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RESEARCH RECOMMENDATIONS

FOR THE BROADBAND TASKFORCE

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P R O C E E D I N G S

MR. SICKER: Good morning. Okay, let's get started. I'm very pleased today to be able to convene this meeting and to be able to bring the folks who I was able to get to join us today.

We're going to have two sessions, a morning session and an afternoon session. This morning we have Dan Atkins; Charles Bostian; Vint Cerf, who's joining us via an ISDN connection -- there's Vint -- hi, Vint -- David Clark; Chip Elliot; and Ty Znati. From the FCC we have Rashmi Doshi; Erik Garr; Stagg Newman; and I'm Doug Sicker.

I'm going to keep my comments short for a number of reasons. I've asked each of the participants to spend about 10 minutes, which is quite a long time, given the short duration -- and I think we have two hours this morning -- but I want to hear from them. These are the experts, so I really didn't say an order, but I would like to just suggest that we start with Dan and go down the line.

Okay, thank you.

MR. CERF: Excuse me, could I interrupt? It's Vint.

MR. SICKER: Yes, Vint.

MR. CERF: Just one point. I have to be out of here at about 10:30 your time, 7:30 my time. So, just FYI in term of scheduling.

MR. SICKER: Well, Vint, would you like to kick it off?

MR. ATKINS: I'll yield to you, Vint, and then I'll go next.

MR. CERF: I don't -- wasn't trying to force myself on you. I just wanted you to know what my schedule was.

MR. SICKER: I'm more than willing to have you start it off.

So, we'll begin with Vint Cerf.

MR. CERF: I'm happy to do that, Doug. So, good morning, everybody. I'm not going to take 10 minutes. I don't want to. What I really want is to get some discussion going here. But I am going to put a few things on the table that I hope will trigger arguments.

The first observation I'd like to make about broadband research is that we are not doing terribly well with regard to the kind of wireless opportunities that might lie before us. The FCC Technology Advisory Committee has from time to time explored ultra wideband possibilities, but I don't think we've ever had much of an opportunity to pursue that, because there hasn't been a lot of available spectrum in which to try ideas out -- things like combinations of OFDM and CDMA and a variety of other sharing techniques -- but I'm personally persuaded by two things, first, that sharing the broadband resources can lead to some substantial efficiencies -- parties cohabiting in the same band; and, second, it's my impression that when you do sort of a general radiometric measure of our use of the spectrum that on average it's very, very low. We allocate capacity in narrowband channels and maybe we're using 2 percent of the spectrum at any one time. So, I think that opportunities to experiment with broadband wireless sharing would be very beneficial, and I'm not aware of much opportunity to pursue that from an implementation and experimentation point of view. So, that's one point.

Second, I think we can learn something from the 80211 experience, which is the unlicensed sharing of bandwidth. Despite, you know, the occasional collisions and the like, it's been remarkably interesting to see how many people have found ways to use that unlicensed bandwidth, you know, given radiation-level restrictions, and the like, in order to permit better sharing. So, I'd like to see more experimentation with unlicensed capacity -- white spaces being a good example of that.

A third thing which I'd like to suggest -- this I think still exists but I don't know how effective it is. This is what we sometimes call the NITRD Program, which is the National ITR&D effort. During the time of the Clinton administration, there was something called the

President's Information Technology Advisory Committee. Some of us served on that committee for several years. We had what I considered to be a very important, powerful, effective, crosscut relationship between the committee and the R&D agencies -- NASA, NSA -- well, NSA was there, but NASA, DARPA, NSF, and DOE among others would come to report what they were doing. They described it in a crosscut way so we could see the amount of research money that was being spent and we could see on what it was being spent. And I'd like to suggest that the participating program managers were, I thought, stunningly cooperative in their work to coalesce and to make more coherent the aggregate research program. I'd love to see the reconstitution of that committee, possibly under the PCAS, which is where the previous administration lodged the responsibility after PCAC was disbanded.

Finally, I'd like to suggest that there are some significant opportunities which appear to be in part underway. The program at NSF, the funding program, the future internet design sort of clean-sheet effort, which I hope Dave Clark may talk about, and the related GENI effort, which I'm sure Chip Elliot will tell us about, represent a foray into exploring what's possible, given the 30 or so years of experience we've had with the internet. Many of us -- some of you sitting around the table now -- are well aware of the shortcomings of the system as it is today, the lack of security, the failure to make heavy use of broadcasts, the poor quality of mobile tracking, and the like, suggest that there's lots of room for improvement.

You asked a whole series of questions -- nine of them. You could take days exploring these, and I'm not going to take more than a few more minutes of your time to do that. You asked about the kind of research that's going on today, and I think a lot of it tends to be very short term. I'm not seeing the kind of long-term willingness to put funding in for possibly years, even decades. If you look at the Arbinet program and the successor internet program, the funding profile went on for nearly 20 years. It involved multiple agencies after DARPA began the program. So, I don't see that kind of consistent funding, and I think in its absence that you'll find people not proposing high-risk ideas because it isn't clear whether will they will have time to explore them successfully.

I think that there is also a question about -- skip down to question 6 with regard to venture capital. The venture capital community got burned in the dot boom. Whether it learned its lesson or not is still to be seen, but they became a lot less adventurous, I think, in the aftermath.

I'm not suggesting that venture capital community should become as silly as I think they were during the dot boom, but they are being more careful about the proposals that they are funding.

The reason I raise this as an issue is that I think we tend to stop short in our research work at sort of working in the laboratory and not necessarily going far enough to get things to the point where a venture capital company would see an opportunity to pursue further. Let me use internet as an example. When it became clear that wrapping a graduate student around a computer to turn it into a router wasn't going to be scalable, companies like Proteon and Cisco were formed to build equipment and sell it to the university research community, and only after that community showed that there was a viable market for this kind of equipment and only after permission was given to carry commercial traffic on the government-sponsored internet (inaudible) phones did we see, around 1989, the beginnings of commercial internet service and once again interest in the venture capital community.

So, what I'd like to suggest is we examine our research programs. We ask do we have paths in place that will allow the R&D side to push further towards commercial viability enough to relieve risk in the -- enough risk -- venture capital world in order to spawn new companies and we hope therefore new jobs.

Finally, you raised a question about technology transfer. I often believe that technology transfer is an oxymoron, and what transfers are either people who understand things and go into companies who make products and services or products which transfer because they're usable.

To give you an example in a case -- Chip Elliott, he might want to respond to this later -- GENI will be effective if the consequences of what goes on there are transferable into commercialization.

I have to say that, with regard to impediments, one of the biggest ones, in my view, is software patents. Somehow we've gotten tangled up in our underwear -- that's a technical term -- and we are somehow inhibiting creativity by interfering with people's ability to use what they've learned in the software sphere. One of the anecdotes that seems to be open source -- and I can certainly tell you that Google has been regularly trying to provide open source platforms in the form of Android and Chrome and now Chrome OS coming next year. I believe that we should pursue that as an important theme, that open source opens up many opportunities for the R&D community to be successful and to build on other people's expertise.

So, I'm going to stop there and thank you for allowing me to blather on for however long I went. I'm going to be able to stay until 10:30 your time, and I'm eager, of course, to hear what others have to say. So, thank you very much for letting me join you this morning.

MR. SICKER: Thank you, Vint. And you're about nine minutes, so you almost used your entire time.

MR. CERF: Well, so much for that.

MR. SICKER: So, the one thing I didn't do -- I actually thought there was another moderator joining me, and I had prepared notes for that person. So, I left out to mention what we're even here for today, and I will borrow from what I had prepared for that speaker.

Well, as you all know, we're here to talk about research recommendations, and the goal is to provide these recommendations as part of the broadband plan, which is going to be going to Congress and out to the public and to other government agencies, and our hope is to articulate some directions and research recommendations as it relates to the process of research, as it relates to areas of funding and some other areas that are being examined and will be specified in more detail in a public notice, which will be coming out I think this week possibly? Yes, sometime this week.

I do want to make it clear that the FCC recognizes that there's a role for it in encouraging broadband and thinking about these research recommendations and research agendas. But we also recognize that there are many other government agencies whose primary job is just this, and our hope is rather to help them and augment a lot of the work that they're doing through this report rather than step on their toes sort to speak.

So, let's turn now to Dan Atkins. Dan's a professor in computer science at the University of Michigan. I would have gone into a more detailed presentation -- or rather a description of who Vint Cerf is but I hope you all know who that is.

MR. ATKINS: Thank you. So, good morning. I'm speaking to you from the vantage point of someone who has conducted combined technical-social work and what I would broadly call cyber infrastructure-enabled distributed knowledge communities -- or CI-enabled, for short. This work includes the concept of science collaboratories and digital libraries, and all of this of course depends critically on broadband digital networks.

I've served as the dean of Engineering and the dean of the Information School at Michigan and recently did a tour as the inaugural director of the Office of Cyber Infrastructure of DNSF. I currently hold the title of Associate V.P. for Research Cyber Infrastructure at U of M and consulting with consulting with many foundations and agencies involved in innovation and learning based upon network technologies.

The focus of my remarks today is that research development and provisioning of broadband networks of adequate performance and reach in both wired and wireless forms is absolutely critical to the nation's future leadership in a globally competitive world based upon knowledge and innovation. It is critical to both research and education. And by "adequate reach" I mean coverage to all inhabitants of our country, as well as high performance appearing with broadband networks in other countries, especially with their national research and education networks for sciences intrinsically global, and our strategy for innovation must include a nuanced mix of competitive and cooperative relationships with research communities in other countries, much of it facilitated by digital networks supporting what are sometimes called scientific collaboratories.

