thinking about it in a sensible policy way, and this will be over lunch.

And then at 2:00, we're going to have a briefing from my colleague Bruce Jentleson, who in addition to being a stalwart with Jim in the Bridging the Gap program which though, undoubtedly, he'll tell you a little bit more about it over the course of the day. He was also the chair for Duke University's provost of a Blue-Ribbon Task Force that spent a number of years thinking very hard about tenure standards and what are the appropriate tenure standards for the modern university. So we're very much looking forward to that.

And then finally and again, in direct flattery is the -- or imitation is the highest form of flattery -- we're going to wind up the substantive panels at the end of the day with the perspective from current and former higher education leaders, and we'll take a half hour at the end of the day to sum up.

The one thing I want to mention as you may notice the not to covert recordings of our session here. We are producing a transcript of our discussions not for circulation of the transcript but as a series of notes for me to use in writing a final report of the day's proceedings. And before that goes public, you'll certainly have an opportunity to comment on it, revise, and extend or exclude remarks if you're so inclined. And so with that, let me turn things over to the first panel which will be ably chaired by our colleague Jim Steinberg, who's the former dean of the Maxwell School at Syracuse University, former dean of the LBJ School -- seems like you can't keep a job. Also somebody who epitomizes the old tradition of Bridging the Gap by not only spending time working in the fields or the growths of academia but also holding increasing high level positions in U.S. governments under various administrations. So, Jim, take it away.

**THE CHALLENGES AND OPPORTUNITIES OF INTERDISCIPLINARY/  
NONDISCIPLINARY PROGRAM IN THE MODERN RESEARCH  
UNIVERSITY**

**MR. STEINBERG:** Thank you, Mike. Can you all hear me? I'm told if we keep the mic down here, it still works, yes. Oh, you have to turn this on.

Okay. Is that good? Yes?

**MR. DESCH:** Yes.

**MR. STEINBERG:** Great. So thank you. In terms of holding jobs, let me say -- and I speak I think for others in the room -- that one of the best jobs in the world to hold is former dean; because you have the benefit of having been a dean, but you don't have to do it anymore. Since there are a number of former deans in the room, I hope they can appreciate my view on that.

I got a little more advanced notice than Steve did about my assignment here. I got about 36 hours of advanced notice. But I embrace it well with great willingness because I really love the concept of this particular panel from the perspective of somebody who works in the social sciences in the policy world to have on the panel people who come not from the social science world primarily. Because I think it is a great opportunity for us to learn the evolutionary path of the natural sciences and applied sciences. Because it's played such a big role in the development of social sciences. And specifically, as many of you know here, the social sciences have always lived in a state of envy with the natural sciences.

The idea that there was a degree of rigor and precision in the natural sciences that the social sciences should aspire to. And therefore, if you look at the history of the social sciences over the last 30 or 40 years, it has been this very strong emulation for better or for worse. And largely I regard you much for worst in terms of what we're trying to achieve here. Because it has taken social sciences,

to some degree, away from a world of trying to have an impact on what happens in the real world of policy and become much more isolated around, like Mike talked about in his introductory thing that, a greater focus on rigor of the elements.

That's the bad news, but the good news, which I think our panelists will share with us today, is that the good news is that natural sciences have moved away from that model, particularly, in the world of interdisciplinarity. And the evolution of the natural sciences handles much more fertile cross-fertilization between the different disciplines and the natural sciences, I hope and believe, can provide a very valuable model for the social sciences. And we look at the biographies of our panelists here and you look at the hyphenated character of the worlds that they work in, they do cross the natural science disciplines. That is something I think you could provide a lot of insight for us if we continue to have and be able to inspire in the direction of the natural sciences have evolved. So let me conclude with that. In my own participation, we had a talk about where we are, I think, in the social sciences and policy sciences after hearing from our colleagues.

I assume we'll go in the order of listing here. Mike, do you have any preferences or the panel, do you have any preferences?

**MR. DESCH:** That will be fine, Jim.

**MR. STEINBERG:** Okay. So that would mean, if we go by this order, let me ask Dick Lanza to go. No, Keith. You go first.

**MR. MEYER:** Good morning, everyone. I want to thank Mike for

inviting me to be a panelist and giving me an opportunity to talk about my experiences in interdisciplinary endeavors in the research university setting. I should mention I've known Mike since he was four years old, so I've watched him as he progressed in his career.

I came to the field of medicine a bit later than most of my peers having enlisted in the navy during Vietnam War to avoid being drafted, but eventually, I was able to return to the university to pursue my post-high school education and get my degrees with the help of the GI Bill. I wasn't quite sure what I wanted to do once I graduated from medical school in 1981. A lot of things appealed to me. I thought I was going to be a surgeon like Mike's dad or an internist. I liked psychology, pediatrics, family medicine, pathology. But I went into internal medicine, and I took an extra year of chief residency to try to figure it all out.

A fluke event landed me in a pulmonary and critical care medicine fellowship and that sent me on a course of becoming a physician-scientist in pulmonary medicine when I was appointed to a University of Wisconsin School of Medicine tenure track in 1988. I was already convinced at the time that I was hired that interdisciplinary programs and collaboration with experts at my own institution and at other medical centers around the world were really of paramount importance in achieving one's research goals. By interacting with colleagues at my own campus or experts from other institutions at national and international meetings, I was able to forge relationships that allowed us to investigate questions

concerning the pathogenesis and management of a number of disease states and perform research that I could never have done on my own. Collaboration not only brought individuals with more knowledge, experience, and skills together to work on specific health issues, but it also allowed clinical investigations to be adequately powered with enough patients enrolled so that outcomes could be better supported by the data that we obtained.

And I'd like to use an example that I think illustrates the power of interdisciplinary and interinstitutional collaboration to benefit patients and improve their health status and, in turn, help them with social issues in their place in society. But I first became involved in the care of adult patients with cystic fibrosis in 1985. There were very few patients who survived to become adults, and patients with CF would most often die of respiratory complications in their early teens. But the genetic defect that causes the disease became known in 1989. This catalyzed many new studies that sought to rectify the gene defect and cure the disease. Pediatric and adult CF centers around the country were joined together under the auspices of the Cystic Fibrosis Foundation, and the CF Foundation provided support and began a comprehensive patient registry and promoted collaboration among CF centers around the U.S. to perform clinic trials of new treatments for the disease. And as a result of these collaborative efforts, we now have therapies that can correct the function of the abnormal gene product and cell membranes, but now adult patients outnumber the pediatric patients. And they not only have improved survival but also a much-improved quality of life; they can

actually do things that normal people do.

But the care of the adult patients with CF also involves disciplines that are not necessarily viewed as directly within the domain of medicine such as social workers, health psychologists, nutritionists, occupational therapists, ethicists, and grief counselors. Such specialists are essential to help patients cope with their disease and be a productive member of society. But one challenge to collaboration at a major research university is to find time, while in a high-pressure tenure track professorship, that really focuses on individual accomplishments to foster interdisciplinary relationships as well as to take the time to treat patients in a humanistic and compassionate way.

In closing, I want to thank the conference organizers for bringing many disciplines together and as Sir William Osler, considered many to be one of the foremost physicians of our time, although he did die in 1919, I'll use a quote of his. He has many. "The incest and concentration of thought upon one subject, however interesting, tethers to man's mind in the narrow field. The practitioner needs culture as well as learning," maybe a practitioner in social science but anyway, thank you.

**MR. LANZA:** By the way, there's another thing that Osler also said who had a great -- he was involved in Johns Hopkins Medical School. And one of his great things is he insisted on taking patient histories himself because he said his famous comment is, "The patient will tell you the diagnosis," and as a good example, when you go into a hospital now to see the battery of tests, on the

occasion, you will see the human at the billing department.

So, when Mike asked me about this, I said well, interdisciplinary; what is all this? So let me give you a two-minute summary, but I started out doing is this elementary particle physics. And I spent a lot of time doing that, and we were studying things that we thought were really great, and we were studying the structure inside protons. I mean what could be more interesting than looking inside protons. So we found these little hard pits called quarks and it was a great experience. And then I came back from running one of these experiments and a colleague of mine, a friend who was at Mass General Hospital, said to me, you guys, you spend all this time with the instrumentation. Can't you help us? Sure. And so that got me into a period of about a decade of building, doing medical imaging. So we built vast PET scanners. We built one of the first CT scanners, and we even went to point of building a very nice mouse scanner, and we were looking at the resolutions and stuff. You can see inside the thyroids of mice. So that was a great series of really fun things to do.

And then, you know, came all sorts of other things that happened, and back in the almost, I guess, 1990 was the explosion of Pan American flight 103, which was a terrorist bombing, which immediately convinced people that we should do something. And so a colleague of mine said, Dick, would you like to talk to these people at the FAA and see what they've got to say. Anyway, so we spent a considerable period of time working on looking at explosives and looking at things in bags and that was sort of an interesting problem in a way. By the way,

it's sort of a medical problem too because there are a billion bags a year that we're looking at, maybe a hundred of them would have something. That's one in ten billion so imagine here you are as a physician and you're looking for this one in ten billion case. You'll never see it. And so that's sort of, you go, hmm, okay, things to think about and other things to think perhaps. And then, of course, there was the 9/11 which immediately got people to going crazy and they want to inspect everything. Whatever it is, we're going to inspect it. One of my students said well, in dog we trust, everything else we x-ray.

But then, from that, I got involved in the whole issue since I was in a nuclear engineering department looking at proliferation and arms control and that's when you really -- I think you really get scared. The possibilities are bad. So let me say, I have a few slides. Okay. The only thing I can tell you on this slide is our previous department head, who I think Frank knows, Richard Lester decided to cut this as a slogan something that said science, systems, and society; so that became our new mantra as to what we were going to do. And we're supposed to talk about universities, but MIT is, well, it's an institute; it's not a university. And this is a quote from one of the founders of MIT, and the only thing you should take from that is the MIT motto, which is mens et manus, mind and hand. So it's a little bit different. It was conceived of in the 1860s as being a different set of approach to academic work. It would include both thinking about the basics and also thinking about the practical, so that's sort of the MIT motto and I think they've really kept at it for quite a few years.

Anyway, so I looked at what we were doing, and I said, oh, wow, what do we have here? A multidisciplinary thing. So, well, I just pulled up a few of these things which are projects in groups that I've worked on recently and you can see. So we have our little nuclear engineering department. We have the folks at security studies. We have CSAIL which is the AI lab at MIT. We've got mechanical engineers, physics, computer science. We do things with the folks at the Kennedy School. We also do projects with the Harvard Medical School. And those are all close, and that's the great thing, I think. I was going to say the beauty of really establishing collaborations is it really helps to be able to walk by the guy's office and have lunch, just walk by and it's a three-minute walk. That's a lot different from being a long distance away. So that's one thing to take into account.

The other things we did: we are part of a consortium for verification technology which is sponsored by NNSA and that's -- we do that with the University of Michigan and about 10 or 12 universities, and, of course, as I said the National Lab. So that's sort of a lot of multidisciplinary stuff and none of it -- by the way, we don't have a formal multidisciplinary center. These things sort of came up spontaneously, and I think that's very useful for having the capability of spontaneous generation like this. Let's see, next slide, I think.

Okay, so this is my classic thing and when we started looking at things and this -- the first part of this thing, I think, the rest of it's sort of cool yadda, yadda, but the first thing is to define the problem. And this is, of course, the crux of this matter. And I read through Mike's book and I said, you know,

guys, we need you and we need social scientists because we want to ask that question, what's the problem? Otherwise, you get people building gizmos, great fun. Gizmos are fun, but the point is it doesn't get you to solve the problem that's important.

So that was one of our first things and this, by the way, is -- I don't know what people are doing today, what its limitations and so on and so forth. This is, by the way, sort of a modification of a classic venture capitalist questions when you try and get money from them. And the other thing I thought, just as a comment, and a great deal of what we talk about, I think, in the social sciences, was Cold War-driven and still is very much a lot of interests and effort on nuclear security. We really got into nuclear security, and nukes became a real driver to a lot of studies and what signals we've sent to the Russians. What are we doing, this, that, and the other? And the point though is, of course, now we have cyber and I point out what's been observed. Two comments here: one is that nuclear attacks, they are sort of threatened but they're not used; whereas, cyber-attacks are used and not threatened. So we've got that as a -- and that sort of difference in time scale and turnaround really, I think, is something for social scientists and theorists to take a look at and see how does this incredible shortening of time change things. So that's one area. Maybe that's the takeaway from here.

And then the other question which always comes up is risk, and this is one from Clark who's at Rutgers is a -- I think is a -- you know, I think he's a sociologist actually -- and raises this question because risk and our perceptions of

risk are absolutely important to understand because that's why congressman X goes crazy when you say there's no problem because he knows something can happen. And so there are the two ways of thinking, and the probabilistic is the classic scientist-engineer who really tells you, look, the chance of this happening are nothing. You're safer than driving your car. It doesn't matter what you say because people are thinking of possibilists. They don't believe you when you say, one part in three hundred thousand. What's that? All I know is, could it happen? And, if you believe it could happen, then you start thinking about that.

This also is one of our problems, I think, today with this whole business of even the depth of the expertise and the fact that 20 minutes on the internet certainly equals your four decades of experience. If you just use the internet, you would save all this time. Anyway, so that's, I think, one of the things we're going to have to think about in terms of, certainly, in policy.

And two other areas I thought about and I looked at was sort of the whole question of you could have gotten all of this incredibly nasty technology out there, why haven't we seen it? Good question. And there is a group of folks that - I think this is a group out at UC Berkley I believe -- and they just say, well, it could be there, and it won't be a potential. It will only be realized when somebody actually does it. And the picture further below is the famous picture using mirrors to burn up the Greek ships at Semite.

And one last comment -- I think, I had one more. Oops, that's it. That's my total and I think I'll stop at this point. So, to say that I think that I mean

it really is important that we do answer the first question which is what is the problem? And defining the problem is really where your expertise comes in, and I think if we don't do it, we're just going to end up, as they say, building nice fun gadgets to build, but we'll spend a lot of money and not get what we thought.

That's my comment.

**MR. WILCZYNSKI:** Yeah, so when I think of this panel, I kind of struggle with what exactly I want to talk about, and so what I decided to do is take a sort of a bigger sort of a vestigial (phonetic 0:33:34.7) approach to think about the idea of collaborative research and something about what we get out of it and why we do it. I think there's a lot of work that's been written about the barriers to collaborative research already. I'm not really sure about -- but some of it is empirical in the sense that there is some evidence for it, but I don't really want to stress that; what I wanted to think more is the development of the field and what this means for collaboration, what it actually is, and how it's defined. And then what we kind of get out of it because I think there are a number of things that you duke it out a collaborative cross-disciplinary or cross-departmental research that is important. But I'm not sure that the different types of interdisciplinary or cross-disciplinary cooperation give you the same thing.

So let me talk a little bit about securing the developmental path of these things. Let me talk a little bit about the developmental path of neuroscience because I'm now in my 60s, and so I've been doing neuroscience for a long time.

In fact, I have a PhD in neuroscience from the 1970s from the University of

Michigan when you didn't really get PhDs in neuroscience at that time. And this opportunity to talk here and also actually reading some of your book -- honestly, I didn't get to the whole thing yet -- made me actually think about the development of neuroscience as a field and just to reflect that, but you think of neuroscience as a discipline right now but, in fact, it wasn't always a discipline.

When I was an undergraduate, for example, then when I went to graduate school at Michigan, neuroscience was the quintessential interdepartmental field of study. It wasn't a field of study that wasn't really anchored in a discipline. It was a field of study that was trying to look at a problem. The problem was how does the brain work in any particular context whether it's a biomedical issue or an evolutionary issue or just a structural issue with the realization, well, you can't really solve that by being a biologist or a psychologist or a chemist or a physicist or a mathematician. You kind of have to get these together because, if you think about the field at the time we went to medical schools, there were anatomy departments that looked at anatomy of the brain or physiology departments looked at the physiology of the brain and your biochemistry departments that looked at the biochemistry of the brain, and there wasn't really a lot of talking. So the idea was that, if you were to understand the brain, we want to have an interdisciplinary approach to this.

Now all these disciplines, it turns out, are biological sciences in some sense driven by medical sciences, something I'll go back to in a moment. But, in the interim, we all -- you don't realize, but I realize it now -- neuroscience

has a become a discipline. It's become a department just like psychology or chemistry. In fact, I'm now in a department of neuroscience that I kind of helped put together. And one of the ideas in order to get a new department or a new academic unit through the Georgia Board of Regents system, you have to argue that while it's unique, there isn't any place else. And I had a really hard time doing that because I had to argue that, well, among the research universities that in the Georgia system which is University of Georgia, Georgia Tech, and Georgia State University, we didn't actually have a department named neuroscience so probably we needed one. But, at the time in 2008 when we did this, neuroscience had become a discipline. It became a discipline which is a stand-alone academic department that reported to a dean just like everybody else did even though it did try to draw in and still has that residual, historical basis of drawing in other people from other disciplines in order to enrich the people.

Well, one of the things that struck me from (crosstalk 0:37:25.7) the great things that people talk about here in terms of rigor versus relevance, the idea that as disciplinary studies become more centralized into an actual unit, in general, emphasis on methodology and rigor as the driving force for what we're doing becomes more important. And certainly, that's true for neuroscience. And my feeling is that even at the upper levels of, principally, National Science Foundation, which is actually at the forefront, I think, at interdisciplinary research. A lot of the emphasis is we need interdisciplinary research to provide new tools in order to look at what we're doing, and any of us who have been on NIH panels or

NSF panels knows that works can grant these, knows that one of the things you kind of have to do in order to be competitive for funding is you have to have a reason for having a grant; you have to have an idea. But one of the things that will put you over the top is if you have new methods. And that's really become part of the developing of this.

I think at least in neuroscience and probably some of the other biological sciences, especially those who have biomedical implications, this emphasis on rigor hasn't come with a diminution of the idea that there's relevance. Because, if anything, the idea of relevance has actually increased in these fields with the idea you should be doing something in your field if you want to get funded that has relevance not to public policy but to something: to a disease state, for example, to an environmental issue. And so, in neuroscience, there's a lot of biology. You're actually faced with two things as you become more of a discipline: you have to increase the emphasis on methodology often -- not often. Sorry to be cynical about this, not often -- but sometimes the exclusion of actually what the problem is. I mean, you -- but also you have to emphasize what the relevance is that you're doing. And sometimes it's lip service, although, I think sometimes it's for you. I mean the idea that you're doing it for that. So, as I look at this, it's rigor and relevance. You want to get -- there's an increased emphasis on new methods and new techniques sometimes to the exclusion of the ideas behind it, but also a greater emphasis on why you're doing this with irrelevance. I don't criticize funding agencies or anybody else from doing this; after all, the

money that we get comes from taxpayer money. And so it does make some sense that if it's taxpayer money or its foundation money that there should be some reason why they're giving it, so I see it as relevant.

So, if we're in this -- again, and neuroscience has now become a discipline, if you look at it from that point of view, then let's start thinking about what that means for being an interdisciplinary participant there. So we're already a discipline now. We thought we were inherently interdisciplinary, and we keep saying that as neuroscientists we're inherently an interdisciplinary field, but in fact, administratively at universities, conceptually in terms of our society is we're actually a discipline. We're like chemistry or biology is. There's subdisciplines in those and just like there's subdisciplines in neuroscience. So how do we sort of do this in a way that makes sense of what do we get out of this? And so I took some time to sort of thinking about this and looking at this. I don't think there's really much data on this. I think it's more of a gut feeling about what's going on, and it would be interesting to look at this.

One of the things that neuroscientists -- and I think that in all biological sciences -- look at is the connection with the other sciences, whether they're in the physical and mathematical sciences or they're in the social sciences. And the idea that your particular discipline is based in neuroscience would be enriched by a connection with people with physics or in math or in anthropology or cancer because of the math depending on what that is. So there is this emphasis from the top down, including funding agencies and at university levels, to support

interdisciplinary research in the sense of kind of ironically defining interdisciplinary research on the basis of different disciplines that can be defined as departments participating in that, not the intellectual contribution but the administrative contribution.

So we often see from funding agencies the very large initiatives at NIH or NSF that emphasizes interdisciplinary or cross-disciplinary projects that are very large but are defined by having different slots filled so from the top down. So, for example, a program that would require you to have someone in the biological sciences and neuroscience but also has to have someone from physics and maybe someone from the social sciences and then from different types of universities or maybe even in the E.U. for example. I think this is a bit of problem because it does impart a bit of tokenism that goes on there, like, we need to have a physicist on it, so let's go find somebody that you'd be willing to deal with.

But there are contributions here. If you look at the contributions there, there are kind of two areas here you could contribute to solving a problem: One is methodological. The other is bringing ideas in. My feeling is that if you look at the kind of large cross-departmental or cross-disciplinary, as you define by department interactions, most of those at least from neuroscience, tend to be contributions that are methodological. And they're important so I'm not diminishing them, but they are important. So, for example, we want to interact with computer scientists because we have this gigantic data set, so we don't know to work with them, but we can court their expertise in working with those. We

want to model how systems work and people in physics, for example, or in math are very good at that, right. They're better than us. Or we want to know how social structures are reflected in neural systems, well, people in sociology, for example, or even anthropology are quite good at that. So, in the larger cross-disciplinary scheme, the disciplines being defined by inter-academic units, what seems to come out of that and what seems to be a major contribution is the methodological, and I'm still trying to struggle with whether that is a good trend or a bad trend. I think it's a good trend by getting the methods from different areas connected into a disciplinary science, but what seems to be missing there are some of the ideas.

So I read a number of different papers on this, these sort of glowing contributions of what is coming into the biological sciences from chemistry and from physics and how important this is. And there are tables about an example like this, and what struck me was they're all about providing tools. They're all about providing computational tools. In chemistry, it's all about, well, developing small molecules so you can probe something. In physics, it's all about having computational models or ways to actually do better imagining. Or, in computer science, it's all about how you handle big data sets -- really little in the way of what is the intellectual contribution from these areas that will actually inform what we're going to do?

So I thought about this and I thought about there being levels of collaborative research, levels of collaborative research starting at the very big level

which again, I'm kind of arguing we increase mythological rigor and new methodological ideas into the field. But then there's this other component of collaboration which is collaboration within what would be defined by a funding agency like NSF or by your own college of arts and sciences left in the department. It is collaborations between individuals within a field who have different ideas and different areas of expertise but then come together and provide new insights or new avenues of research that are not based on new methods but are based on new concepts.

And I'll give you three examples of this that are within the field of biological sciences in general and also touch upon neuroscience and not go into details but just to give you kind of flavor of what I'm talking about: One is this area called evo-devo which evolution and development. So, some time ago, looking at a number of individuals who are in the field of evolutionary biology started to interact with people who are in the area of developmental biology. And they did this on the basis of very old ideas that had to deal with how do you actually involve these structures? And the idea that the evolution of the brain, for example, or any kind of structure really comes down to changing developmental programs. And people in developmental biology were very interested in developmental programs, in the channelization of different developmental trajectories, and how gene expression changes or develops to give you different things. People in evolution were interested in the adult forms of things, how you actually end up with different genotypes for different physiological processes.

And what happened there is some cross-fertilization of methods but also cross-realization of ideas, like ideas that really changed the way people in evolutionary biology thought about how evolution actually occurs through the changing of developmental programs and how that might work.

The second example is in this area of social genomics. Social genomics is a field which asks how is it that -- what is the gene expression differences that define differences in social states or give us different social behaviors. Again, it's within the areas of biological sciences where people who are neuroscientists or animal behavior scientists got together with individuals who are molecular biologists in genomics including development biologists. Individuals in that developmental genomics side of things were individuals who thought very deeply about the idea that during development, genes are expressed.

It's the control of that expression that gives you different developmental patterns. It turns some part of the body into a hand and other parts of the body into a jaw. All those cells have the same genes, but they have different expression patterns that occur. How are they controlled? Why don't we keep growing hands after you get born? You know, things are turned on and turned off, and those ideas were important to people like me, for example, who are interested in social behavior among adult animals. And, as the basis of social experience, how do we change our brains so that we're now very different animals in terms of our responses to stress or responses to aggression or responses to threats? And the idea that there must be gene expression control in the neurons of

the brain that are very like the gene expression regulations that occur during development and these changed adults in an ongoing pattern here, and so the idea that you could look at the molecular bases of social behavior while using the ideas in molecular biology, about gene regulation, gene expression, and how this changes more morphology cells and their function.

And then the third thing, I think, is the most recent one, and it seems to at least be more compelling to my undergraduates than anything else and that's this idea of PET brain signal. You may have read about this, but as I tell my undergraduates, you have to kind of think about what you actually are as an organism. You do realize that you are an organism that's composed of about ten trillion human cells, right? That composes your -- but interdigitated within those 10 trillion human cells are 100 trillion microbial cells, mainly bacteria. So you are not exactly human. You're sort of some hybrid of human, bacteria, fungus, and viruses that have invaded themselves into an individual, and most of that is in your gut.

And there are people who have been studying gut bacteria for a long time independent of anything having to do with behavior or neurological disorders or anything like that, but it became apparent that the bacteria, the microbiota that you have in your gut, especially, is extraordinarily complicated. It influences not only physiological systems within your gut -- meaning the disorders like irritable bowel syndrome and Crohn's disease -- but it turns out that they also have a bidirectional communication with your brain. And there's a lot of research coming

out now that's very correlative because we don't understand how this happens which suggests that gut microbiota may be at least correlated with a number of things including depression, autism, a number of different neurological disorders.

There are very bizarre scientific studies looking at transplanting in gut bacteria among rodents of different phenotypes and changes in their behavior just by switching their gut bacteria in different strains. There was a study that actually just came out that transplanted gut bacteria from depressed humans -- there are also control groups in this as well -- into rodents -- because you can't do it to people -- into rats and as a result of this, the rats expressed a number of behavioral phenotypes. We can't say they're depressed, but they have different stress responsivity. They tend to have a number of biotypic (phonetic 0:52:05.3) responses that are very similar to what depressed patients have. It's they're lethargic. They developed the (inaudible) disorder system. Remember all you're doing is transplanting your gut microbiota.

So this is an area that's revolutionized how people think about individuals as organisms. It's not just the methods, of course, it is part of the methods that we (inaudible) . As a neuroscientist, I wouldn't know how to study gut microbiota and people who are microbiologists who study gut bacteria have no idea how to do brain markers or how-to phenotype rodent, but if you get those individuals together, not just methodologically but conceptually, you are fertilizing ideas.

So, what I just want to point out is that this type of collaboration

may be more common than the big collaboration. It actually may be more valuable in cross-fertilizing ideas, and it may be more under the radar because it's within a discipline. It's all biology, and you don't recognize these often in this big scheme of more multidisciplinary or encouraging multidisciplinary studies here.

So I just wanted sort of point those out and as things to think about as we think about what cross-disciplinary and collaborative work meant. There are multiple levels of this. We tend to think more about cross-disciplinary and collaborative research as being the big sweep of things. It's, can we get biologist and physicists together? When I think there is another level before the individual investigator, which is the collaboration between individuals and subdisciplines within regular disciplines, which it actually may be more fruitful in cross-fertilizing ideas and also for overcoming some of the barriers that are widely recognized for hindering the effective process of their research. In part, because at least within that sphere, we kind of understand each other. We can value each other. We don't speak exactly the same language, but at least we can translate a lot better in those individual terms. So that's what I wanted to point today.

**MR. STEINBERG:** Great, well, as Steve's presentation sort of proved, my hope and expectation that the experiences in more of the areas have been natural sciences would be particularly valuable in reflecting on the challenges in social sciences. And I truthfully like that the focus of some of this early discussion about thinking about the challenge as problem-centric ways of thinking about organizing ourselves versus tools or method-centric methods and

ways of organizing ourselves.

And going back to Steve's reference to Stanley Hoffmann and the dumbwaiter, you know, for me the nature of the challenge here is it's all well and good to have people putting stuff in the dumbwaiter and the dumbwaiter going up and delivering it. But it's only valuable if the stuff that's put in the dumbwaiter is useful to the people at the top who are getting it. And the challenge here, in terms of what the problem is in this discussion, is to make sure that there is a connection that's more than a mechanical one between the people in the kitchen putting the stuff in the dumbwaiter and the people at the top who are consuming what's put in the dumbwaiter. The people at the top have to let the people in the kitchen know what they want to eat and what would be useful because, if they produce something down in the kitchen and the people upstairs are allergic to it or they're interested in having dinner and not breakfast, it doesn't do much good. And the whole focus on Bridging the Gap really is to make sure that this dumbwaiter process is one that actually produces something that takes advantage of what people in the kitchen can do, what their skills are, but in producing something that the consumers up at the top can use.

And so I want to use that to think about the challenges in the social sciences in particular in the serving international relations side. We talked about the fact that, as Steve pointed out, that Mike's book looks at taking the history of the thing. And the history is very important because if you think about the evolution and development of social sciences, say, at the great founding period of

the social sciences in the 19th and early 20th century, we didn't have disciplines at all. If you think about (inaudible), we wouldn't know what kind of social scientists they were. They were concerned about social problems of the day -- saying the same as Marks -- and they thought about how do you try to understand those problems and they want to understand them, not just to admire them but to do something about them. And that was the attraction and strength we need and why is this way of thinking about the academy as a way of contributing to that understanding and the emergence of social science in the academy?

We have Frank here at John's Hopkins where a lot of this early emerged, but as I mentioned in my opening remarks, once it came into the academy, the social sciences became preoccupied with proving that (inaudible) is an academic discipline and it became heavily focused on the tool side with less of an emphasis on the problem side and trying to prove its value by the rigor of the tool and the sort of rigor on the theoretical analysis rather than thinking about what good it was, what it was going to be used for. And so, when got to talking about it, you know, how particle physics leads to detectors like CAT scans and PET scans. It's not that we don't need people thinking about particle physics but, particularly in the social sciences, having those tools are only as valuable as those tools are in terms of understanding the problem. And so, as the social sciences led by the economic fields sort of moved towards a greater focus on the tool, there became sort of -- began to lose the sense that the value was not just in the rigor of the tool but what it was being used for and where this is an old one and the

foundations played a big role in the 1960s in helping us to first recognize that in this translation of social sciences in with the academy that they were losing the idea that it was designed -- it should be designed to contribute to solving problems not just developing tools.

And so a number of solutions began to emerge, and I was asked to talk a little bit about, first, the emergence of these so-called APSIA schools, the Schools of International Affairs, which were very much influenced by the foundations which were concerned about this problem. And the idea was to create a new kind of institution that was more focused on the problem which is preparing people to deal with challenges in the real world both in international relations and also in domestic policy by creating a new kind of institution which was a standalone school of international affairs on public policy to have as its mission producing both the human capital and intellectual capital for dealing with real-world problems.

And it's important to emphasize that the mission was two-fold which is why they were created as professional schools to train people so people who are actually going to go into the world of public service and problem-solving, but they were not just supposed to be that. They were not just supposed to be teaching institutions, but they were also supposed to be research institutions. And they were supposed to be also, because they were part of a university, generating knowledge and research to support not just to teach people but also to support the intellectual development.

And I think what happened -- and I'll be interested in my colleagues hearing this -- is that the schools were reasonably successful in the teaching mission in terms of producing a kind of a large group of people with professional training that prepared them to go into public service -- both policy and international relations -- but they were less successful on the research side. And why was it? I think there were a number of reasons for it.

Part of it was because of the way that these institutions existed in the university. Because they were standalone schools, for the most part, they operated in parallel and, particularly, in isolation from the rest of the universities themselves. So, if you look at, for example, the school that I was first dean at, the LBJ School, it was a standalone school, but it existed in the university. And it had people, teachers from the various disciplines from economics, political science, sociology, anthropology, and the like, but it existed in a university that had separate departments like political science and economics and anthropology and there was almost no communication, cross-fertilization. They taught different people. They worked on different problems. And so rather than having them be integrated deeply into the work of the university, they became this sort of standalone institutions. The same was true at the Kennedy School, like most of the schools of public policy that were created during this highly fertile period of creation in the last 1960s and 1970s.

Second, because they had been focused on producing students for the world of practice, they produced very fruitful right into the world of teaching.

There were a small number of PhDs who came out of these programs, but even the PhDs who were in these schools tended not to go into the academy in part because of the kinds of people who were attracted to do PhDs there but in part because, if you were interested in going into teaching, it was a big risk to take a PhD in public policy school or an APSIA school because the chances that you would get hired into the interdisciplinary part were very small. If you wanted to be a political scientist and teach in a political science department, you had to have a PhD in political science not in public policy. And so the universal potential job opportunities for PhDs from these schools was very small and, if you took the path of doing a PhD in economics or political science, you could still teach at a public policy school. But, if you did a PhD in a public policy school, you couldn't teach in the political science department or in the economics department.

And so, although, each of these schools had some PhDs, they became very kind of marginal to both the schools themselves and had very little impact more broadly on the university because their graduates were not getting into the broader world of the university and the university's basic research. And so, though they made a significant contribution to the world of practice, in reality, the very few of them, I think, have had a particular transformational impact on the universities in which they were situated and became sort of isolated. Some self-isolated. Some isolated because of the cultures of the universities in which they were based.