Education and learning, likewise, can benefit from networked-enabled international cross-cultural experiences and socially based learning. How might world understanding and even world peace be nurtured through new forms of digital diplomacy.

Research in broadband networking must itself be broad, involving carefully selected large-scale pilot projects well instrumented and of long-decade duration, as Vint said. We need investments that enable networking researchers to test out their ideas at scale. Despite the heroic efforts of the GENI communities and others, I would argue that this is still difficult to achieve within the current NSF funding models.

We need more investment in research in which networking is both the object of research and learning, as well as a platform for research and learning, to work in the Pasteur's Quadrant for those of you that know that metaphor. We need to pursue basic research together with potential applications for next-generation networking.

It's documented in dozens of reports sponsored by the NSF and similar funding agencies around the world. The conduct of scientific and engineering research is being revolutionized as we move into a platform of information technology or cyber infrastructure. This movement is also called eScience and, more generally, eResearch and is being pursued through investment in most all developed countries and increasingly developing countries.

Cyber infrastructure -- this kind of awkward-to- say term -- was recently adopted by the NSF and intended to connote two important things. The cyber part connotes augmentation of the physical world of atoms with the reduced barriers of time and distance afforded by the virtual world of bits. We mean here -- by distance we mean distance in three senses: geographic, organizational, and disciplinary. The infrastructure part is a reminder that IT must be afforded the high status of infrastructure, one of the most complex and expensive things undertaken by society. It's of course a lot more than technology, more than boxes and wires and software; it's a lot more than occasional discreet purchases of stuff. It's both physical facilities and supporting organizations, people, and policy; sustainable models of continuous improvement. It is reliable, supports broad connectedness, and provides a platform on which others can effectively build and tailor applications critical to their missions.

The framework of CI-enabled research includes high-performance computing for modeling simulation, prediction, and increasingly data mining, data, and information creation and stewardship services and online instruments and observatories. These services, tailored to specific projects, are linked by networks, middleware, workflow, visualization, and collaboration services to create what I'm calling today collaboratories -- laboratories without walls -- in which scientists work together globally with their colleagues, their tools, their data, and their digital libraries in a workflow through all four variations of same and different time and place. People, information, and tools can be linked in all four quadrants of this 2 x 2 matrix of same and different time and place, and so we can say that these teams are working together in four-quadrant organizations.

Some of these collaboratories are becoming functionally complete in the sense that they include all the relevant people, tools, information and facilities for a project, and therefore the collaboratory becomes both necessary and sufficient for participation in the project. They also have the potential to support groups working with not only implicit knowledge, knowledge that you can write down, but also increasingly with the tacit knowledge that can only be created and conveyed through social interaction and practice. It can support not just learning about but starting to support learning to do as well as learning to be -- to be a scientist, to be a physician, and so forth.

Dr. Arden Bement, the director of NSF, has said in many public talks that our nation's leadership depends critically on provisioning and applying cyber infrastructure and maybe a real -- maybe it'll be a determinant in America's continued ability to enervate and compete successfully in the global arena.

Although great progress has been in understanding and applying the technical and social behavioral factors necessary for a successful collaboratory, there's still much to be done. One of the barriers is the general lack of adequate end-to-end networks spanning global campus and residential venues. It's particularly challenging as science becomes more and more computationally and data intensive. The needs for wide area transfer of terabytes is becoming commonplace, with some communities moving to petascale requirements. We need enhanced networking infrastructures to support the science community in increasingly data-intensive distributed science. And the challenges are not just about size and ubiquity of the pipes. They include use-driven research spanning the entire stack of transport through the collaborative applications.

And now although CI-enable research has been the priority to date, CI-enabled learning is now emerging under a variety of names as the priority for private as well as federal funding agency. There is much in common between a collaboratory to support research and one to support education and learning.

A perfect storm may be brewing for our nation to revolutionize the way we learn, the way we teach, and the way we assess both, especially within the K-12 system. Department of Ed is currently developing a national education technology policy document, which reportedly will advocate a vision of a so-called culture of learning resting upon a learning eco system that is always on, life- long and life-wide, lending both formal and informal education. Network-based infrastructure would be required to support not only access to information but access to participation in learning networks.

It is a platform for seamless integration between in-school and out-of-school activities, including mobile learning and learning on demand. In such a world, learning need not be dominated by the traditional transfer model but more by a participatory socially based learning model that enables much more learning to be intertwined and made more relevant. The National Academies has likewise suggested that there is a crisis in laboratories in the school system, and cyber infrastructure could obviously provide access to research-quality telescopes, electron microscopes, and so forth for that community.

And, finally, I note in closing that the economic as well as the green and ecological pursuit of the movements and opportunities I have tried to briefly describe will likely rest on our continued adoption of the emerging models of cloud computing, the long looked-for goal of utility-serviced computing that is finally becoming more real.

Cloud computing involves massive consolidations and economies of scale on the server side but massive diversification on the client side and devices, desktops, laptops, web books, Smartphones, eBooks, wearable computers, media players, and so forth, and more diversity and location and in application. Realizing the full potentials and benefits of cloud computing depends critically on research development and provisioning of the next generation broadband infrastructure.

MR. SICKER: Thank you, Dan. Next we have Charles Bostian. Charles is a professor in electrical engineering at Virginia Tech.

MR. BOSTIAN: Thank you. I'm Charles Bostian. I'm pleased to be here.

Doug said he needed an old radio guy for the panel, so I qualify at both.

I am going to focus on some ideas about radio, how it can help broadband, and I want to acknowledge the contributions of several colleagues to this, particularly Preston Marshall, formerly of DARPA, who many of you know, who has recently completed a doctoral dissertation, which really quantifies the benefits that cognitive radio and related technology can offer to broadband.

In looking at all the questions that Doug sent out, I chose to -- concentrated on the ones in the box, and I think the biggest shortcoming is lack of integration, and this is related to what Vint said about lack of long-term efforts. I think there are few research efforts looking at a complete picture, say, dynamic spectrum access to robust wireless networking to mobile applications. And there are very few paths from small-scale basic research to large- scale deployment and experimentation and testing. Most university researchers are either working in up to about 10 nodes of intelligent wireless networks or they have a lot of nodes but they're in about three rooms.

So, these are two things that I would like to advocate. In terms of what could we do to have a substantial impact, I think this is taking advantage of the inherent capabilities that wire networks don't have, and I'm going to address one of these in probably too much detail about spectrum sharing and spectrum reuse and then touch on some other topics.

Everybody knows we need more spectrum in the right -- we need more spectrum in the right part of the spectrum, the attractive frequency ranges. So, we talk about spectrum reuse. We talk about white space. We talk about how we're using spectrum inefficiently. But I think the academic approach to this has been less helpful than it could be, because we've really --

I think the white space idea of finding and using vacant spectrum is impossible -- what is really vacant spectrum, and can you build a business on it? One person's vacant spectrum is something somebody else paid a lot of money for. I think the whole focus needs to be on how we can develop technologies that share spectrum easily and efficiently. One example, of course, is WiFi where it's developed to be collision tolerant and no one user has it but we're able to share it.

If we look at making spectrum reusable, I think we need to change from the idea of having no interference to the idea of managing interference. Having no interference makes sense of course for some applications, like public safety TV bands, but I think if future research is going to help, it's really going to need to focus on how we make spectrum more reusable, how we encourage sharing. And our traditional approach to efficient use of spectrum, I think, has been counterproductive to this, because my radio colleagues and I have been working for years on getting more bits per hertz through a channel.

But the way you get more bits per hertz through a channel is really that you raise the transmitter power, because you have to increase the energy per bit divided by the noise power density. So, you raise the transmitter power exponentially when you're increasing energy per bit, and once you develop, it is a system that will go a long distance and will interfere with stuff at a long distance.

So, I have a graph stolen from Preston to illustrate that. You can imagine that transmitter power is the vertical axis and areas of horizontal axis, and for given transmitter power, for a given required Eb/N0, there is an area where you can communicate effectively, and then there's a much larger area where you can't share the spectrum, because if another transmitter like you is in that area, you will interfere with it. So, the key idea I have stolen from Preston is that we should be looking at spectrum reuse efficiency with a metric which takes the number of bits per hertz that we can transmit and divides that by the area over which the user has to have exclusive access to the spectrum, and that makes things different, because if you lower the Eb/N0, if you lower the effective number of bits, you actually get a smaller interference region and you can get more efficient use of the spectrum that way as I'll show you.

Now, how big this interference region is depends of course on propagation conditions, and that can be modeled fairly simply by an exponential path loss, which is about -- exponent -- which is about 2 in line of sight and can go up to about 4 when you're over the curvature of the earth and behind buildings. If you put some numbers in this, you can see that the most efficient modulations for sharing spectrum actually use between 1, 2, maybe 3 bits per hertz, and we could do a lot more with using spectrum efficiently if instead of putting a small number of users with a large number of bits per hertz in an area if instead we used a large number of users with relatively simple, relatively power transmitters. And you can play games with that and do things like a head on the right where you can find ways to get a huge number of users and a small number of frequencies very efficiently and allow the thing to scale and allow all of the users to have pretty good access.

These techniques have been known to the cell phone people of course for a long time, and they have to do frequency planning, but they haven't really been taken into account in the part of the community looking at dynamic spectrum access and spectrum sharing.