And it's interesting having gone from one of these types of schools,

the LBJ School, which was really, I mean, even physically separate from the rest of the university like a separate student body who didn't interact much across with separate faculty. It was interesting too, then, come to the Maxwell School which was created much earlier than this occurred in the '60s and '70s of creation of these new institutions as a solution to the problem.

Maxwell, which was great, in the 1920s had a very different model which was to create a school of social sciences with all the social science disciplines in it and then added to that a department focused on public policy and international affairs as one of the departments within a broader school. And the theory there was quite different which was to not have policy-relevant work separated into a separate institution from the social sciences and to have one dean who was dean not just of international relations and public policy but of economics and political science and to have a sense that the school was as a whole had a mission that brought together the different disciplines.

Whether this worked in practice, of course, was less satisfying than the concept. But the concept was a very different one than having a standalone one in at least to try to respond to the idea that the discipline should not be separated from the world of people contributing to both the teaching and training of practitioners but also a generation of knowledge to be useful. I understand those two experiences is important to us thinking about the paths going forward.

And then I want to talk plainly about a third model, which I had the privilege of working with Mike Cavanaugh again with the support of Carnegie as

you've heard which is a project that we launched now -- what, six years ago, Kirk (phonetic) -- which is called the International Policy Scholars Consortium, which was an attempt to try to find a way without doing institutional reengineering either along the lines of the Maxwell School or along the lines of these standalone public policy and international affairs schools, to try and see if there was a way to use the interests of students in having an impact on the world as the change agent in the system.

And the idea here was to try to find a way without having to rewire the institutions, as Steve Van Evera's been trying to get us to do for some time, is to accept the constraints of the existing institutions but try to provide what one of our colleagues called nutritional supplements to young and aspiring scholar-practitioners in a way that allowed them to both have an opportunity to be successful in the academy but also to contribute to both ideas and the world of practice. And the idea here was to create a consortium of schools that had both public policy programs and smaller social science PhD programs, but to take the PhD students from the traditional disciplines and expose them both to the tools and the mindset of the world of the schools of public policy and international affairs by creating a set of programs and activities outside the normal PhD programs that would allow these students who were discipline based in their main degree program to be exposed to both the different disciplines and to the world of practice through a series of seminars that take place during the school year and workshops that take place during the summer, in which are breaks, to both give

them the opportunity to get exposed to interdisciplinary ways of thinking and scholars and practitioners from different disciplines, but also to develop a cohort of students who have shared interests and commitments to the world of the policy realm at work which allows them to sort of build a support network over time as they move into their careers in the academy.

And we've been at this for six years. We now have gone through six cohorts of PhD students so there are now about 90 -- that's about right -- 90 students who have gone through this. A number of whom have now moved into a tenure track of academic departments and have created this sort of sand script where there is a community of people who have an opportunity -- because they have the right degree in their disciplines to have an opportunity to work within the academic university system but have both the intellectual tools and the support networks to allow them to begin to explore the opportunity both to do more interdisciplinary and policy realm at work. Again, Mike and Steve, I think, we believe that this is something that's beginning to get traction, whether we've been successful in persuading esteemed bean counters, he will tell us. But the fact that we not only have had this large cohort of people who are in these traditional disciplines, who are eagerly staking out this kind of set of experiences, and the fact that we continue to get significant numbers of PhD students seeking to participate in these programs suggest that people -- this is, both meeting an interest and demand by the students. Or the fact that they are, then, going on to be reasonably successful means that they're at least not paying a price for been having exposed to

this different kind of approach to their work but also that they are now being seated into the traditional academy and work related over time will bring these mindsets and sort of problem-oriented and ideas-oriented solutions rather than method-oriented solutions to the work that they do in the academy. Time will tell but just the infectious nature of working with these young PhD students and their continued commitment to the program after they graduate from it and their continued involvement with us suggests that there are real opportunities here to try to follow the path that the natural sciences have had by building these networks and wanting the students themselves to be able to view the kind of collaboration met on multiple sides. So I'm going to stop there. Do we have any questions? Thank you very much. Do you all have any further comments and response to each other or comments from the audience here? Jim.

**UNIDENTIFIED MALE:** Okay. Thanks. Very interesting set of presentations. I wanted to follow up with something that's sort of stimulated by what Walter was saying and what Jim was saying. I'm very interested, Walter, in the way in which neuroscience developed from interdisciplinary to becoming a discipline and wondering whether there's any prospect for that with international affairs. But here's my question because I was dean of a school that was like LBJ. It's like LBJ in that it's made up of faculty from lots of different disciplines. All of those disciplines are represented as well in departments elsewhere on campus and for the young -- especially for the tenure track faculty -- I would like to try to foster with money interdisciplinary work and there was some very interesting

projects that developed but really if you're a young anthropologist in the School of International Affairs, you want to be seen by your discipline of anthropology as a great anthropologist. If you're a historian, as a historian. You want to publish in your disciplinary journal, right. The idea that you're going to do something great to answer some big question in international affairs is really secondary to being seen as a great disciplinary scholar which is very limiting in building interdisciplinary work. And I'm just curious how that -- whether that was a problem when before neuroscience because a discipline and whether that -- presumably what changes when neuroscience becomes a discipline is that you then have your own journals and people are publishing in neuroscience journals, and so they can be successful in an interdisciplinary way where they might not be able to before medicinal (phonetic 1:12:16.7) discipline.

**MR. WILCZYNSKI:** So I just started thinking about this as a -- coming here. So I had like a scholarly thing about how to present it. But what you're saying is a real problem and was a real problem. I think it's less of a problem now because, as you become a discipline with your own department, then it becomes more like any other department so now that's Earth sci- -- if you're a scientists, 35,000 members, right, just in society, probably more than that. It has like 30,000 people just going to meetings every year. It has its own journal, right. It always had some but now it has a journal for science and a whole bunch of other neuroscience journals, so you publish there, and you actually now become of your discipline. Before that, I think, it was kind of problem. What also has happened

as you form your independent academic units which are either departments are now at least a part of one -- ours are -- then your tenure lines are there, right?

**UNIDENTIFIED MALE:** Right.

**MR. WILCZYNSKI:** Your resources are distributed by your department chair, not by someone else's department chair. You're working with colleagues there so that becomes easier. So, when I was at the University of Texas before I was at Georgia State, University of Texas, we had an interdisciplinary neuroscience program that potentially got developed there, and it was exactly like what you're saying. Everybody was a faculty member in a different department. There was an interdisciplinary PhD program in neuroscience that took students, at least a few students, but then how would you support it? So there was a training grant that supported them but getting them supported by other departments even though a faculty member was a member of that program was a nightmare. So it became just very, very difficult.

And then the faculty members, of course, especially junior faculty members, who might be in the interdisciplinary neuroscience program, would look at that and then -- we hired them because they were smart, right? So they were smartly utilized, well, I'm going to out for tenure in my department. Then neuroscience says he's going to have absolutely no say in this. They're going to look at the publications that I have in my field, right, and they're going to judge it there, and the neuroscience student has nothing to do with that. So, as you become an actual discipline, I think some of those problems go away, but before they do,

then they're extraordinarily difficult to deal with.

**UNIDENTIFIED MALE:** I'm curious about how often an interdisciplinary activity does or does not evolve into a discipline and what are the circumstances that drive that? My instincts are that it's driven by the ultimate markets for the product of that interdisciplinary activity either the human capitalists created and the student who studied there and whether they can find positions either in the university or elsewhere. And second, whether the consumers of the intellectual capital find it worthy of buying or paying for it. This question of whether the social scientists have been misled in a way by the natural science paradigm comes to mind, so I'm curious. Do you have good examples of really powerful interdisciplinary activities that never made it to becoming a discipline? The problem got solved or the problem went away.

**MR. WILCZYNSKI:** So I don't know. I was thinking about that as you were talking. Only we are examples are failures, right. There are things that we've tried and were not successful, right. So why weren't they compared to the ones that were, and I've never actually thought about it until you mentioned it, so I don't really know of any, but maybe there are others. It seems to me that neuroscience, international affairs are fields where they've got traction. Molecular biology is another area that got traction here as well. I would be curious whether there are --

**UNIDENTIFIED MALE:** I have one example. You might want to argue with me on this, but we may be in the midst of the death of nuclear

engineering as a discipline. And it's driven by the energy that you're globally, in all the natural gas we found, there just isn't a demand for nuclear power.

**MR. STEINBERG:** Sure. I think that's a good example because the question is there's a certain market failure for the problem which is if presumably what allows these things to either flourish or to die is because there'll either be a market need or they're not in some sense, right.

**UNIDENTIFIED MALE:** Right.

**MR. STEINBERG:** And I think the problem and, unfortunately, I talked (inaudible) in the academy since I've seen it. He said there's no market discipline on these disciplines that are brought on to start doing irrelevant stuff that people are doing things for which there is no market. There's nothing much that's coming in the dumbwaiter, but nobody's getting fired or losing their job. They don't go out of business the way a kitchen that produced a bunch of garbage would do if they kept sending up the dumbwaiter, no customers would come. They would have no revenue at all. All the bad cooks in the kitchen would get fired. And so it's, at least, my perception of this exercise is that you don't get -- the work is done. It's not particularly valuable at least outside the academy and yet nobody is firing the cooks that are producing these bad meals.

**UNIDENTIFIED MALE:** But over generations of students aren't attracted and those who are attracted can't find jobs.

**MR. STEINBERG:** There's not a lot of connection there between that, I mean, in the sense that, in part, because the research part of the university's

different than sort of -- I don't know -- just the regular student part of the university. Most of the universities are here to produce undergraduates, right, and there's been this disconnect between the research side because all these departments are shrinking. I mean, a lot of them are shrinking in terms of their student enrollment but it's (inaudible) maybe it just takes a longer time for that part to find the support.

**MR. LANZA:** A couple of comments on that, not being a nuclear engineer, but I think in some ways, there's several issues in that field. And one interesting thing is to compare it with the German experience which you know has been touted as the future direction we're going to go. And, of course, as you know, you can get lots of papers on this, the price of energy, electric energy, has skyrocketed in Germany, carbon dioxide emissions have skyrocketed, and a question to ask is, what a minute. Did we miss something here? Well, of course. And people wanted to believe things were going to be true and things that really irritate some people is they -- in one day, we, in Germany, produced a hundred percent of our energy by renewables. Wow, that sounds great. I mean, who's going to argue with that? Then they say, wait a minute. It was on June 21 where we absolutely peaked on our solar and oh, by the way, when we talked about energy, we're not talking about non-electric energy. And so MEMA is here producing 90, 100 percent of energy by renewables; the autobahn is filled with cars driving along; there are steel plants running. So, you know, in a way, I think that nuclear engineering certainly is going to have change direction, and nuclear --

the navy, we have a large program that thinks they can do fusion really. And I don't know you can get into fusion because a colleague of mine once proved that we should have had it in 1934. And his argument is the U.S. had pressure in 1954 and was 20 years into the future and then 1964 was 30 years into the future so we drew the line back to 1934.

But the other comment, the non-nuclear comment, was, for example, MIT has a department and it's called Brain and Cognitive Sciences which is quite different. It really sort of came from computer scientists and that was one of the big things. There were programs in artificial intelligence. There were programs in natural language. And so it came from a completely, I think, in a different point of view because MIT didn't have a medical school and that was -- and so the drivers for it were considerably different.

**MS. ACOMIDA (phonetic):** Thank you very much. My name is Mary Acomida and I'm actually here from the European Union delegation here in Washington. And I wanted to bring maybe to the discussion a reflection on the European position and particularly the role of funder in promoting interdisciplinarity. And the example I want to give is the European Union's framework of our research and innovation. We've had a number of them. We're currently in the ninth one or preparing for the ninth one. In the past, we always funded by -- pretty much by -- but not exactly disciplined about transportation research, environmental research, health research, et cetera. And so we allocated a certain budget for each of these disciplines and we're in the fortunate position of

having set in your budget perspective, not an annual budget like here in the United States.

And, in the ongoing framework program, which is called Horizon 2020, which will end at the end of 2020. We approach it a little bit different and we said well, rather than talking about specific areas of research, we talk about societal challenges like health, well-being, and demographic change and try to get the scientific community to start thinking in this way. I propose research projects so they would fund with this perspective. It worked to a certain extent, I must say, but perhaps not as much as we would have liked to probe the interdisciplinary aspect.

So the next framework program, which will be called Horizon Europe which will start in 2021, the idea is quite different. Firstly, our old directorate general has been reorganized starting yesterday, and the main funding part will be in three pillars: clean planet, healthy planet, and prosperity. So these are the objectives of the sustainable development goal of the United Nations, but secondly, we will no longer as of 2021 -- at least this is the plan -- will each scientific area like transportation research or health research or environmental research have a dedicated budget. The budget will be a common pool and each of the former disciplines, so to speak, will have to come together and identify together the topics that they think will be important to have calls for proposals so, therefore, the scientific community will almost sort of -- it would really be a very strong nudge to the scientific community with impact to work together starting

with forcing the funding agency and disciplines within the funding agencies to work together because no longer will they have their own budget. They would have a shared budget, and they'd have to propose what they want to do together in an interdisciplinary way. So that's just a little bit of an example of something new that we're trying in Europe.

**UNIDENTIFIED MALE:** Great panel. It follows well on Louise Richardson's talk last night and I think the themes we're wrestling with really, to me, are just -- they are really fundamentally about universities and as institutions in society which on the one hand are looked to to have a multifaceted mission. But it is probably fair to say we are a politically liberal and organizationally conservative because we're talking about many different ways of routing around the fundamental structure, right.

So we talked about the rise of interdisciplinary, absolutely, almost to the point where over the decade or so it's become a buzz word, right. Everyone loves it even if they're not serious about it. So, in our -- Terry Sandford spoke about the policy too. I think we have nine different types of PhDs on the faculty. We're not organized into departments, so, on the one hand, we are inherently interdisciplinary. But as a couple of people said when they think about it, as Jim said, they think about their tenure case, their promotion case. It's well, who are the economists that are going to review me as external reviewers? And because they do work that is a bit more problem-oriented, they may not be publishing in the (inaudible) journals in their discipline, yet they're doing very important scholarly

work consistent with the mission of the policy school, but that becomes a wrestling match in a very difficult issue. Whether it becomes you're involved for interdisciplinary to a new unit in political science, you could argue that the political theorists talking with the former model builders even though they're saying discipline is a form of interdisciplinary, right.

So I think we really keep looking for ways, but a structured change has become very difficult. It's really with science and policy, but I think back to the generation of nuclear arms control and it hurt the Herb Yorks and the Sid Drells and the Hans Bethes. And I guess my question is I get the sense that while it's very important in science to do the basic research that not just about policy but that affects society in a lot of different ways and extremely important and it's this is not an either-or proposition that the incentive structure for young scientist to become the next Herbie York or Sid Drell within the academic world isn't the same as it once was, and so how do we create this pluralism?

And there are many other entities within society. There are all these think tanks who are doing some private research labs, but I think fundamentally it's the universities probably arguably have changed the least of any sector of society in the last quarter century. So there are various efforts to try to route around or create incentives to do that and I think what we're struggling with is the importance of not taking away the core mission of academics in any discipline and just further academics. You know that's the advancement of knowledge upon knowledge worth saying but how to really adapt to fulfill a range of other issues

that we should be better at as institutions in society. And to the scientists, I think it has its own challenges that are similar to social sciences but in their own way different.

**MR. DESCH:** If I could just piggyback on that really quickly and I wanted to ask Keith how much of what Jim was talking about in terms of the different models for professional schools of international affairs in their relationship or not with the constituent social science disciplines, how much of that is analogous to the challenges that medical schools face in terms of relationships with the natural science disciplines themselves?

**MR. MEYER:** Well, I don't see the problems being quite as serious as to the social sciences may have in terms of communicating with each other, but on my campus, I'm able to, as a resource, go to people in other departments, biological sciences, and talk about ideas and collaborate and our new motto is collaboration. That's really important. There's a big sign in front of the school now about talking to each other about things. And so, anyway, I think it's not as much of a problem at my own institution, and I, personally, and others have thrived on collaboration among a lot of basic scientists in different departments, and I've seen patients too, so I can take things from the bedside to the lab. I have a basic science lab and try to translate it into benefit for patients. There's also more of an emphasis at UW on taking the riches of the university to the populous and then, in that way, there's been some change. And my wife runs the program that's now kind of becoming the flagship of -- and she's an English literature person, but

she's got this program of taking people who have no hope -- homeless, prisoners, et cetera -- getting 30 of them into a program per giver and getting -- because they have the smarts and -- helping society in that way. So that's kind of a change for UW anyway.

**MR. LANZA:** Just another question -- you talk about the Sid Drells and Herb Yorks, but remember this whole thing was driven by the Manhattan Project. That was the defining issue and was the defining issue of the fact that you can organize big science and it was filled with academics and it was filled with incredible scientists, but they sort of have that as their creds if you will. I mean, if you look at someone like Dick Garwin who many of us know who is an incredible scientist, and he was present at the beginning. He wasn't quite at the Manhattan Project but soon thereafter. So that's why they were able to very well articulate their concerns and also, I mean, one of the other great things is they were -- you couldn't tell them, well, if you knew what we knew, you wouldn't have -- that sort of argued a lot of bureaucrats. We have this secret magic sauce and knowledge that you don't have. Well, the point is they did have it. And I think that carried through the whole ABM issues to all people who have actually been in the business.

**MR. STEINBERG:** But I think part of the problem about this as you know is what's concerned us a lot is this adverse selection problem which that what you want is you want people who care about these things because you want people who care about this and about arms control to go into the academy. But for

them to do that, they have to believe they can succeed in doing both, right, or else they're going to have to make a choice and they don't want to make a choice. If they have, if they can't succeed, they'll going to either have to say, well, the only way to succeed in Kennedy is to not care about arms control and just do physics, or I really want to hear about arms control. I can go in a think tank or something, but I'm not going to go to the academy.

And so what ends up happening is unless there's a reasonable prospect that people who want to be interdisciplinary and do these things can succeed. They're just not going to go down this path, and that's been the challenge, right, is how you get it so they feel optimistic enough about the future to feel like the things -- by the time they get to the point where they need a job in the academy or they're going out for tenure that they're still going to have a chance. And that's when, I think, they're not really trying to untie for some time which is why, you know, it's possible that over time the market will work but what's inhibiting in commerce are people who could be the forces to make it happen aren't willing to be the risk takers to try to change it. We've been trying to make you feel more optimistic or comfortable about taking the risk to do interdisciplinary work and still feel like you're not sabotaging your career.

**UNIDENTIFIED MALE:** Just a brief comment in response, there's evidence that the people who need novel interdisciplinary problem-oriented research tend to be more senior faculty who have earned their spurs and can take the risk. If you're advising a young graduate student, you probably should advise

them to stay in a discipline initially; in time they will have the freedom.

**MR. STEINBERG:** That's right. But the problem has been that, one, young people, it's a lot of people who care about this priority's going to be willing to defer for 15 years, number one; and number two, even if they come in with that, after 15 years of (inaudible) they lost all the ability to do it. So I think you're right but there's still -- it doesn't always work out. People, all of a sudden, can -- after 15 years of doing all the kind of right things, staying in their lane, all of a sudden, blossom once they get tenure.

**MS. OLSEN:** I'm just going to kind of follow up with a lot of the discussion, but it was a statement that you made about rewiring the institution, and it might be this is something for the prospective higher education leaders. I'm also a neuroscientist, right, but I actually, in the '70s, went to a department of neuroscience. Why? Because it was in the '60s -- '65, '66 -- the new schools that were established -- UC Irvine, UC San Diego, and SUNY Stony Brook -- all got to start from the ground up and UC Irvine made a department of neuroscience, ecology and evolution, biochemistry, and molecular and cell. All of these are now disciplines, but back then they weren't. And so why is it so hard for university? Unless you're starting from the beginning and from the new to be able to change because everyone saw that trend. Everyone saw that neuroscience was going to be involved in discipline. Now it's a department in almost every single university. Even Notre Dame started an interdisciplinary neuroscience program four years ago, right. But why does it take so long when you know it really drives the

excitement of the science. Why can't universities change?

**UNIDENTIFIED MALE:** The other side of it and you know what the answer is. What led those pioneer universities to be willing to do it? And what was the leadership there that said, we're going to do this and we're going to develop it?

**MS. SCHOR:** I think the other interesting thing that might be a follow on to several of the comments that were made is that often when these interdisciplinary kinds of departments spring up in the context of biomedical research or in medical schools, they spring up separately in the medical school from what already exists on the undergraduate campus. So there are many schools that have brain and cognitive sciences or even neurobiology on the undergraduate campus and a separate department of neuroscience in the medical school as if that is a completely different discipline. And I wonder if that's the right way to configure things. It certainly isn't a way to start a dialogue across the campuses.

**MR. LANZA:** A comment about that -- I agree with you but I mean on the other hand, for example, as I said, MIT had this brain and cognitive sciences which was driven a lot by people in linguistics, people in completely -- areas which were not super popular and I think it's resources. I mean, honestly, it's resources and I think the UC system at that point -- do you remember? I think, in the '60s, amazing. My God, they had money. They had all sorts of things and they were willing to try a lot of new ideas. I think, so we're now resource constrained, although, I have to say, the latest MIT -- I don't know if it's a -- what

do you think, Steve -- cross-disciplinary thing or trends there in the college of computing.

**MR. DEL ROSSO:** Schwarzman?

**MR. LANZA:** Schwarzman. Well, that's supposed to be a combination of artificial intelligence. It's going to bring biologists -- everybody in biologists, chemists, everyone will be linked into this common thread of computational approaches and it sounds great and you say that's a good thing to do but the problem is it's going to cost, as we say, done. It's going to cost a billion dollars, and that's serious money. I don't know what they're doing, but do you know what they're doing?

**MR. DEL ROSSO:** No, think about them selling their soul in the process of raising all that money.

**MR. LANZA:** Yeah, boy, I'll bet.

**MR. STEINBERG:** Well, our leader has subtly signaled by standing up and barking out to me and pointing at his watch that our time together has come to an end. Thank you to the other panelists. This was great.

**MR. DESCH:** I think the most optimistic takeaway of this panel was the last point; that it's only about money and, fortunately, in our field, money is no object: satisfactory or not. Our colleague, Steve Del Rosso, has assured us. We have another panel. It's grown like poppyseed on the band side that will start in five minutes, so this first panel really got us off to a superb start, so I thank you, Jim and Walt and Keith, again.

## **THE “DEMAND SIDE” FOR INTERDISCIPLINARY/NONDISCIPLINARY RESEARCH**

**MS. OLSEN:** So my name is Kathie Olsen, and if you see any microphones, (crosstalk 1:50:44.0). I think this panel is fantastic because the last comment was about resources, and this is the panel that really represents the resource. I just want to emphasize the importance of the power of the federal government. I spent most of my career -- I retired about eight years ago from the federal government because they really can make a difference and I want to talk about one comment.

When I was a program officer at the National Science Foundation in the '80s in neuroscience, our colleagues were engineers. And I like to say, if you share coffee, you share DNA, and so we started talking, and we said well, engineering's a system. It's a brain system and so the program officer -- this is not top down -- this is program officers, Nat Pitts and I and a couple of program officers in neuroscience, decided to put some money together and say, okay, we want a bona fide engineer and a bona fide neuroscientist. You need to come together and work on systems that would benefit both neuroscience and engineering so that was the criteria. Basically, the first year what we got was proposals that were the people that were already doing it: the MIT, the University of Illinois Champaign. By the third year of the program, we got such innovative

research ideas that it was incredible for three years of dangling money.

Most of the robotics that we're supporting started in that program in the '80s by program officers and the government setting it up. But I have a concern a little bit today because, to me, when we support interdisciplinary activity, it's because we have a research call or announcement that's interdisciplinary, but we have the individual young faculty member -- and part of this was discussed a little bit earlier -- that basically has an idea. They now have to evolve their idea to actually fit a program within the government because their tenure requires federal support. Okay. To get federal support, they might be very interdisciplinary, okay, yet, if it doesn't fit, then they're not going to get funding.

And I'll give you examples. I work with the University of Notre Dame. I work with the young faculty and I help them, right. We had a grant returned from the National Science Foundation because it was so interdisciplinary -- he got his post-doc with Eric Lander at MIT -- that NSF says that there's no home for it and then returned it without review. In another instance, we had a young woman, a mechanical engineer, working on the brain, working at diorite (phonetic 1:53:33.3), sulfide how it develops with the mechanical forces. She was going into computer science and they said, you're an engineer. We don't want to review it. She rewrote the proposal. She did get funding, but it was like she was stressed because she says, I don't know what to do because, again, these young people are so driven by the science.

And so I just want to say that the government plays an important

role, but you also play an important role with the direction that science, engineering, and technology is going for in this nation. And I wanted to say my final remarks is the fact one of the things that I'm actually most proud of in working very closely with NIH was the fact that co-PIs are considered PIs now. And one of the reasons we did this is because, if you're going to do it interdisciplinary research, the mathematicians, the biologists, the computer scientists working together, all need to get credit, so they can't be a co-PI and only the PI gets the credits. And so we actually did this together, and I think it really changed the framework to encourage interdisciplinary research, but you talk about resources.

The reason on an NIH grant you can only have five total PIs is because it would cost almost a billion dollars to change fast lane, our computer system, and so that's how that decision was made that you could only have five PIs to do interdisciplinary research, again, the importance of resources and how we do all of our programs. So we're going to go straight from the book and Nina's going to start first. Evan, who agreed to do this on Saturday actually, will go last and everyone's going to remark about three to five minutes and then we're going to open it up for discussion. Nina.

**MS. SCHOR:** Thank you and it's a real pleasure to be here. This is like a study break for me. I can't tell you how wonderful it is to be with you. I, actually, almost did the inverse of what Kathie described. I spent most of my career in academia, and I've been at the NIH now for about a year and half. And I

thought that what I would do is talk a little bit about the historical aspects of how the NIH has come to and from, if you will, the notion of social responsibility as part of what the academic enterprise is all about.

I think from the standpoint -- and you've already heard some of this already this morning -- from the standpoint of the clinical aspects of the medical school, there is no question that enterprise interfaces very, very closely with the community around it and with the medical and health needs of that community. I think it's less obvious for the research component of medical schools. And I would say about 30, 35 years ago, if you were applying for NIH funding and you didn't have a hypothesis and a mechanistic component of the studies that you wanted to do, it didn't make any difference how important it was for the people around you, you weren't going to get funded.

And so the emphasis was really on generating understanding of basic scientific principles, albeit, principles that might be applied to health and disease but that didn't need a direct hook into some clinical problem that was out there in the community. I think as the taxpayers, the Congress, made demands on the scientific funders and the community, as things like the war on cancer, the battle against AIDS and HIV, the most recently the opioid crisis, began to be viewed as problems. The solution of which could be contributed to by the medical community. There has been increasing emphasis on the direct clinical relevance of the work that's funded by the National Institutes of Health. I think probably about ten years or so ago, entities called Clinical Translational Science Institutes

became a reality. These were institutes that were mandated, funded, a dedicated pot of money at the National Institutes of Health to fund these around the country at universities. And the notion was that they would bring in interdisciplinary character of individuals together around the health problems of the communities around them. The interesting thing was that, although, I think, at least to some extent, the clinical and the basic science communities were beginning to talk to one another, the CTSI, these Clinical Translational Science Institutes, began to bring in the social sciences because so many of the determinants of health and well-being are social determinants.

Even when I was a resident, like 5000 years ago, we talked about the fact that the most important number you can know about a patient when they come into the emergency room, is their zip code. And I think that's a critical piece of information that the medical community, at least in its research enterprise, managed to ignore for a very, very long time, and these Clinical Translational Science Institutes I think brought us back to the reality that there are many things that are not biologic factors that determine health and disease. I think what has happened now is a swing of the pendulum almost to the complete opposite side, and I'm not sure that that's a good thing either. The truth usually lies somewhere in the middle between the two extremes, and I think we've maybe gone too much to the other end of the spectrum.

I say that because about five years ago, the institute with which I'm affiliated, the National Institute of Neurologic Disorders and Stroke, did sort of an

assessment of our grant portfolio. What exactly is it that we are funding? And we divided it up into basic -- basic, the things that just related to, I think, the pamphlet called it Knowledge for Knowledge's Sake -- basic clinical -- the notion that you could study mechanisms but of a targeted disease or of a targeted therapy -- clinical research and the translational research that was aimed at bringing something from the laboratory to the clinic. And when we looked five years ago, we found, much to many people's concern and surprise, that the portfolio that was constituted by basic science grants was plummeting in size and that most of our grants at that point were either clinical or very obviously translational looking at mechanism and therapies for diseases in animal models.

And there now has been a push to move somewhat back in the other direction, and I think the reason for this is usually not a surprise to academicians. The fact is that the road from the laboratory to the clinic to the community is often neither a straight line nor a predictable line, and so that there needs to be to feed the pipeline into the translational and clinic spheres, there's needs to be some quantity of very basic science research that we perhaps can't predict ahead of time what the application is going to be down the road.

I am currently -- I mean it's funny having been here only a year and a half -- I'm currently leading our strategic planning initiative and one of the major components of that is going to be to try and make some decision about the optimal mix on the one hand of basic versus clinical and applied research that we should be funding. And also and I've heard this morning some allusion to this in other

spheres as well, what is the right mix between investigator and scientific community-initiated research and taxpayer government agency mandated or requested research? How much should we be putting RFAs out there for applications in areas we want addressed and how much should we let the scientific community drive what it is that we fund? And so I'm very much looking forward to being one of the driving forces for asking that question, but right now, I don't think we have a satisfactory answer to it. Thanks.

**MS. OLSEN:** Geoffrey.

**MR. GAGE:** Thanks, Kathie. Good morning. Gray is a great navy day outside. We've got what we call San Diego weather prevailing here in Washington D.C., which is fantastic. It's also an important day -- first day 77 years ago the three-day battle of Midway which was considered to be one of the decisive turning points of World War II. A lot of lore associated with that and something that our chief of naval operations likes to highlight. You know it wasn't -- there was a lot of black law circumstance that converged there. It was also really representative of an incredible amount of resilience that our country showed. Not long after Pearl Harbor, the ability, for example, to turn an aircraft carrier around -- a battle damaged aircraft carrier, the Yorktown -- literally in like single digit days -- three days or something like that in the Pearl Harbor Naval Shipyard -- to get it back out to sea to go engage in this sort of serendipitous battle. Folks were working on the ship as it pulled out to go into harm's way again. It was ultimately sunk, but only one of ours versus four of the Japanese.

And of course, there's the story of the cryptologic work that went on to break the code so it was really -- you had industry at this incredible depth of resilience in the country so a great success story and one that, I think, a lot of it is questioned whether we could reproduce today just based on some of the efficiencies that we've taken so an important day to commemorate and maybe one to look at on and wonder, can we pull it off again?

So it's an honor to be here. Thanks to Mike for putting this on and I've got a couple of colleagues from the navy staff, Pete and Francis, so hopefully during the Q and A can kind of help leaven and fill in the blanks that I might leave with some interjectory remarks here.

I am a naval aviator by trade. I joined the navy not unlike Dr. Meyer to avoid not, in this case, the draft in the army but to avoid going to Wall Street out of college. I was successful. It's been an incredible career, and it's an honor to be here to sort of share a little bit about our perspective from the demand side of interdisciplinary work.

So I decided to do it really quickly through what those in the military refer to as sort of the top five paragraph order. Literally, we can assign any task by describing the situation, the mission, execution, and then some admin, and the command and control aspects. And the situation that we find ourselves in now is one of great power competition. We bureaucratize it like we always do. We refer to now as an acronym GPC for short. We probably oversimplified something that is, in fact, I mean, by any definition, a complex problem. You know, this is not a

Cold War. This isn't even the inner warriors, these are -- we're talking China and Russia, both very different, but rising great powers in addition to some other folks out there. So we're in the midst of a complex problem though between the national security strategy and the national defense strategy and the navy strategy. We actually have what, at least in my career since the early 1990s, has been what I think is an unprecedented alignment of sort of a strategic vision. We've got clear-eyed recognition that China and Russia are, in fact, competitors. We think that China overall's our priority competitor just based on a lot of factors even beyond the military and then, of course, Iran, North Korea, and violent extremists organizations remain concerns as well that we cannot ignore. So we've got real alignment.

We also have a general understanding of what we consider to be key operational problems and challenges that we have to face. They're pretty broad brushed, things like for example, how do we project combat power in a contested environment? something we've not had to do literally since World War II. And those problems can be further and must be further refined. So we also understand though that this competition is characterized not in a binary peace or war type style but we're referring increasingly to a spectrum of competition and conflict. If you imagine sort of on the left side day-to-day peaceful operations, to the right side total war at one end, and the fact that we may find ourselves operating across that spectrum sounds heinously either in physical space you know, somewhere in the Pacific, somewhere in the Atlantic, when you factor in all these demands like

cyber and space in addition to the terrestrial and physical demands. So that's another indicator of our thinking on this.

The mission for the navy remains essentially the basic functions, to do sea control, to do power projection, maritime security, and ultimately the real no-fail missions strategic nuclear deterrents. Those missions have been pretty constant but the environment that we're doing it in has obviously changed. How do we think and what is our demand for getting smarter about this given the new environment? And that's gets to sort of our execution.

The Chief of Naval Operations Howard Richardson talks about a sense of urgency. We divergently compete with China, with Russia, with our adversaries. How do we do that without a Pearl Harbor? How do we do that without a 9/11? I mean, that generally in our society, right, with all our build-in frictions, that's usually what's required is a crisis for us to really get on with something in a united way. I talked earlier that a lot of us question our depth of resilience whether it's the industrial base or just the population in general, their tolerance for something like that. So how do we initiate the sense of urgency without an actual emergency? I don't have the answer to that but, I think, collectively it seems, to me, that we need to be talking about it more and sharing with the population not just our leaders, military and civilian, but also with the population in the sense that we really are in an existential fight for our way of life. So that's a concern for the navy.

Along that spectrum, remember from the day-to-day to the violent

conflicts, we've got a lot of capability, a lot of technical means to sort of fight the high-end war. We've got stuff. We've got ships. We've got airplanes. We've got submarines. We've got missiles, weapon systems. On the left-hand side of the spectrum, we still operate with that equipment, but we also need to think about how we do things differently, more conceptually in this day-to-day space. Great examples are so we've got our ships and our submarines and our aircraft that we fly, sail, and float around the world. How do we operate them differently in the face, for example, of the Chinese adventurers in the South China Sea where they're sort of incrementally doing land grabs short of war, short of a violent conflict? And that's really where we need to think differently conceptually, and I think that's clearly a place where the social sciences in addition to the physical sciences can help us think differently, understanding the problem, understanding the adversary.

One of our biggest impediments is frankly Mandarin. We don't have a lot of folks really throughout the country, but certainly in our military in the navy, that speak the language of our adversary. And I'm not talking about reading their secrets; I'm just talking about reading their newspapers and understanding what's going. We're moving in that direction. but that's one example of an impediment. So we need to think differently conceptually. We need to think differently -- how do we marry technology and the human, the man-machine teaming? We know that the first to get that technological advantage of really leveraging man-machine teaming is going to have an advantage, not just in the commercial sector but in the military sector that was referred to earlier during the

discussion of risk. And, I think, that's a job pretty much for all the sciences to consider.

We talked open-sourced understanding of our adversaries and of ourselves frankly, and then, of course, nuclear deterrents with the downfall of some of our -- with the potential, let's just say the potential change, in the treaty environment, the development of new nuclear weapons, us going to some more tactical nuclear weapons. That might require a revisiting of some of the nuclear deterrents theory so a real potential for thought and contribution in that area.

We've got some in the navy. I won't go into detail now but, having to talk about it later, some internal initiatives given the understanding we need to move out urgently. We need to become a better learning organization and notwithstanding the fact that we're part of the joint force obviously responding to the president and the secretary of defense. The CNO, chief of naval operations, in addition to being responsible to man, train, and equip the navy, he's also part of the Joint Chiefs. He has to advise them, and so what does the navy bring to that fight? And we are internally doing some organizational redesigns to try to tap into some latent capacity for learning and getting smarter within the navy lifelines, but also looking to develop a broader ecosystem of network, of again, not just technical but also the social sciences outside. But again, we don't even know, in some cases, the questions to ask which I think we all understand is the first step -- is asking the right questions. So I look forward to your questions and suggestions and with that, I yield the floor to the front.

**MR. RITCHIE:** Thank you. Thank you, Geof. It's good to be with you here today. I appreciated this opportunity that Mike has given us to explore a little bit on the demand side. I think that as someone who is a political economist by training, I'm deeply familiar with this debate about interdisciplinary research. And now finding myself doing commercialization inside a university, I also see this demand side in terms of the need to get from the university out into practical application in people's hands the things that are being discovered. The Milken Institute estimated, just looking at patents inside a university, that over 80 percent of what gets discovered in a university stays inside the university. So there's a lot -- and I'm saying that's a good or a bad thing. I think there's a lot of research that is done that needs to be done just as an exploratory effort to then build on other things.

But there's a lot of things that could have a practical application and do good in the world that never sees the light of day. And part of the challenge is that the distance between that discovery and somebody using that in the private sector whether it's in the clinic or whether it's in industry or whether it's in wherever that discovery is best suited. We call it the chasm of chaos and you're not even to the wilderness yet. This is long before you think about how it's going to get funded and who's going to lead it. These are problems -- these are solutions looking for problems, right, I mean in a lot of cases, you don't even know what you have.

And so I think that there's an opportunity to think about how do you

move from just the research side to also the development side, you know, this translational research? How do we engage not just multidisciplinary or cross-disciplinary but multiorganizational and cross-organizational kinds of research? And these are difficult challenges especially given the way that universities are structured. I loved the comment that universities tend to be politically liberal but structurally conservative, and I would say maybe even more than structurally conservative, maybe we're just like concreted into place. I love listening last night to the keynote speaker who was talking about her institution that's still thousands and thousands of years old at this point, more than 1000 I think or close. I don't know. But anyway, the point is very old.

So there's really three areas that we've been thinking a lot about in terms of what drives from a demand side this cross-disciplinary research: One of these I'm going to talk about in terms of just once we get a discovery and the process for commercialization is often we'll get a disclosure of a discovery that's been made by an academic, and then, because it's so early, we'll find that there are other things that need to be done. And by the way, this might be social sciences, social science research, but this could be chemistry research that needs biology help, or wireless research that needs nano- help or any number of different kinds of research that would finish out or finalize that research. And so, what we'll often do then is go back to that researcher and say, for this to be commercialized, it needs this other component. Do you know of anyone else who does this kind of work? And often there will be someone inside their own department or across

inside their college that can help them with that research.

The second kind for us is really being driven by cross-university research. I'll tell you a quick story. When I was at the University of Utah, we had discovered a quantum dots technology and we thought we were -- we were pretty excited about it. It had great potential and we talked with Samsung and we talked with a number of other big companies that might use it, and their comments -- in fact, this is almost always the comment we get when at first is discovered is -- that's interesting. Get it to here and then come back and talk to us again, and we'll see if we can't do something with that. And so we thought we were fairly unique. As we went out though, we found out that probably 30 other universities also had quantum dot technology. And we started to go down the path of saying, could we combine the intellectual property from all of these universities into one portfolio which we could then take out to industry and that we could see if there was something that we could do with that?

It turned out to be a very difficult process because of the (inaudible) policies that -- what happens is universities now own that intellectual property. They don't have a good feel about how to combine that intellectual property, how to work with other universities. And over probably a 15-year period of time, we've gone back now. It turns out -- this is kind of painful to talk about but -- so Samsung has implemented quantum dots technology and none of it came from the university. They actually re-engineered around all of the intellectual property that the universities had come up with. We got together with about eight or nine of the

universities. It included MIT and Cornell and Stanford and a number of others, and we just did some research on outside a small molecule drug discovery, how many technological discoveries don't work that had been adopted by companies and implemented in a way that made more than a million dollars to the university? Do you know what the answer is to that question? The answer is zero, which is a really interesting outcome given the opportunities. So there's something here. There's an opportunity there. If we can think about ways to motivate and incentivize universities to work together with those intellectual property discoveries had we been able to go with a full portfolio, I think the outcomes would have been very different.

The final area I will just touch on really quickly is actually being driven by government, and I'll say local government, and companies that are coming to the academy looking for help in their research areas. They may be bringing in the intellectual property or they may be just bringing the problem. We've got a number of these where companies will come and they will say, you know, we're trying to solve this problem and we might have part of the answer, but we're looking for the rest of the answer. Can you help coordinate the right group of people together that will solve this problem? Almost always, that includes more than one discipline. A problem requires more than one discipline to get connected. But we can act as a funnel or a connecting point that brings in those opportunities then works with the researchers on the university side to solve that problem. Again, this is often translational in nature. It's not so much in the very

fundamental research side, although, sometimes we can take some of that research that's been done there and work through that. They also come with resources to do that. They'll bring commercial-sponsored research or government-sponsored research.

In South Bend right now, many of you have seen Pete Buttigieg now is a presidential candidate. He coined something that I thought was really interesting. He said, South Bend should be a beta city. This opportunity that we could take technologies that have been developed at the university and actually try them out. Let's take them and see what we could do. There were some sensor technologies developed at Notre Dame that we used in South Bend that are now a company that does sewer analysis about the water volumes that comes through. And turns out it saved the city almost two million dollars a year then created a company that was just purchased by a large water conglomerate as well. It was a very successful outcome.

But we're doing all kinds of things in terms of neighborhood management and people observation. What happens in different neighborhoods and how they gentrify? How they grow? How they develop. There's a real confluence of social science and also hard science research that's coming together in some of those areas and anything that we can do from a policy standpoint. I think about the ways universities influence policy but also the way policy influences universities that would encourage this collaboration around intellectual property, implementation, what we call de-risking in the sense of moving these

early discoveries through the process to being actually having impact in being used in the market place I think is a very important opportunity. So I'll stop there.

**MS. OLSEN:** Matt.

**MR. STROTTMAN:** My name is Matt Strottman. I'm the chief operating officer at In-Q-Tel. For people not familiar with In-Q-Tel, so we're a 501(c)(3) nonprofit. We get funded by the government, the intelligence community specifically, and invest in innovative start-up technology companies. So, in case you haven't figured out, I am not an academic. I'm completely a practitioner. I did go to Wall Street outside out of business school to go do technology investment banking before coming to In-Q-Tel.

Essentially, what we're looking for and I don't know if this is going to be great news for the academics as far as academic research, but what we find it's very similar to the government where there's almost a bubble set up around an ecosystem where, I think it was to Brian's point, where there's -- technology's being developed by a certain ecosystem but it's not necessarily world class and it's not necessarily being able to be productized. And so, by that, we mean there's not -- because we completely look at everything from what is the problem that's trying to get solved and what technology can be applied in order to solve that problem?

And so, I guess, by that nature, I think the term interdisciplinary has been thrown around. I think, by nature, that's what everything that we're looking for. It's because we're not necessarily going to care about if it's a neuroscience problem or a data problem or any other kind of material science physic problem.

We're more looking at it and saying, how can whatever technology come to bear come to be to solve whatever that problem is? And so we have the unique lens as far as we're not trying for a financial return; we're really trying to solve and use these innovative technologies that are out there to solve a lot of the government's problems that they may not be seeing because they are operating within that bubble of people who actually are following what's going on in that ecosystem. I think our belief is there's no monopoly anymore as far as who the smartest people are in the room. We have to go out and actually search those people out and find them.

So whether they reside in government, academia, commercial sector, that's really what we're looking at and what we're driving for. And so, for context, we make about between 50 and 60 investments a year. We have about 500 portfolio companies and meet with over a thousand companies every year, so we have a pretty broad context about what those -- what's going on and so quantum dots were brought up. We had a couple of quantum dot technologies. Again, none of those eventually -- and I think we did actually have the one that Samsung ended up buying, so it was one of our portfolio companies. But I think really what we're looking for is -- and we actually don't invest a lot out of the universities, and it's tough for us to go through and even invest out of stuff that's coming out of the government just because the thought process is so different from how it is that the typical leaders that we deal with operate under.

There's always, in venture capital, things are financed to a milestone,

and so what we're really looking to drive towards is what's being achieved for a certain dollar amount and how long is that going to take? And so that's really what we're kind of driving for on these companies that are serially financed. And what we're looking for in our business model is to we want to make sure that there's something consumable by the government at the end of that product at the end of that process, and so we're applying our dollars towards developing or enhancing the technologies that already exist with a lot of these companies. And we're really trying to get to that pilot stage with the government.

In-Q-Tel's been operating for about 20 years now and we've really gotten that to be more of a machine. We can know that it's about 75 percent of the companies we invest in get to that pilot stage, and then what we hope for is then the government actually buys those on their own. That tends to be of those piloted technologies about another 70, 75 percent so roughly a little bit over half of the companies that we're investing in actually get purchased and used within the government. And so I'm curious to hear what questions people have because this is a different environment than I'm used to speaking in, so I'm interested to get context here, but I'm happy to answer questions people have.

**MS. OLSEN:** Evan.

**MR. HEIT:** Okay. Thanks. Evan Heit from the National Science Foundation. I'm division director of the Division of Research on Learning and temporarily I'm helping start a new program at NSF that's called the Convergence Accelerator, so I'm going to be telling you about that today. But let me just say,

first of all, I'm so happy to be here today. Thank you for the invitation. I'm glad it worked out that there's been just such an interesting set of presentations. I'm going to try to keep it going and try to address some work-related issues, and I even brought a few slides here.

First of all, I just wanted to comment on some of the points that were raised by Walter and also Kathie. Both had spent time at NSF. So I'll say that at the National Science Foundation, we fund both disciplinary and interdisciplinary research including both highly interdisciplinary research and moderately interdisciplinary research. Our strategy perhaps is to have a diversified portfolio where we don't invest in only but one kind of work.

To give you an example of this, I'll be telling you about this Convergence Accelerator Program. It's very interdisciplinary and we've had some pushback from the field. Well, why aren't you investing in more basic disciplinary research? And what we'll respond is, first of all, it's a pilot program. As you can see, there are always trying new things. We want to see what works. We just didn't want to do what we've always been doing. But also, it's less than one percent of the NSF budget, so there's not any one crux that dominates what we do. As we fund new disciplinary research, we're certainly aware of challenges, but because we place so much emphasis at NSF on the review process, we think about just how do you review an interdisciplinary project as opposed to something that's just from one discipline. And we always have this concern that if some projects and proposal encompass multiple disciplines, if it just goes to a single panel, it

might not do well. There might be conservatism. It might look like it doesn't have enough from a particular discipline, so we do sometimes a more internal review, say, by program officers rather than an external review, and that's one way to try to address this conservatism or potential conservatism.

But I also I was saying to Walter earlier, his challenge just how do you define interdisciplinary? is something we do think about a lot in NSF. I thought his challenge was how do you define interdisciplinary without referring to disciplines? I thought it was kind of nice puzzle. I was just looking at our internal rule book yesterday. I forgot how we identify interdisciplinary proposals and there were three criteria for a particular program we call rays (phonetic 2:27:38.1). That's particularly for interdisciplinary projects. Rays projects are defined as projects that don't fit within an existing NSF program, that they lie with the boundaries between two or more disciplines, and also they have the potential to be transformative. So at least two of the three criteria do refer to disciplinary programs so that's a really nice challenge.

Now let me get to telling you about the Convergence Accelerator at NSF and, in particular, just what is the Convergence Accelerator. It's not business as usual. It's definitely a pilot program. We're really looking to see how this goes and looking for feedback from the field, getting back to make various comments. Like the Horizon Program just for them, the Convergence Accelerator is a pooled budget, so it's not a budget that belongs to individual disciplines and that could have interesting effects on our decision making. And if you look at the next bullet

point and this is really why I make this slide because every word is very carefully chosen here. So this is kind of the key point. The Convergence Accelerator is meant to accelerate from discover to use -- to use the phrase that was used before - - in a fairly rapid way, getting things out of universities and how to serve it to the public, is focusing on use-inspired research, so not pure basic research but research with some use and application in mind right from the beginning. It is convergence research that we're supporting.

I know NIH has also used this term. I'll give you my definition of convergence research, which is compatible with how we'll talk about it at NSF. It's research at the maximal level of interdisciplinarity that addresses a grand challenge. And I would say these two criteria go together well because, as others have said, being able to address a grand challenge, an important societal need, often entails having people from multiple disciplines who are taking multiple approaches and trying to, indeed, come up with new approaches. We are funding projects in areas of national importance so, of course, we're a federal funding agency. That's our goal, but we think that this particular mechanism's particularly appropriate. And finally, we are funding partnerships between academic and non-academic stakeholders, so this perhaps is most clearly how it's not business as usual at NSF.

So we typically fund researchers at universities. If we get a proposal to Convergence Accelerator that's only researchers from the university, we wouldn't fund it from this program. ,So if you're at a university and you need a

partner from industry or a partner from a nonprofit, for example, and again, being able to do something that can translate from ideas and discovery to application's something that we think it's -- who's could be particularly useful to have these kinds of partnerships.

The way it's going to work -- and I should say that at this point, in fact, I'm going to go back to the office this afternoon and help review proposals -- we are going to be starting about 40 projects in September that they're going to be team projects that are going to work in a cohort. They're each going to receive up to a million dollars each for further building out their teams and research planning in a relatively short period -- just nine months -- nine months after September, next spring they're going to pitch for Phase 2 funding. This is sometimes called DARPA/MIKE (phonetic 2:31:08.0) or DARPA/ESTO (phonetic) the way we're doing this. And a smaller subset on the teams -- about ten teams -- will receive up to five million dollars each, and the key word here is that there's an expectation of deliverables. So we're looking to support teams that will have some concrete tool, platform, application, solution that could serve the public within two years from, I guess, next spring. So that's the way it's going to work.

You might be asking if it's a hidden kind of structure now or which is what are these projects about? Here are the national challenges that we're initially addressing. The first open knowledge network is addressing the challenge of building our national data infrastructure. So you're surely familiar with Siri and Alexa which are proprietary knowledge, and it works that can answer questions

using proprietary data but what about the data owned by the American public, U.S. public data, whether it census data, geographic data, economic data, and so on? What kind -- educational data -- what kinds of questions could those data answer for individuals, consumers, industry, scientists, and so on? So we're supporting building these open sub-adjutant works using U.S. public data.

And second and third themes are related to future work of human technology frontier. So, when I came here this morning, I took an Uber. I don't know if anyone else took an Uber and like driving many jobs including jobs that many of us in this room have the potential to be greatly changed or Uberized in the coming years. On the radio was a commercial from Indeed.com, which is a website that matches up employers and workers. It's probably not the way most of us found our jobs, but increasingly, that's the way people find jobs in the future. The OECD and others have pointed out that many jobs are going to be transformed, if not eliminated, due to changes in technologies.

So AI and future jobs, that track of the Convergence Accelerator, is aimed at building tools for individual learners, individual workers to anticipate where jobs will be going in the future, what skills they have now that might be relevant to future jobs, and how they can prepare for future jobs and incorporating educational technology as well. The National Talent Ecosystem complements that. It's a way of supporting employers. As they look at their workforces, they look at skills they have so they have -- and coming up with platforms, tools, recruitment mechanisms that will help them build their future workforces as they

try to address new technologies -- say, data science, AI -- and someone really might not have all the skills that they need now.

So I'll close with a request to all of you here. So I told you what the three tracks are in the convergence -- oh, thank you, yeah. So you mean, if I just turn the piece of paper, it doesn't turn the slide. That's really interesting. Okay. Yeah, okay. Thanks. So this is perhaps the most important slide where I ask for your ideas. I've shown you the three tracks that we're starting within the pilot. Next, we really want the ideas from the public whether it's people in industry, people in nonprofits, people at universities. What should the next tracks be for the NSF Convergence Accelerator? Perhaps the most important thing it's Googleable, it's DCL, which stands for Dear Colleague Letter, 19-065. If you Google that, DCL 19-065, it will take you to a request for information. It's actually just a short survey where we're asking all of you what tracks should we try to apply this mechanism to in the future.

We're looking for new topics that require a convergent research approach that are a potential interest in academic and non-academic stakeholders. We know in this program, we want to fund things that simply stay in universities, address a challenge, a grand challenge, both for science and for society, and finally are ready to be accelerated within a period of two to three years to practical solutions. I'll just conclude by saying thank you so much for the opportunity to talk, and I really do hope people will have a look here and send us some good ideas.

**MS. OLSEN:** So I'm going to ask the first question among the panel and then open it up for discussion. In the previous panel, Richard Lanza made a statement a couple of times. It says, what is the problem? And when we were listening to these panelists, they were talking about sort of the -- and I say the challenge is for their institutions including Dr. Cavanaugh (phonetic) who is representing another federal agency in a sense of supporting research. All the organizations do it differently from top down, bottom up, et cetera, but what are really the problems. How do you address the problems that you need to be solved? And what are you thinking about in terms of doing things differently so that you can really like this program where you're trying out this pilot so that you can work with academic, industry, the federal government, the state governments, so really to solve some of the societal problems that we're facing not only here but around the world? Who wants to go first? Okay. You've got the microphone.

**MR. RITCHIE:** Okay. I'll go. Thanks.

**MS. OLSEN:** While still keeping the importance of basic research.

**MR. RITCHIE:** Yeah, so I think that the biggest challenge for us is the things that are discovered are so just so far away from practical application. This is what I think Matt was talking about a little bit. In terms of moving things through a milestone-driven stage-gated process that takes them from discovery to the point at which somebody can use them is a lot of work. It turns out universities are great at research; they're not very good at development. I'm really actually pretty happy to see what Evan -- right, it's Evan -- was just talking about

here in terms of not only are we interested in solving these big challenges but in terms of a scientific breakthrough but we want to get them to actual practical application, so that gap is really the challenge. And then you'd have everything that goes into that skill's gap in terms of funding, leadership, scientific capacity and ability, bringing the right stakeholders in both inside and outside of academia and that just requires a group of people who are focused on that both inside and outside in terms of a partnership to make that happen.

**MS. OLSEN:** Yes.

**MR. GAGE:** I'll take a stab at that. I mentioned some of the initiatives we're doing internal to the navy. We recognize just within the lifelines people who are already in uniform or that we could order around. There's like in the hundreds if not thousands of folks who are either going through navy post-graduate school, Navy War College, other graduate-level educational opportunities that we really just sort of let them do their thing. And with just a little bit of focus and alignment, we could tap into that, not to deny them the academic freedom that we want them to have. But we really don't provide that necessarily, so we have some control within our own lifelines. And then, externally we're trying to figure out what that looks like, that outreach, and we've got, again, through the technological research, the research labs, and what not, that system is pretty well set, but in the social sciences, it's much more informal and so that's kind of work to be done.

Brian and the folks talk about this idea of getting something that's

sort of ready for the market. That's a huge impediment with DoD. We talk about wholeness. You can't feel the weapons system a widget unless it's whole, i.e. yeah, you know how to build it, but you also have to sustain it, right. You have to train people to use the widget. You have to integrate the widget into existing platforms, and unless you're whole, it's really hard to get anyone to buy a new system that may be otherwise a real asymmetric advantage. Well, take a look out in the real world. Who are the Houthi rebels there over in Yemen? No one would say that they have a whole system of coastal defense cruise missiles, but with just a couple of truck-launched missiles that they've lobbed out into the Strait of Bab al-Mandeb in the Red Sea area, they caused everyone to operate differently. Major navies had to operate differently. So turn the table, what can we do if we admit maybe to a little less than the wholeness on some of these asymmetric advantages? And, again, that's material capabilities but also different ways of operating, and that's where we're looking for ideas, crazy ideas. I don't think anybody's been thrown out of the CNO's office recently for having a crazy idea. We're looking for those. So we're sort of victims of our own conservatism in that regard, but really on the nontechnical side, we're not even sure of the questions to ask but we're open to that.

**MS. OLSEN:** Eric, you want to go next?

**MR. HEIT:** Sure. I'll just say that at NSF, we pride ourselves as being a field-driven funder, and most of the ideas we get are the best ideas from the field. And that there are a lot of advantages to that, but perhaps one

disadvantage is that there are always these different topics, and they often work individually just addressing separate problems. Everyone in the room could be addressing a somewhat different problem in this Convergence Accelerator approach, which is just a pilot, and we'll evaluate it and I hope it's going to be successful. And then when we learn, I hope that it's going to be successful. I think that part of the secret sauce, part of the reason that we think it's going to work is that we are funding cohorts who are all working on the same problem. So, if you are supported to work on jobs of the future and aids for workers who are trying to prepare for the next job, there are going to be 19 other teams in the room with you, and you're going to meeting them potentially over the past year. So this is an opportunity for us to pose challenges to the field and get some kind of coherence that, I think, is different for NSF but I think it has the potential.

**MS. OLSEN:** Yes, and then, yeah.

**MR. STROTTMAN:** Yeah, and so I guess we look at it a little bit differently because a lot of the discussion's been around funding, but some of it's just about coming up with the ideas and getting the idea people around the same problem. So, like for example, we have an initiative that we worked on, didn't really cost us any money at all; it was called SpaceNet. And so the idea was one of the agencies that work with is the National Geospatial Agency, which has a ton of geospatial imagery, and through one of their partners that they work with DigitalGlobe, and we just partnered with them, who gave away free imagery data, and now we can put that out there, open sourced it, and allowed people to go out

and create -- basically use it as a data set for any of the -- to try and get their algorithms for their machine learning AI algorithms.

And sort of stuff like that didn't cost any money for anybody enables us to, basically, encourage people to come use that data set, to train their algorithms, and eventually solving a problem for one of our customer agencies. But that agency didn't have to fund anything. And so a lot of it's just looking at it differently and allowing people to come and gravitate toward it who have an interest in something that's out there. And so a lot of it's just using information that either the government or the universities may actually have that's already out there. How can you utilize it, make it more open and free to allow people in order to come and be attracted towards what it is that people are working on, build that community of interest?

**MS. SCHOR:** So I think that in the biomedical community, we're still extraordinarily good at solving the very small, very well-defined problems, the problems where you vary one thing at a time and ask did anything change. But those are not the real-world problems with which humans are faced. And I dare say we are just barely scratching the surface of even understanding how to frame the messy problems that humankind seems to get itself into. I think the interesting thing for me -- interesting is a kind of a euphemism, I think, in this but -- the interesting thing for me is the fact that we have now gotten computationally so good at collecting absolutely everything we can think of collecting and mining those data to find the proverbial needle in the haystack that we have stopped

posing hypotheses in thinking about ideas.

And I think the problem with that approach is that -- I mean, we used to call it a fishing expedition and now we call it unbiased approaches. I mean, talk about euphemisms. I think the problem with that approach is that we are nowhere near discerning which of those needles in the haystack is a significant or important needle and which one is an epiphenomenon. And I think that's the next level of development of technology that has to evolve. I think it's fine to create ways of collecting data, but unless you know that what it is that's important is in the set that you're collecting, you're going to miss the mark entirely or even worse, be completely misled. So I think data science for us in medicine is certainly more advanced than it was even five years ago, but it's nowhere near where it needs to be in order for us to decide what does or does not matter.

**MS. OLSEN:** Question? Yes.

**MR. DEL ROSSO:** Thanks. Steve Del Rosso. I wanted to follow up on Evan's mentioning of this focus on issues of national importance for the Convergence Accelerator, and obviously the three national challenges you talked about seem pretty benign and seem to be issues of national importance. Drawing from the discussion last night of our keynote where Ms. Richardson was offering some cautionary words about a potential too cozy a relationship between the academy, the presumably pristine objective academy, and the dirty potentially maligned policy world -- I'm exaggerating the point -- but her argument being that the academy should keep its distance. Now, this is more a question of the national

security realm, but we all know about the controversy over the Minerva Project, the Human Terrain Project where anthropologists were embedded with military units in Iraq and Afghanistan. Geoffrey, you and I had an interesting -- we haven't met personally. We met on the phone and had an interesting conversation the other day about the navy's interest in behavioral psychology. The question is then how can you assuage the concerns of people like Louise and others in the academy who feel that there may be unintended consequences of some of the research they're doing not only unintended, but if they knew about the intention of sort of dual-use idea, then we would not be too pleased about it. How can you assuage their concerns that these ideas that come from the academy may not be used for issues or problems that may be deemed pernicious from the academy side?

**MS. OLSEN:** Yes, we're going to get back here and --

**MR. HEIT:** Well, thanks. I think we might have whole lot different answers about this, but at NSF, we're very careful about what we do and what we don't do. And from our founding, I believe, in 1950, we are supporting the advancement of science, but we're also supporting the prosperity of the nation. And, if you look at these particular topics, that they're linked to that in a fairly transparent way. We've also been supporting at NSF, and NIH has really taken such a lead in this, open science where we're trying to encourage everyone who's supported by NSF to share their data, share their publications, and in an open way let everyone know that just what it is that you're doing.

And finally, I would say that we're supporting tools here. We're

supporting platforms. Something we don't do at NSF is we don't try to influence legislation. For example, we're a non-political organization. We certainly would like to fund research that would inform policy, but we don't recommend policy. So we try to have a dividing line that way between policy and what we support, but that is something that we need to be very careful about. When you first said that, I thought you were actually going to say, how can universities be careful to be insulated from what goes on in industry? You can kind of raise that point as well, and we certainly at NSF are in a pilot now where we're navigating these issues and trying to do good. Thanks.

**MS. OLSEN:** Does anyone else want to talk? Yeah, I figured. Geoffrey.

**MR. GAGE:** I would just -- I understand the point from where you're coming from. I'm not an academic, but I referenced the Midway effort. Someone else earlier talked about the Manhattan Project. We, in the navy, love to harken back to the glory days of the 1980's maritime strategy, which I think one of the beauties of that -- actually that's where I sort of started learning about this stuff while I was in college -- is that folks, social scientists pitched into the argument whether it was Mushimer (phonetic) or folks. They engaged, but ultimately, I'll say, you've got to pick a side. People who were in the Manhattan Project picked a side. So yeah, there are going to be some people that are not comfortable with that, and that's great and I honor that but at some point, this system that we're trying to defend that we need the help of the sciences and social sciences, like I

said, I think it's an existential -- we can have existential problems so some folks are going to have to get on board and take a side. That's my opinion.

**MS. OLSEN:** Okay. Go and then I'll have you.

**MR. JENTLESON:** Bruce Jentleson from Duke. It actually starts at the same point as Steve's but it's about the national sense, and I think this may relate mostly to Evan and maybe Brian and Nina, but anybody else. As I think about these programs and our efforts to innovate and I think about your mention of the opioid crisis has come to the attention of your institute, and my concern sometimes is in sending the university world, we tend to associate with our peer institutions. I'd like to mention last night; community colleges are not really members in the American Political Science Association is one example. All of this is extremely important. Is there a way of factoring in a non-just, you know, silly interest group or a political way sort of differential needs in the country, and I'm thinking here of parts in the country that have been demonstratively left behind technologically and in a public health respect? And so, if you get any proposals for AI, for example, this may go into the panelist at lunchtime.

We're not the truck drivers, right. We have an environmental impact statement we started a number of years ago on various projects. Is there a way of factoring in -- you've got two proposals for different things or for commercialization or whatever, but if one -- and I'm going to overstate here -- might just really help the coast, right, they sell a quarter in Northern California and the other might actually have a significant relative impact on parts of the country

that are suffering in terms of innovation and other opportunities. Isn't that something that should be factored in in terms of how we set our priorities on a national basis or even in an opportunistic way for universities and commercialization.

**UNIDENTIFIED MALE:** I'm glad I came today. This is great.

**MS. OLSEN:** Nina, do you want to start or Brian? Brian and then we'll have Evan.

**MR. RITCHIE:** I think this is a really interesting -- one of the things that we've recognized at Notre Dame is that commercialization -- really there's two ways to do it. Okay, I mean, you can be -- you can do it the MIT or Stanford way. We could talk about with just being incredibly gifted at being located in the right place, right, or everyone else has to use the process. And, I think, that if we can figure out how to do this, you know, some of the most interesting communities that are up and coming right now are places like Boulder, Colorado and Provo, Utah and Gainesville, Florida and some of these other places that are taking their research university -- and by the way every one of these innovative societies have at their heart a research university but the reverse is not always true. Just because you have a research university doesn't mean you're going to be an innovative society so ways in which those are connecting -- and for us we certainly are asking what -- we have limited resources and so we're saying which has the most opportunity to do the most good for the most people? We're certainly trying to prioritize, but the problem is, and I think Matt would agree with

this, is you don't know (inaudible) a lot of times. I think a lot of us looking at Facebook 20 years ago would have had no idea. I can remember when it was MySpace and Facebook. I'm like, what a dumb technology. Whoever invests in this is an idiot, right? And then look what it is today. It was, I don't anybody foresaw that, and so the ability to look into the future and see what's going to matter and what's not going to matter, or how it might be used for good or for evil, is really hard to know. With that said, if we can get the right processes in place to evaluate these things so that places like in my case, South Bend or Toledo, Ohio or Mumbai, India, wherever can figure out how to do it innovation and do it well, then we just might change the world outside of the coasts because nobody's going to be Silicon Valley again, I don't think. But could you Provo, Utah? Probably, if you could.

**MR. HEIT:** (Inaudible).

**MS. OLSEN:** It doesn't matter.

**MR. HEIT:** Okay, I'll just, first of all, talk about truck drivers. In the work that we're funding at NSF on future work of the human technology frontier and also the Convergence Accelerator, because we're taking a convergent approach, we're not just funding projects that are going to be tools and technology that are just going to make cars work without people, that these are projects that are including social scientists, education researchers, and so on. And built into the projects, built into our expectation is that they are addressing the societal implications of these advances as well.