If we go beyond this, the long-term recommendations, it's really to look at the inherent things that wireless networks can do that wired networks cannot. If you have a wireless network with dynamic frequency access, you can control the topology by assigning frequencies. Instead of answering the question how can I route this with the typology I have, you can ask the question what's the most effective typology to route this information.

There is a lot of technology out there that does these things -- disruption tolerant networking, for example. One of the big differences is that these techniques apply to wireless networks and not wired, that they really require communication across all of the layers, which I think is something that we should be encouraging as we go forward in wireless technology for broadband applications. And I think if we can promote research in these areas that some of these ideas can really lead to order of magnitude improvement and network performance. And these things are already out there in military networks. It's a question of getting them into the civilian research community.

Thank you.

MR. SICKER: Thank you, Charles. So, I do want to point out that I did not say an old radio guy; I said a radio guy. Next we have David Clark, who's a senior research scientist at Massachusetts Institute of Technology, and when I think of the internet I think of Vint and David and a few others, and I'm honored again to turn the mike over to David.

MR. CLARK: Well, thank you for giving me the opportunity to participate here.

You posed a number of questions, and I

thought I would organize my comments around those questions. I wanted to start with your last question. You asked about the breadth of what should be in the research recommendation, and narrowly we could be talking about the deployment of broadband in our country, and more broadly we could be talking about not only the technology but the innovations that define the use of the access -- the cyber experience, if you will -- and I would argue for a broader approach. I would argue that if we care enough about broadband to make its deployment a national priority, we should care about the range of issues that make it valuable.

You asked about the state of research funding for broadband-related research. I don't have quantitative answers to this question. My answer is more on the form of impressions. Overall, I believe that the level of funding for network research, which I take is a proxy for broadband-related, has been inadequate to meet the needs of the nation and certainly the research community. I see bright students receiving PhDs in the field and choosing not to go into academia, not go into research at all, because they see the job of a junior faculty member, even at a prestigious university, as difficult and unrewarding and giving them little opportunity to accomplish anything meaningful.

I talked to faculty that have left the United States for universities overseas, and they comment on the much more supportive and productive environment they find there and the high quality of the students they have to work with.

We see some changes in the strategies for funding right now. Vint alluded to the FIND program or the junior program at NSF. NSF is trying to expand the sorts of research it supports to include projects that are larger, more integrated; and I applaud that. DARPA has been largely absent from this sector for a while, but it may come back.

I think a relevant question is whether the research community would be able to use a high level of research funding while doing research only of the highest quality, and the answer to this question is unambiguously yes. The consequence of increased funding would be to change the sort of research being done, and you'll hear an echo of what Vince said and of several other things.

Instead of projects that involve funding primarily for graduate students, projects can be undertaken that also involve professional staff, including programmers, hardware designers. The ability to do larger projects that reduce ideas to practice and demonstration would make federally funded research much more compelling and relevant.

You asked about shortcomings of the current process. Aside from the overall funding levels, which I just mentioned, I had to identify the following points. First up, the merit review process used by NSF sometimes tends to produced what I would call conservative outcomes, and I think a diversity of evaluation methods for proposals can help ensure that ideas of all sorts -- incremental ideas, long-ideas, high-risk ideas, contrarian ideas, multi-disciplinary ideas -- all have a good chance of funding. I think it's important particularly to encourage the longer-range, high-payoff but risky research, research where it may be hardest to make a clear assessment of quality up front.

Secondly, projects funded over -- larger projected funded over a longer period allowed qualified research teams to focus on the research rather than focusing on grant writing, and I think this single change might be the most significant in increasing the research productivity of our best contributors.

You asked about industry research and how such research should influence federal funding. I think the important consideration here is not the topic area but the nature of the outcome. It makes more sense for industry to invest in research when it can appropriate the results of that work. Enhancements that might advance the state of the world as a whole but not the players that funded the research are hard to justify in an industrial lab. Federally funded research is more likely to result in open standards, industrywide architecture, socially beneficial outcomes. Data in itself is an example of open interfaces and license-free standards that resulted from federal funding.

Industry research and development is more likely to lead to innovations that preferentially benefit the owner of the research results. Today the interesting work that is defining the cyber experience is moving up -- and remember, I'm taking a broader definition of the research scope here. So, we're moving up through the layers away from the technology that physically transport bits and towards standards that define high-level abstractions, social networks, physical location identity; and I believe implementing these concepts using open standards and industrywide architectures is critical to the future of the internet. So, I would argue that public sector funding of work in these areas is critical even if we see industry (inaudible) already. You asked about commercialization. In general, I see my colleagues being quite creative and effective at commercializing their ideas. I would observe that one should not look at the commercialization, assuming that the only success model is small business venturing and entrepreneurship. Some ideas, like open standards, can transform an industry and create new growth opportunities without spinning out a new company.

You asked about broad research funding where we enable the unexpected or major discoveries. All of the subpoints you listed under that question are indeed very important. That's a great checklist. Unfortunately, the most direct path to a broad agenda is the more liberal availability of funds. When funds are scarce, there's a natural tendency to focus very hard on arguments about best use, which tend to narrow the target's objectives where success can more easily be measured and assessed, and these are often the short-term, low-risk projects.

As I said above, the second means to ensure a broad range of outcomes is to have different sources of money allocated using different methods. If you do collective review, combined with career grants, which focus on the person as well as the specific topics, funds at the discretion of a single program manager so they can make bets -- DARPA's done contests; there are creativity awards, which I would describe as funds that allow someone to go and think for a while -- all of these produce different sorts of results.

You asked about the role of venture -- we -- none of us get to think. You asked about the role of venture capital. I'm not an expert on the rules that govern venture capital funds, but most funds can only use their money for the specific purpose of funding a company at some stage of its birth and growth. Most fund rules preclude using the money for prelaunch research funding. The venture capitalists I've spoken to tell me that they're dependent on activities funded from other sources, including federal funding, to seed the innovations that they in turn can fund. I believe that federally funded research is critical as a source of ideas that spin out into venture-fundable innovations.

You asked about technology transfer, the specific issues of intellectual property and institutional barriers. I'm going to sound like Vint. The appropriate approach to intellectual property is a longstanding debate. I would suggest you either delve very deeply or do no more than acknowledge the debate. I think the proponents and opponents of open software, license-free standards, etc., and the like, have put their cases clearly. Many institutions, including my own -- MIT -- have made clear they give the researcher the choice as to how best to exploit the results whether by licensing or by open release. I think this is the best outcome we can have. An institution that takes that control away from an inventor and demands that an idea be patented even, if the inventor doesn't think it's the best path to exploitation, is probably not acting in the spirit of the intention behind the funding.

You asked about priorities. I could have given a talk here about my own choice of topics. We can certainly identify topics for the future of the internet -- better security, continue economic viability. If it's necessary to demonstrate that there are pressing issues in order to make the case for funding, that can be done. But I hesitate to embed lists of priorities in a document that sets a national agenda. Might have the consequence of narrowing the funding agenda of one or another agency. However, it might be helpful to make the case that cyber space is not done. There are new opportunities to exploit, new innovations to make continued benefit to the nation both to the economy and the citizenry from continuing to invest in the field.

Many of the issues we might raise here -- many of the issues I am raising -- are broader than those that relate to broadband specifically. They have to do with comparative policies across nations as to the most appropriate level and structure of research investment in order to sustain the competitive advantage of those nations. As such, these decisions are part of the nation's overall economic policy and the role of public funding for research within that policy: priorities for training, long- term investment to stock the intellectual shelves for tomorrow's innovations, or short-term academic industry collaboration to take the ideas off the shelf, and so forth. This landscape and the place of the United States in this landscape is changing substantially right now. I personally feel that we as a nation do not have a coherent way to analyze, model, or take control of our future in these shifts.

There are issues as diverse as immigration policy, primary and secondary education, our response to the current economic downturn. All of these shape how we support research and how we benefit from it. Perhaps our response to these changes is the best we can do, but whether or not these are bigger issues within which a discussion of a single priority like broadband must sit, there's no doubt in my mind that if today we doubled the national investment in IT research, the nation would be better off in 10 years.

In a narrow sense I think -- again, I'm speculating -- the return to our national coffers from the tax revenues on the resulting commercialization would pay back the investment. I've had other countries tell me the exact same thing. Taxation is much better than holding equity share in a company. You get a much higher percentage of the profits.

But I can't substantiate what I just said. I can't prove what tomorrow might bring. I'm obviously an advocate for investment in tomorrow. Right now I see other nations trying to out-invest us, and this makes me a little sad, but I can't quantify this. All I can say is if my point of view were to have an impact, it would be to empower people of vision to use that vision to invest in the future and not to defend the spreadsheet of numbers.

Eleven seconds to go.

MR. SICKER: Thank you, David. I have to say I'm sure the academics here resonated very strongly with your comments on funding and particularly the process side of the difficulty in getting research monies.

We now go to Chip Elliot. Chip is the chief engineer at BBN, and also he's the lead on the GENI initiative, which I'm assuming you're going to focus your talk on today.

MR. ELLIOT: Well, thank you, Doug, and thank you Stagg, for inviting me. Yeah, I'm going to give a couple minutes' background on GENI and then go into a specific recommendation for this task force.

Let's go to slide 3 if we can.

MR. BOSTIAN: If you could -- you could take the clicker.

MR. ELLIOT: Is there is a clicker? Oh. Thank you. How very modern.