More generally at NSF and anyone who's ever applied for funding at NSF or served on a panel or worked at NSF, you'll know that we have two review cards here. For every proposal that comes in, one is intellectual merit which is how it contributes to a particular scientific field. The other is broader impacts: how it has a positive impact on society. And there are lots of different ways that that's defined, and every single proposal has to be scr- that we fund has to be strong on both. I'll give you one example from my own experience. In a previous life -- not that long ago -- but starting in 2005, I was a founding faculty member at the University of California Merced, which was the new University of California campus in the Central Valley of California, which is kind of like California's Appalachia in terms of being educationally underserved and underserved in so many ways. From the time the campus was being founded, it was majority first-generation college students and minority students. And I could tell you that we did very well in NSF funding from early on in the early days of UC Merced, and we hired interdisciplinary faculty. We hired people who, I think, were very good but it was also that they were able to put in proposals that demonstratively not only had intellectual merit but they had boarder impacts as well because they were able to say that each research grant, each dollar you give to this university is not only going to get scientific research done, but it is going to build infrastructure. It's going to build educational opportunities in part of the country that hasn't been served before.

**MS. SCHOR:** At NIH, we think about this I mean very much like

NSF all the time and so, for example, there are mechanisms for grant funding that you can only apply from if you're from a state -- we call them idea states -- but they're states that are underpowered relative to the coasts if you will, relative to overall NIH funding, and you can't apply unless you come from one of those states. We have in some of our nationwide initiatives things like the All of Us initiative where it's another one of these data based generating things following a cohort of individuals from pre-birth all the way through their first 20 years of life. We have made really special efforts to get into communities that don't typically participate in clinical research studies and to actually make this not only appealing to them to be a part of this but to make it easy for them. People who don't necessarily have transportation to get the place or don't have an MRI scanner in their community or whatever it is.

And then the other thing I wanted to mention that's a little bit outside of that box, but, I think, will get us to asking about populations that maybe we don't serve as well as others is that our BRAIN initiative, which really to date has been an initiative at developing novel strategies and technologies for looking at the nervous system, now has a neural ethics component that I know Kathie has been involved in as well, but that is -- I always feel as if the BRAIN initiative asks what can we do and then the neuroethics group looks back at us and says, but should we do it? And I think that's a critical piece of this to ask, how do we get these technologies to people to would stand to benefit most from them or how do we avoid their use for nefarious purposes?

**MS. OLSEN:** Bob.

**MR. LATIFF:** So want to respond directly to this question.

**MS. OLSEN:** I figured you did.

**MR. LATIFF:** I should point out that I spent many, many years in the military, and I am probably as cynical as they come about military operations. I guess my question is or my answer is more of a question. Do we want our mil- -- they're going to do it anyway whatever it is. Do we want our military to operate stupid? Or do we want to help them deal with a smarter -- I hope they do? We saw this with Google with some drone targeting thing that Google decided not to participate in -- saw it with Microsoft. Microsoft said thank you for your interests; we're going to do it anyway because we think it's a good thing for the military to have. If the university doesn't want to participate in developing new lethal technologies, I get that, but it shouldn't be one size fits all. We should look at if we want the military to be smart about what they do, we should actually be willing to help them. So that's my speech. Sorry.

**MS. OLSEN:** And I'm going to let Michael ask the last question or the last comment because to keep us -- well, unless you give us five more minutes.

**MR. DESCH:** I will give you 15 more minutes.

**MS. OLSEN:** Perfect. Oh, this is great.

**MR. DESCH:** In my beneficent, but these were all really terrific presentations and lots of really interesting issues to talk about. I just want to ask one question to Evan because this Convergence Accelerator program looks very

exciting both the effort to foster interdisciplinary particularly the grand challenge piece of it.

NSF though, historically, seems to have struggled in this area. I mean, the origin story of NSF -- I mean, it didn't start in 1950; it started in '46 but didn't get off until '50. A big part of that had to do with the balance between basic versus applied research, the Mansfield Amendment in the late 1960s. A broader impact criterion, as I recall, came about as a result of the intervention of Barbara Mikulski in the late 1990s and then by this issue. So what that history suggests to me is it's a challenge and the question is what is the source of the challenge that makes this a recurrent issue that NSF keeps having to come back to and try different ways to address. Have you thought about it in that context? Or how are you viewing this in terms of the previous similar initiatives in NSF history?

**MS. OLSEN:** While he's thinking about it being that I was the deputy director of NSF, this is something that I did. Louise Richardson made a great comment last night and she was really talking about the, I like to say, the privilege of doing fundamental basic research. And this is sort of my concern now with China and some of those countries too because they're getting to a point where they cannot just invest in more of applied but they can invest in the C-CORE in terms of fundamental research and NSF has always had that yin-yang on you need to keep the individual investigator, okay? You need to keep interdisciplinary research. You need to keep that balance but what is that balance? And when they started the Science and Technology Centers and the ERCs, the

Engineering Research Centers, it's still only what about five percent of the total NSF budget or maybe ten percent, yet it still causes all that angst.

And I'm very interested about the NIH now and the neurologist to relooking at that balance because you need that fundamental research but then it just sits there, and it doesn't get out of the university. It doesn't get to you. It doesn't get to the military, then we're in that ba- -- so this is something that is constantly being revisited, reassessed, and I do believe similar to your new programs too, they're based on societal issues, but you're going to have the fundamental research and the applied all together. We don't do it right, but we have to have those pilots, and we constantly need to have that balance and relooking at it. And you know, you talk about for the (inaudible) car from going to pure basic or like you were saying hypotheses driven to the more of applied. We swing this way; we swing this way. We've never really found that balance right there.

**MR. HEIT:** Well, I'll talk about two challenges. One is the challenge of bringing research to application. And the other is the challenges supporting interdisciplinary research. So the challenge of bringing research to applications is not something that I think applies to all universities, and it's not distinctive to NSF, but I am reasonably optimistic. Although we're doing a pilot right now, that the particular way we're addressing this is based on the program at NSF has called I-Core, which is Innovation Core, and it's been a successful program that has been adopted by other federal agencies that allow individual

researchers to start to commercialize their research. They go through a cohort. They go through entrepreneurship boot camp and so on. It's been a fairly successful program, although, as I was saying earlier, we tend to find a lot of different projects on different topics. You might have 40 different I-Core projects in this room so by following this I-Core model yet having a focus cohort coherent portfolio addressing a particular problem in some ways, it's not a radical change from some program that's already successful now. But we're doing a few tweaks that are addressing these particular issues. So maybe you'll have me come back in a year or two, and I hope we'll have some good news because I know we've been addressing this for a while.

The other challenge is funding interdisciplinary research and I was chatting with Walter earlier who worked at NSF in the 1990s and some of the challenges that we are facing now, just how do you define interdisciplinary research? How do you support it? How do you review it? How do you lead, help individual researchers? New faculty, how do you help them succeed? That's something that I know that you've been working on for decades, and I think they're still some challenges there. So you mentioned earlier someone you work with sent something to NSF pretty recently and because they were in the wrong field, they didn't get a good reaction. That's something I would try to never do myself, but those things still do happen.

**MS. OLSEN:** I know there's a person over there, a couple of people over there. But it's an interesting thing too because you also have to talk to the

review community because they tend to be also very structured and stovepipe to get those ideas. But what worries me a lot is when you have a young faculty member and they change what they're doing to make it more likely that they're going to get funded versus when you do have the fundamental research, when it really should be what is your really creative idea? I think you and then you. There was someone back there.

**UNIDENTIFIED MALE:** I just wanted to make one comment that the notion that there is a balance, in this case, between basic fundamental research and more applied research is, in my view, not the way to think about this. You start first with the needs of the nation. I mean, you're a government agency. You're spending taxpayer dollars and those needs change. If the nation is at war, what you fund ought to be different than if we're in a peacetime. If the needs of a particular population -- the opioid crisis emerges, you ought to have the adroitness to respond to that within a larger framework as a supporter for research and the country. So the notion that there's a simple balance that never changes forever is maybe what the devil's, the research, enterprise that we keep trying to do what we've done rather than being a little more nimble and a little more participatory and a little more responsive to the nation's needs. To me, the tricky question is, how to do assess those national needs? Presumably, the Congress is one method and the administration is another, but without getting too edgy, that's fraught with problems.

**MS. OLSEN:** Yeah, we were in testimony one time and Congress

was going to give NSF a big increase and they said, but I know what are you going to give me next year? Sort of like your comment when he says, what we're going to give you is well-trained graduates, undergraduates, et cetera. We can't say we're going have that product or we're going to have that new computer, but we know that if we don't invest in it, we're not going to have it. But that's a very hard thing to say to those kinds of issues.

**UNIDENTIFIED MALE:** But what can I do? But why is it a hard thing to say? I get the problems and I had to deal with Congress a lot. But this is - - it is the public money. And, if we believe we need to do basic research, then we need to be able to make the case to the Congress and to the American public. So my question is when you all think about this, there's a part of the academy that says, well, this is the unwashed out there. They don't really understand all the good stuff we're doing. Don't let them decide. We know better. And we're already tainted by all these things, and you all were in the front lines of having to mediate between the two. I just need to hear your thoughts about, how do we do a better job? And how do the mediating institutions like you persuade the Congress and dissuade that, because if we don't, we're not going to be successful and then having them say, we know better than you. This is what's called I think they talked about it and Steve earlier about this backlash against expertise. It's because we haven't made a case. And so people are going to say, we know better than you and, so just don't ask us any questions about what we're going with this funding and this research.

**MS. OLSEN:** And I apologize on that and then let Nina talk. I didn't mean to say that.

**UNIDENTIFIED MALE:** No, no, no, I agree.

**MS. OLSEN:** What I'm saying is I can't tell them I'm going to have a new computer. I can't tell them that I'm going to have a new sensor; we may. That's the hard part when they want exactly --

**UNIDENTIFIED MALE:** But my point is saying that, how do we do a better job?

**MS. OLSEN:** I know.

**UNIDENTIFIED MALE:** But that's not the right criteria, right.

**MS. OLSEN:** That's not the right question. Yes.

**UNIDENTIFIED MALE:** Because if we don't, we could take they're asking for criteria. We may all agree that those are the wrong criteria, but we can't persuade them, and ultimately the American public, that this is the right criteria. We can be right but it's irrelevant.

**MS. OLSEN:** Nina and then you.

**MS. SCHOR:** I'm not sure the right question -- and I may have been guilty of framing it myself, so I'm not sure that the right question is the balance between basic and applied research and the right question is, to what extent should we do research that is reactive to the problems that already exist and to what extent should we do the kind of research that it might anticipate and prevent the problems that we can't even name yet? And I think that's sort of what

my dad in industry used to call blue sky research. There is some fraction of our portfolio that has to be that if we're ever going to avoid problems rather than just being just reactive to them.

**UNIDENTIFIED MALE:** So I wanted to bring in a different tech funded than what we've been talking about, and I'm curious the sort of the way you're thinking about this at NSF or NIH and that's sort of the modern-day Andrew Carnegies. And the demo I'm going to use from my own experience, and at a certain point, you should probably close your ears. You're my new provost. So Robert Smith -- so Robert Smith spoke at my commencement, School of Leadership Service commencement, about four years ago. His big news recently was his Morehouse commencement in which okay there's a societal problem: student debt. He goes to Morehouse and says, I'm going to eliminate your student debt. Four years ago, he came to SLS he said to me, tell me a problem in your area that you think I would be interested in. And I knew the kinds of things that he was funding. I said, well, our field is very nondiverse. The number of African Americans going into the national security field is very small in academia, in foreign service, the intelligence community. Oh, tell me something you could do. What could we do?

So we came up with a proposal for working with predominantly African American high schools in the district and in Prince George's county. I mean, what we were used to, we were like, well, we didn't say anything about how much, so we have them a two-million-dollar proposal. He was like, come on, Jim,

like I was thinking \$15 million. Okay, fine. This guy's got a 15-million-dollar proposal. And he just started this foundation that he has, and we were like, by the way, we talked to talk to the point of contact for the foundation. Like what do you do with respect to indirects? Ah, you know, we usually do about ten percent. Okay. So our university said, well, their foundation, they don't publish their indirect rate on their website. And I was like, yeah, but they said they'd do ten percent. Well, but they don't publish their indirect rate on their website, so if they don't do that, you have to charge them the federal rate. Well, they're not going to pay the federal. So the university sent the foundation a letter saying, you don't publish your indirect rate on the website, so we're going to have to charge you that 45 percent that (crosstalk 3:14:27.8).

**UNIDENTIFIED MALE:** We don't publish ours either, by the way.

**UNIDENTIFIED MALE:** Yes, well, you can imagine Robert went ballistic, right. Your university's trying to rip me off. I want to spend my money on these African American kids in the district and Prince George's County, and there's no way I'm going work with you and, of course, we were mortified. So there's a lot wrapped up in that story including some rigidity at a university, but the bigger thing is right here. Here's a guy who's thinking I've got a lot of money and I'm going to -- there's not an RFP. There's not sort of the normal process, the competitive process. I have things I'm interested in, and I'm going to put my money in these things that I'm interested in. And what's the prospect for

government agencies that provide funding working with these new philanthropists who have a lot of money and may be interested in the same kinds of issues that you're interested in funding?

**MS. OLSEN:** I think it's noon, so this will be the last question and we'll discuss afterward.

**MR. HEIT:** I'll be very brief because I know it's time for lunch and people's tummies are grumbling. So a year ago, NSF issued its five-year certification plan. And we have something in there called an APG, an Agency of Priority Goal, and as Kathie will know, sometimes we have a few APGs, Agency of Priority Goals. Last year, we only had one agency priority goal which was to increase partnerships and partnerships that are defined in a particular way where dollars are changing hands. We're working with some outside entity, and we are increasingly funding research programs that are co-funded by other outside entities.

I'll just mention two of them briefly, but there have been others recently. One is Boeing made a donation of ten million dollars to the National Science Foundation to fund research and develop on engineering education. This would be something that would benefit both Boeing but the nation more generally. Amazon made a donation I think it was at least five million. It might have been ten million. This was just in the news to support a new program on fairness in artificial intelligence. We're for certain programs, so we're increasingly running programs of that kind. As you might imagine, there are complications at working

with industry on projects like this not least intellectual property, but that is something that we're doing increasingly. We're terribly trying to be agile at NSF.

**MS. SCHOR:** I think NIH is --

**UNIDENTIFIED MALE:** And just very briefly, NIH has a foundation that's related to us so people can donate to the foundation for NIH and then we do joint projects between money that's allocated to us federally and the foundation for NIH.

**MS. OLSEN:** So I want to thank the panel very much for participating and --

**MR. DESCH:** And Kathie, thank you very much. We have for our next scheduled event, not only food for the stomach, but food for thought. A panel on addressing the question of whether the brave technological changes outpace our ability to think about it systematically. Now, normally I would not schedule a panel and have live food in the audience, but I'm so confident in this panel that we can risk people being armed with messy weapons during the presentation.

What I'm going to suggest is the food should be lunch, which is portable, should be available out in the atrium. We take maybe 15 minutes and victual ourselves bringing it in here. In the meantime, Steve and his panel will set up front and will plan on beginning the formal discussion about 12:25 or so. We have flexibility built into the schedules, so the whole day is going to work. So please reconvene back here at the latest by 12:25 and we'll have a working lunch panel. Thank you.

## **CHALLENGE CASE STUDIES: HAS THE PACE OF SCIENCE OUTFRONT OUR ETHICAL AND PRACTICAL FRAMEWORK**

**MR. VAN EVERA:** I've been told by Commander Desch to herd the cats, so here we are. Thinking about the topics for today, there's, sort of, two big questions on the table, and we've been talking about question number one.

**UNIDENTIFIED WOMAN:** It's not playing, but I'm --

**MR. VAN EVERA:** Call your broker.

**UNIDENTIFIED WOMAN:** -- but I'm --

**MR. VAN EVERA:** Sell him short. There you go.

One question is, when we think about the academe -- academic world in society, is, first, do no harm; and the second question is, how can we help more? And the theme so far has been, how can we help more? How can academe, and other knowledge producers, including the state funded and state research organizations like IARPA, how can they better serve the world that they live in and that funds them?

The second question is, first do no harm. How can academe minimize the risks that it will do things that have unintended, negative side effects, which is a question that's kind of historically not much thought about and not much worried about, but there is increasing concern out there that the world of science in particular is bringing us new discoveries for which we're not ready, morally, ethically, politically, socially, and raising the question of, are there duties

for those producing that knowledge to some way pre-assess or early assess the possible downsides -- the threats they pose -- to include outsiders in assessing those threats to notify the wider world of those threats.

And sort of as an ancillary question, related to the things we're talking about here, is there any way to do that without having it be multidisciplinary, to which the final answer is, no. This has got to be a collective activity.

What's the paradigmatic framing of the problem would be in Martin Rees' book of 2003 called *Our Final Hour*. And he's a British cosmologist who wrote a book arguing that, the long-term trend in the -- coming to us -- coming to the world from science is for the discovery of more and more powerful means of destruction. And those means of destruction, more and more relentlessly, outrunning means of defense against them.

And a third attribute that he argues about is his -- observable as a trend, his -- that these new means of destruction will be democratized. And, unlike nuclear weapons, you're not talking about means of destruction that could be mastered by poor actors, small actors, weak actors, non-state actors -- terrorists, sociopaths -- and in his book he talks about the nuclear revolution and what it all means. But then he says, are we now on the verge of seeing new LOEDs (phonetic) come into being from bioscience? Could bioengineering offer the means to make hyper lethal, hyper contagious, delayed symptom pathogens? And if you hit that trifecta, you've discovered something that could end the species,

which would not be good, or, in other fields, if it was distributed. If you produce the technology that could produce that result, and it was attained, you know, non-state actors demand -- mastered.

And the sort of quick coda on that, why take it seriously? We, the human race, have found only one way to prevent the use of weapons of mass destruction. I'll repeat, we've found only one way to prevent the use of mass -- weapons of mass destruction -- deterrents. That's the only way we know how to do it. Therefore, the logic is inexorable. If we permit WMDs, the hand -- to fall in the hands of non-deterrable actors, we're in lots of trouble. By definition, we've created a problem we can't solve.

So when -- the next question is, well, what about in other versions of this problem, too? New technologies that have downsides. And is there a duty on the part of scholars to forecast those downsides and at least, at a minimum -- maybe this is an uncontrollable process -- maybe there's nothing one can do, but at least you can warn the wider world that things are coming. But, you know, how many other ways can you see a downside? Again, some of us were talking earlier about, you know, when plastics were invented, did anyone foresee that they would become a huge pollutant that would threaten the oceans, or would internal combustion engines, when they were invented, did people foresee they could produce enough side effect to actually affect the climate and hence the economy and the welfare of the species.

So this question of forecasting problems is our duty, and if so, how

should it be done?

And, Jason, how are you?

**MR. MATHENY:** Good.

**MR. VAN EVERA:** Good to see --

**MR. MATHENY:** Good to see you.

**MR. VAN EVERA:** It's good to meet you.

**MR. MATHENY:** Yeah, likewise.

**MR. VAN EVERA:** He wrote a very interesting little talking point paper about how IARPA does this, the Intelligence Advanced Research Projects Agency, what's this, sort of, checklist of questions that have to be answered before a project is okayed. But you can tell everybody about it.

But, anyway, I'm going to sit down and let everybody talk. Just saying there's these big questions on the table, but the final -- third question is, if there -- you can all quarrel about, you know, is Rees right? Are we really looking at a small problem or a big problem when we talk about technology with harmful - - discoveries with harmful side effects. But if you agree that that is a problem, then, what to do about it. What should -- what norms should govern the behavior of the discoverer world, and -- including both in government and in the academic world.

Why don't we just start -- should I move from right to left?

**UNIDENTIFIED MAN:** Whatever you want.

**MR. VAN EVERA:** Go ahead. Run down the line.

**MR. LATIFF:** I like the question that Mike posed in our -- was it our technology outstripping our ethics.. I come at this from a national security, military approach. So my career's not important, but I think it is pertinent to say that I spent a lot of my 32 years in high tech research and development and acquisitions by chasing a couple years. I spent several hundred million dollars on research and twice or three times in my career, I managed large weapon systems, procurement and development systems. So I've seen, kind of, both sides of it.

I'm not a big fan of prepared remarks, but since I wanted to do this right and make Mike proud of me, I've prepared some remarks. About all through the morning I cut about half of the words out, so I'll kind of flip through these, and then we'll give everybody a chance to ask questions, so --

I talk about Russian technology to war. It's very -- or there's important ethical issues associated with our aggressive embrace of the mass technologies for war fighting. And just to name a few, the highly uncertain aspects of artificial intelligence, the pervasive use of cyber techniques, and the impact with automation on military decision making and command and control.

In general these technologies are moving so fast and we're bracing to incorporate them so quickly, I fear we may be getting insufficient thought on the potential downsides and some of the unintended consequences of their ethical implications. We're seduced by technology. I love this. You know, consumers line up to buy thousand-dollar smartphones when they don't really need one. They -- it's just available. Air forces demand new generation fighters when the previous

generation fighter hasn't even been delivered yet. Political leaders don't understand AI. They just know we need some of it.

But we've seen, you know, innovation cut down some. Now at tomorrow's levels will employ technologies like AI and neuroscience and cyber, none of which we yet fully understand. The effects of these technologies are far more difficult to predict than classical, conventional weapons which have really deterministic effects.

Hypersonic missiles and aircraft and high-powered laser weapons and computer attacks will strain the capability of human operators to keep up. The very short timelines creating dependence on, you named it, Artificial Intelligence. Rapid advances have been made in neurocontrol prosthetics and in the broader, general area of brain-machine interfaces. And I think Hank will probably talk a little bit about this.

And research is ongoing into the use of chip implants to restore brain function. And, of course, work is going on in the use of enhancements using neurological techniques. They raise deep, ethical questions and concerns about compromised character, justice, and coercion. What effects do neurological enhancements have on human essence? What will be the effects on personal autonomy and free will? What about privacy? How will soldiers be viewed on the battlefield?

Unmanned systems with new, potentially autonomous and lethal capabilities are now available, and can be controlled by artificial intelligence.

Robots and autonomous systems allow us the -- to reduce the risk to our war fighters, but there are a lot of strong objections to giving autonomous systems lethal capabilities, not the least of which is the idea that taking the human moral agent out of the firing decision is just wrong.

Some issues surrounding the research and potential employment of autonomous weapons are complexity and unpredictability. Some of these things have millions of lines of code. And with millions of lines of code, some are going to be in error, and some of those errors could be fatal.

The Department of Defense is developing lethal cyber capabilities and cyberspace munitions, frightening additions to the tools of war, the ramifications and consequences of which are huge and largely unknown. Then there are a lot of unanswered questions about the use of methods of deterrents in cyber capabilities. Operations at computer speeds is such a complex fear of (inaudible) the capability of even highly trained cyber warriors, not to mention the intellectual capabilities of our decision makers, such as they may be.

Again, and this domain as with many other weapon systems, AI will play an outsized role in future militaries and future war. And while AI can do a lot of great things for us, it is also unpredictable and vulnerable to unique forms of manipulation. An anathema to military planners. Researchers are still at a loss about how to adequately test these systems and ensure that they can be trusted to give correct answers. Potentially, AI systems -- potential problems with AI systems are their inability to explain their decisions, and their vulnerability in

computer hacking. When I was a young lieutenant in the Army, and I made a mistake, I had to tell my captain why I made such a stupid decision. AI systems can't do that yet.

Recent reporting on the dual deep mind AI system, it was trying to play a game, determined that it had an unexpected aggressiveness to it. They suffer from an inability to detect flawed data, and they have an unusually high susceptibility to malicious inputs. AI systems can exceed human performance in many ways, but they can fail in ways that a human never would.

So with all of that said, let me add that I may get these numbers wrong. Jason can correct me. But this year the DoD alone is going to spend \$8 billion on AI systems. The DNI people had told me that about 80 percent of the new questions they get from the field have to do with the implications of artificial intelligence.

The laws are in conflict, and international humanitarian laws remain important. In an age of -- the new age of the war on weapons, it's even more important to analyze how changes affect, and are affected by, the age-old concepts of military necessity, discrimination of combatants, proportionality, and the avoidance of unnecessary suffering. Forty-four years ago, scientists from around the world, meeting at a seminar in California, agreed to a voluntary moratorium on some types of recombinant DNA research.

In present day, we have scientists defying requests for constraint, and publishing details of how they were able to weaponize a deadly strain of bird

flu. And, because of our strange collective and willingness to engage in arms control and non-proliferation agreements, lesser developed nations and groups will increasingly gain access to these advanced technologies. It is not clear that their ethnical structures will be similar to ours.

Speed. One danger is that now, because of the speed of technology development, we've rushed into the use of these technologies. Everybody remembers Vladimir Putin saying, the nation that wins in AI will rule the world. So the 20th Century theologian, Reinhold Niebuhr, wrote -- in 1952 -- the American military power created the temptation to become impatient and defiant in the slow and sometimes contradictory processes of history. The development speed of new technologies is even more incongruent with Niebuhr's comments.

In discussing cyber capabilities, author, Lucas Kello, in a new book called, *The Virtual Weapon*, said that in our area -- era of rapid technological change, threats and opportunities arising from a new class of weapons produce pressure to act before the laborious process of strategic adoption era concludes. In his 2003 Army War College report called *A Crack in the Foundation*, then Lieutenant Colonel H. R. McMaster, spoke of the new theories of war that had taken on names, invoking a sense of control, things like network centered warfare, information age war, that it minimized the concepts of human imagination and initiative, and placed decision making squarely in the computer.

Command and control were grounded, and the assumption that technology would lift the fog of war. Near perfect information would make

possible wonderful decisions and make our troops safer. And here are, two decades later, with AI, cyber, and the omnipresent Internet, we're still looking to technology to be the savior, even though, as McMaster noted, things did not turn out quite the way the visionaries had hoped.

AI is beginning to again be touted as a technology which will revolutionize command and control and decision making. And how much decision -- how much should decision makers depend on these systems to give advice to humans, to make so-called time-critical decisions? How can we ensure and trust that these systems are not biased and telling us things that are not correct?

So before we go in full tilt incorporating AI into our most critical decisions, and a host of other emerging technologies into our weapons and our soldiers, we need to be sure of what we're doing, and just as importantly, why we're doing it.

So that's a much-truncated version of what it was I wanted to say, but I --

**MR. VAN EVERA:** Thank you, Bob.

**MR. LATIFF:** -- kind of look forward to the questions.

**MR. VAN EVERA:** Jason.

**MR. MATHENY:** So I'm going to speak for about ten minutes about my experience with ethical constraints on research at IARPA, which is a federal organization that funds research for the U.S. intelligence community. It

funds a variety of research in machine learning, computing, biotechnology, neuroscience, cognitive psychology, and sociology. I'll talk about a few forms of ethical constraints that have been practiced by IARPA, and by other national security organizations.

The first are standard requirements for ethical human subjects research, including informed consent, and the use of institutional review boards. IARPA doesn't fund any human subjects research without such ethical reviews. This is also true of non-human animal use, for which -- or which requires reviews by Institutional Animal Care and Use Committee. Every federal organization that I'm aware of follows these rules. And in my ten years of experience at IARPA, I'm happy to report that I know of no exceptions.

A second type of review at IARPA is a civil liberties and privacy protection review. This is a review to ensure that the research does not violate the civil liberties or privacy of U.S. citizens. This review is uncommon among research organizations in the federal government, and I'm proud of it, even though it's very often inconvenient. I'll give an example. IARPA worked on a program called Open Source Indicators that analyzed large volumes of data to detect disease outbreaks, economic crises, and other societal events. These data included social media.

It would have been legal for IARPA to fund research that backed up global social media from whatever location given that these were social media accounts that users had agreed to make publicly available. But we placed several

constraints on the research. First, researchers were limited to social media that came from outside of the United States. Second, they could only analyze data that were publicly available and could be accessed without establishing connections to the users. Third, they could not misrepresent themselves, for example, by creating fake or non-attributable social media accounts. Fourth, they couldn't analyze individual accounts or extra any personally identifiable information. They could only analyze broad trends in social media, such as whether the keyword frequency of flu or fever across a million updates was increasing.

There are a variety of cases where constraints on analyzing foreign social media would be inappropriate because of compelling national security reasons. But this was a research program, and the benefits of analyzing individual foreign social media accounts was, in this case, too small to justify.

In national security research, there are other ethical constraints that have been historically important. And these generally overlap with international agreements or norms about weapons and targets that are disallowed in warfare. These include disallowed weapons such as biological weapons, chemical weapons, laser blinding weapons, as well as disallowed venues, such as the Antarctic or outer space, and disallowed targets such as civilians, whose definition has changed over time.

To me, the case of biological weapons is especially interesting; and I think it's one that (inaudible) draws special attention to. The U.S. had a large and advanced biological weapons program dating back to the second world war, and in

1969, the U.S. unilaterally ended this program in what could be described as enlightened self-interest. The U.S. assessed that biological weapons would become increasingly easy to develop, while also increasingly devastating due to rapid advances in biology.

In contrast, nuclear weapons require access to rare materials and extensive infrastructure, are generally accessible only to industrialized states, while biological weapons rely on no exotic material, and are accessible to those with sufficient technical training, which is provided for other commercially important areas of biotech. The possession of biological weapons by the U.S. might have been marginally improving of its own strategic position, but it's substantially increased the strategic power of poor states and non-states that didn't have the resources to develop nuclear weapons.

So in 1969, the U.S. decided to abandon its biological weapons program, and to encourage a moral taboo against their development, ultimately resulting in the Biological Weapons Commission in 1972. This example was one that we thought a lot about at IARPA as a case of enlightened self-interest that we might apply to other areas of technology. And in the last few years, we added to IARPA another layer of review, which was a process for deciding what research to fund that incorporates the potential risk to ourselves of resulting technology.

Richard Danzig, the former secretary of the Navy, recently wrote an excellent report titled *Technology Roulette* about the risks associated with advanced technologies, whether due to theft or to accidents. Danzig notes that

secrets don't last long, definitely not as long as they used to last, say, 20 years ago. It's much easier for classified information to be stolen, to be exfiltrated, and to be shared. Even when we develop a technology in secret then, there's a non-negligible probability that, due to theft or insider threats, that technology will eventually leak.

In addition, many advanced technologies are inherently more dangerous than past technologies. This is, in part, due to the power of emerging technologies and their complexity. And to the recommendation to read Martin Rees' book, I would also recommend reading a fascinating paper by Nick Bostrom called *The Vulnerable World Hypothesis*, in which he thinks there are a number of thought problems such as, how would the U.S. have controlled nuclear weapons if, in fact, they had been substantially easier to develop? And if Leo Szilard had thought about a development approach that didn't require access to rare fissile material, how would the world have responded?

So along these lines, before IARPA funds a program, it asks program managers the following set of questions. First, what's your estimate how long it would take a major nation competitor to weaponize this technology you're proposing to develop after they learn about it? What's your estimate for a non-state terrorist group to weaponize such a technology after they learn about it? If the technology is leaked, stolen, or copied, would we regret having developed it? What, if any, first mover advantages are likely to endure after a competitor follows? How could the program be misinterpreted by foreign intelligence? How

can we reduce the risk of a foreign intelligence service misinterpreting this weapon system and its intent for use?

Can we develop defensive capabilities before or alongside defensive ones? Can the technology be made less prone to theft, replication, and mass production? What design features could create barriers to entry for unintended users? What red team activities could help answer these questions, and whose red team opinion would you particularly respect?

For the last three years, IARPA has asked these questions of its program managers before we approve any new program. And sometimes the program managers don't have good answers to those questions, in which case we decide not to pursue that technology. There are some biotechnologies that have fit into that category where we did not pursue those technologies because we couldn't figure out a way of ensuring that those technologies would be used only defensively or in ways that weren't especially vulnerable to misuse.

Other times IARPA has satisfactory answers to these questions, but the questions raise risks that need to be addressed, and safeguards that we build into the technologies. There are some artificial intelligence systems, in particular, that fall into this category. And one that I -- we've been preoccupied with in the national security community, is the vulnerability of most AI systems to fairly simple attacks, sometimes called adversarial attacks, which can include spoofing AI systems, poisoning datasets so that the systems misbehave, or inverting the system so that you are able to reverse engineer the training data upon which a

model was originally developed.

That's an overview of the applicable constraints at IARPA. I think IARPA and other organizations are quite eager to get feedback from the research and ethics community on what other questions we should be asking. What angles are we missing that could make us regret some of the investments that we're making or could make. So I look forward to the discussion.

**MR. LATIFF:** Great.

**MR. VAN EVERA:** Thank you, Jason.

**MR. GREELY:** So the question is, is science outrunning our ethical and practical constraints? And the answer is yes, no, and maybe. I've got about nine minutes and 40 seconds to expand on that.

I'm listed in some of the materials as having something to do with neuroscience. I'm not a neuroscientist. I am a law professor, though I do work on neuroethics, and am currently the president of the International Neuroethics Society. But I'm also, perhaps, the person here who's done the most bioethics work around genetics, stem cells, and other things. So I thought I would start by giving you very quick one or two sentence examples of ethical and practical problems in neuroscience, in stem cell research, and in genetics, then talk a little bit about the context of controlling those, what the -- whether -- what determines whether our knowledge has outrun our abilities to control it or not. And then, finally, end with a couple of ideas.

First, neuroscience. If I could offer you, for free, a test that with 90

percent accuracy would tell you whether or not you were ever going to contract Alzheimer's disease, how many of you would take that test? How many of you would not take that test? Which half of you are crazy? That's an ethical question. It's an ethical question for people trying to make a decision for society about whether you encourage it for insurers, for employers, and for others. I can't offer you, honestly, that test today. Come back in ten years, and I suspect I will be able to.

Very different neuroscience question. In April, a Yale researcher named Nenad Sestan published a paper in which his team had taken the heads -- the decapitated heads -- of pigs that had been slaughtered at a slaughterhouse in New Haven, hooked them up to a pump that ran a acellular perfusate through them with hemoglobin and glucose four hours after they had been killed. The cells almost all seemed to revive. There was no EEG. We don't -- and he likes to say, and I like this, the cells were alive; was the brain alive? Not nearly as clear. But it's not clear what that means. It's not clear -- one thing that is clear though is that our conventional wisdom that creates those die irretrievably after about 10 to 12 minutes without oxygen is not true. And that's going to have implications, not so much for pigs, but for human medicine and other areas.

On another neuroscience issue, my friend, Jack Gallant at Berkeley has done amazing work at trying to decode thoughts in people's heads using FMRI. He can tell what you're thinking about in a general sense. Are you thinking about food? Are you thinking about pets? He can tell whether you're

thinking about a dog or a cat usually. He can't tell what kind of dog or what kind of cat, and fMRI is a very clunky, expensive, and not wonderfully -- not wonderful technology. But people are working on better technologies to try to read minds, sometimes with somebody's assistance. The scary ones are being able to read minds surreptitiously. And there are thoughts on that.

Stem cell research. There are currently about 700 clinics in the United States that are selling illegal, and largely bogus, stem cell treatments. They are clearly illegal under the Food, Drug, and Cosmetic Act. A federal court just ruled yesterday and closed down one of these sets at FDA's request. They're widely used -- our legal construct doesn't seem to be able to constrain the political and the popular demand for cheap and easy repairs to knees and other things.

Second, a researcher at the Salk Institute in San Diego, Juan Carlos Belmonte, was doing some really interesting work trying to deal with organ shortages. He was trying to use human stem cells to grow human organs inside pigs, organs that could then be used for transplants into humans. These are not pig organs that have been humanized. These are actual organs from human cells grown inside pigs. Now that's kind of an interesting use.

I wrote a book in 2016 called *The End of Sex and the Future of Human Reproduction*. It's not as gloomy as the title makes it sound. One of the key aspects of that was another stem cell advance. We are going to be able to take your skin cells and make them into eggs and sperm. We can already make them into something called induced pluripotent stem cells, which can become all cell

types. If you were mice, we could already make those cells into eggs and sperm and give you healthy progeny. The human work is a little behind, but we're currently two steps away from being able to make human eggs this way.

We're a decade away from knowing that it's safe and effective, but that also is coming. And being able to make eggs and sperm will immediately make happy lots of infertile couples who are infertile because one or the other doesn't make eggs or sperm or used to make eggs but doesn't have any useful eggs left anymore. But it will have broader implications, as well.

Genetics. The human side of CRISPR has gotten all the attention. CRISPR: clustered regularly spaced short palindromic repeats. I'm very grateful that Francisco Mojica, who came up with the name, picked something that had a nice, easily pronounceable English acronym, but we haven't paid enough attention to non-human CRISPR uses.

We've heard a little bit today percolating genuine, appropriate concerns about biowarfare using CRISPR. Smallpox is gone, we think, except for one lab at CDC, and a lab someplace in Russia. But there are a lot of pox viruses out there and mutating one pox virus using CRISPR into a different pox virus shouldn't be that hard. But there are well-intentioned things that could go wrong.

If we could eliminate the ability of the Annapolis mosquito to become infected with the malarial plasmodium, we could save 440,000 human lives a year. People don't give people malaria. People give mosquitos malaria, and mosquitos give it back to other people. Genetically tweak those mosquitos so

that they can't get infected, they can't infect people. A problem, though, is, if you release those in Requena (inaudible) with Molly or Mauritania. Who controls it? What happens when unanticipated things happen with those?

Human embryo selection. We already are able to take invitro fertilization embryos, do a genetic test on them, and decide whether this embryo is going to have a particular genetic disease or not, whether it will be a boy or a girl, and some other traits. Our cheaper whole genome sequencing is making that more and more possible to do more and more things with it. There's a company that's offering it for intellectual ability, claiming that they can do three or four or five IQ points. I don't necessarily believe them, but they're selling this already. This is also going to expand.

And then last, designer babies, CRISPRed embryos. He Jiankui in China shocked the world in November by announcing the birth of two girls whose embryos he had CRISPRed. I don't have the seven hours it would take to tell you just how terribly unethical, stupid, and reckless that experiment was. I can only hope that the two little girls will be okay. But he's probably not going to be the last person to try to do that.

So those are just three times three, nine vignettes from three areas of science, biological science, where things are moving rapidly. Is it out of our control? It depends. It depends on a couple of things. One is, what kind of regulatory system do we have set up in advance? The biospace, in the U.S. we're better off than we are in most, because you need FDA approval to do much of this,

or you need human subjects or animal subjects approvals to do much of this.

In software, there's no such preapproval, premarketing approval system. In most of the economy there's no premarketing approval system. In medicine, we have it, although we don't always use it very well. That's a help. On the other hand, as these things become democratized, and cheaper and easier, it becomes harder to control, even if you are legally regulating them. As a practical matter, regulating every bio high school, every bio college major, with a garage and access to CRISPR, is this practical matter going to be impossible, let alone non-state actors. And literally high school students are doing CRISPR experiments. It's not that hard.

So those are some issues. On the other hand, two cautions. One is, something we regulate too soon. When Dolly's birth, the cloned sheep, was announced, countries went crazy. There were all sorts of hysteria of that clone. Armies have cloned warrior slaves. The Brits very smugly -- do we have any Brits here? Very smugly in a very British way announced, oh, well we've already taken care of it, we've banned human cloning.

And it turned out seven years before, in their regulations, they had a line that said, ban on human cloning. They had a section that described the scientific technique they were banning, which was not the technique that was used to make Dolly. If you regulate too soon, sometimes you regulate poorly because you don't know where the science is going to go.

The other issue is, we take lots of our ideas about how things are

going to come from fiction. Fiction is a great thing, but fiction has a bias. Fiction needs conflict. There are far more dystopias in fiction than utopias, and far more utopias than realistic portrayals of a world where most of the time you muddle through. So don't -- I think we sometimes underestimate the ability of human societies to muddle through. It doesn't mean we should count on it. What should we count on? I think we need to spend more time and effort monitoring technologies, both new technologies, but tech -- both insipient technologies and technologies after they've been introduced.

If we don't spend very much on figuring out, are they working? How are they working? Who's using them? And who's using them for what? It's like university development. It's really neat. It's relatively easy. You get somebody to give millions of dollars to build a building with their name on it, it's really hard to get them to give money to have janitors to keep that building up. The monitoring and maintenance are crucial. We underinvest in it. I think that's a real problem for us.

Another issue goes to something that Steve has talked about. I think science needs at least an aspirational goal of reporting -- snitching -- on people who are doing badly, unethical stuff. It needs more -- it needs to be more than just a goal. The doctors and lawyers are supposed to report their fellow doctors and lawyers who are practicing in unethical and dangerous ways. Scientists should have that same goal, but they need a support structure to make it easy.

And if you go back to He Jiankui, at least eight different academics

around the world knew about this experiment in advance, and -- or at least knew that he was thinking seriously about it. None of them informed anyone. Some of them have subsequently said, you know, I think I should have. Three people at Stanford were investigated for their non-reporting, all cleared. One of them, Matt Porteus, said, you know, I wish I'd reported, but I didn't have any idea who to report to. There's no structure out there.

So I think there will be challenges. I think monitoring the technologies is going to be important, and I think a self-reporting ethos would be - - or self-reporting it -- a snitching ethos, and even using that word shows you some of the cultural barriers it'll run up against. Will it be useful in helping us understand where the technologies may or may not go? Thanks.

**MR. VAN EVERA:** Thank you, Hank. We have -- how much, Commander Desch?

**COMMANDER DESCH:** About 35 minutes.

**MR. VAN EVERA:** There we go. So speeches, comments, questions, editorials?

**UNIDENTIFIED MAN:** When IARPA does its analysis of whether to fund a particular area of research, if somebody walks in today, after that decision and say, we know the Chinese are doing it? Would it change the decision?

**MR. MATHENY:** Usually not because usually we're most concerned about the risk to ourselves than the technology simply existing. So the

fact that the Chinese is developing the technology will make us want to pay a lot of attention to China, but not necessarily develop that technology ourselves, which simply increases the total risk service.

**MR. VAN EVERA:** Bruce.

**MR. JENTLESON:** So a quick comment on the pig's brain revival.

**UNKNOWN MAN:** Whatever, something.

**MR. JENTLESON:** Anyway, there's an article coming out here in (inaudible) *Magazine* in a week or two that my son-in-law happens to be the journalist, and he was telling me about this last week. And it's pretty --

**MR. GREELY:** What is his name?

**MR. JENTLESON:** Matt Shaur, S-H-A-U-R.

**MR. GREELY:** You got to talk to him?

**MR. JENTLESON:** Yeah, but, anyways, it's in the works, so for those of us now interested in this.

**UNKNOWN MAN:** So just three comments. One is kind of generational. And one is about our human reasoning. The other's about policy. The generational one is, all the problems we are talking about, seem like they'll be even more complicated as millennials and generation Z, you know, moves into positions of doing things in power. And I don't mean that as a slur, but I mean, you know, technology's so much a part of their world. You know? It almost seems, like I know with my own students over the years, if there's a way of using technology and a way of not using technology, even if the second one might be

better, you know, like your point about the iPhone you don't need, you'll opt for the technology.

And in some respects, even when you talk about a lot of things about international affairs, the key factor they run to quickly is technology. So there may be -- I think you put it, sort of a worship of technology that's there that could be even more problematic over time. But also, for all of us, in terms of the basic human reasoning, you know the thing about AI, you know, it struck me as the same as some of the other things we talked about. And it's the importance of differentiating between a premise and a proposition. Right? Whether it's an implicit bias that goes in from the researcher, separate from does it work, and what complications and what kind of counter's going to be done.

But, you know, if we don't treat things that should be propositions or hypotheses need to be tested, and we assume -- we take them as assumptions and premises, whether it's an argument being made by a lawyer or an argument, even, by an academic or programming into AI, it's going to be a problem, and how one really, you know, an honest way, a sincere way, not even implying negative motivations, how that gets checked upon when there's a sense of if it works technologically.

The third is the policy point. I mean, my goodness, you know, you think about the efforts to develop rules and norms internationally, right? Concerns of the United States came out in an era in which, you know, if you go back, the rules and norms that were developed in (inaudible) and some with the Brits. We

argued were good for everybody.

But in today's world, you'll have not only national interest kicking in -- I don't want to bend my definition of a rule in order to be so certain of a national interest, but differences over what those rules and norms should be based upon history and culture, and that's even just at the state level, let alone a non-state actors, you know, or others. And the point that you're making, Hank, about, you know, two soon regulation can be problematic, you know, our question is, should we be studying efforts where we have succeeded at different points in history and at different sectors of activity of making somewhat consensually enforceable rules and norms. You know?

The Geneva Convention. I never really thought about -- I mean, I know, you know, cattle proongs were used in World War I, et cetera. But it seems like we could start to think about scenarios about how one tries to make policies in a semi-generic way, and then figure out how they apply to these different areas because it's going to be so complicated, you know, for a world which has so many different actors in it. It is very different than any World War, whether it was Geneva after World War I or the post-World War II world, that making rules and norms was much more, oh, geez.

**MR. VAN EVERA:** Jim Steinberg. Well you -- did you want to comment?

**MR. LATIFF:** Yeah. Well, I think your last point is a really important point. I've actually been to China and Russia, talking with some of their

scientists about the ideas of how we might go about doing arms control agreements for some of these new technologies. And it would be very, very difficult because they're so dual use. But we need to -- you know, we're pulling out of treaties all over the place, and actually we need to be going in the other direction. We need to recognize that I think the captain today talked about the fact that we have competitors.

And sometimes it makes a lot of sense to talk with your competitors and recognize that we no longer have a monopoly on some of these technologies. So even kind of going back to your idea about (inaudible). I think that's a really great idea.

**MR. GREELY:** Let me get closer to the homogeneity or lack of homogeneity. I think, in some areas, and I think for the most part the biological sciences have fallen into those areas that are not so deeply associated with national security. There is a homogeneity among the scientists in a sense of the scientists owing an obligation to science more than to an immediate national interest in some cases. He Jiankui is sometimes used as an example that the Chinese have no ethics. This is a dangerous group, et cetera. I don't know that we'll ever know just how rogue he was because I don't trust the Chinese investigations, which haven't been published anyway. But it was very unclear, at first, whether China was going to herald this as a wonderful accomplishment of Chinese science or condemn it as a rogue.

Within 12 hours, 122 Chinese scientists, mainly scientists, a few

ethicists, published something on WeChat, a letter condemning his experiment and saying that it embarrassed and shamed China in front of the world. And three hours later, Beijing announced that this has embarrassed in China in front of the world. So I think -- I don't want to put too much stress on it, but I think the culture of at least some sciences may be a help in some circumstances. In a World War II context, maybe not so much.

And one last example that stem cell research around the world has been almost completely governed -- the research part, not the quack treatment clinics -- by guidelines put out by the National Academy of Sciences in 2005 -- 2004 -- '04 or '05. Those were voluntary. And yet the scientists liked them. They adopted them. They're -- the International Society for Stem Cell Research adopted them. And they are, one way or the other, pretty much universally followed around the world, even though they are very, very rarely legally required. So I think there is some oath for collective action on the part of scientists in some context, at least.

**MR. VAN EVERA:** Jim Steinberg.

**MR. STEINBERG:** Great panel. It's a terrific discussion. Now one other comment coming off this last point, and then a question for the panel.

The comment just on the possibility of -- and I'm not a war commander, but about how we deal with China on these things because I'm part of a group that's been convened by a former Chinese vice minister for affairs down here. She wants me to ask if there is an institute that's bringing people to talk

about whether there are (inaudible) by AI and its application that could be agreed, not necessarily by governments, but at least (inaudible) basis across different countries, and it involves people from different disciplines and from different countries around the world.

And the fact that the Chinese, you know, launched this initiative, although it's not done from (inaudible) but by somebody who was clearly part of the government establishment, suggests to me that we are way underestimating the possibility of at least to try to find common norms, and even if we don't get full agreement, just the fact of seeing this as a common challenge is really important, and really valuable, right, the -- for the reasons that many of you have suggested. It's in our self-interest.

The reason we banned chemical weapons, because they're -- they realized that the control was -- you -- it was hard to use them effectively without introducing all these undesirable consequences. And so, I don't think we should just assume it's too hard or that the competitors just want to get an advantage on us. I think we have an obligation to explore these possibilities.

The question, and it links a great conversation and it started a story about possible ballistic thinking with your questions about regulations. I'll have you all reflect on the application and the usability of the idea of the cautionary principle as one of the ways of thinking about this. I mean, it's a regulatory principle. The Europeans have seen this as, sort of, a way of thinking about how you deal with these uncertainties. It's been something that we in the United States

have largely resisted. It might be too constrained in the technological innovations and sort of debatable GMOs is the posterchild of the core question about precautionary principle.

But how should we think about that and regulating activity as a possible means of dealing with some of these uncertainties?

**MR. VAN EVERA:** Does anyone want to comment?

**MR. LATIFF:** As I remember, the precautionary principle, you have to prove that it's not dangerous before you proceed.

**UNIDENTIFIED MAN:** I mean, it's proven in quotes, but yes, I mean, that you have your other obligation -- you can't simply, because you can't have them prove that it's deleterious. Right? It's like the old delaying clause, right, for -- the -- you used to have to show that there's zero risk of cancer, which was an old regulatory policy.

**MR. LATIFF:** So in my own very personal opinion, I think that that would be too constraining. Researchers need to have the capability to operate fairly freely. For me, the big question is not so much the researchers, and there was one comment that, you know, people like Jason and Darba (phonetic), you know, have these structures in place. The research organizations have -- many of them -- have these structures in place.

I think where it all really breaks down is when the technology is proven to be possible, the problem comes when somebody decides to pick it up and run with it. And those structures are not in place, and particularly in the

weapons development business. You know, program manager, get the technology. And it's, like, okay, bar the door. I'm going to get this thing into the field fast. And I don't think they have the same kind of -- so it's, I think, research is sort of kind of taken care of. It's the development and current acquisition that isn't quite taken care of.

So that's a long answer to your question. I think the precautionary principle might be too constraining and actually major [inaudible].

**MR. GREELY:** I'm not a fan of precautionary principle. I mean, I'm a fan of the basic idea almost [inaudible] that you should worry about the consequences before you do something, and if that's all the precautionary principle, then I'm very much in favor it.

But the idea that you need to prove something is going to be safe before you try it, I think is way too broad, and it avoids the logical -- I think logically unavoidable. A decision not to do something is an action of its own. And so you can say, let's not CRISPR mosquitoes because we're not sure what the environmental effects will be. But by doing that, you're saying, well, let's continue to let 440,000, mainly African, mainly children, die of malaria every year.

So I'm not a fan of the precautionary principle. I think it's too strong. Denying action also needs -- the consequences of the non-action also need to be waived. But I do think that thinking about the consequence -- having a process for some thought about the consequences of the actions -- of your actions -- are important. IARPA's process sounds like a good one. I don't know enough to

say that for sure, but it sounds like a good one.

And I want to second Bob's, kind of, about implementation. I'm a Californian, so I'm half human, half car. And automobiles have completely changed everything about my state. But it wasn't the invention of the automobile that did it. It was the invention of Model T. It wasn't the first cars, which were expensive and reliable and, hence, rare. It was when they became common, when they moved -- those two CRISPR girls didn't change the world, but if there are 2 million CRISPR babies a year, that begins to change the world.

So I think you're right. The rollout is, the more important and the less regulated often.

**MR. VAN EVERA:** Richard Lanza.

**MR. LANZA:** Yeah, I think one question I was going to ask about AI, and that is, of course the critique is that when it works, you don't know why. And when it doesn't work, you don't know why which is, sort of, I think, a little bit distraught -- disconcerting to me.

But the other part is, the numbers I've heard is the weapons systems are maybe about 30 million lines of code, but modern automobiles, there's probably been in excess of 100 million lines of code developed for that particular vehicle. It doesn't all get used, but the question is, can you ever write 100 million lines of code without error?

And then you hear guys saying, oh, no, no, we have this self-checking -- you have a self-checking code checker, which will ferret this out. But

I think that's a really disconcerting thought is the fact that you can't ever check that something doesn't have something buried in it.

And an example of another thing in the hardware level, we're doing a project for IR as a matter of fact, which we're look -- trying to find whether someone had covered up chips. And so looking at chips of ten [inaudible] resolution level, well, that sounds great until you realize that a chips that's a centimeter by a centimeter has 10 to 12 voxel -- pixels in it, and a few more going down. So you're trying to figure out an error in one ten to the fourteenth (inaudible), sort of, line-by-line.

**MR. VAN EVERA:** Any comment? Steve Delloso (phonetic).

**MR. DELLOSSO:** Thanks. As the panel was speaking, I couldn't help thinking of that famous line from Einstein at the Donovan, nuclear age, atomic age, that everything has changed except our mode of thinking. It seems to me that because of the -- both the speed and the proliferation of all these technologies, it's even -- exponentially were difficult for those modes of thinking to catch up. But we remain hopeful.

But my question was, it builds on something Bruce said about the comfort level that generation Z has with technology. It's sort of almost part of their DNA now. And I wonder if there isn't a potential risk and an overreliance on the technological silver bullet, which might delay or even constrain action that could be taken today with the assumption that, yeah, we believe in technology. It's moving so fast that we don't have to act today because some day some scientists,

somewhere, will find a solution to the problem.

**MR. GREELY:** Yes. And it's a possible concern. It's a moral hazard problem that you think that you're deterred from doing something because you think there's something better out there. A great example of it is in climate change where there are a whole bunch of -- you know, we know how to reduce the amount of greenhouse gases in the atmosphere, but it's expensive and difficult and inconvenient, and politically and economically costly. And we don't want to do it. So some people are saying, well, you know, we should explore geoengineering more. We should explain these other techno fixes.

I actually think we probably should explore them, but we shouldn't rely on them. And I share your concern that the technofix can deflect attention from lower key, less sexy, but more proven effective things. I mean, you don't want to get lung cancer, don't smoke. That won't be a perfect prevention, but it's a lot better than waiting for the perfect lung cancer drug to be invented.

**MR. LATIFF:** So it's just a (inaudible). General Mattis, or Secretary Mattis, when he was General Mattis, running the Marine Corps, some commander in the Marine Corps actually would make his Marines train without the technology from time-to-time because he was absolutely sure that when they got into combat, the technology wasn't going to be there for them. And so, I mean, I think that recognition by the part of combat commanders is a really important one.

**UNIDENTIFIED MAN:** Can any of us drive without GPS

anymore?

**MR. LATIFF:** It was just within the last five years that the U.S. Naval Academy reinstated the training on the sextant. So for ten years before that, they could get them up -- gear that up because they had GPS. It's kind of interesting.

**MR. DESCH:** So I'd like to ask the panelists to connect this issue with the issue of interdisciplinarity that we were talking about this morning, and specifically to ask, what are the models of different sorts of disciplinary backgrounds or experiential background that ought to come together in the contemplation of these sorts of issues? Where's it been done? Where has it worked? You know, and what are the challenges to making it work?

**MR. VAN EVERA:** I'd like to sharpen that. What would you -- what do you say to the proposition that -- causing researchers to do, especially university faculties? To estimate the downside risks of discoveries is actually very hard. And they're very incentivized not to consider the risks, not to talk about the risks, not to assess the risks, and not to see the risks because they tend to often be monocultural outfits that don't work on society, don't work on society questions. They're not going to even see a problem.

Like, for example, with CRISPR, one problem that the folks outside the science world say is, hey, CRISPR's going to recreate the world of -- a brave new world where you have, essentially, or the world we had, you know, 80,000 years ago of homo sapiens and Neanderthals, two species, and all of the ethical

horrors that follow from having one-half of humanity not consider the other half to be human are going to follow for that.

You're going to break down the entire moral order of the world, under which people respect that all God's children matter because God loves everybody and we're all people. Well, that's gone. That's gone as soon as you have (inaudible) babies, CRISPR. But that's a question you're not going to see if you're in the bioengineering world. It's not a question that comes to the surface.

So I guess I'm just following onto Mike to talk about multi-disciplinarily as a necessary prerequisite to assessing these threats.

**MR. MATHENY:** Can you give one example that IARPA will be - started a project called Bright, which took -- hence, I think excellent suggestion about figuring out better mechanisms for snitching. And the goal of the project was to team up biologists and social scientists to set up a bunch of experiments, looking at ways to encourage whistleblowing about bad behavior in laboratories.

And we actually ran randomized experiments within national laboratories, DOE laboratories, to look at which sorts of incentive and reporting systems would strike the right balance in getting people to report odd lab behavior, which we engineered. I mean, it turned out it was, you know, benign bad lab behavior. I mean, it wasn't going to cause any real health risks. But the people who were observers were unwitting of that.

So you, on the one hand, want to increase reporting of bad behavior. On the other hand, you don't want it to be used as a, sort of, weapon socially by

people who are disgruntled and decide to try to get their co-worker fired for something they didn't actually do. So this required a multidisciplinary approach of social scientists and biologists and technologists teaming up.

We don't know yet the answer to the question of what works, but I really do think that a research effort focused on appropriate whistleblowing or bystander recording is important.

**MR. VAN EVERA:** Any more speeches?

**MR. GREELY:** Well, I'll say something.

**MR. VAN EVERA:** Sure.

**MR. GREELY:** I don't know. I want to get more information from you about that. Because I'm writing on that topic. I didn't know about the IARPA program.

First, I -- Steve, I think you underestimate the degree to which the scientists actually had an interest in the consequences of their actions. I wouldn't say 90 percent, but I would say a little more than 10 percent of bioengineers are interested in what happens with what they do. We actually, at Stanford have an active bioengineering undergraduate major that has an ethics class that is a requirement, and is not just a requirement, but is popular.

They are very interested. It's not a perfect control, but it's not as off -- it's not as -- the two worlds paradigm from C. P. Snow isn't as complete as we sometimes think that it is. I'd say that the interdisciplinarity really varies a lot from field to field. My field, broadly, would be bioethics. It is not -- it is on the --

it is in the middle of being a discipline versus being an area. I have friends who say it's a discipline. I have other friends who say it's not a discipline. I think I'm in the latter camp. I think it's sort of an area. It's like Latin-American studies. You come to it from many different places. You're looking at the same kinds of things, the same context, but with different expertise.

There are only three PhD programs in bioethics in the U.S., I think. I think that's an important marker of its disciplinarity or not. That makes it a real advantage for assembling interdisciplinary groups on these issues, and part of it is, in my world, the issues are almost always presented as problems, as concerns. It's not usually when something's out in the world and making trouble, but when there's a new article like the pig brain article. And people go, holy shit. What are we going to do about this pig brain issue?

And you know that you need neuroscientists involved. And you know that you need physiologists involved. And you know that you need lawyers involved and philosophers involved, and others involved. Nobody can do it all by themselves. So where I live, the interdisciplinarity is relatively easy, but I think that's partially an artifact of how the problems come to us, and the fact that we lack the kind of really strong disciplinary -- rigorous disciplinary system that many other fields do.

And let me say one other thing about interdisciplinarity, and this doesn't have anything to do with what we've just been talking about, but I sit on the Faculty Leadership Council of the Bio-X Program at Stanford, which was

intended to create interdisciplinarity, mainly in a stem world, get engineers talking to physicists talking to oncologists. We have fellowships. We give out 26 full ride, three-year graduate fellowships a year.

We require the fellows -- we give them to interdisciplinary projects, and we require the fellows to explain why they are interdisciplinary. There is real money at stake for those fellows, for those grad students, and mainly for their departments who otherwise would have to come up with the money for them. Some -- and we end up arguing a lot about what's interdisciplinary. Is it collaboration between an oncologist and an immunologist really interdisciplinary? Most of us would say no. Is one between a mechanical engineer and a pediatrician an interdisciplinary? Usually we'll say yes.

And they're required to have advisers from those two different fields, as well as to have a project that is interdisciplinary. I think that has helped create more interdisciplinary work among the graduate students at Stanford. But it's cost a lot of money.

**UNIDENTIFIED MAN:** Do you have time for more?

**UNIDENTIFIED MAN:** Yes, of course.

**UNIDENTIFIED WOMAN:** One question. If you look at the ethical spectrum that, I think, we sometimes struggle with, particularly -- I come from the (inaudible) side, and I'm an industrial policymaker for over 20 years. And you see in (inaudible) in other fields, problems that really rely on understanding the immediate, the mid-term, and the long-range impact. So

transitioning a country to democracy, the people who are living the first moments of that, are in real trouble. It's a horrible time to be in that society.

Perhaps 10, 15, 20 years later, you got a much better society. Who decides how equitable that is, and how do you factor that in as time arises in the difference between -- and I think it goes to your point about implementation and monitoring over time, because the first adaptation of something is often okay, or sometimes it's the worst part, and then the future may be much better or much worse. And so how do you think that through about this, the different impacts over time, ethically?

**MR. MATHENY:** So I -- I'm not very sophisticated in moral philosophy, but I have, I think, been persuaded that applying a discount rate to values in the future is probably not morally justified. So that people who just happen to be born later aren't worth less, morally. If we believe that, then I think things that deliver benefits, predictably, in the far future, are still quite good, even if it is the far future. And the same way the things that are distant from us geographically aren't necessarily less valuable.

I think what makes making long-term predictions about, say, whether a country's democratization yields long-term benefits, what makes that difficult is the lack of predictability of the outcome.

There is that one special category of things that we can predict are good for the world, which is people should probably continue to exist in the world, and I do think then that raises a special, sort of, moral category for risks that are

existential. And I think, you know, those are risks that we worried about a lot at IARPA, particularly in biology, but also related to the nuclear weapons and long-term effects of nuclear weapons like nuclear winter. Those are long-term effects, and they're very low probability events.

But to go back to Jim's point about precautionary principle, one place where I think the precautionary principle probably makes a lot of sense to be thinking deeply about in its application, are cases where the effects are irreversible, they're transgenerational, and they're terminal. We don't know much about the far future, but one thing we do know is that if there aren't people in it, that's bad. Okay?

**MR. GREELY:** I can't resist pointing out that for almost of all the biosphere, except dogs, cattle, rats, and cockroaches, a world without people would probably be a better world.

**MR. MATHENY:** Man will die in a billion years because of the adapt of the earth. So if there's any long-term prospect for animal life, it will be thanks to humans.

**MR. GREELY:** Oh, and you think we're going to make it for a billion years.

No, but to get to the question, I think it's a really interesting and hard one. And I think you've got -- but I do think those of us who are around and competent to make decisions now have to be okay with decisions. You can't say, well, I'm sure that our great-grandchildren will like this, so we're going to institute

it even though everybody right now hates the idea. You can try to convince them that their great-grandchildren will really like it, and that's a good reason for them to do it.

The flipside of that, though, I think is a little trickier. I think we need to -- sometimes we say we shouldn't do these things because in -- our great-grandchildren will end up with a, you know, a culture that's different from ours. And I think they will inevitably. And they should. And they're just -- you know, I don't think we have the right ethical answers for all time right now, and if our grandchildren have different ethical views, we shouldn't act in such a way -- we shouldn't assume that what would be right in their world is what looks right to us. But it's complicated, so --

**MR. VAN EVERA:** Thank you.

Thank you, team. Outstanding.

So, all right, we've got a break on the schedule, and we're going to reconvene in 15 minutes at 2 o'clock. And our colleague, Bruce Jentleson, is going to make a very interesting presentation on a task force he chaired on tenure standards at Duke University.

So please feel free to rehydrate, re-cookiate, or re-coffiate here, as the case may be. And see you at -- before 2:00.

**MR. DESCH:** I want to move on to our next presentation. It's my great pleasure to introduce my colleague Bruce Jentleson who's a professor of

public policy and political science at Duke University. I could talk about all of his books and articles and other honors but that would cut in far too much to his presentation. I would say, however, that Bruce is one of those people who not only talks the talk but walks the walk-in terms of bridging the gap having himself served in the Department of State on the policy planning council and also on the Obama 2012 campaign. Bruce, along with Jim Goldgeier and Steve Weber from UC Berkeley, are also the principles of another Carnegie sponsored Bridging the Gap project eponymously titled "Bridging the Gap" which provides a couple of weeks every year of really terrific hands-on training for young academics who want to speak to a broader audience. He has a new book out *The Peacemakers: Leadership Lessons from the 20th Century Statesmanship* that I'd be remised not to mention. But the topic that he's going to talk about today is literally "In the Trenches" for the issues that we're concerned about and that's where does the rubber meet the road in terms of tenure standards. And he has co-led a task force at Duke over the previous two years that is trying to establish a new set of tenure standards for the university that will take cognizant of some of these broader issues. So, Bruce, we're very much looking forward to it.

**KEYNOTE SPEAKER: "REPORT OF THE DUKE UNIVERSITY TASK  
FORCE ON TENURE STANDARDS."**

**MR. JENTLESON:** Thanks very much. Let me turn this on. So thanks to Mike and I also want to thank Steve Del Rosso who had to leave. He

told me not to take it personally for all his support for our project, Mike's project, the project that Jim and Frank of the Carnegie Corporation really has been a leader in the foundation world in efforts to connect (inaudible) and the policy world particularly in our area of international relations.

As Mike said, I was co-chair of this task force Tenure Standards Committee at Duke, and it's interesting because there was actually a relationship to Bridging the Gap. I think Mike mentioned this morning that -- so Jim will talk more about our program in the next panel, but, basically, we've been working in our area of international affairs, public policy, political science, and sort of what you might think was a bottom-up strategy trying to provide opportunities training for PCs, for faculty, et cetera to do it.

And we realized that at some point the bottom-up chart needs to try to beat the top-down, so about three years ago, we convened a conference of provosts from universities to talk about us as really a case study. And some of the provosts we knew from our relationship, but we had about 14 provosts there from a range of universities: biggest University of Wisconsin provost at the time Sara Mangelsdorf was there so a big public university; some other Tier 1, some HBCUs, some small colleges. And provosts are busy people. They came to this conference and spent a day because they believe it was worth their time even though they didn't quite know who we were. And so we're trying to figure out to broaden what we do in the sense that the presidents and provosts with the range of constituencies that they have. They were feeling more, I think, the pressure, the

interests, consistent with the multifaceted mission universities have to broaden what we do as institutions. So my provost Sally Kornbluth with was part of that. Ironically, the provost at Penn at the time Vince Price was part of that. And so when the Provost Kornbluth decided to form this committee, she asked me to co-chair it because she knew the work I was doing at Bridging the Gap. It also turned out about a year later, we had a new president at Duke that happened to be Vince Price from Penn. So a lot of things were coming together.