I think it's undeniable that global networks are creating extremely important new challenges both for research and for all of us. There are, loosely speaking, science issues where we, the people who design and build these networks, can't understand or predict their behavior, and that's kind of a bad situation to be in. There, I believe, are substantial innovation issues. Many people believe that the network itself is becoming harder and harder to innovate within. You have to innovate kind of at the edges or above the network.

And, finally, there are society issues that all of us increasingly rely on the internet but we're unsure we can trust its security, privacy, or resilience. So, I think these are very important issues.

In response to this, the National Science Foundation has set up an interesting and comprehensive research program called Network Science and Engineering, and Ty Znati is here, so I won't go into this research program, but it is directly addressing the challenges I previously mentioned.

In parallel, the National Science Foundation has started up a project to build a large-scale suite of infrastructure in which these ideas can be tried out. It can be distinguished from the internet as such by being very deeply programmable so people can program all the way into the network and run experiments throughout it. It is envisioned as being virtualized, so different experimental services run, in essence, in parallel planes that are called slices within the infrastructure, and it is viewed as being fundamentally federated so that it is owned and operated by a lot of different organizations and different people.

This project has been underway now for about a year of active prototyping. We've just finished the first year and are starting the second year.

Let me tell you our basic strategy for creating prototypes of GENI. One of the things that's most important for these new forms of research is to get large numbers of human beings -- real people -- into these experiments. The internet nowadays is not a collection of machines; it's a collection of people who use services.

Now, it's clearly infeasible to build research infrastructure as big as the internet. So, that is not a path to do to make kind of a parallel set of research infrastructure that's just as big as the internet that people can explore futures in. So, we've adopted the strategy of what we call GENI-enabling commercial equipment, and we would like to be able to go to absolutely standard vendors and simply buy equipment and have it ready to do research on. And then we want to use this in the production infrastructure -- for example, in campuses, in the national research backbones, and so forth. So, we'd like to run production traffic in parallel with a variety of large-scale research experiments.

This is kind of an eye chart, but these

are the teams that are currently doing prototyping in GENI. The fine print lists a number of academic industrial teams, many of them composed of several different institutions, and the kind of the wall of logos on the right shows the companies that are currently involved in prototyping GENI.

It's very important to us to have companies deeply involved at this stage, because, again, we would like to have commercial equipment that is what we think of as GENI enabled.

We've just started this October building out what we call a mezoscale prototype, that is, a prototype across more than a dozen campuses in the United States and two of the national research backbones -- Internet2 and National Lambda Rail. Our goal is to get into and through these campuses. We would like to get to students in their dorm rooms, through their WiFi, through WiMAX, because these are people who can participate in these very large-scale experiments. So getting and through campuses is a key importance to us.

At the bottom I've shown some of the commercial equipment that is being GENI enabled and we hope will be deployed into these campuses and into these backbones. It's a key goal of ours to have more different kinds of commercial equipment GENI enabled.

Okay, now let me switch to a very specific recommendation on broadband research. So, I propose that the FCC require that all broadband infrastructure that receives federal subsidies must be research enabled. So, if you are going to build out infrastructure using federal funds, you must open it up for research in parallel with production traffic.

Well, what does this mean? We don't really have enough time to get into all the technical details, but the data plane must be capable of carrying experimental services designed, say, by academic researchers in parallel with commercial production services or other forms of production services.

The control plane must be compatible

with control software that permits on-demand allocation of this infrastructure resources whether for production or experiments. I would recommend that both wireline and wireless broadband be covered by this. As a technical note, in some technologies quality of service, good isolation will be easy; in others it will be hard. I think this is an area that probably you should just do what's easy.

And I note that many, many different kinds of technology are already compatible with this kind of recommendation.

Let me give some specific examples. In broadband optical networks, such as national backbones or regional networks that might be federally subsidized, you can satisfy such a mandate by allocating either entire wavelengths or packet-level traffic engineering or so forth. So, there are many different ways that you could open up optical networks to run experiments in parallel with production traffic.

Campus networks are already beginning to do this with funding from the National Science Foundation. You can satisfy such a mandate with Ethernet V-lens, Wifi, and so forth. There are many ways to do it.

Radio and cellular systems is a particularly interesting area as various people have mentioned. Here you can do it either kind of classically by, say, setting aside spectrum allocations for research, but you could also do it in other ways -- for example, setting up mobile virtual network operators dedicated to research purposes. So, I think there's a lot of ways you could try to do that.

I would argue that such an approach has relatively few downsides. I believe it add little or nothing to the cost of broadband builds since most of the equipment you buy already has this kind of capability. And it does not require additional infrastructure to be built. It's a relative neutral proposal. It doesn't favor one vendor over other vendors. Doesn't favor one type of operator over other operators. Does not favor one kind of research over other kinds of research. And if it's a bad idea, it can very easily be undone. You simply stop running experiments in parallel with production traffic.

Finally, I argue that such an approach would bring widespread benefits. It opens up broadband structure -- infrastructure -- for research experiments and innovation. It gives many, many people in their dorm rooms or, ideally, in their homes or through their cell phones ready access to experimental services.

A researcher tends to think of what they're innovating in as an experiment. But an end user will think of it as a novel service, which they should be able to get in their house or through their phone or what have you. If you follow this path, it removes the barriers between a successful experiment, an academic research experiment and a real service, because it's an imperceptible shift from one to the other. It's useful for a very broad range of research. I gave a GENI example. Dan mentioned many forms of cyber infrastructure that could use this in fields ranging from physics through biology, and so forth. And, most importantly, it's quite cost effective, because specific research projects will no longer need to build their own infrastructure. You can simply use the research portions of the broadband.

Thank you very much.

MR. SICKER: Thank you, Chip. That's a very interesting idea. I would like to discuss that more with you maybe after -- during lunch.

So, now I'd like to introduce Ty Znati. Ty is the division director at NSF for Computer and Network Systems, and, more importantly -- for me -- he was the person who introduced me to networks long, long ago. I've known Ty for about 20 years now, and he taught me my first network protocols class.

Please, Ty.

MR. ZNATI: Thank you, Doug. Thank so much for inviting me and allowing me to share some of my thoughts with you and with this committee.

Again, the curse of having a name that ends with z. By the time you get your turn to say something, practically everything you wanted to say has been said, so I'm going to just kind of elaborate on a couple of points that NSF is trying to do but also try to share some thoughts with you with regard to what we should be doing in broadband.

So, I know that the mission of NSF is really to support basic research and that the approach then is a (inaudible) is really a bottom-up approach, a distance to the ideas that come from the people, and then basically fund those that are meritorious and hopefully fund them at the right level of funding, which we cannot really do in many cases, but trying to fund that transformative research and then fund the people who are the most promising people to be able to do that work and conduct that research.

Okay, NSF is one of 13 what we call NITRD (character for long i added) or NITRD (character for short i added), depending on what group you really are, and NITRD agencies support research and development in networking and infrastructure. That is really what we define as broadband, and within NSF, CISE plays an important role and contributes greatly to funding research and broadband. So the projects that Vint mentioned, which is FIND -- Future Internet Design, or the GENI project is actually being funded from within CISE and particularly from my division.

Okay, so the type of project that we focus on in broadband are really not specifically tailored toward either access network or any specific aspect of the (inaudible) network that we tried to develop and then to support, but we more fund -- fundamental research that actually enables the evolution of global-scale networks, and the services that depend on these networks enable them to evolve and to achieve the level of trustworthiness, the level of reliability, of robustness, and so on so forth that these networks are supposed to achieve. So, most of our research is focused on end-to-end issues challenging research issues, such as scalability, performance, trustworthiness, manageability, usability, and so on so forth. So, evolved (inaudible) looking at a network as, let's say, a technology but as more like a social technical network whereby it is technology but also it's being used by humans and therefore it should actually involve or network the human within that technology itself.

When we look at what's going on despite the, you know, large investments in networking and research and networking, we're still really facing extremely difficult challenges to be able to achieve the strategic plan in terms of social and economic benefits that we can harness out of this -- out of information technology and networking. As a matter of fact, a couple of reports, one by OECD, ranks the United States as the 30th nation in terms of broadband penetration. Even some other studies that focus on metrics beyond adoption growth rate also point out that we have -- we're falling behind in terms of funding basic research.

So, let me briefly define what we mean by (inaudible) what we fund in terms of broadband. We look at broadband as an end-to-end issue. It's about the last mile, the first mile, and everything in between, including the humans that use this technology and the humans that develop services and applications on top of these technologies.

Okay, so one of the foci that NSF has invested funds in is this understanding of the complexity of our system, and the GENI effort, for example, is part of that agenda. We built systems; we have built the internet, which is an incredible success story; and we did really have fundamental understanding, you know, that goes beyond the type -- to enable us to understand the type of behavior that the system exhibits so we can actually build them and engineer them to be at least adaptable so when things happen we can always respond to those emergent behaviors as they occur.

So, I think there is a need for investing into that type of science and engineering and a need to invest into the infrastructure that will allow the scientist and research to gain that understanding. And, again, GENI is one example, but I think there is a need to actually fund more of this type of infrastructures to enable the science and to advance the state-of-the-art in terms of building complex systems.

There's the other aspect of broadband, which is networking at the edges. I think we have made significant progress in understanding access network from the fiber to the wireless, and I think Charles has spoken about a few of the challenges that we're still facing, but nevertheless I think the full potential of broadband technology has not been realized yet specifically with respect to the emerging applications and the fact that they're allowing users actually to create contact. It's no longer about contact, you know by companies and by industrial organizations but also by the users themselves.