The theory of the case here really, we've talked about a lot of things. And this morning we talked about trying to chip away and create site incentives, but the tenure system, and by that, it's really a misnamed report because we really talk about the whole food chain, not just tenure but promotion. And it really feeds into the hiring process because we've all sat in committees where somebody says, they're doing really interesting work and it's very relevant, but it'll never make it through the central committee, what we call APT at Duke. And so the notion is if you could make some changes -- and I don't want to over say we're proposing. We're not revolutionizing the system, far from it. But if you could make some changes on the lines of public scholarship and the like, you could begin to really get at the crucial aspect of universities and a flat governing system with a lot of faculty governance, it would be really important.

So what I want to do pretty quickly and it'll leave plenty of time for questions and discussion is sort of run through the process we use for doing this report; key parts of the report with a particular emphasis on the part that deals with

what we call the report public scholarship, and I'll define what that is; and then give you a little of a sense of where it is in terms of the Duke policy process. It was a multidisciplinary effort, and the recommendations we made were in every level from the department to the school to the university. We had a range of views on the committee, people from systems perfect, all the risks on the site unchanged, to blow the thing up. We got unanimous support for the report which should mean everybody agreed with everything in there, but, as a good decisional as a political scientist, the bulk was on the thrust of the report and anybody that wanted to write a minority email to the provost could do so and, in fact, did not do so. We did at the very end of the year, but he was tired, right, so that helps it.

And it's really, not so much for Duke, but we think that it has a lot to say. It was based on a lot of research that we did in the higher-ed literature, a lot of outreach and the like, so I want to talk about it in ways that might be of interest at a range of universities. So what we created -- it was part of the strategic plan set up by our provost, Sally Kornbluth, and actually defined it. It was very much, as the goal of this committee, was to address increasingly diverse forms of scholarship. The fact that increasingly diverse forms of scholarship had not been taken into account in our appointment, promotion, and tenure process. While progress is made on interdisciplinary efforts, so we're talked a lot about that, and it is not to say they have been solved but we've been working on that for at least since the early 2000s. Criteria for rigor and impact in a less traditional, alternative, or emerging forms of scholarly expression including online education,

public scholarship, and policy outreach have not been well-defined. The committee was to re-evaluate the criteria for tenure promotion to clearly define criteria that would reward the many scholarly activities we value as a community.

So it was in August 2017 and in October when the former provost at Penn Vince Price was inaugurated as the new president of Duke and made part of his speech was along these lines as well: "We must prevent our research from ossifying around practices that were designed to confront another century's challenges and that limit our ability to confront the emerging problems of today. The landscape of human knowledge and human challenge has changed and so, too, must our maps and tools for navigating them. Are we bold enough now to invent more productive and sustainable ways to organize and catalyze scholarship around pressing problems? Are we broad-minded enough to collaborate across the full range of scholarly perspectives, disciplined enough to draw resources just to support this work, and," flexible enough to alter expectation of what counts as valuable research? That bold is my addition of it. It wasn't necessarily in his speech, but I think that's really what we're trying to get at here in this process.

The specific charge and mandate to the committee was to identify and define what types of knowledge creation and dissemination should form the basis for tenure and promotion, to create rubrics for evaluating and assessing excellence, including measures of engagement and impact, ensure Duke's commitment to excellence as well as its commitment to diverse and innovative scholarship. This is probably arguing against the notion that might come from

more traditional areas. Is this moving away from what we consider to be research excellence, and the argument is no, it's not. It's providing perhaps more of a pluralism of what we consider to be excellence. And we're focusing on research dimensions of tenure, not on teaching or nonpolicy engagement services.

I'll talk a little bit about that with Louise Richardson last night about the role of teaching in the process, and it just wasn't part of our many -- we actually had an academic year to do the research and to write the report. And that's its own issue that we did touch upon last night, but we didn't do it. I can't really read all this, but the committee had members, my co-chair Anne Allison was from culture and anthropology. We had representatives from the Fuqua School of Business, from the Nicholas School of the Environment, from stats, from chemistry, from econ, from the school of engineering, from molecular genetics and microbiology, romance studies, divinity school, African and African American studies. Every unit except the law school which, for reasons I don't understand, doesn't do its tenure through the provost. And the part of the medical school that was on the clinical side was not part of it. The part of the medical school that did a tenure process were part of our process.

We had a number of administrators on as ex officio members. And we met on a regular basis. We met at least once a month and that we formed into sub-committees which track with the parts of the report that I'll mention in a second that kind of do work. We had a little bit of staffing to do it, a significant investment in time on my part and my co-chairs. Part of the process is a lot of

faculty input and outreach. Every one of those members of the committee started with a liaison to their departments or more broadly their schools with a series of questions that were standardized to get responses on about people's sense of how the system worked and how they might be initially thinking about the issues we were dealing with.

We presented to the dean's cabinet both early in the process and then this past October on the report completion; to the board of trustees academic affairs committees also twice during the process and afterwards; to our academic council, which is our faculty governing body; academic programs committee; our APT, appointments, promotion, tenure; and there were a lot of individual consultations both people that said they wanted to talk to us and people we thought it was important to talk to both for their ideas and for the politics of the processes.

The external outreach we tapped a lot of provosts from the Bridging the Gap network. We did a lot of research on what other university guidelines were on these things, on public scholarship, on digital and the like to see what we could learn and adapt. We met with a bunch of professional associations from a variety of disciplines and there were some related initiatives at other universities.

The research was really important. Like a provost said to me, she says, you know, when it first thought about this, I thought you'd have a bunch of people in the room and you try to get consensus, but if you read our document -- and I'm happy to circulate it and I'll tell you it's available on the provost website, but I can circulate it at anybody's request -- it actually reads as a research paper

because we try to make it empirical. And I'll give some examples of that so that it wasn't just a set of opinions, and there was a fair amount of research including on metrics for measuring and evaluating productive output.

So the report itself is structured really with five chapters. There's an introduction; a chapter on existing policies and practices which if we're changing nothing else, what would be some of the issues we would want to change with an existing policy and practices; a chapter of diversity; one on public scholarship; one of digital scholarship; and one in the arts. The arts chapter was often questioned of whether we should start tenuring what are typically, in our jargon and others have similar jargon, professors of the practice in the performing arts, and in fact a number of universities that don't have schools of the arts had been doing. That's not as relevant as today, but I just want to mention that that was part of it in terms of that.

Let me quickly talk about the existing policies and practices. There was one section on procedural issues, and these were a bit administrative. They may similar to other universities. Just some examples: We found that the third-year reviews around the university really vary widely, and it is important that they be clearer of guidelines given from the provost office about how to conduct those; and there were some concerns including things that could go into legal cases, about what I was told I had to do with the three-year review and I interpreted that almost contractually, and et cetera, et cetera, et cetera; changes in Annual Scholarly Activity Reports in part to be more inclusive of public scholarship,

policy, engagement, in digital scholarship. The report you did to your dean in most parts of the university and that would also get in the point of the sort of main website so people can go and find information on the faculty didn't have sections on public scholarship. It didn't have sections on digital. It didn't have sections on policy engagement. Various issues regarding external reviews, issues between book-based disciplines and article-based disciplines and sub-disciplines, and also the question of interdisciplinarity.

This part's a little more interesting: the substantive metrics. And Mike has been working on the exceptions in his book, but he gives these presentations, as some of you may have seen, on the National Research Council rankings. The metrics that are used in terms of substantive aspects of these reviews and the idea of -- and what we tried to -- what we showed was there were flaws in the usually quantifiable and scholarly world-based metrics because there was some sense that we know what the gold standards are over here and we're moving into these worlds of digital and public scholarship and they're a little vaguer. And the notion was that not to throw these out, but to show that they had their inherent flaws too. One was the perverse incentive and citation counts that favored incremental scholarship and what one member of the community called bandwagon style research conducive to high citation counts but also less intellectually rich and innovative, right? So one of my staff professors who did some of the medals for this said, things that I write that everybody cites, I would argue, he says, they're less important than articles I write that are deeply

intellectual about changing the field, but the incentive was just to put those out there to get your citation count up. So whether the citation count is really a measure of impact on your discipline, intellectual quality, is a little bit of a question mark there.

In the literature, they used the term citation "cartels" which would be not that Mike and I go into a room and we -- but there's a notion that he'll recommend to his PhD students that they cite me if I cite him, and we haven't really said that, just like other cartels. And there's a lot of literature on demonstrations of this aspect of it.

The journal impact factor, again, through some of the studies done by my center said that there's a tail effect. So, if I write an article for a journal that has a very high impact factor, for my article contributes a very small number of the citations to that and Mike writes an article for a journal impact factor that's a lower ranking but is responsible for a significant number of citations, if you just look at journal impact factor, I'm going to benefit from that metric more than he does. One study that was done in the *Journal of the Association for Information Science and Technology* did a link between high-quality articles and journal impact factor is "weakening steadily since the beginning of the digital age."

The H-index is another index used. One of my economist colleagues showed how he had a higher H-index than at least two Nobel Prize winners. He said, I think, he was as good as them, but he had a point to make. And so the recommendation here was to continue to use such metrics but take a count of their

limits and flaws as indicators of scholarly excellence including, as people said, sometimes people don't read files; they just go by the numbers. And so it was important to just give a wake-up call on those issues.

The diversity issue: again, we started a number of studies from a number of disciplines that showed that factors such as gender, race, place of birth, or prestige of the affiliated institution can impede one's chances of publication regardless of the quality of research. Some examples: a woman in the economics department is less likely to obtain tenure the more they co-author peer-reviewed articles. Top political science journals did not reflect subfields more highly represented by female scholars, conferring publishing disadvantages. Now the top flagship journals aren't as interested in the things that female scholars might be attracted to so, therefore, you're not likely to get into those journals definitively. Studies of international relations subfield found biases in citations of articles written by female scholars and racially based expectation patterns were found in a number of disciplines including philosophy, anthropology, sociology, and communications and, again, this was an effort to show the empirical basis for the report.

In terms of recommendations, again, it could be longer discussions that this, but one of our points was that it was important not to sort of just raise these issues if you have a female faculty member or somebody from a minority coming up, but that as a general matter and separate from a particular case, departments should review relevant studies in their disciplines such as those cited

in the report. That one was a seminar in your department, so people are a little more aware that this is going on as a general sense of how they approach reviews. And there will be a periodic reevaluation of the process by which departments and APT account for diversity and take note of aspects that should change. So those were, again, two parts.

The public scholarship, it's the part that really relates to us most in policy engagement. Our recommendation here is to maintain disciplinary and interdisciplinary scholarly work as their principle basis for tenure and promotion, while giving greater weight in relevant cases within the research-scholarship basket to public scholarship. It's very important for this to be in the research basket and not just in the service basket where we talk about policy engagement. And it wasn't so much to say, and instead of, but if it somebody has done work that is public scholarship, that should be counted in a significant way, but it's also important that they show they're doing work within their discipline. So what types of public and policy engagement merit consideration and what criteria for impact, influence, excellence, and rigor should be applied?

The definition we work with the public scholarship is pretty much taken from the report a little bit abridged is publications whose primary audience is not confined to the scholarly community, but which draw on the faculty member's scholarly specialization and are subjected to expert review whether peer review, editor, or another comparable form. In the sense of the committee was this applied in different ways across the disciplines. What my chemistry professor

said, if he develops his research leads to a patent and he just doesn't publish it in the flagship journal, why should that not be counted if it was genuinely intellectual scholarly research? People in the humanities who might do projects with museums to increase -- sort of in the spirit I think that Jim Steinberg and others were raising to let us think about -- public awareness and the importance of what they do through museums through their own scholarly research.

See, there was an example that came across for every discipline, not just those of us who do international relations or foreign policy. Does it include if I'm real upset about gun control, and I write something on gun control? That's not public scholarship because that's not my specialization. That's just my own personal preferences but acknowledge that you could use your scholarly specialization for publications in which the primary audience is not confined to the scholarly community. It included the development of databases for use by individuals or organizations extending beyond the scholarly community, say patents and intellectual property based on scholarly research that have manifestations other than, or in addition to, peer-reviewed publications.

We talk about policy engagement as well, which is your actual involvement -- testifying in a congressional committee or an expert task force or serving in government as well -- but we really put the emphasis here. We give a lot of emphasis on the importance of that in many ways but here we're really trying to crack the nut on this notion of research because our current practice is, if you do something like that, it's kind of considered over here as service or outside

activity and the like. It also included research and other forms such as websites and documentary films complementing articles and books, no less based on relevant scholarly research in order to reach audiences and achieve objectives for which these forms are particularly well-suited.

So that was the definition that we sort of hammered out. Why? A lot of what we talked about last night and today, the importance of the role of universities in society: local, national, global. This is not a commentary on the current situation, but it really is the sense that we are, I believe, privileged institutions in society. We get a lot of benefits from society. We do have a multifaceted mission, and the mission is not strictly to produce things for people who are within the university world and that it's very important for universities. And this is what we got from those presidents and provosts at our 2016 meeting.

Wholistic conception of faculty excellence: That the scholarly value is the scholarly value; it's not just based upon whether it appears in a disciplinary journal. It's kind of a movement for universities and competition for human capital. I mean, the argument is just why this is a food chain thing. We don't say, you know, go get tenured, go get promoted, and then do that for a number of reasons that have been brought up earlier, and brain scientists tell us that late 20s, early 30s can be your most productive, creative period. But we find in our work with PhD students in our field, that there are many who say, I really care about the world, and the reason I went to a PhD is because I want to care about it as a scholar. But if I've got to go through the next three years of finishing my PhD to

be competitive in the market and do something that I don't really feel is as valuable as everything I gave, and then next six years, then six years, we lose a lot of human capital. And, if we care about universities and institutions, all our missions including our students, then we really have to take this competition for human capital very seriously. And it benefits the faculty and all of us can tell who've worked in the policy world can say, you know, there are distinct opportunities for intellectual interactions that can help refine ideas and generate new ones and generate data gathering. You can either directly or indirectly establish a relationship with somebody at the Bureau of the Census or at HHS or wherever, and then you know where to go for the data and then you develop a certain amount of trust. You expand professional networks. You apply knowledge to public problems in ways that are intellectually gratifying and socially meaningful. They're really important, so it's not just the first thing, well, I have to do this at the university -- but the value that it can be for faculty.

If you look at the literature coming out of professional associations, so just some examples: The American Sociological Association calls public scholarship an essential component of scholarly excellence of university's obligation to society. The American Political Science Association that Betsy mentioned last night is surveying departments about inclusion in public engagement in tenure and promotions. The AAAS has a whole program in AAAS's website on why public engagement matters. And one of the points they make is, through engagement, scientists and the public participate in discussions

about the benefits and risks of the science and technology impacting our lives. Many scientist value public engagement with science but are not aware that their colleagues feel the same way. One of the NSF STEM grants is evaluated on intellectual merit and broader impact, defined as "potential to benefit society and contribute to the achievement of specific, desired societal outcomes." I know Gavin and others spoke this a little bit this morning. A number of universities, both R-1 and others, are working for greater valuation of incentives.

And the British, that we mentioned with Louise last night, they have this Research Excellence Framework which is based on "the quality of outputs: publications, performances, and exhibitions and they're impact beyond academia." It's a complicated process. It has strengths and weaknesses, but it's an interesting thing for the U.K. funding that she was describing that Oxford and Cambridge, the (inaudible), and others who are dependent upon, there is this crisis now across the university of trying to have a framework for measuring research excellence in those ways.

There are a number of caveats as it relates to this. The first one is the goal here is greater intellectual pluralism, not a new fixed mandate. A political science department still needs people who may be building formal models that really don't have any outside applications who are developing theory qua theory as any other discipline. But it is an argument that we need more intellectual pluralism in people, again, in cross-disciplines who are oriented to public scholarship.

As we started the process of policy for Duke I'll talk about towards the end, it's a federalism concept, that there's a core-centered university, there needs to be core-centered university norms and rules.

**MR. DESCH:** Bruce, can you go back a slide? I think you --

**MR. JENTLESON:** Oh, did I flip a slide? I'm going the wrong way. A core-centered university norms and rules, but the tailoring and operationalization by schools and departments. In other words, what we're trying to do, and we'll hopefully do in the next few months, is have the provost set up what this definition of public scholarship is, what the expectation is, and then have departments operationalize it, right. Some will be grandfathered in. You'd say, well, I'm on my third, fourth year. And again, it's not for everybody. It's not a one size fits all, but they'll need to be reassured, continue on the path that you're hired on and you were working on. And the appropriate assessment measures and parameters need to be applied.

I made this point before about not just after tenure, nor wait until full professor before -- and take into account in hiring with the example I used before, at least how some positions are defined. We used the term earlier, problem-oriented research. In my field, some nice people view as policies the means to the ends of theory development. It's fine, but a lot of times you can use theories and means to the ends of understanding policy or addressing a problem. And so this would feed through that food chain.

In fact, it really needs to affect the organizational culture and PhD

students who are interested in non-academic careers. It's fair to say that I've known colleagues who would say, well, you're a PhD student who got their PhD and then went to work on the seventh floor of the state department really is a failure, and my PhD student who got a tenure-type position at a very, very obscure state university somewhere is a success, right. Both need to be seen successes and PhD students clearly keep their organization tools and I think Jim and Frank were very much -- they are our programs and the programs we run are very much oriented towards helping people with that and building support of cohorts among faculty and graduate students.

So the evaluating quality: the kinds of questions that are being asked in developing metrics are not that different than for academic publications. Is the publication at hand well-grounded in disciplinary or interdisciplinary theory and research? Has it been reviewed at a peer-review like process, or it's going to be editors as well? Does it demonstrate knowledge of the required format, needs, and frames of reference of the audiences it seeks to reach? So, if you write for foreign affairs, you won't have a lot of footnotes in it. And you try to express these ideas you have in a different way than if you're writing just for world politics or something. And then, well, not in the same style as more strictly academic publications, is the empirical basis and conceptual logic of findings claimed and arguments made effectively presented?

It's fascinating; we have a division at Duke called ScholarWorks that has this website called Understanding Research Impact. That's a useful guide to

various metrics and approaches to assessing research within, as well as outside, the academy, and they're, like, over here in the library. And they were not really being drawn on by, like, tenure committees and they have a variety of metrics none of which are perfect. I don't know if people have used this one called Altmetric Explore, but it's one that is used for a lot of online publications. It goes beyond how many hits on the website or that sort of thing. And it's a useful heuristic for general visibility and for reaching sources and outlets that traditional citation indexes do not. So there are some metrics. There are a couple of others that we talk about in our report. My own sense of this is we need to have metrics combined with the kind of questions that we ask which don't necessarily require common quantifiable measures but are qualitative judgments about quality. Start using them, getting them out there and, in this spirit of cumulative knowledge like anything else over time, they will get improved and perfected. But as long as they're not used in a serious way, that's not going to happen.

External letters: well, what do you do with external letters? So it happened as we were going through this that I received two letters from the same university asking me to be an external reviewer for one person who was being considered for tenure in a traditional social science department, and another person who was being considered for the equivalent of tenure outside that social science department in another part of the university. And the letters kind of track like this and then the one that really wanted to know about public scholarship asked me to address the candidate's commitment to understanding or addressing important,

contemporary issues in ways that illuminate problems, propose innovative solutions, develop novel methods for approaching problems, formal or informal influence in policy circles, their ability to translate scholarship into policy formulations -- reasonable guidance for writing two very different letters in terms of that.

The British system as I mentioned has a lot of complications.

Colleagues who have been involved and it's very cumbersome and bureaucratic as Louise acknowledged. Some of the questions that they've been still wrestling with is what about an important and high-quality scholarly contribution that is rejected with policymakers for reasons other than its scholarly quality? Or we might have some examples. One of the ones that are followed and end up blowing up the world. How would a scientific insider or discovery be considered this highly impactful eventually but only after an extended period of time? These are tough questions. But, again, I submit that they're relevant as well and to strictly academic side.

In digital scholarship, I'm going to go through really quickly because I want to finish and just open it up, but we ask very similar questions and some of it was overlapping. How do you measure and assess scholarship in a digital age? Digital and technological transmissions, circulation, presentation of scholarship, and that there was a disconnect between the encouragement of digital innovation but not a comparable adaptation of the tenure and promotion system. Here's some examples of the kind of things that different disciplines would consider as digital

scholarship: databases, digital tools, new code languages and algorithms, open-source initiatives. You can see the list there. This was sort of an aggregated aspect.

The metrics: some of them wanted as similar to the public. They tried to get at the rigor of the scholarship. The originality and the impact of the research within a designated or emerging field and how robust the review process is. The questions being asked -- and some of this we actually took from the guidelines of other universities that were actually starting to work more than we were on the digital. Is the product based on and/or presents original scholarship? Were there serious reviews? Is the venue prestigious? Does it have recognized scholars who also published in the venue? Does the product make a significant contribution to our understanding of an important topic? Is there a process assuring duration and longevity of the project? How do the uses of technology compliment, extend, or enhance what this scholarship would have otherwise achieved by conventional medium? So again, these are the kinds of criteria or rubrics that you can develop. That's really all.

This was that the arts, which is interesting in itself, that aren't relevant to us.

So we finished this report in May 2018, and we spent most of this year presenting it to different committees. We are politically liberal and organizationally conservative, and I promise I would go back into government. I'll live with my criticism of bureaucracy and slowness of process. I'll probably forget

that in a moment, but we are very bureaucratically slow. So we've had an initial step this year in which the provost asked also within the departments to report on their current policy including how public scholarship is valued and assessed. And when this went to the APT committee, myself and my co-chair were asked to read all the reports and meet with them. And pretty much across the board, I would say the dominate -- there were very few responses that got it, that were where we want to be. And most of them were, we recognize that it can be important, but we really don't want to encourage you too much because we still don't really evaluate, basically, they said. And every word it takes away -- Jim, I'm going to steal one of your lines you always use who says that -- if you write an article for foreign affairs by staying up late at night after you finish your journal article, if you'd stayed up late to watch *Game of Thrones* nobody would criticize you, but somehow that you did this means you're not using -- you should have used that extra time for the next article.

So we got all those reports in by the end of the academic year, we reviewed them and the chair of the APT -- well, he's very supportive of what we want to do. We're moving into a process now where we're going to apply the report and make recommendations including the caveats and, hopefully, have a process over the next -- the first semester where we'd have a get in how would you take those guidelines that you have and adapt them with some of this -- some of the key recommendations of the report as guidelines? We're going to try to put an article -- post (inaudible) or some other publication.

Because, again, this is really not just about Duke. I mean, I actually think for a university in the sense we've gotten is, if we were to make a solid move, not revolutionary but serious revision, that there are a number of other universities and we see that as a precedent that they could then use in their own processes and then, over time, we get to a better balance and, again, none of this is so you kind of keep it stretched out. This is to say that there shouldn't be research, quality research, that's just for the academic world. But, I guess, I strongly believe very much for us as institutions that this intellectual pluralism is extremely important to universities. It affects our teaching. It affects so many other things. So that's where we are. I'm happy to answer any questions or take any comments in the time that we've got left, so thank you. That's it.

**UNIDENTIFIED FEMALE:** Were you able in any way to take a look at what you determined and say some pool of 10, 20 people would have gotten tenure using these new standards versus would not, did not?

**MR. JENTLESON:** You know, I think that would be very hard to do. I could think of ad hoc some cases that I've known at my university or others. But I actually take it back to that food chain problem. I mean, I have had a number -- and again, as a policy school, we have nine different types of PhDs, so we're higher economists, high historian and scientists, but a number of people, including outside my discipline, whose work I thought was really interesting. It was solid scholarship, but it was addressing a policy problem whether it was problems of lack of daycare and how it affects women in the workforce or the

right thing. And you sit in these meetings and people say, that's really interesting, but the other candidate, who's writing strictly for journals, is more likely to get through the APT process, so there's call it a deterrent or preventive effect.

We've had and I know in our experience in our Bridging the Gap program with a lot of self-selection people coming through as PhD students and faculty are oriented, and we've been seeing tenure happening in a lot of places partly because they do the disciplinary and the other, but, again, I think it's really important to keep those people. I have nothing against the think tank world, the policy world, the NGO world, and the private sector, but in a selfish way, I want universities to get the best human capital out there for what our role is in cross-discipline so that would very hard to measure. Yeah.

**UNIDENTIFIED MALE:** Just out of curiosity, did you run this by young assistant professors? What's their comment on this?

**MR. JENTLESON:** Yeah, in fact, I spoke at a conference that the provosts convened for newly tenured professors across the university and some of them with total cognitive dissonance, right, but I just want to keep doing what I've been doing that got me here. And others were, wow, the provost -- and, again, we haven't even finished the report at that point. This was February of last year 2018. But those are -- that would be great if we move in that direction because I really want to -- I know I haven't been able to do it and I've been suppressing it.

I can tell you from our work and others who work with young faculty and PhD students that in our field there is very high demand. I mean, this

younger generation of people coming into our field really are coming in -- and some of them have worked whether it's in an official policy world or the NGO world, had been in the military, and I feel like the same thing. My sense is from my colleagues in other disciplines in the sciences and the humanities.

So I feel like if we surveyed, it would be self-selecting, but I think and, again, it's not -- I don't envision 51 percent of positions in any discipline over the next decade making a big emphasis on public scholarship but we got a decent percentage that really helps in a tremendous way. Yeah.

**UNIDENTIFIED FEMALE:** If I could ask the question again a little bit from the European perspective, you're probably familiar with Plan S in Europe which is a move towards open science and moving away from publishing traditional journals that are not open access, fully open access, which will theoretically have a huge impact on the scientific publishing industry and indeed on something like tenure. Because, for example, in Europe and I think now at least 15 different funding agencies in Europe have signed up to Plan S, so by 2020 anybody who wants to get funding from one of these European funding agencies, including the European Commission as of Horizon Europe, will have to publish in fully open access journals which tend not to be, shall I say, the journals which today are considered the prestige journals. And this will have a huge impact, and it will have a huge impact on tenure. And I'd just like to know if you thought about this as, hopefully, it's the future.

**MR. JENTLESON:** We did I think in one of the -- I think I may

have mentioned or if not, I left it out just to condense the report about open sourcing kinds of materials and they are very important, after all, there's no paywall, and secondly, chances are people writing for open source, they may actually write it in somewhat more accessible English without diluting the quality of the intellectual quality. And so that's a good example where, if you get an intersection between universities changing their criteria and the scholarly publishing world changing its, you begin to get a number of forces working for change, right, and that's happening in your perspective. I know from the time I spent at Oxford that at least one of the colleges was getting a little concerned they were going too much to the American model, which was very heavily in a particular type of social science.

So I think all of those factors -- if there was a notion that bringing in a range of types of excellence of scholarship in the kind of people we staff our faculties with, our tender line faculty, because it's really not the only important factors but are really the only ones who are full citizens in university governance and you get a range of factors. I really think it helps universities in tremendous ways and I don't think it -- you know, Louise was very concerned with the problem of getting too close to policy and coming that -- I think many ways, many paths along the way don't get to that. And by the way, policy we mean a lot of things. We have a lot of people working in the NGO world with a prior accepted role they play in conflicts around the world, so it's a very broad conception of policy in political science let alone looking at the physical sciences and the others

as well.

**MR. STEINBERG:** Yeah, I just one comment and then one question. So we brought a number of the Carnegie grantees together last May in New York and Bruce made this presentation, and we had a little bit of a discussion on the question -- most university scholars are rated for both annual review, merit reviews, and tenure promotion on their scholarship teaching and service, and in many places, people aren't really sure where to put this type of work that they're not -- some places, they're not really willing to have it count in the scholarship section but if you put it in the service section, it tends -- they never count very much for anything anyway. So there was a conversation last year that I think it was -- I think maybe Debbie (phonetic) said it -- there was somebody who said that they were talking about creating a fourth category for evaluating faculty of this type of engagement that would give people a place to showcase it, so I mentioned that.

My question is you mentioned at the onset, and I remember when you presented this before, that other universities had moved in this direction and that you had gotten reports or other guidelines from other universities that were trying to do this. I'm just curious whether any of them are far enough along that you were able to get a sense of what it's meant on their campuses?

**MR. JENTLESON:** Yeah, on the public scholarship, we really didn't find anything other than, you know, sort of the lip services to it that I was talking -- and the digital I can tell you Indiana University had done a lot of work

on digital because they have cultural studies there and that was one of the ones that we really looked at and talked to and tried to derive from. So I think we found more interesting ideas on the digital side than on public scholarship. And your other point, I mean that's why, even if there was another category -- research teaching, service, and public scholarship -- we know that the tenure process really is -- how many people -- this is going to give anxiety -- well, they're not a very good teacher, but they're really doing good research; they'll get tenure. Well, the research is okay, but they're a great teacher; chances are no. So unless you get this into the same basket as other research, it's just not going to have as much of an opportunity to get serious attention. And we actually -- the public scholarship definition I showed you we developed -- there are three or four definitions we found in the literature that we footnote, we kind of drop on it and develop our own. So that's really crucial.

I think in the service, we also say, look, service lumps together -- service to your professional association, service to your department and policy work -- and we wanted to try to break out things that were external to the university as another part of that. And as I said, the annual report that people do, they didn't even have any of this on it as you report to your dean and then gets posted on the standard website that anybody goes to find it. These weren't even in there as categories. So some fairly basic things need to be done as well.

**MR. STEINBERG:** When you say get it into the basket of research, I think that's right, but the issue is how it gets in the basket? Is the part

of the cake or is it the icing? And it gets back to your *Game of Thrones* comment because people, I think, are willing to treat it as a plus if they've got everything else that they need, but it won't substitute for the disciplinary research and that's the real question. That's the real rub is how much are we going to allow it to substitute? And so I'm wondering kind of what the discussions and the conclusion about that substitution work.

**MR. JENTLESON:** Yeah, I think it was the sense that I think the definition that I gave -- where was my -- maintain disciplinary interdisciplinary scholarly work as a principle basis for tenure while giving greater weight around the cases than to the public scholarship. And our sense was so you could be in a situation in which somebody has done enough to establish themselves in their discipline, you know, as a quality scholar and then also has done really quality public scholarship, which is different than saying that it's just the icing. So, if you want to measure it, it can replace a certain amount, a certain margin of public scholarship. We try not to specify that but that's -- you know, because it's just very hard to do. It's the two articles and then another, but I think and at your three-year review sometimes you can get a sense if these things are there from the department. But it's really it can't just be, oh, you know, you did everything as that person is doing, no public scholarship, and you did this. And so it's going to have to develop into how it gets applied, but that was sort of the phrasing that we wanted to do. And we always counsel people coming through our programs. You know, this has to be in addition to, not instead of the other. I don't envision and I

wouldn't recommend somebody getting tenure at a university who only does public scholarship? But I do feel strongly that it should be given a significant portion of the -- and then how do you do that? It's just like the metrics we're using. You can't really put a number on it, so it has to be a general sense and application across cases. Jim.

**MR. STEINBERG:** I think it's a really important point though, Bruce, which is that, I mean, put yourself as many of you been in the position of the tenure track professor and it is the *Game of Thrones* problem. If you're saying that it isn't a substitute, it doesn't count in the same way that the others do, then it is a bad choice for the aspiring tenured person to spend time with that because the same amount of time spent on the other thing is going to count for more. And so I realize it's hard to operationalize but if the message isn't that it counts in the same way that you -- just like in the disciplines now, you get three articles in the second tier versus one article unless people have some understanding about what the trade-off is, so maybe it's two public engaged scholarship versus one peer-reviewed, but if there isn't some way of seeing that there's some of this that you can get that counts as much as the other. There is zero chance that people do it because it would be irrational for them to do that. I mean, it does become that problem and, before you -- so it just adds one more feature to that which is the external reviews. You touched on it in terms of the letter, but for me, the big issue was not so much what was the instruction to the external reviewers. Who are the external reviewers, and how do they get selected?

**MR. JENTLESON:** So on that second point, there was a reason I was asked to write that, right? I mean, they did ask the person that they thought could speak to it, right. And I know you used to talk about how you would have one letter at Syracuse that was specifically about a person's --

**MR. STEINBERG:** Right, but we had to make that a part of the process because if you'd left it to the departments to pick the external reviewers, there was zero chance because they were going to get their friends who had the same view about the profession that they had. It became a self-replicating institution.

**MR. JENTLESON:** Yeah, part of this is if you get established -- and part of the reason of your process, if we get a strong statement from the central administration that we want to factor this in, then we're going to get a lot of different ideas and some of them will be rejected and some not. And I think the substitution is a little bit like -- it's not a one-to-one ratio of substituting for strict work, but it's probably after a certain point of establishing that, there is a degree of substitution in effect.

Two other things and that one is we finally found -- so Bridging the Gap now, we have our three original directors -- Jim, Steve Weber, and I who are senior faculty -- and three of our younger folks who started with us as assistant professors at three very different universities and all have gotten tenure, right, and they spent a lot of time in our program and they've also done publishing. And we've had a number of our participants now get tenured, and so we started this

book series with Oxford University Press that's called the Bridging the Gap Book Series, which is designed for books by scholars with one of the most prestigious presses that is we say is genuinely policy relevant. So it's not a bullet-pointed Deputy Secretary Steinburg do this, but it's also not turning point papers in three paragraphs.