So, we really need to invest in research to understand these emerging applications, because they have a potential role to revitalize the economy and revitalize civil sectors of our society, and I list a few of them here: health care, education, commerce, and entertainment, and so on so forth. I think it's important to increase the level of funding to enable this radically, innovative way of thinking about this technology and these applications.

But we also have to understand as this technology becomes more and more symmetric in this sense, it's no longer about downloading issues; also by uploading and by creating contact at the edges and so on so forth. I think the power of this technology can also create all sorts of issues in terms of security and vulnerability. I think we need to invest into new frameworks that goes beyond the perimeter model to understand how we can secure our system so we can harness the benefit without having to pay heavy penalty in terms of security and privacy.

And I want to mention a couple of other issues. I think there's a lot of other understanding to be done in terms of the structural changes and maybe investment incentives that have to be in place in order to enable the ubiquitous penetration of broadband in the U.S. and as I said in one of the studies we don't like very well with respect to other countries. I think that's an issue that FCC is well positioned to be able to address. And here we may specifically pay attention to the complexity of the need for quality of services requirement that will allow people to see value in these broadbands.

I'm going to say a couple of things here on some of the factors that have influenced or influence currently the way the adoption and use of broadband technology are affordability, usability, and the value that the users perceive on there. Those are difficult questions. Yes, we may proceed with the development of the infrastructure, but I think it's equally important to have people understand how can this infrastructure be developed so it is usable and it's affordable by the people who are supposed to be using it. So, there's a lot of understanding there to be done.

And, finally, my last point is collaboration of partnership. I think the problem is bigger than what NSF can do by itself or maybe any other agency. If you remember, until very recently there was a lot of partnership that were -- that used to be commonplace between industry researchers and academic researchers. In many cases they have propelled technology and allowed this -- maybe I'm going to use a term that Vint does not agree with -- this transfer of technology thing to happen a lot sooner than otherwise would have been possible. And I think one case of that is the Gigabit Project of the 1980s that allowed the penetration of optical fibers in development of this technology at a much more broader and deeper scale than what could have happened if the research only was done by industry or by academia.

I think the research in the future generation of networks has to follow some model that such a -- a model like that one, and definitely the renewal of this partnership is very well needed. Maybe the government, industry, and academia can look at this type of partnerships and collaborations and look at ways where the intellectual interests tinge between the stakeholders can happen in a much more flexible and efficient way.

MR. SICKER: Thank you, Ty. Since Vint has to take off soon, I'm going to ask first if he has any comments. I know we have some questions for him, at least one, but I wanted to turn it back over to Vint and see if he wanted to respond to any of the other speakers.

MR. CERF: Thanks very much, Doug. Well, first of all, I found this extraordinarily

thought provoking, and so I appreciate the group that you've assembled to raise a lot of these issues and to respond to your questions.

Just to go back to Ty's comment about my disbelief in technology transfer, it's not that I don't believe that technology can be transferred; it's just that I don't think it transfers on its own. I think that it comes about by moving people from the research world into industry to take their ideas and actually implement them or create products that can be propagated and used as opposed to simply the technology behind them. But this is not a good time, I think, to have a big arm wrestling match about that.

I think the one thing I'd like to emphasize is the importance of being able to do experimentation and in some cases to take advantage of infrastructure that might not be naturally accessible to the research community.

To give you an example of that, I want you to think a little bit about cable and satellite television for just a moment. Right now we have huge amounts of capacity dedicated to transmitting digital content in a broadcast fashion either over cable plant or, in the case of Verizon, over their FIOS optical system or from satellites that are, you know, raining digital bits down on hundreds of millions of receivers.

The thing I'd like to suggest to you is that if we could repurpose some of that capacity to rain internet packets down on people, and if we were to develop protocols that took advantage of knowing that this is a broadcast medium and this packet will be received by multiple parties, we could be doing some very interesting experiments, possibly creating new businesses for companies which up until now have used these large amounts of capacity simply to deliver decrease in quality video material, so I think that they don't recognize the possibilities inherent in this broadcast medium. That's an example of an experiment that I think would require a partnership between the research community and industry, which has access to the capacity.

Let's get to your questions, Doug, because, you're quite right, I need to escape in about ten minutes or so.

MR. SICKER: Okay, turn it to Stagg Newman.

MR. NEWMAN: Okay, I have a question. First for Vint and the other speakers, but you get first crack, Vint.

A friend of mine who went from academic research to large-company research to now CO of a VC-funded biotech company suggested a 10-year program for research. The first four to five years would be funded federally with basically no strings attached of fundamental research, and then to go beyond that would require an industrial sponsor or sponsors kicking in some of the money but still primarily government funded, and then the last three years they'd be on their own to get industrial or VC funded.

Is such a model practical? Does it address the concerns? Is it implementable?

MR. CERF: First of all, Stagg, it's a strikingly interesting proposition. It reminds me a little bit of the engineering research centers or centers of excellence that NSF sponsored for some time. Let me use an example. Deborah Estrin's work at UCLA, the Center for Embedded Network Sensing, had something like two four-year funding tranches of substantial quantity. If I'm remembering correctly, it's about $20 million per tranch over that eight-year period. She's now coming on sort of towards the end of that eight-year program and looking to reconstitute the effort with potential industry participation.

It's not easy to make that transition, but that formula that you offer reminds me very much of NSF's other innovative approach. You'll recall the shutting down of the NSFnet and the creation of Network Access Points and their declining funding profile, which basically said we'll fund you for a while but you need to become self- sufficient. I really like that tactic a lot, because it would, in my opinion, allow the entity that's doing the research to deliberately push the potential for commercialization in the direction of industry, whether it's through venture capital or through established businesses that want to engage in perhaps a new line of business that's enabled by the research. So, I like the formula a lot but very interested to hear what some of the other participants have to say about it.

MR. SICKER: So, I wonder if before we -- I wonder if we should try to take other questions and circle back around. If there are any other questions for Vint while we still have him on?

MR. CERF: Yeah, why don't you do that, yeah.

MR. SICKER: Rashmi?

MR. DOSHI: Yeah, I guess I would add one more question for Vint only in terms of experimentation. The FCC has put out their experimental testbed with NTIA and seen some of the use for spectrum in terms of experimenting. There hasn't been that much of a taker. Is there a reason why some of that may not necessarily be useful or whatever when Vint said that we should put out some testbeds to others?

MR. CERF: Okay, so, Rashmi, it's -- my first reactions are that if there wasn't a great deal of uptake, could it be that people didn't know about the existence of the program? Is there a problem with, literally, advertising? Was there any constraint in access to the facilities or the way in which the program was structured that might have limited interest? I'm disappointed to hear you say that, because I had hoped that things like this would trigger some serious work. What about collaboration with some of the research agencies, whether it's NSF or DARPA or some of the others? Was there an opportunity for that kind of synergy to be applied? Maybe you could elaborate a little bit more?

MR. DOSHI: I guess the list -- I don't know if the constraints -- I guess constraints were that perhaps there were no independent funding associated with it, and that may be the issue why people -- at least academic institutions and others didn't participate. We had a smaller number of venture-funded companies participate, too, because they thought some short-term commercial opportunities, but I'd be interested to hear, I guess, maybe now or later, eventually, whether FCC should do something more or different associated with that.

MR. CERF: Well, if I could just jump in, Doug, and say that this is a perfect example of an opportunity for coherent and collaborative planning of research across agencies. We have enormous opportunities for the program managers in these various agencies to work together on a larger-scale program, each of them bringing particular capabilities to the table. In fact, what I would suggest is a conversation between the FCC and at least Chip Elliott, NSF, and the GENI program to ask whether the facilities that might have been made available under the FCC program could be fitted into the GENI infrastructure and allow for some research in that dimension.

MR. SICKER: That's personally why Colorado didn't go after it. We had all the interest but none of the funding to do it. I mean, it was a great opportunity. We talked to some companies. But it would have been great if NSF or DARPA would have stepped in with the funding.

Any other questions for Vint before --? I do want to add one thing. My most valued

publication that I have -- I don't know if Vint will remember this from some years ago. There's a 1974 -- the original copy of a packet -- no, a protocol for internet packet interconnection. Do you remember signing that copy for me, Vint?

MR. CERF: Yes, yes I do. And, by the way, do you know that Softees has just auctioned off a copy of that publication for $25,000.

MR. SICKER: Do they have -- I have Bob's signature on that, too. Is it signed by both of you?

MR. CERF: Yes, it was, so you now have at least a $25,000 property. Hang on to that and it'll probably be worth more when one or the other of us expires.

MR. SICKER: Oh, my goodness. I just want -- I hope that we get to see more of these kind of publications. That's what we need, right?

MR. CERF: Amen.

MR. SICKER: I'm sorry?

MR. CERF: The chief internet evangelist was saying amen.

MR. SICKER: Amen. Thank you for joining us, Vint.

MR. CERF: Thank you so much for allowing me to participate this way. I have to tell you, it was fabulous. The audio was terrific. Video was very good. We have to do more of this. You can do a lot with this kind of technology I think. So, anyway, good luck with the rest of the meeting. Thanks so much for inviting me to join you.

MR. SICKER: Can I add that this is actually over ISDN and I thought that was kind of ironic, given that -- something broken about that, I'm sorry.

So, Vint, I will ask that you allow me to follow up, because we're getting this process started, and I'm sure I'm going to have follow-up questions for the chapter that I need to write.

MR. CERF: Absolutely. No problem. Good luck, everyone.

MR. SICKER: Thank you. So, can we follow on, on Stagg's question, then, with the rest of the panel?

MR. NEWMAN: David had his hand up.

MR. CLARK: I did. I think the suggestion is pointed in the right direction, but I want to be a little careful about casting it in too formulaic a way. I would - - there's a report that the CSTB at the National Academies has now revised twice, I think, which has got a picture in it which is colloquially called the tire tracks picture which shows timelines for a whole bunch of innovations within the IT space in the emergence of the internet or the Web or computer graphics and games and things like that and risk processor, databases; and what you see, if you look at the picture, is that the pattern of evolution from initial research funding and eventual commercialization is often not a straight line. It bounces around. Sometimes industry starts funding something and then they drop it, and academia picks it up, and then an industry research lab will pick it up and then it goes faddle for a little while and then it pops up again. So I would really say that industry ought to understand its place here, but we shouldn't be formulaic about it.

And with that as sort of a precautionary -- and, by the way, 10 years may not be a lot -- enough for some of these ideas. When I was a budding grad student, I was told by my mentor that if I wanted to understand the world

years from now, I should look in the labs, because if it wasn't in the lab now it wouldn't be in the market in

years. I think that horizon has been compressing. I think Nabster managed to do it in a month, but that's a special case. I think that with flexibility, he's getting at an important idea there.

MR. ATKINS: I'd like to build on that.

MR. NEWMAN: Actually, it's a she.

MR. ATKINS: Oh, excuse me.

MR. SICKER: This is that picture. I mean, it's a very fascinating picture of you. If you walk through it and look at how these technologies have interacted with one another over the timeline, it's a great depiction.

MR. CLARK: Well, I think they're going to revise it again.

MR. ATKINS: I wanted to just build on Dave's remarks from a slightly different perspective. So, I threw out this term "Pasteur's Quadrant" in my talk. Let me elaborate on that a littler bit, because, again, I agree with the spirit of what you're saying; and I also agree with what Vint said, that that's very much consistent with the spirit of the ERC programs that Eric Bloch pushed very strongly at NSF in which many of us were beneficiaries and believed in quite firmly.

When Venivere Bush kind of set up the framework for the National Science Foundation 50-some years ago, he was influenced by the pretty traditional linear model of having pure, curiosity-driven research over on one side and then having application and product development on the other and kind of a linear flow over quite a period of time sometimes through fairly arm's distance mechanisms, and somewhere even advocating you have a big diode in the middle of that so that the application and near-term issues don't get back and pollute the curiosity-driven thinking of the great minds.

A few years ago, a guy named Donald Stokes wrote a book called Pasteur's Quadrant in which he asserts that we really need to think about that not as a one-dimensional process but as a two-dimensional process where the one axis is the extent to which an activity is focusing on curiosity-driven research and the other axis focusing on the application and product development and that we have, for example, Niels Bohr appear purely curiosity driven, and Edison, the inventor, out here with -- and then he used Louis Pasteur as the kind of prototype of the person who not only contributed in both dimensions but built synergy by grounding and informing what they did in the theoretical and the applied side.

So, many people have alluded to the fact that the challenges and opportunities we have before us in this areas need to be placed in a much broader context. They must be placed in the context with a value proposition. They need to be viewed as emergent systems, not deterministic work. They need to engage the social, technical, economic, legal, policy issues together with the technological issues, and so I would claim that, you know, and consistent with what I said and Dave and some others reinforce, is that we need these large-scale broadband but broad goal kinds of pilot projects to drive that, and so a pure, curiosity driven at the front end and the pure tech transfer at the second end -- although that could be part of the program you're advocating, I think we need these long-term Pasteur Quadrant-type projects as well.

MR. SICKER: Very good point. I did want to add one thing, and we keep touching on this issue and I think it's important. I think Ty and some others mentioned it as well, which is we're talking about research recommendations for broadband, and that needs to be kind of unpacked. What does that even mean? Because it's easy to say networking. It's easy to say HCI and these different areas. Where does that -- you know, where do we get the most bang for the buck? What are we trying to do in this -- with these research recommendations? And I agree with what I think most people are saying, that it's more than just the specific technology, and we need to think more broadly, and we also need to think also about computing in general as part of that.

I think Erik actually had his hand up.

MR. ZNATI: If you have --

MR. GARR: I have some more questions,

so if you want to stay on this topic, that's fine.

MR. ZNATI: Actually, I just want to mention that I agree in essence with what Stagg was saying in terms of enabling a longer-term research with sustainability in terms of funding, in terms of pursuing these ideas. And to a certain extent I think the FIND that David Clark has actually a lot to do with in terms of structuring it and organizing it has -- is pursing that. But I think I'd be leery in terms of putting a time on when research is going to be done. I mean, that's really something that will be difficult to achieve. If you look at -- just look at what we did in the local access networks, and this research starts back in the '80s, and we're still trying to find out how best to share, you know, media among, you know, different users and so on so forth. And keep changing from avoiding interference to actually dealing with it and basically harnessing what interference can allow you to do and so on so forth. And that thinking kind of starts up at the individual level and then emerges into bigger and better, you know, kind of research agenda. That's what we should do. We should basically think about phases of research as opposed to just some sort of a rigid structure to how we do research.

And the other point is really -- the issue is not about what research should be doing but what -- how can we enable collaboration that used to be a strong one back in the, I would say, '70s and even late '80s that is really diminishing right now, and that's the collaboration between researchers in industry and research in academia, and find modalities whereby we can -- we enable that collaboration in a very easy way, which is really not happening right now.

MR. SICKER: I hope we can bring that point back up this afternoon on part of the Industry panel. We have Victor and some others here who I think would have some points.

MR. NEWMAN: Why is that not occurring now?

MR. ZNATI: Well, because the industry

-- they're not really investing a lot in basic research. The majority of industrial people are not doing that. And, further more is some of the federal agencies -- they don't fund industry in most of the projects actually they support, and that should be looked at.

MR. SICKER: David.

MR. CLARK: I wanted very briefly to come back to the question about spectrum use. And I'm not a wireless guy, so I would instantly defer to you, but I was going to pass on a comment from a couple of my colleagues that do work in wireless, which is that building a flexible piece of experimental apparatus in a given piece of spectrum is actually a big project, and, you know, you can say well, we have softer radio and I'll just mess around with the head end and so forth, but there actually aren't that many experimental platforms out there, and to move into a new piece of spectrum with a new characteristic involves a project that I think, again, is an excellent example of collaboration here, because a number of people could share a radio if it were done. But there's an awful lot of papers today that are being published using some twisted version -- and I don't mean that in a hostile sense but some twisted version of a WiFi radio. Why aren't they doing it there? And the answer is because that's the experiment apparatus they could afford. And if you have spectrum, this is a beautiful example of the place where collaboration to develop the other part of the infrastructure, which is the experimental radio -- you know, power aware or whatever you want to do, power control -- and there has to be some debate within the wireless community. What should the features of that experimental radio be? But that kind of collaboration is probably what it takes to make a piece of spectrum with experimental -- a piece of -- a healthy experimental platform.

MR. BOSTIAN: I couldn't agree more. And the development that is going on like that is military, and the platforms are not available to the civilian research community. I wish there was a way to develop those and had something like GENI-enabled in the radios.

MR. SICKER: I'd like to follow on, but Charles and I have a proposal in front of somebody here who probably will put us in conflict, so I'll just --

Erik?

MR. GARR: So, it's a great point, and I'd like to tell a quick story and then ask kind of a high-level question related and then a very specific question.

I've had a -- I'm not a researcher. I had the great pleasure of working with Alan Kay on some projects once in a while, who told me great stories about the early days of Xerox PARC and how the researchers would talk to the "suits." Alan very quickly called me a "suit." So, I say that, because I think that's kind of what's going on here. It's really great to talk about how do we collaborate between the great research that you all do and the interests of the American Corporations. We've kind of got a language problem that often shows up.

Alan's view -- and I -- you know -- Alan, if you're listening, I apologize in advance -- was we just kind of did our thing. We didn't really -- you know, we didn't really worry too much about what the folks from corporate said. We did our thing, and history has shown that, you know, the choices that they made were probably pretty good choices and they did some really important work. So, the kind of high-level question is how do we form this relationship or re-form it or reconstitute it in a way that we really get private industry in the right way and government in the right way and the research in the community in the right way to form around these topics? Because I think we fundamentally have a scale problem. Most of the things I'm hearing from all of you is that, you know, a little project here and a little project there is good, clean fun but it's not really going to move the ball forward, and the type of infrastructure challenges we face demand really big things, which means we have to figure out how to form capital in a very big way around these things, and that's going to cost government and industry. So, that's kind of the first kind of high-level question.

The second, more specific question is as we make the case to the suits, again of which I'll ascribe myself to be one, one thing that really I think would make a powerful case is something that I've heard from some of you and that I read anecdotally as I try to follow this issue is where are we competitively with other countries at a detailed level? So, are we really spending more or less? Is someone else spending more effectively, etc.? I think the more that we can understand that in some specifics -- that helps us make the case that, hey, this is really important and as a country we need to get organized around that.

Up for grabs.