We now have, like, nine books in the series. We're getting proposals in different junior faculty as well as senior faculty, very interesting gender balance going on. And so we did it with the Oxford because we did it with a third-tier university press. People said, oh, yeah, that's a publisher that do public scholarship. So it's sort of like the point about the open-access journals. As a scientist, you build a lot of these things in that creates opportunity, so somebody will say, oh, they published with Oxford. That and they still got to do a policy-relevant book. So it's a tricky process which is why I feel like, you know, I'm hoping that we'll be confident enough to launch it and have enough reliable factors to work with and then the only way you know is you road test it and refine it over time.

The worst-case scenario would be tenure somebody you shouldn't have tenured, which I don't want to happen, but let's just say it happens already, right? I don't mean to be cynical about it because you'd really want to have success because people who are looking to discredit it would say, oh, look at what you did over there, so it is a tricky business. And that was kind of about as far as we could go, and the next step is to see how well it gets operationalized. Yeah,

Mike.

**MR. DESCH:** And this should probably be that last question so we can go to the next panel. I was going to say Jim Goldgeier and I did this survey of APSIA deans and the top 50 political science chairs. We're in the process of writing it up. With one of the really interesting findings, really there's two albums to it. One is the increasing convergence of APSIA schools and departments in terms of disciplinary criteria for promotion and tenure, but also the APSIA deans in addition to that wanted all this public scholarship and public engagement stuff. So that's an accurate reflection of what's going on. Again, the rational newly minted PhD might say, boy, a professional school of public policy might not be the optimal choice given a differential expectation.

**MR. JENTLESON:** But I think that's a measure of where we are now, right? This is kind of off topic but in some ways it's -- I keep thinking of the arguments about public opinion polls and impeachment, right. You know, do you do what polls are or you do it where you think they might go, et cetera, et cetera? So, I mean, I think that if you started to change things that there would be some impact. I don't think every prestigious political science department in the United States would change. But I think again, to make sort of the space for people that want to be affiliated with an APSIA school or a political science department is really the goal, and then I believe it all had a positive impact and over three, five, ten years to have its impacts read. And the argument that we've been talking about with the president and provost is it would really, I think, in this age -- and this will

get into the next panel of higher-ed leadership, but I think if we do this in and do it well, it would be a good statement for us in terms of how the president and provost and the roles they're playing and where higher ed's going now. So that's all the baseline which is why we caution people now. And the goal is to figure out change and then have what's affected by that the next round of output demonstrate to the change on impact.

**MR. DESCH:** Bruce, thank you very much. Also speaking of higher education leaders, Jim and Dan and Martin and Jack, you guys, are up. I have to say Bruce, I have great admiration for this project. A lot of work, a lot of really great ideas. Also I'm very hopeful given that we regard Duke as the Notre Dame of the Southeast. If you guys move in this direction, we'll be right behind you.

**MR. JENTLESON:** Because we took your athletic director away, right?

## **THE PERSPECTIVE OF HIGHER EDUCATION LEADERS**

**MR. GOLDGEIER:** I am delighted with this next panel. We have a former president, a chair of a board, and a provost, so we have higher education leadership well represented and Bruce mentioned through our own Bridging the Gap project that we, for a long time, had really been focused bottom up on training academics, particularly PhD students, and junior faculty or the tenured faculty also

come to our workshops on -- people have expertise of one and learn how to do more policy in public engagement.

And we then had this idea a few years ago that we should seek out what we consider to be our natural allies which were university leaders who, in our view -- I mean this was our hypothesis -- that they felt under pressure from all sorts of different directions to show that their universities had a broader value to society. And I think that it's worth noting, meaning we did get 13 of them to -- well, one of them was our own at AU and one of us Bruce's at Duke but -- there were 11 others who showed up for this one day meeting who had no idea who we were and we took that as something that there was a lot -- and they really were interested in thinking about these issues and we're really pleased that several of them now have moved on to become university presidents not just -- I mean it was a big win that Vince Price became the president of Duke but Sara Mangelsdorf has become the president of Rochester momentarily and David Harris from Tufts became the president at Union, and so it's been important for our network.

We followed that meeting up with another meeting where some of the provosts came back, and we had some folks from the foundation world to talk about the interaction, and there was clearly interest in, sort of, on their part in thinking about can they promote more of this type of work on their campuses? To Jim Steinburg's point just now, they said from their standpoint, the biggest blockages were first, their own tenured faculty, and nothing seemed to anger them more than the fact that their tenured faculty would tell their junior faculty, you

shouldn't do that because the provost will not think that's a good idea. And they said that that really ticked them off that they -- because they were like, if anybody -- in fact, we had a junior faculty -- one of my junior colleagues was there and my provost agreed with that, and he was like, wait, really, because mine got the message that had been to him, don't do this kind of stuff because it's going to -- your provost won't appreciate it. And the other thing they complained about was the external review process, so then their view was even if I can get this change on my campus, the letters are going to go out to people who aren't going to value it and so then what happens when that file comes to me, I'm kind of stuck, so that's a problem.

I had let our panelists know that three of the questions that I came away from the meeting from that I was going to ask them to engage with either in their opening remarks or during out Q and A were first, how can you provide incentives for faculty who want to do work that engages the public in the policy community but who don't feel they'll get rewarded for it or even that it might harm them at tenure time? Second, how can you provide incentives for faculty to ask big questions that would necessitate a multidisciplinary and interdisciplinary approach when the discipline they belong to don't reward those types of publications? And then should universities be providing structural support for faculty who have expertise but don't really have a lot of experience engaging the policy and public communities?

For example, at the workshop, Bruce's provost, Sally Kornbluth,

mentioned that she had worked with their Nicholas School of the Environment to create these which she called policy navigators, basically, on the assumptions that the faculty didn't really have any idea how to engage the policy community but that she could bring people who knew how to engage the policy community to the school that they could help the faculty do that type of work. So those are three questions that I had from the meetings that we've held. And I think we'll just go down the line here, Dan, Martin, and Jack, and opening comments and then we'll open it up.

**MR. MYERS:** Okay, great. Thanks. I thought I was going to be the last because I was last on the list. So I was going to say it's a dangerous place to be on a panel, you know as people leave people fall asleep, but it's more dangerous for you because God only knows what's going to come out of our mouth at this point in the day. I have a well-deserved reputation for not being able to stay on script and, in fact, I've been changing my comments as we've been going through the day today. If I can read my own writing, perhaps, it will still end up being semi-coherent.

I started off just, you know, I read Mike's book, and I listened to a lot of these arguments over the course of my career and talked to many of my fellow provosts about these kinds of issues and, in fact, my own sort of trajectory is I won't say typical but it's not something people won't recognize. You know, I started out as a sociologist thinking, and I believe I even said something like this in print in a blog or something along the lines of, if you're not here to fix social

problems, then get the hell out of the discipline. But then, you know, over the course of my career, I learned about methods and rigor and I really got involved in some things that people would perceive as being pretty esoteric in that even most of my colleagues in the social movements area probably wouldn't be too interested in reading. And so I really got into the production of the -- you would call APSR - - but I call the ASR style type of production and scholarship but then I got dissatisfied with that because I knew I wasn't doing anything in the real world. And so I spun off and did some other things that I really designed to hit a pulse to the audience. I wrote a couple of books with a friend of mine to try to do some of that kind of work, so I've kind of been swimming around in this.

When I was at Notre Dame too, I was involved the whole time with the Kroc Institute for International Peace Studies which is part of the Keough School here now and, you know, we had people on tenure track in the Kroc that were really much more interested in doing policy stuff, addressing the policy audience and writing and those kinds of ways and with those kinds of audience, and I really had to grapple with what kind of standards we were going to apply to them. Some of them had written in their contract letters about what role the policy kind of publication is going to play. So I mean I've actually thought about this quite a bit. And sometimes I feel a little like the taste about these things is kind of dour. We just can't do this. Everybody's stuck over here in esoteric kinds of research, and there's a lot of almost mudslinging at academia about the kinds of things that they do that other people can't appreciate or don't think they're relevant.

So I wanted to try to spin aback a little bit and try to end on a little bit more positive note and so kind of two pockets of thoughts here. One, first of all, maybe it's not all that bad. And the second one is signs that things might be getting better. And we've heard some of this during the day, so excuse me for repeating a little bit of this, but I think it's worth noting some of the trends that are going on.

So first maybe it's not all that bad, and there's a possibility that we're kind of overstating some of the problem here and I'll just -- if you'll excuse a little rant. I have a little spasm often about attacks on methodology and how detailed and how hard we work on methodological kinds of problems and, you know, the reason we do that isn't just because we want to look smart with equations; it's because we're trying to get it right or as close to right as we can. And that's healthy.

I recently gave a talk just a couple of weeks ago where the university I was asked to speak at a little conference about high-impact research, and I started with saying, oh, the first thing you'd want in high-impact research is get it right. That's the first thing you have to do and that's caught up in this whole enterprise of some of the methodological rigor that comes in. And sometimes, I think, some of that attack on methodology actually spins almost into an anti-intellectual stance which is, I don't understand what you're doing so it must not be relevant. It must be useless and so on, too esoteric. That points to a different problem and that's translation. I'm going to talk about that in a second. But back to the sort of the

why we think things are so bad and why maybe they might not be beyond the overemphasis on this methodological detail.

In Mike's book and somebody brought it up yesterday too, there's that graph, C woman's graph (phonetic 1:06:46.7) with which there's the downswing in policy recommendations in APSR and it looks terrible for policy recommendations. I don't know if in 15 or 20 years, it's a flat line on the bottom. But what does that really mean? Well, there's this temptation to interpret it that political sciences only care about policy or that political science research doesn't have any policy implications or that, if policymakers want advice, they aren't going to political scientists. Or even worse, beyond political science, all of the social sciences and all of academia, everyone is not doing work that really has any relevance to the real world or in policy, but that's not what it really means. What it means is that there aren't policy recommendations in the APSR. You have to make some really giant leaps to get to those other kinds of conclusions.

For one, so suppose that the ideas that political scientists aren't putting policy recommendations in ASR because that's just not part -- it's not relevant to the work that they're doing. Well, that doesn't mean just because they're not in APSR doesn't mean they're not somewhere else and someone mentioned blogs in the last two days here but it's more than blogs. I mean there's lots of outlets where people take their work and they put it in outlets that are more accessible to others. Just because something isn't appearing in one outlet anymore, doesn't mean it's not appearing somewhere else and I'd like to see a graph of other

things where other places where people put policy-oriented work and see if maybe there's something on the increase.

And the old joker said somewhere in the '90s, MTV stopped playing music videos but that doesn't mean music videos weren't being produced or being consumed by people. I know my kids are glued to YouTube all the time.

So where are the policy recommendations if they're not in ASR? Well, there's books, there's people writing policy briefs. People write reports for government agencies, op-eds and so on. I mean, there's a lot of stuff out there quazi-academic outlets and so on. So does the growth in that kind of work outweigh the loss in policy recommendations in ASR? Well, I don't have the data, but I bet that that amount of stuff absolutely swamps whatever could have been in the ASR and the amount of consumption by policymakers of policy recommendations that were in ASR. So I think we need to sort of rebalance a little bit what's really going on out there.

I also thought last night during the Q and A, I couldn't help thinking that maybe we should just admit that we're doing something different with APSR or ASR or any high-end social science journal. That kind of research even if it isn't complicated or informal modeling or mathematical, it's meant for a certain kind of audience and it's not the kind of work we would aside a thought. I thought of this when we were talking about teaching. You don't usually assign much what's in the latest APSR in your intro to American politics class, right? And you're doing something different when you're doing the teaching tasks and when

you're doing the research task, and that's one reason why they can co-exist at places that care about research and teaching.

The cutting-edge research task is different, and so it's the same thing with investing a policy audience. You're doing something different and that's something APSR isn't intended to do that and maybe we shouldn't be promoting so much that it doesn't do that because it can be done in other ways. Let's see. So that doesn't mean this work that's happening can't inform others, but you have to do something different, and you have to do this translation test and that's really what it's about. There are lots of ways to do that, but we have to rewrite our working to a different language for a different audience and sometimes there's people around who are really good at this. Maybe I'm not -- I tend to think I am -- but maybe I'm not, but some people aren't but there are people. And speaking as a sociologist, I can't help but be irritated with how much money that damn Malcolm Gladwell is making for taking and repackaging social science ideas and putting them into a form that's more consumable by a lot of other kinds of people. And I have to admit, he's really good at it. He's really been very effective and so, fine, let people who have that skill and wants to spend their time doing that, work with them to try to get the stuff that's important in our work out into a different audience. Okay.

So that's the end of maybe it's not all that bad. I think we'd agree despite however bad it is, and we don't have to agree on how bad it is, but however bad we think it is, I think people in this room agree that it would be a good thing if

the collective of social scientists had more impact on policy decisions. If we were more involved in the big questions and it was more a plot that connected to applied environments, so we agree to that. So what do we do about that, and are some of these things happening? And we've heard plenty of stuff actually today to suggest some optimism about what the trend line currently is. As I look out in the landscape, I see lots of science that things are moving in the right direction. Several people have mentioned things, but I think especially Bruce was -- what Bruce was saying about the hunger or taste amongst younger scholars for doing this kind of work is a really important sign that we need to pay attention to and try to support.

But another part of this is that interdisciplinary work is on the rise. I mean, I don't think there's any question about that. In fact, you know, when I went through interviewing for provost positions, it's all people want to talk about it seemed half the time. I got sick of talking about it, actually. But it's been the biggest thing in higher ed for years. We talk about it and we try to invent structures about it, and there's more than just talk. You know, you could go on any university's website -- I went on Notre Dame's last night just to see, and there's some 50 centers. There's more than 50 centers and institutes and almost all of them, from my knowledge anyway, are cross-disciplinary, multidisciplinary, interdisciplinary enterprises and that's a really important development. You can see it at any university. I got the same list at my current university at American with the exception that we've discovered in the null of the last board meeting that

we forgot to put one that was just funded by one of the board members and we were talking about it and he can't handle a lot. We got it fixed by the end of the meeting, but nevertheless, but it's there.

It's everywhere and people are trying to do this. They're trying to use that kind of mechanism to develop it, to develop those interdisciplinary connections and those interdisciplinary enterprises. You can see it too in the development of disciplines and people have talked a lot today about neuroscience and brain science, so I won't perseverate on it, but that's a great example. Some universities don't have departments in that college anymore. They have brain sciences. So it's really changed a lot, and I think that we'll see more of that. In the motivating document over this -- I think it's Steve, there was a quote from Steve about we need a revolution in how academia is organized, and I actually think this our -- and it said something else. Some people don't think this is feasible. Well, I don't not think it's feasible. I think it's already happening but it's not a revolution. It's more of an evolution as we see people starting to appreciate this work and build structures around it.

I'm running out time here, probably. So I'm supposed to be representing the provost, so I should I talk a little bit about what universities can do to push this all forward with engagement with policymakers. So let me just rattle off a couple, and we can take them up more in the Q and A. One has been highlighted here by Bruce, but it's the fundamental incentive structure. You always got to go back to that and that, for us, is tenure and promotion. I mean,

that's really the core. Explicitly building that broader impact stuff into standards is very important. Now it has to be done under a disciplinary specific way. So I was interested to hear what he had to say because you're trying to approach this on a university-wide level in some sense, but what you're really trying to do is induce the disciplines to talk about how this kind of stuff comes into their particular kind of work. And you just have to keep asking them to do it.

I think every year I've been a provost, I've asked some departments or even the whole university for them to -- I didn't tell them what to do, but I asked them to go back and look at their tenure standards and see if they're current with respect to how they approach this particular issue -- policy relevance -- but there's other things too. I asked them -- at Marquette, community-engaged research was a big deal, and so I asked them to look at that. I asked them to look at electronic publications because those things are changing around us, and they need to constantly be interrogated.

The second thing is providing some resources for attaching policy translators to research work. We do provide internal support for lots of different parts of research. Well, here's one we should explicitly add to our little portfolio. There's not a ton. Somebody asked earlier why doesn't something happen at universities, and the answer is because it's expensive. So there's not tons of resources to do that, but if it's important enough to us, we ought to start putting some money on the table to support that explicitly.

Third is to continue to build these kinds of topic or problem or

policy-based research clusters or centers and so on. I really do think they work, and I think if people look around, you can see lots of success from some of those. And they're not only great for doing this kind of research, but they're incredibly good for drawing funding into the university. That's what funders and benefactors want is to feel something that's a little more focused, and those centers give an opportunity for that.

And then demand dissemination strategies when you're giving out funding. That's got to be part of the package when someone asks you for money, well, you have to tell me how you're going to disseminate this and get it to the right policy circles. I think that's very important. We do have developed this thing at a lot of the universities where communication shops are at work very hard at getting us out into the public eye in various ways. But there's a different audience when you're talking about policymakers. It's not the same as trying to get in the newspaper. And how to get to those is something that we don't have -- we do not know of a lot of muscle on it. We need to think more with our comm folks and others about how to do that.

And then the last thing is just booster this. As a leader in an academic institution, whatever you want done, you don't get to tell people to do it. You got to keep boosting it. You have to talk about it all the time, and it's amazing, actually sometimes, how much you can get out of just repeating the message that this is important.

**MR. JISCHKE:** Thanks for this opportunity to comment from the

perspective of a higher-education leader. It's kind of a nice title actually. I bring to this discussion 23 years of experience as a president of four public research universities, 3 of which were land grant universities. I'm also currently a trustee at a technological university and, in the interest of full disclosure, my disciplinary background is science and engineering. I'm not a social scientist.

Any discussion of the societal purposes of university, I believe, has to start with the institution's mission and, for research universities, that technically includes research and teaching. Some universities, land grants in particular, add service or engagement as a third mission in order to capture the idea of using the university's capacities to reach out beyond the institution. Each of these three elements of a university's mission in my view, serve broader societal purposes. They develop young minds. They create new knowledge and, in particular, they engage the public beyond the university in order to facilitate societal development. Given that our universities have multifaceted missions, it's almost inevitable that issues of relative emphasis and proportion will arise -- and this is what I want to at least first talk about.

It's my view that the emphasis along and within universities, research universities in particular, on these three aspects of the university's mission vary quite substantially. In particular, the extent to which service or engagement, public purposes are seen as an explicit mission of the university, it will have a very significantly and differentiating impact on the perception of the larger public as to whether the university is, in fact, serving public purposes. The difference between

the attitudes of faculty in the arts and sciences college and the agricultural college are quite substantial in this regard. Insularity and a narrow disciplinary orientation are a much more likely consequence of missions that explicitly ignore engagement. Engagement outside the university, in my experience, reduces the insularity that I mentioned and often lead to an interesting opportunity for both learning and research that can only be tackled with an interdisciplinary approach. So I conclude that engagement with the public is absolutely essential to invigorating the social purposes of the university. You can't do it in isolation.

There are several obstacles to this kind of interdisciplinary, socially purposeful, and engaged university and I just want to mention a few of them. At the top of my list is the seeming inevitability of a disciplinary organization. That structure, the disciplinary structure, of universities seems to be a natural consequence of their intellectual foundations in these specializations they feature. It also reflects the very large scale of modern research universities. They're very big and complex organizations. It also reflects the way we house people. Every department has a building. Every college has a quad. They're separate and distinct from one another. And finally, I think there's a human dimension to this that has to do with tribal cultures that arise in such organizations.

The second obstacle is the financing of the university. Modern research universities are increasingly financed by tuition from students and private donations with shrinking support from the government for both basic teaching and what I call engagement activities.

The third major source of funding, research support, is increasingly for quite specific purposes. There's little institutional discretion. Private donations in my experience, and I've raised a fair amount of money, are also for quite specific and restricted purposes. So institutions as institutions have quite limited discretion in placing funds into so-called social purpose activities.

A third obstacle is the Bayh–Dole Act of 1980 which allows U.S. universities to exploit the intellectual property that's developed for research supported by the government. It's made the private benefits of research rather than public benefits a much more explicit motivation of the research mission. And the beneficiaries are faculty researchers, typical policies given a share of the proceeds. Second, the university which also gets a share of the proceeds, and thirdly, those who license the intellectual property. There's not a cut for the public, if you will.

A fourth obstacle, the absence of social purpose in much of our teaching.: The disciplinary focus of research universities can make teaching quite academic and sterile from a student perspective. Students often come to us motivated by both social purposes and, of course, personal, professional development. Encouraging faculty to focus some of their teaching on the societal impacts of the subjects they teach will help students both better understand the importance of what they're studying and also the broader social purposes of higher education.

And the fifth, and final, obstacle I'll mention is the limited engagement of the faculty with the public. Few faculty at research universities

take time to share the fruits of their research with the broader public both to educate that public but also, through that interaction, to better understand what the needs of society are and whether they're being met by the research efforts of the university. The faculty reward system underlies this particular obstacle.

Let me next talk about three examples of opportunities to reinvigorate the social purposes of the university. The first one is to restate the mission of the institution. Making explicit the social purposes of the university by including it in the institution's formal mission statement, I believe, is the first step to reinvigorating this aspect of the university's purposes. Regular statements by institutional leaders including its governing board and reports of activities in support of the social purpose mission are required, not only to recognize those efforts, but to educate the broader university community about what constitutes such social purposes and how issues like faculty hiring, tenure, and promotion criteria can and should be changed to realize those social purposes.

Second, an opportunity comes from allocating university resources to support such activities. My experiences as a president is nothing speaks louder to the university community, especially its faculty, than the uses to which financial resources are applied. Fortunately, rather modest funds can have an outsized impact on the social purpose mission; it doesn't take a lot of money. Examples that I've experienced: community service grants to student organizations, grants to faculty and others to assess the impact of the university's social purposes, and support for partnerships between the university and the various communities

outside the university with which it interacts.

And the third suggestion is to encourage the integration of the university's social purposes into the teaching and research missions of the university. All the discussion today has been largely about research, I believe teaching is another area where this can be done to very salutatory effect.

Realizing this desired impact of the social purposes of the university and the students and faculty will be facilitated -- I know it because I've seen it -- by making it a more explicit part of the teaching and research of the university's programs including, in particular, interdisciplinary initiatives.

Let me close with two caveats. The first one is that an explicit commitment to the university's social purposes does not mean that all the university's efforts will become interdisciplinary and socially purposeful. The university requires both disciplinary and interdisciplinary efforts for its success. I do not believe that it is possible or desirable to eliminate the disciplinary structure of the university. Indeed, in my view, it's stronger interdisciplinary programs can only occur if there are strong disciplines to build upon. The goal ought to be to foster both disciplinary excellence and interdisciplinary work.

And the second caveat, a failure to respond to the social purpose agenda, in my view, invites risks for universities. The failure to embrace the imperatives of the social purposes of the university invites, first, a risk of competition from new more relevant degree programs that can marginalize existing degree offerings as well as make the traditional academic programs

increasingly unattractive to talented young faculty, one adjoined universities that fostered the societal impact of their research. Just look at the enrollments in various programs to get some sense of how attractive they are.

It also jeopardizes, in my view, the public's continued financial support of our institutions. If we want the public's money, and I think we all do, we ought to pay attention to the public's needs and their hopes and expectations for higher education. My reading of the history of higher education is that as the needs of society change, more often than not, those needs are met by new institutions because the existing higher education institutions are unwilling or unable to change. Public universities, land grant universities, community colleges, and online institutions are just examples of how society has responded to the changing needs they have from higher education. So there's a warning in that for all of our institutions if we don't keep up and respond to these changing needs.

Thank you.

**MR. BRENNAN:** Thank you, Martin. Dan, being last means I'm having to look at my prepared remarks if you had said -- you guys stole all of it. I'm going to sound like your caddy. I don't have 23 years of experience running large universities. I'm the board chair at Notre Dame, a great privilege. My thoughts from that perch -- and I'll try to be generic but it's hard to do so, and I think, and I just want to make a comment about that because I think it's important.

We have a very short mission statement at Notre Dame which is to be a force to do good in the world. It's people like me can't remember more than

five or six words, so it makes it easy. But on your point, it actually helps us, I think, in terms of thinking about this issue of the social good and public purpose. And I'm going to talk at a very high level with just -- and I think there are really six things that the governing body should be focused on with respect to this issue. And two of which are environmental and then six specific steps. The first is -- and it's very much in your last point, Martin. Somewhere, I'll say you are industry -- I'm a spectator at some level -- has lost the narrative. It's on your point then. It's not as bad as it seems in terms of the disengagement. I read Mike's book as well. I hope you bought it; he gave me a freebie because I was going to say you sold two copies but maybe he's only sold one. I thought it was fascinating, and I thought it was a thesis. I'm sure it's debatable. I know it's been debated. But it is a very -- from where I sit, it is. The academy institutions are disengaged. I hear it when I'm here for Notre Dame as the board chair, and I think it's a shame. I think it's overstated but it's real on some levels. So that's environmental point number one.

Number two, and again, it gets to a point you made, Martin, and the demand out there for relevance and engagement is very high and you see it. You see it when you talk to students. You see it when you talk to benefactors. You see it when you talk to people who are observing what goes on on prestigious universities' campuses. They want the engagement. Parents' frank -- they want their kids to get jobs which I find nice but a little neurotic at times that they're so focused on it at this stage. So those are the two environmental things that I think

about and I come back to how can you accomplish what you've been talking about for the last day? And I don't have the answer, but I have sort of a set of steps that, I think, we try to think about. We've had a very simple mission statement, to be a force for good in the world.

And the first is we have five university goals. And, in some ways, on this broad topic of social and public good, being a faith-based institution's an advantage. It's an absolute advantage. So our five university goals are these: ensure that our Catholic character informs all of our endeavors; offer unsurpassed undergraduate education that nurtures the formation of mind, body, and spirit; advance human understanding through scholarship, research, and post-baccalaureate programs that seek to heal, unify, and enlighten; foster the university's mission through superb stewardship of its human, physical, and financial resources; and then number five, and it's the most relevant for this event, is engage in collaborations that extend and deepen Notre Dame's impact. And make the invisible, it's visible. This is in our board books in every meeting. And my point to our trustees is, if we're not talking about one of these five things, we're not talking about the right things.

To accomplish that, one of the things we did as a board and it's relevant to, again, another point about the organization. We changed our organization from administrative, which you might call disciplinary of our committee structure, to strategic so that we can have this kind of conversation at a strategic level of our undergraduates and our faculty research and graduate

programs. They're the two key committees for Notre Dame. We're a year into it and, in a year, we've changed dramatically the quality of the conversations, I believe, at those committee meetings and our ability to be helpful in some way. You might think that's an odd -- a helpful board might seem like an oxymoron; it's not. We're trying our best, okay. I know Mike was nodding saying yes, it is.

But we did it purposely, so we can think of the holistic nature of being an undergraduate or a faculty member. It's teaching. It's mind, body, and spirit. And we found that at least when we deal with the members of the administration and faculty, they find it much more valuable because we're discussing issues like this rather than very narrow stovepipe issues as a matter of course. So that change was very important for us as a governing board. I don't know how long the other ones have been in. It's the advantage of having change and I asked a very simple question, why are we organized like this? And everybody said, I don't know, but we don't actually like it very much, so we had to change it.

Now we come back down and run through quickly how we think you go about addressing this issue. First, is putting the mission in the strategic plan. It starts simple, we have a long mission statement on the website that nobody knows where it is or what it is. But pretty much, if you step on campus for more than a week at Notre Dame, being a force for good in the world is something you understand. And it's very visible on the planet. This facility, this institution, the Keough School of Global Affairs is the best tangible manifestation of

collaborations to deepen Notre Dame's impact. Dan mentioned there are a bunch of institutes, many of them have been put together under one -- our first new college in a hundred years to make our impact very real.

The second plan for it -- and plan for it in many different ways once it's in the plan. Physical is one of them. I'm a business guy as I don't understand why it's so expensive, but we now have collaboration rooms all over the place and it's a nice thing, I guess. I look and I have to put 15 workstations in there, but it is driven by interdisciplinary work. It's a physical manifestation that we believe interdisciplinary work matters at our university. And again, people who've been involved, Mike and others in the faculty side, I think would say that it's very different than the physical planning that was done 20, 30, 40 years ago, and it is expensive. It is expensive but it's important. It's organizational. It's, again, Keough being a great example of that. So put it in the plan, plan for it, and then fund it.

And funding, some of it, is going out for benefaction, but funding it in other ways is do you put pots of money available for interdisciplinary and public engagement work? And the answer is yes, and it has to be very explicit. It has to be available. It doesn't have to be a lot of money. I'm shocked at what particularly some of the humanities in social sciences can get very excited about fund dollars, no offense to you guys. If you were a bunch of scientists, you'd want two Mercedes-Benz and a Jag, right. And it's amazing. You fund it, make it visible, make people compete for it. Make people compete for it. Who has the

best ideas to make a difference in the outside world?

And then the last two things are kind of Business 101: measure it and report on it. Again, because we're a faith-based institution, we produce a report every year called a Report on a Catholic Mission, which, one of your institutions Martin might have said, might have turned into the report on our public engagement. And it's something -- we have an organization structure. We have a board and then we have something called a board of fellows which is half clergy and half lay people. I have the privilege of being a fellow and have had for a while, and it's probably the most important meeting we have is when we get that report on Catholic mission. You could change it to public mission because much of it is how we're making a difference in society here and around the world. How or where are we funding? What are we teaching? How are we teaching? But again, as a businessperson hoping to make a small difference at an academic institution, as are most of the trustees that mostly come from that world, that idea of measuring it and reporting on it is a critical element. If we're going to go back and go back to one of our five overall goals, how do we extend and deepen Notre Dame's impact as a matter of course? I think that's a set of marching orders for most everybody and against some of your questions that Jim, how do incent people? You need to be very explicit.

The last point I would make is a pretty simple one and a fun one because there's some great examples. I know someone on our campus but in others were encouraging academic entrepreneurship as a fantastic way. We have

something called the Lab for Economic Opportunities which might, and Mike I'd take your judgment on this or Dan, maybe the place that has the most visible engagement in some ways and from a policy standpoint at this stage. I know they're not in your department, but it's got group economists (phonetic 1:40:41.2) who now it's a full-fledged high-impact organization. It's not ten years old, not ten years old. They're entrepreneurs. They raise their own money. They now get funding from lots of sources, and they have had a huge impact on public policy both sides of the aisle down here, around health and around social services and social well-being. So encouraging intellectual and academic entrepreneurship is the last point I would leave that we as a governing body love to see, probably because a lot of us are from that kind of world, but we think it is a difference maker and it's a market-based answer to the question of how do we make an impact. I'll stop there, Jim.

**MR. GOLDGEIER:** What a panel. Good job.

**UNIDENTIFIED MALE:** Great panel. I have a question for both Martin and Daniel. That was about this interesting observation about the rise of interdisciplinary centers and activities in a disciplinary-based entity. And you were citing that as success. Part of me thought to myself, well, if the disciplines were doing what we all hope they would be doing, would these institutions be necessary? It's been sort of my experience observing that oftentimes these interdisciplinary institutes emerge largely because other audiences see the disciplines as not doing their job or not meeting some of these goals, and this is

especially true in the world that Jim, Steve, Mike and I are in.

I think of some of these, probably the most well-funded prestigious one is one at Stanford, the Freeman Spogli Institute. It's got hundreds of millions of dollars. It's created largely so Stanford can have this visible impact on the world, yet it's engagement with the cognitive disciplines is very weak at best, and in fact, they've had challenges trying to recruit people and cross-fertilize. And I think the reason is because, in a university setting, the disciplines have the power. They hire and they tenure. These interdisciplinary centers are nice add-ons. They're band-aids, but unless an interdisciplinary center can actually be involved in the crucial process of what makes a university run and what counts -- tenure -- then they're really not going to make a difference. And so the Stanford example is an extreme one. We have a political science department and how they study international relations doesn't look anything at all like how this interdisciplinary center does. But I can think of other examples too. And so I guess I wanted -- I want to hear your case more that these institutes and centers are actually a good thing as opposed to actually a reaction to the fact that disciplines aren't doing what they should be doing.

**MR. GOLDGEIER:** And I just so just to add onto your point.

What's interesting at Stanford is that Freeman Spogli is able to offer tenure.

**UNIDENTIFIED MALE:** That's unclear.

**MR. GOLDGEIER:** I mean at Hoover, Hoover seems to be able to. Is the same hippiness?

**UNIDENTIFIED MALE:** No.

**MR. GOLDGEIER:** They can't. I thought they were able to do -- okay.

**UNIDENTIFIED MALE:** In fact, if you look at those who are hired by FSI --

**MR. GOLDGEIER:** Well, they say they do.

**UNIDENTIFIED MALE:** They say they do because I remember years ago when we were doing this stuff at Texas going out and talking to Chip Black (phonetic) about it. He was the most innovative guy in this field, and I said, the only way you're going to fix this is by getting tenured positions. We've been trying to figure this out, and one of the things we did at UT and LBJ and the Truman's Instrumental along was actually creating these sorts of rubber band lines where you could actually do that. But it was clear that all the money in the world could not solve this problem unless it dealt with the disciplinary aspect of tenure and hiring and Stanford hasn't, and the people that we know who seem to have -- they're not on the faculty senate. They can't share dissertation committees. They can't vote on tenure, so they may have some aspects of tenure but they're still second-class citizens in the university.