MR. CLARK: The question about industry funding and industry cooperation I think is a tricky one. Industry today feels very, very pressed to use money in ways that's responsive to Wall Street pressures and so forth, and they're just not going to go out on a limb. You know, the margins that were driven out of the telephone industry basically destroyed Bell Labs. I'm now watching BT severely downsize their lab in England. Even though they are a much more dominant carrier, the margins are still chasing them out of the business.

It's clear if you look at some of the countries like the Asian countries that are really pushing here, they are using federal funds much further down the R&D path, and as I said these countries have a very indifferent industrial policy than we do here in the United States, which is that the use of money for -- the use of government money for the D part of R&D pays off for them in terms of competitiveness and, as I said, a return on investment through taxation. It's really hard to figure out where are competitive, and I think that, for example, some of these OECD numbers -- we should identify where we're weak, but I don't think we should flagellate ourselves over these OECD numbers, because, you know, they have weirdness in them. And I don't mean to criticize the OECD. I know those people. They're good people. But I like to point out we have -- the latest Pew survey we have 63 percent of the households on broadband, and we have about 107 percent on dial-up. That gets you to 70 percent. Twenty-two percent of the people that they query say they don't use the internet. You know, it's passing the House. We don't have a deployment problem; we have an update problem. And, you know, when Ty talks about usability and demonstrating value, that's why I was saying define this problem broadly. Broadly. You may -- you when we finish the plan to push broadband into the parts of America that are not properly served today, I think that 63 percent may tick up by a couple of percent, and I think that's a good social buy. I don't think there's anything wrong with that at all. But that's not nearly as big a payoff as understanding why do these 22 percent think the internet is not worth taking. "Couldn't make me" -- they get answers like that. You know, "couldn't --" what's going on?

And you might ask well, what percentage of homes in the United States and what percentage of homes in, say, Korea or Singapore have PCs? I don't think the OEC gives you that answer gives you that answer right off the bat. Maybe the do.

So, I think we have to ask these questions very craftily. But when it comes to certain areas like wireless -- and I've talked to people from some of the Asian countries -- again, they may be posturing; it's hard to get the data -- but they say yeah, we see the United States faltering and it's a good time to trample it from behind and take the lead, and generally once we take the lead they never get it back, and they point to semiconductor memory and things like that. So, that's a lot of data gathering to really understanding what's going on. And the cross- cuts are really tricky, because, you know, you go to Europe, a lot of the money coming out of the European Commission is part of the seventh framework, the frameworks and so forth. It looks like a lot of money compared to us. The part of the European Commission that deals with telecommunications has about a billion euros a year, but of course that's not all research; some of that's the equivalent of the European-level FCC. I mean -- but, that's all academic industry partnership money, and my colleagues who take it say, yeah, there's really great, large quantities of money. But industry, because of their involvement is always pulling the horizon, and we can't do long-range research. We're doing the short-range stuff, and they hate that even though they like the money, so, you know, let's recognize our strengths, too. We have a very powerful vehicle in the relative independence of the National Science Foundation to go fund things that doesn't have to make sense to industry on day one.

MR. DOSHI: Let me just build on that a second. So, I agree that we -- that different countries have different ways of doing things, and many have this -- more of a centralized policy in management. I've worked extensively some years ago with the digital library community within the European Union, for example, and there I think they were initially stymied and actually doing what academics would think of as true basic research because of the economic interest of the publishing communities, and so there were real constraints on the horizons or the boldness that could be pursued under those programs.

I work extensively with eScience community in the UK, and there at first blush you could say that they're getting more bang for their buck in their investments in cyber infrastructure within higher ed, because they have this thing called Jisk that kind of defines kind of a common infrastructure and has a lot of financial clout that can lead to more coherence and interoperability and economies of scale and digital libraries and eScience infrastructure within higher ed, and so in some ways they're further along, but when I talk to these people over some beer in the pubs they all are envious of what they think is a better balance between autonomy and policy that we enjoy over here in the NSF kind of a culture. I would be very reluctant to try to tweak that too boldly.

MR. GARR: Yeah, no, I -- I mean, I'm glad to hear that, because I think there's a -- you can be seduced by why don't we just get away together and we'll figure it all out and throw a bunch of money at it and it'll work, and I think there's certain -- there clearly is value, and this is what Alan taught me when I worked with him. You know, the value he ascribed in the early days of PARC was that they were left alone and they could really -- you know, great minds thinking about problems and do their work. So, there's a tension here that I think we need to figure out.

MR. ATKINS: There's actually an NSF report funded a couple years ago on the history of infrastructure with the notion of learning lessons vis-à-vis broadband and cyber infrastructure, and one of the things this points out is that if you look historically at the evolution of any kind of infrastructure, it's a very long-term Darwinian organic process. It's not one where someone sits down and prescribes it and it gets built. You nurture infrastructure, and it's very complex competition and cooperation. So, we have to be careful of not being unrealistic that we can sit down and deterministically determine what would happen and exactly how it should happen and so forth. I can give you a pointer to that report.

MR. GARR: I agree.

MR. SICKER: We circle around on this issue of infrastructure and education and broadband and I -- this is a key point and we've talked to NSF about this recently, which is we really need to think about what broadband means for academia, and it's an order or better magnitude, you know, bigger pipe we're talking about. It's not enough (inaudible) to have a 10-megabit-per-second connection; they need a hundred. They need a gigee, and that's going to be a part, I think, of this path going forward, that we really actually need to think broadband deployment for academia and to get to those data rates that are needed to ensure that we can do what we need to do -- the scientists need to do.

MR. ATKINS: And from lab to lab and home to home, and full end to end.

MR. SICKER: Right. Other question on that end?

MR. ZNATI: Let me add a data point here. There is also an important report that was -- it's called The Size and Engineering Indicators. It was published by the National Science Board. Actually it provides a broad base. Have you seen that one?

MR. GARR: Yes. It's really big.

MR. ZNATI: Well, actually -- yeah, that's -- the report itself provides this broad base of quantitative information of the U.S. and also international -- of an international science and engineering enterprise. But the chapter 4 specifically provides a comparative analysis in terms of investment of the U.S. in comparison to other countries like China, Korea, and the G7 in general, and I think that report points out that we're falling behind in terms of investing in basic research, and that raises the question that the issue has to be addressed if you are to remain competitive, and then I think if you focus just on chapter 4, there's a bunch of bullets there that provide different types of comparisons and different types of sectors and research foci.

MR. SICKER: Thanks, Ty.

MR. ZNATI: You're welcome.

MR. SICKER: And so, Rashmi, do you have more questions on your end?

MR. DOSHI: Yeah, let me go back to some of the points that Dave and maybe Charles made, again going back to the issue -- I'm a little bit unsure as to what are some of the problems that are faced in academia to try and -- what should FCC do? There was a proposal under GENI that perhaps we should somehow require the creation of an overlay or at least set aside in terms of spectrum or perhaps -- I'm still looking to see what are some of the actionable things that perhaps FCC or the Commission, in terms of the planning, should do in the broadband plan with respect to easing or making aware of it. On one hand we have the issue of, say, we put aside a spectrum we thought was potentially experimental, but the difficulty that the people needed to build infrastructure that would compliment that was perhaps holding back doing that. What are -- how do we do things going forward, and what are the actions of some of the concrete things beyond just throwing money kind of to NSF and say here, do it? Are there some ideas that could be explored?

MR. SICKER: And, again, and beyond even just the issue of what areas, David had talked a good bit about the process, and that's the thing that in the last eight of my life as an academic that I found was difficult. By the time you get to the third year of funding, you got some really great stuff going on, really great ideas -- that's when it really happens, and then all of a sudden the funding's gone, you know, so.

David.

MR. CLARK: Actionable for the FCC -- I mean, that's what --

MR. DOSHI: Well, again, within the broadband plan.

MR. CLARK: No, I understand, I understand. I mean, the FCC itself does not have as part of its charter any kind of grant making.

MR. DOSHI: Should that be part of the proposal?

MR. CLARK: Well, that would be radical. As I've said, I've argued for diversity of grant-making mechanisms, and I have to say that various studies that the CSTB has done around IT -- we repeatedly struggled with the fact that within the IT space, there is no department of IT. We have a Department of Energy. We have -- you know, we have a national -- we have Health, we have Energy, but nobody that we ever can lean on, a CSTB committee had the courage to say the government should start a new agency. That's sort of a high-overhead, high-risk activity to get something that takes several years just to figure out what it's doing, like the, you know, DHS, and --

MR. GARR: Yikes.

MR. CLARK: If the FCC wanted to be in the grant- making business, the question is would Congress go along with that. I see no problem with saying well, that's an interesting source of diversity, you know. You can call for collaboration. Does the FCC participate in the NITRD convenings?

MR. DOSHI: Not directly. At least not -- I mean, we occasionally participate in some NSF-specific grand version program evaluations, but nothing more formal or substantive as far as I know. And again I'm looking to see are there things that are new, something that allows us to bring forth or at least break some of the barriers that we're talking about here.

MR. CLARK: I mean, I have to say setting aside spectrum for experimentation is just a really, really, really important thing to do --

MR. DOSHI: And, you know, having done that, now the question is how do we allow people to participate in it to do that? Should we do different type of --

MR. SICKER: It could be that the FCC reaches out to NSF and says hey, you know, we're making this available; we need to coordinate our efforts. We're making these airwaves available; now can funding be provided or something like that. I mean, that's a recommendation that I could -- I can see NSF -- I mean FCC making.

MR. CLARK: Has anybody ever had a workshop around the question of how to use that spectrum?