**MR. MYERS:** I think it's a false dichotomy that they're either a success or the indicator of success or failure. They're both -- they're an indicator that what was working in these stovepipes and the silos wasn't getting it done, and so they emerged and then they have had some success. Now there's lots of

different kinds of centers and institutes and some of them are terrible. Some of them need to be stamped out, and they're just letterheads to make somebody's ego feel better. And then there's other ones that are legitimately doing really interesting stuff on a shoestring, and then there's big ones that really have a lot going on. And I would point my experience at Notre Dame with Kroc and Keough on both. They don't tenure themselves, but they have a very strong relationship with the disciplinary departments that the faculty, in essence, joint appoint between the institute. At least that's the way it was when I was there. They joint appoint between the institute and the department, and so they do play a role in the tenuring process. Kroc certainly did with people in my department. And so they have some strength there, and they have purse strings which matter a great deal. I explained it --

**UNIDENTIFIED MALE:** What role do they play in the tenure process?

**MR. MYERS:** I'm sorry?

**UNIDENTIFIED MALE:** What role do they play in the tenuring process?

**MR. MYERS:** It depends on the particular appointment, but I know of a couple of cases where we hired people specifically, and it was written into their contract that there would be participation by faculty from the Kroc Institute that were not in the department on the person's tenure and promotion committee.

**MR. JISCHKE:** Let me pipe up on this too. At first, regarding the

tenure issue, at Purdue, we do not tenure people in interdisciplinary activities particularly research activities and the reason is absolutely fundamental. We envisioned the interdisciplinary research activities as very opportunistic in not having the permanence that the academic departments have at the university. So people who come to Purdue who are wanting tenure have to be tenured in an academic unit. With the interdisciplinary capacity does is provide them access to support and relationships that make it possible for them to do research at a scale that a department cannot afford.

The best example is we've got a hundred-million-dollar nanotechnology center that has state-of-the-art world-class equipment. No department at Purdue could afford to get that equipment or to service and maintain it. What the faculty also found out about these interdisciplinary capacities; it enhanced their ability to recruit talent. It really made a difference in bringing very bright young people to the faculty. And the ultimate bottom line is over about a seven-year period, we doubled the level of sponsored research. And of that doubling, half of the increase came from interdisciplinary research activities. So it facilitated what the faculty wanted -- bigger research programs, bigger problems to go after, and better colleagues -- and they embraced it.

The most amazing statistic I can give you, to me amazing, of the roughly 2000 faculty at Purdue, half of them had a relationship with one or more of the interdisciplinary research centers we created at a place called Discovery Park. The faculty embraced it. There was no fundamental disagreement between

the administration, the deans, and so on. One last comment, I was a little lucky in that when I became president, 10 of the 11 deans retired. So I don't know if it's because I came --

**MR. DESCH:** Was that causal?

**MR. JISCHKE:** But every dean that we recruited got a little speech from the president about we were deadly serious about interdisciplinary research, and if you were going to come to our university, you had to be supportive. And we explained to them why we were doing this, and it worked.

**MR. GOLDGEIER:** I have Mike and then --

**MR. DESCH:** Just very quickly, it's ironic because Dan chaired the first committee that led to the establishment of the Keough School. A decision was made at some point, and I can't remember exactly when, for Keough School to have its own tenure lines. So the old model of the big institutes hiring jointly with the departments no longer exists. I don't know the sort of back story although, having been at other graduate schools of international affairs, many of which started out hiring joint and then moved to being able to hire and tenure their own faculty. I have my guess about why that happened. Just three quick points -- I sound like I'm defending my book. I'm selling my book; I'm not defending it.

**UNIDENTIFIED MALE:** Stop giving it away Mike if you want to sell it.

**MR. DESCH:** Blogs will save us. Dan, you're a storyteller. I'm a big end data guy. I actually have data on what policymakers are looking at and

reading, including data on academic blogs. Even the best of the academic blogs in my discipline *The Monkey Cage* is way down compared -- and the really in the weeds of academic blogs don't even register. The online sources that policymakers are going to are overwhelmingly foreignpolicy.com or something like *War on The Rocks* which is a hybrid with a lot of practitioners on it. It's like best at times, worst of times. A lot of academics are using blogs as an alternative outlet. That's to the good if you believe in engagement but it's not having a big impact. We have data on that.

Translators: I think it's a mistake if academics let other people translate their work and this is my Bruce Russett and the democratic peace story. Bruce Russett, a very distinguished professor at Yale, one of the foremost experts on the democratic piece. The Bush administration, Bush 43, included in its rationale for both nation building in Afghanistan and the invasion of Iraq in 2003, but an argument that if these countries were democratic, many of our problems with them would go away. How serious that was in terms of their rationale maybe another map. In 2005, Bruce Russett publishes an article in a scholarly journal lamenting the lunkheads in the Bush administration for misapplying the democratic peace. I read it, completely persuaded by it, but I'm saying where were you in the summer of 2002 and why is this in the one of ten thousand International Studies Association journals rather than in a place with *The New York Times* where policymakers would have seen it. So, I think, if you think that your work speaks to policy issues, you're safer generating your own policy in some

occasions.

Finally, I think good scholarly work comes from deep engagement with concrete policy issues, and here I'm going to revert to storytelling. We have a regular thing where we invite six of the top pre-docs ABD IR (phonetic 1:53:36.0) young people to present their work. We just had one of these emerging scholars' meeting. There was a guy who's finishing his PhD at Stanford who, before he went to get his PhD, was a presidential management intern and then worked in the state department after that. He got very interested in how the United States and other countries dispersed military aid. And what he concluded was something every policymaker knew that often if you were going give military aid to one country, there might be another country that a beef with that country that you were also going to have support, so the classic example was Israel and Egypt.

Why did we give three million to both of those countries? It's because that's what keeps the peace. Now, this kid goes to Stanford, and he's in the political science department. He's a high-tech social scientist, so he's well-trained in using all the tools. He looks at the literature, the scholarly literature, on arms transfers, and he thinks, holy smokes, of everything that's been written by scholars on this, nobody has observed this dynamic, so I'm going to write my dissertation which is going to produce some great articles and a big book, but he would never have gotten that insight if he had stayed within the guild. It was only the fact that for two years he was in the belly of the beast in making policy before he went to graduate school that he came up with this insight.

And I think there are a lot of other illustrations of where -- and I don't think an argument by the way, that all policy-relevant academics need to go into government. But I do think that you learn an awful lot by doing it and enriches your scholarship and Mark Ginsburg (phonetic 1:55:42.6) at Stanford, you're going to hear his name as a hotshot young PhD before too long. (crosstalk 1:55:49.2)

**UNIDENTIFIED MALE:** He's one of our IPSCON people.

**UNIDENTIFIED MALE:** He's in the IPSCON program.

**UNIDENTIFIED MALE:** We gave him a pre-nup. He was like -- it was like the walls, when he started working with us and he was so desperate to get out of the Stanford quad-something (phonetic 1:55:58.6) to hear what we were doing. It was as if he -- it's like the Wizard of Oz when you go from black and white to color. And he's one of the most grateful -- he'll do anything for us because of his engagement in our program.

**MR. GOLDGEIER:** Given the time, so but whatever Hank's comments on the table and see if any of the panelists want to close out.

**MR. GREELY:** This isn't deep. I just wanted to clarify the record. I thought the description about Freeman Spogli was wrong and I looked it up. There are about 45 fellows who are jointly appointed as faculty, many of whom are tenured. The senior fellows do count as members of the professoriate members of the academic council and can be PIs. The center fellows, which is the lower rank, makes up about a third of the fellows are not members of the

professoriate, are not members of the academic council, and can only be PIs by exception. The main advantage of being on the academic council is that you can be a member of the faculty senate, which having shared the faculty senate, I can say, is really not much of an advantage.

**UNIDENTIFIED MALE:** Look, I mean, this kid Mark Ginsburg is one -- we could have a long discussion about how Stanford has hundreds of millions of dollars it has spent on centers devoted to this stuff that a kid like Mark Ginsberg never interacts with. He has to come to Syracuse to interact with us so and --

**MR. GREELY:** I was just speaking pure factual statements about the fellow's program.

**UNIDENTIFIED MALE:** No, I believe you. This is something we know very well.

**MR. GOLDGEIER:** Jack, Martin, any final comments? Dan.

**UNIDENTIFIED MALE:** Jim, can I just -- one more comment. Sorry. Because I like Dan's answer which is what if we even talk about it interdisciplinarity? And I think one of the things that has kind of gone through this whole thing is we have a model of interdisciplinarity which consists of a bunch of people each of whom come from a discipline to work together, right. Let me suggest as another way to think about interdisciplinarity which is actually -- and this goes to Martin's point about teaching. There are actually trained people to be interdisciplinary, right, that we train them not in the disciplines. We actually

train them to have the multi-methods people who actually have learned from these things and themselves can apply it.

So it's not a question of each of the individual trades, the plumber and the electrician and everybody coming together but it's another way of thinking about how to think about problems where you have people who come to it who start by thinking, well, I want to deal with climate change, and so I need to learn a little bit about chemistry and I need to learn a little bit about these things. And I'm not saying that should be one or the other, but I think we've lost some idea that there is this second model of interdisciplinarity that leads to different kinds of professionals who are trained to be dealing with the problem and then can draw on all these other things. And one of the problems with this entirely discipline-based thing where we -- especially for our graduate students -- where we only train them in the disciplines as if that's the narrow little stovepipe that they're in, and maybe they learn to talk to somebody from the other disciplines. But I think there is an alternative opportunity to think about how we organize ourselves around that. We actually train PhD students.

This is what area studies was all about, right, which was to say, let's think about how you deal with the Middle East. You have to know some economics and you have to know some history and you have to know some anthropology. And so I just wondered how you see that model of interdisciplinarity and how that might fit into the way we structure our universities.

**MR. GOLDGEIER:** I just want to say on this point. I mean, the way universities -- the requirements they have for how PhD students put their committees together. At Berkeley, I assume this is still the case, it was back in the day. I mean, you had to have two people from the department on your committee and you had to have a third person from another department at the university which has now filled that block from society (phonetic 1:59:56.6) and it ended up being on my committee. I learned a ton from him and the literature and so on which I would have never had done if I -- if the department said you have to have three peo- -- I mean, they required us to have that one of the people had to be from outside of the political science department.

**MR. MYERS:** I mean, I think there's value in that for some people for sure and, I mean, there's a fear about it too that they're trained to be so diffused that it won't be focused enough to get them anywhere, so they have to be careful about that. But I grew up in that realm too in Wisconsin. We had to take a 12-hour minor in a completely different department. You had to have an external person from another department on our committee. And I found that to be very valuable in rigor, in fact, it still is -- some of the stuff I got from that experience is still used in my work what little there is left as I'm being a provost.

I wanted to go back to two things Mike said just because I don't want him to have the last word, but I know he's going to close the thing off so he's going to get me in the end one way or the other. If these policymakers aren't looking at blogs or whatever we think they might be and you know what they're looking at,

we've got to figure how to attack the thing that they're looking at. The answer probably isn't to start putting policy implications and APSR and hope that the policymakers are going to start reading those. We've got to figure out how to get to them.

In one of the things we read, it said something about, unfortunately, they've turned to lobbyists. Well, if that's the bottom access, then we need to think about how to attack through that route to get some of the information that we know that we research and generate into that. And the second thing about doing your own, which I agree, if you can and you have the capacity and you know how to do it or you could be trained up to do it, great. But a lot of people don't have all of that and partnering with someone, not just letting them do it but partnering with them somehow, so you still have your fingers in what's happening but you're getting the expertise of somebody else who knows how to hit that particular audience, I think, would be really helpful for a lot of faculty.

**MR. JISCHKE:** Just a comment. My experience has been that students both at the undergraduate and graduate level have a broader range of interesting job opportunities because they bring this skill of being able to work on complex problems in an interdisciplinary or team way and do that productively and the private sector desperately wants it. My sense is the world of higher education because of the direction of the research agenda is going are looking for people that have that kind of background.

**MR. DESCH:** Anything?

**MR. GOLDGEIER:** No, good.

**MR. DESCH:** Great and thank you.

**LESSONS LEARNED AND NEXT STEPS/STRATEGIES FOR  
ENHANCING THE UNIVERSITY'S SOCIAL PURPOSE**

**MR. DESCH:** Okay. We have one-half hour to go. And the title of this panel is Lessons Learned and Next Steps and Strategies, and I'm going to chair this, and I'm going to ask Frank Gavin, who has been probably in more different types of departments than anybody I know. He's a disciplinary historian but don't hold that against him. Although he was at LBJ but then he was a practicing political scientist for a while at MIT, and now he's back at Johns Hopkins site so he's seen a lot of the land out there. We'll ask Frank to make some observations and then I'll ask for any other comments from the remaining group about sort of big lessons learned that we ought to flag from our discussions so, Frank.

**MR. GAVIN:** Great. Thanks, Mike, and thank you for putting this together and I've just learned a ton. This is absolutely fascinating. As Mike said, I'm sort of a disciplinary homeless person hence my sort of strange comments earlier because, again, I'm a historian. Not only was I in the political science department, but I actually kind of enjoyed the company of policymakers even though I've never been one. So I have kind of odd thoughts on disciplines.

I wanted to make four sort of comments on some of the stuff I've done just very quickly and then talk about some of the things I learned today. The first thing is I had this wonderful experience at MIT, and this is getting to meet the great Dick Lanza. This really kind of showed me what the possibilities for disciplinary and interdisciplinary work was. The Stanley Foundation, which funds nuclear security work, was looking to endow a professorship in the MIT nuclear studies in the engineering department and he asked me to co-chair research in nuclear science and engineering. And talk about something that I was utterly and completely unqualified to do, but it was perhaps the fascinating intellectual experience of my life. And I got to see how a discipline could function with some of the best aspects of interdisciplinary while keeping the good parts of the rigor of the discipline. There were material science people, physicists. It was very problem driven. There were very curious data people. They totally embraced and welcomed me in a way that I found -- and so I've always held that experience up as the best of all worlds and that's why it was great to hear a lot of what Dick said today.

The second experience about in this is one that Jim mentioned, and I won't go into great detail with this IPSCON project with that student Mark Ginsburg participated in, which has been this project to help -- it's probably about I'd say two-thirds political science and one-third historians to help them engage -- to have their work engage policymakers.

And just to give you two examples of things that have really been

successful: Every year we do these online seminars. We have one on the writing of the national security strategy. As part of our group, we had the three people who had been involved in writing all of them in the last 25 years -- Jim, Phil Zeleco (phonetic), Peter Feaver -- and it's fascinating. All three of them disagreed within different national security strategies, but the students then get to see how the process worked. We even had Nadia Schadlow come who was writing along with the Trump administration to present and she actually ended up asking Jim, Phil, and Peter more questions about how it was done. And it was one of these wonderful experiences where the students could see, ah, okay, this is actually how it works. And how it connects to their academic work.

Another exercise we do that's been really popular is we go off to this beautiful, quaint little resort that Syracuse has up in Middlebrook up in the Adirondacks. And one of the exercises is for the junior scholars to take their dissertations and present it to an interdisciplinary group and then some policy people so that they're forced both to explain their work beyond the discipline but then they get advice for how to translate -- not even translate -- how to make their arguments more salient for communities outside of their own discipline. And they loved this. They absolutely loved this. So that's just been the second story.

The third story is something involved in the *Texas National Security Review* which is a journal that's funded from the climate center at the University of Texas. This is the center I was involved in helping create. The idea is to create an interdisciplinary journal that's rigorous but also engages policymakers, right. And

so Jim actually has an excellent article that went through peer review on the North Ireland Peace Process. It combines academic literature with the practical experience of policymaking.

There have been some interesting challenges along the way. Somebody from the European Union was mentioning their way of doing things. And, as I go around trying to get young scholars to publish in this, one of the things that I've found is that you pay scholars and European scholars won't publish in it because it's not on their list and their tenure files go through this whole point system, and so it's been very hard to get some of that off the ground. But our explicit mission was to keep the rigor and the peer review of disciplinary journal while making it relevant and open access -- you can get any of it online -- and making sure that it ends up in the office of the Secretary of Defense and the Secretary of State and it ends up, since it's associated with the war in Iraq, ending up having good readership. And one of the things we say to the younger people is you may not have heard of it. We are peer-reviewed but there's a far greater chance that an undersecretary is going to read this than in any other journal. This is one of the efforts of what we're trying to do.

The fourth story that I tell really quickly which shows some of the challenges. I run a center, this is the Center for Global Affairs at Johns Hopkins and we have two chairs that Mike Bloomberg generously funded, a distinguished professorship, and the requirement is to get over this interdisciplinary question is that the person be in two different departments of schools to sort of hit this issue.

And one challenge is that most of the disciplines and departments are in Baltimore or Washington, so it was kind of distance problem. But the greater challenge is even though this is a free chair, I can't get other disciplines or departments to buy in on it. They're very suspicious of it. They go to the history department. They don't want to do it. We've gone to the business school, political science, applied physics lab even, even the engineering school, and we'll end up getting these filled eventually but it's been -- Johns Hopkins, which is a very, very interdisciplinary place, it's highlighted the challenges that I think that we know exists in the interdisciplinary study. So that's sort of my background experience.

Just some quick observations from today at least what I've learned and again most of these comments are what I've seen in the world of science and engineering that was described today that are things that we could learn in the social sciences.

So the first thing, I think this came through also on the last panel, is that disciplines are where the action is at in universities and if you're going to bring about any change, you've got to confront the issue of disciplines. Earlier today, there was this great conversation about how disciplines evolve, transform, change, grow, or die, for whatever reason and it's particularly in engineering but also the natural sciences this process of change and evolution works much better than it seems to in the social sciences. As someone who's from John's Hopkins where modern social science disciplines were more or less invented, they're pretty much the same as they were in the 1870s and 1880s, whereas in science and

engineering there's been much more evolution. I think that's interesting to understand why. As part of that discipline issue, I was struck very much by the earlier comments -- was it Walter -- about biology and also Dick's points about nuclear science and engineering, those disciplines that were broader and bigger and, I guess, accepted sort of what Jim had took that earlier and the assumption of interdisciplinarity within the discipline actually avoided some of these problems. So I thought that was a really interesting model which I think that, if we're going to live with disciplines, they should be broad.

The second thing I've learned from today is that the engineering disciplines very much were problem focused, right. Methods didn't lead. Methods were seen as tools. You oriented yourself towards problems and this resonated with hanging out with nuclear engineers during the year that I did. They didn't really care what it was you were trained in. They wanted to know what was the problem you wanted to solve and how you were going to solve it and what were the tools you were going to bring to it. And I think for whatever reason, for reasons we don't fully understand that hasn't always been the case in the social sciences. I think we could learn a lot in that.

The third thing I've learned is that, certainly in the sciences and engineering, there's far more emphasis on collaboration, teamwork, and multi-person team projects, right. Historians don't do any of this. We're kind of lone wolf type. Political science is a little better with some coauthoring, but I think one of the things that well, maybe we need to start thinking about a part of it as lab

culture, part of it is encouraging different disciplines that work with each other. Whatever reason in the social sciences we don't have that same ethos, and it seems as if at least in engineering and the natural sciences having that has been beneficial.

The fourth thing that I took away from today is that I think we often have this false divide between pure and applied. I think some of the great presentations in some of the biomedical stuff, Dick's point about going from particle physics to medical and weapons detection shows that oftentimes in social sciences, oh, we have our pure people and applied people but I think a lot -- there may be more of a false dichotomy and we need to learn the lessons or look at what our biomedical engineering friends, our engineering friends, others are doing and I think that would be very helpful

The fifth thing and this is -- I don't know how to think about this but part of it was conversations throughout the day with Dick -- universities are strange places, and I'd be very interested in hearing your view on this. They each had very specific culture, right. I was on a committee at MIT that was worried about competition with Stanford in terms of engineering and one of the things we came in conclusion of is that Stanford and MIT may look similar from the outside. They could not be more different in their culture and how they go about things. And this is sort of an int- -- I don't know how to think about this. Universities are strange thinking institutions. They're very, very different. You've been involved in a bunch of different institutions. They all have their different flavor and culture

and we need to think about that, and they're good things to that because it means they compete. What makes it tricky though it's very difficult and this has been one of the disappoints in our IPSCON project, and I've heard some resonance of this in some commentary today. It's very hard to do multi-university consortium work. You guys at Bridging the Gap have been more successful than anyone, but it's just very hard for universities to cooperate even beyond the fact that they're competitors, just those different practices. Like when I was hearing Bruce's sort of speech about tenure, I thought, can I show that to our university? You would know that their reaction would be, well, we're different. And so that's something to think about.

The sixth point was I thought it was very interesting, particularly the second panel, how much of what in the sciences and engineering was demand driven that there were these key actors outside of the discipline, outside of the university that not only drove much of the funding but shaped some of the tenure decisions in the senate. And we don't really have that as much in international affairs. I can think of there being good and bad points of this. The only negative point in the whole second panel, I didn't hear much about teaching but on the other hand, having this actual external actor that was helping to shape what the intellectual agenda was. You can see positives and negatives, but I think that's one of the differences. I'm trying to think that through. I think that would be important.

The final point I would make is that I have been struck like a lot of

people in this room -- Mike, Steve, both Jims -- we've been at this issue -- and Steve Del Rosso -- have been at this issue for quite some time. And it's a heart issue and it's a mysterious issue. I think that there has been, Daniel's right, there has been some improvement. The scene is much better than it was ten years ago but if you were to describe to the outside world that a chair of a board of trustees and presidents and provosts all universally want the same thing -- change that we're talking about -- but they can't get it done, and they can't get it done largely because it's within 12 disciplines that no one can identify or find and exist across these institutions. And if they're what run the entity, they'd say, well, what an odd way to organize this activity. This is really, really strange, right. I mean, the fact is that presidents and provosts are really pretty -- you know, they can throw money at things but it's very hard to bring about real change and I think this gets to some of the points about interdisciplinarity and some of the mystery behind this process. Anyway, those are my observations for the day for whatever they're worth.

**MR. DESCH:** All right. Well, they were terrific and very helpful. We don't have a lot of time left and maybe at the end of the day, people are sort of suffering from carbon dioxide poisoning and the other maladies from sitting in one place too long. The one observation I would make and then throw it open to any comments from the floor is that it seems to me pretty clear that it's in our interest as faculty to take control of the relevant/broader engagement agenda. The nightmare, I think, for us is the British system, the REF system, where the criteria of relevance are generated by the government and the bureaucracy outside of the

guild. I don't think, and the lesson of the NSF specifically for political science makes it pretty clear, that we can avoid any longer justifying or answering the question that Eric Cantor asked ten years ago about how the government can justify giving a dollar to our discipline that might be given to NIH to find a cure for cancer. We've got to have the answer for that and in having the answer for that, we can also, I think, achieve a better balance between the real and legitimate demands within the disciplines for rigor but also acknowledging the reality that we're part of a larger social unit and that we also have to respond to society's needs as well. Any other bowlows (phonetic)? Jim.

**MR. STEINBERG:** I hate to do this again at the end of the day but at this point, I feel very strongly about and it's one we haven't discussed, so I just wanted to mention this especially if anybody has any ideas. I would love to get help and it's something I've talked to Del Rosso about because we're trying to think in our Bridging the Gap project about how we would start to incorporate this issue a little more.

So the project started 15 years ago. The issue was with one that you identified in your book. If you're an academic and you're interested in doing this type of engagement, then why isn't there support for it? Why isn't there any training for it? Why the incentives are aligned against it? And so that was the focus and the thing that we have started to really understand much better in the last couple of years and we are incorporating more in our own programs but really would need a lot more thinking is that scholars' experience with public and policy

engagement differs greatly depending on their gender and race and ethnicity and other identities.

I'm sorry Betsy Super isn't here. She's coming to present at our workshop next week because the APSA did a survey of political scientists and asked them ques- and she has this data now on their own experiences doing public and policy engagement and there's a huge difference, right. White men doing public and policy engagement just do their public and policy thing. They have their ideas. They present them. There's no -- they don't have to deal with the issues that women have to deal with all the -- you know, women on social media have a whole set of issues to deal with that I don't have to deal with. Persons of color, same thing. And it's showing up also in their experiences engaging with the policy community and whether -- how their ideas are viewed or not again, depending on gender and other identities. I think this is something that for those of us interested in these sets of issues, I think that wrestling with those sets of questions is, to my mind, is really the next big thing. Certainly, it's what we're talking about in Bridging the Gap.

**UNIDENTIFIED MALE:** You're saying that the data shows that women try less or that they are rejected more or are they harassed more for being different scholars?

**MR. STEINBERG:** Well, they're haras- -- yes, their experience -- when they do it, their experience is much more negative than it is for men. So they have to put up with things that someone like me doesn't.

**MR. DESCH:** Martin.

**MR. JISCHKE:** I want to make a comment that is intended to be helpful. I'm amazed in reading your book with the conclusion I think you came to that lots of people in the defense department did not find the policy of advice from academic social sciences, and I contrast that with I'm a fluid dynamist engineering kind of person where I have ways of finding out whether I got the right answer or not. Sometimes it's experiments. There's a well-established process and the answer is unmistakable. It's right or it's wrong. I think you've got to start asking the question of how do you, from the point of view of structure and research, in finding out how to do it better, you've got to come to grips with this reality if you want to influence those people. A legitimate other answer is I don't care what they think. This is what I think. But if you really want to have an impact on policy, you've got to confront, apparently, the reality that the people you're trying to advise don't like your advice. I mean, I don't for a minute underestimate -- I think I may be in an easier field in that regard because the process is pretty clear. But if you don't know why they don't like it and you're unwilling to change to satisfy their needs, then you need to reorder the agenda.

**MR. DESCH:** That's a huge question and it seems to me to involve two things. One is was it Isaiah Berlin that said that he went into philosophy or he went into -- I'm trying to remember who said what. But somebody who went into philosophy because physics was too hard, and then somebody who went into philosophy or economics because philosophy was too hard. I do think the issues

that, especially at the macro policy level, that social science engages with are very hard and they're very hard because you aren't going to get the fluid dynamic solution to the particular problem that you're trying to engage with it.

That's sort of the bad news but the news is that and I think that this is the good news story that I tell in the book that social scientists have had in the past a salutary influence on national security policy and could do so again if they're willing to address the problems that the policymakers face and in a way that the policymakers find useful. So I don't think it's a completely pessimistic story at the end of the day.

**MR. JISCHKE:** I've asked some of my social science colleagues how they reacted to this situation and one piece of information I got that makes sense to me is they try to create active learning experiences for students at a very early stage. Undergraduates and certainly graduate students so that they are better prepared for the real complexity of policy analysis and they try to give them opportunities to gain experiences. I think people who'd be good at this have a lot of practical experience doing it and they sort of learn the craft.

**UNIDENTIFIED MALE:** That's exactly right. The great analogy that our friend Philip Zelaco has always used is the difference between engineering and science, which is if you want a bridge built, you don't go to a particle physicist. You go to an engineer and the reason is that although not that engineering isn't based on, and may be ultimately based on, physics and you have to not make ultimately bridges stand up because whatever's happening quantumly

in the atoms are working, you don't really need to know much but you need to know how to pour concrete and do these things. So it's the prac- -- but what's happened in social sciences is too much of it has gotten away from exactly what you're saying which is we're -- too many social sciences are trying to be particle physicists and they're not trying to be engineers that doing the practical engagement and what, I think, a lot of us have been advocating for is to get the social sciences to think of ourselves more in the engineer world than in the particle physics world with that engagement and practical learning.

**MR. JISCHKE:** I rather agree with that and thinking about the fact that basic science could not provide what society wanted and engineering emerged as a response to that need. In principle, the physicists, the chemists are plenty smart. Newton's Second Law is still Newton's Second Law. The fundamentals are there, but there's a whole craft that goes with solving real problems that you just can't give up on because your bridges won't stand up.

**UNIDENTIFIED MALE:** That's the two engineers in that because even though you're a physicist but in an engineering firm is this word craft.

**UNIDENTIFIED MALE:** He's gone native.

**UNIDENTIFIED MALE:** Well, I remember because the one funny thing about MIT was watching the department of physics people look down in nuclear science and engineering and vice versa but in a good-natured way, right. That using terms like craft drives scientists crazy because of craft right, but we all know that -- is there a lesson from the engineers about how they engage with their

natural science brethren in a way that at least from the outside seems more productive and doesn't have the problem of the social scientists? Instead of trying to become the engineers trying to become this sort of pure scientists. Are there lessons there?

**UNIDENTIFIED FEMALE:** Yeah, the reason is the engineers that are here too. I mean they are driven by solving the problem. Whatever it takes to solve the problem is what they are looking at and that's what brings forward as many that do that and so they are not worried about what they have to do. They are worried about how to get that done and that's why the teamwork works.

**MR. JISCHKE:** Two answers to your question. All of the engineering disciplines are credited by a set of criteria that includes exposure to the sciences. The sciences are seen as a foundation in every undergraduate engineer as a foundation and second, there's a pretty clear path for a lot of engineering from science -- chemistry to chemical engineering, nuclear engineering from nuclear science, mechanical engineering from mechanics -- and yet the sciences did not take on the applications of their science as part of the discipline in the same way that engineers do, and maybe craft is too loose a term. But there's a whole set of details in the application of say physics to mechanical engineering that the physicist want to cover all of physics and they don't have time to do that stuff and that's how the disciplines emerged and alternately then the people who follow the science path versus the engineering path, those are different paths clearly. They're clearly different. And what's interesting to me is now

today, most science departments could not survive without the service teaching load they have for all these engineers.

**UNIDENTIFIED MALE:** Of the engineering undergraduates.

**MR. JISCHKE:** I mean there's a symbiotic relationship in the financing in those departments. I mean, we're a big university. There are 45 thousand students. I think we've got 80 or 100 physics majors and a faculty of probably 60 or 70 physicists. You couldn't afford that faculty if they didn't teach all of the engineers. So there's an interesting ongoing relationship that both have an interest in.

**MR. DESCH:** Dick, were you --

**MR. LANZA:** Well, I'm just trying to think this issue of a craft but let me go back to something I mentioned earlier. We talked about the Manhattan project as being sort of an example of "solving problems" but the fact of the matter that people who are physics people there actually did invent new engineering techniques. They said, okay, we can't -- we'd love to write nice analytic formulas to tell you what's happening, but you know what? You can do it. It's too complicated and they invented Monte Carlo methods. They invented systems of new ways of solving equations which because they were driven to get something. I'm always amazed at if you look -- the other area, by the way, is to look the MIT radiation lab during World War II which was sort of a big -- was the development place for radar. You know, at the end of it, they produced this I don't know how many volumes series and you look at the names of the authors and they're physics

guys who wrote a book on serial mechanisms for God's sake. How did you know that? Well, we invented it. Or did it somewhere like that so I think you can -- I think physicists are often very good engineers. The difference is you only have to do it once, okay. For many physics people, they say, God, we got this whole thing. We put it all together. It worked. I got my data. Good-bye. And then the whole thing falls apart, whereas, engineers might say, yeah, but what about the second one. Nope, sorry.

**MR. DESCH:** Good. Any other take-ways? Or lessons learned? Please.

**UNIDENTIFIED FEMALE:** Just an observation that two things in terms of how policymakers respond. A lot of it is the packaging but some of that is I wonder why universities aren't making better use of their -- if they a business or communication school or other schools so that we have usually in a policy school a workshop that's trying to help with a real problem but you don't have in the communication school projects where you're trying to help the university communicate its findings to people where it would matter so making use of what you already have or making use of a neuroscientist to help you understand how you can effectively influence people. There's data there that the universities aren't maybe drawing on in the way they could that they have within themselves.

As a policymaker, when someone comes to me with just the one piece of the problem, that's lovely, and if you've done it in a nice clean concise way that I can use, great. But my job was always to actually ask ten other people

because I had to know what the NGOs thought, what the impact was going to be on the poor people as well as what was going to happen to the weapons system as well as what were the privacy implications as well as what are the legal implications. And so, if you're bringing your policy and proposals also with those interdisciplinary answers, here are the potential implications across what you, a policymaker, should be thinking about. I think that's also very helpful because otherwise, I have to search it out already and depending on how good your piece of it is, you may or may not have triggered my desire to act. But if you bring me something that's a lot more complete or I already know I can answer ten of the questions that I'm going to be -- because policymaking is also about convincing everyone that this is the best path forward to get, hopefully, either the influence we want or the effect we want to help people. And you've got to convince enough people that's true and that the downsides aren't so big and that it's worth us spending taxpayer dollars on or using intellectual capital on. And so I find that if someone could help me answer four of those questions instead of just one piece of it, it's a lot more compelling. So it's just a thought.

**MR. DESCH:** Great. Thank you very -- that was very helpful.

We're past the witching hour and I'm cognizant that I don't want to overstay the hospitality of our Washington, D.C. Notre Dame Center hosts. It's like the in-laws that came for a day and ended up staying for a week, so I don't want to run afoul with them so I think at this point unless anybody else has any burning questions, I'll thank all of you for coming, all of the panelists for superb presentations and

wish you all safe travels back to where you're going, so thank you very much.

**MEETING ADJOURNED**