MR. DOSHI: Again, I guess this is going back to the presidential task force that was set. There was a plan that the NTIA would put aside a certain portion of the spectrum and the FCC put forth another -- now, as you argued, I think that one of the issues became the fact that it's not well, you can get equipment off the shelf that you can modify correctly because it's in the 400 MHz range; it's much lower in the spectrum than the current unlicensed spectrum, and that may be a hindrance in terms of experimentation. Stagg -- I guess --

MR. NEWMAN: I wonder, would it be more helpful to enable you to use what's a commercially available spectrum but in some God-forsaken place. In other words, there's a missile test range out in the middle of Nevada; there are certainly areas in Nevada and Alaska and Montana where there is not much use of the commercial spectrum. Would that -- it wouldn't be a nice place necessarily for researchers to go, or maybe it might be nice but expensive. Would that -- you know, is that the type of thing that would work if you had a geographic area where there's not much commercial activity?

MR. SICKER: But that also -- yeah, I mean, that also goes to the point of is that as interesting as an area where there is a lot of congestion but totally for --

MR. NEWMAN: It should be easy to simulate interference, right, and so it's --

MR. SICKER: So, it's funny. It's just funny when you gear.

MR. BOSTIAN: Yeah, I think that making spectrum available would be very useful. Having workshops, encouraging people to do it would be more so.

From my perspective, when I write a proposal, what spectrum I'm going to use is almost down in the noise, because I have go sell the innovative ideas for these radio ideas to a network research community that kind of assumes that well, yeah, I can build a radio and I can get spectrum, but that's so far down in the mud it didn't mean I'd get there. And as Doug said, it's at the end of the three- or four-year process.

I think the FCC has done exactly the right thing in making the spectrum available, but there's no -- nothing in the rest of the government to motivate us to do that. I'm not sure how you do it. Maybe have a contest.

Somebody mentioned DARPA.

A really neat thing that the Irish counterpart to the FCC did back in 2007 was to make some very nice spectrum available for the IEEE Dynamic Spectrum Access conference, DySPAN, and allow people to bring their DA systems there and compete in that spectrum. You did a little bit of that in Chicago but you put us looking up at the most powerful TV transmitter in Chicago, so it was a little bit more challenging. But I think perhaps there are some things you can do to motivate researchers to think about using that frequency.

MR. ATKINS: Let me just add to this, wearing a hat as a former dean and a former NSF official. First, offering resources to faculty members like spectrum or access to laboratory equipment or something without the requisite support for release time and graduate student support and so forth, you know, it usually gets second-rate attention. It's just not practical for them to pursue it.

Secondly, Vint used the term "advertising." I would use the term "community building." In other words, a lot of -- even at the NSF, if you offer something, it usually -- well, even specifying what to offer and the terms and conditions for offering it usually involve some community involvement, workshop, and so forth to articulate it and then you build a community around the opportunity, the idea, and build brokering channels for people to work together. So, that needs to be done proactively.

And then, thirdly, there is quite a bit of history of NSF and other federal agencies working together where if you could bring money to the table, NSF could work with the processes for allocating and reviewing and so forth, and that's fairly commonplace, and so you wouldn't have to establish your whole department of research, review, and infrastructure. You might want to just try that out in some pilot way if you could pull it off.

MR. ZNATI: Of course I need the money

before --

MR. ATKINS: Yeah, I understand -- I mean, one of the outcomes of this process would be creating at least any -- you know, again, it could be a limited term kind of experiment.

MR. ZNATI: Actually I want to add a point from a research perspective here. I think in the -- when I talk to the community I try to see what their needs are and so on so forth in term of do their research, and we get to discussing what FCC enables, and many minds of the research, which is -- I'm not agreeing that that's a right state of mind -- they think of FCC as a constraining body whereby the regulations are rules constraining what they can do. So, they take that. This is basically where we can play, and now let's try to innovate it in that space. But when I listen to a good friend of mine, John Peha, talk about that's not really the perception that the FCC wants to convey to the researcher, but that the idea is really actually to try to innovate and that FCC will help you articulate the policies and mechanisms that will allow this technology to actually foster and be promoted so on, so forth.

I think along with -- promoted the use of spectrum, I think it's also a good idea to talk to the researcher and then tell them kind of change that their perception a little bit and then allow them to think FCC rules and regulations as a framework but not necessarily constraining one.

MR. GARR: That's a really good point, and, you know, there's been a lot of discussion in the public over the last couple months about spectrum, and I think that the chairman's been pretty clear that, you know, this is a critical national asset that is managed by this body and that we need to be better at how we do it. And I think it's really -- one of the other reasons we want to have this workshop, and we expected we'd hear some of these things from you, is lots of people around the country need spectrum, and we can't forget places like the research community, because we have lots of other people coming in here asking for spectrum who want it for corporate purposes, which we need to do as well. And part of the issue with the research community may just be us being more active and more flexible in saying that look, for a research interest we should be willing to, you know, be more flexible on rules and be more flexible on how we manage the spectrum. My only comment is we sort of get that we need to do better on that and that's our intent.

Doug, I can make up a question if you want, but --

MR. SICKER: Hold on, we have questions.

MR. GARR: Yeah, I figured.

MR. SICKER: I just want to make sure we have enough time here for the panel, then turn it to the audience.

MR. GARR: Sure.

MR. ELLIOT: Let me, if I can, briefly advocate a nationwide spectrum as opposed to or in addition to something in a remote area, as in my experience people often make devices in their lab and would like to take it outdoors and would like to send five or six to some friends and so forth, and that is a very low barrier to entry if there is legal spectrum wherever those labs happen to be or wherever the outdoor experiments they wish to run. Now, you know, also having kind of unusual or interesting spectrum in a remote place is good, but in my experience having some reasonable nationwide research spectrum would be very helpful.

MR. SICKER: So, would it be possible that it might not be nationwide research spectrum but streamlining of experimental licenses -- I mean, turn this to Rashmi or --

MR. DOSHI: I mean, obviously everything's on the table in terms of understanding -- first to understand what the constraints are and what are the needs to try and then see. In fact, the rules actually allow a lot more flexibility, so it seems partly just to kind of educate what flexibilities current rules allow. I am not sure if that kind of outreach has been done through the FCC or even through some other opportunity. So, that's probably an immediate thing you could do -- just education and say the current rules are a framework and there are things you could do within the framework. And the next would be, then, finding opportunities where you really need to do some experimentation and review what things can be done.

MR. SICKER: I mean, this was a very good point. I mean, Jon and I were at a meeting with a number of the folks that were on this panel here six, seven months ago, and a lot of the researchers weren't even aware that there were experimental licenses, so, I mean, just being aware of these things could help a lot.

MR. NEWMAN: Yeah, let me suggest maybe if you all could get back to us with concrete suggestions in four dimensions. My thesis was actually on infinite dimensions, but I'm only able to come up with four today. One is geography, okay; second would be the time -- in other words, I don't know, you know, maybe, you know the commercial carriers probably wouldn't care if you were doing research between 2 a.m. and 4 a.m. -- that wouldn't affect their network load. There's a hypothesis. The frequency domain; and then the other is the interference- level domain, you know.

MR. CLARK: Can I just very quickly point out this question of nationwide spectrum or something like that? Remember what I was saying earlier that you really need a consortium of people from different institutions to get together to build a sufficiently flexible, powerful, cost-reduced piece of experimental apparatus that you can have more than two of them. And if everybody has his own different chunk of spectrum, or maybe if it's within a very narrow band you can move the head in around. But fundamental, I'd like to be able to make a radio and share it with somebody at Berkley, and that's why you like consistency in the spectrum allocation so you can share the apparatus.

MR. SICKER: So, I'd like to kind of change direction a little bit with a question, and there are some other folks in the research community who would I think jump all over this issue, and I wonder if there's a rule for the FCC, particularly as it relates to data about the internet and the difficulty that a lot of researchers have in getting information on how the network operates, how peering is done, how routing is done, stability of the network, instability issues. There's a lot of folks who have made a career of this, and they all point out how -- just how difficult it is.

I know when I was here back in the '90s, Stagg and I worked on getting the carriers to report outages on the internet side. They were used to reporting outages on the telco side, and that was an uphill battle, and we actually ended up doing it through part of the Department of Defense to get that scrubbed so that we could put together some information on it. I think it's a little more accessible now, but I'm wondering in that space is there something, in terms of understanding how the network operates, whether that's routing or higher up, that could be useful, and I'd like to hear particularly Chip and David, but I open it to everyone of course.

MR. CLARK: Yeah, so I could try to channel, say --

MR. SICKER: Casey.

MR. CLARK: Casey. Yeah, Casey (inaudible). You bring up a -- it's very specific issue, but I think it's a very important one. Most people in the academic community -- I could just say yes, I mean, to your comment. I mean, yeah. Most people in the academic community actually may not really have a good idea about what's going inside the internet. They have little cartoon-like versions of the story. We certainly don't know all of the diversity about things like peering and what kind of routing constraints are imposed, and it's much more complicated in a richer space than you think or that many academics think. So, when people work on routing protocols and so forth, there's a question of whether they're actually solving the right problem. The problem is the range of information you need to gather is rather complicated. I think that if academic research was seen as being more -- having higher potential to be relevant to what the operators are doing, the operators would be more willing to partner with academics in order to help reveal the information, to help them do the right work. So, I don't know that the right approach is to have this vast public repository that everybody dumps information into, which is a very hard thing to negotiate, because a lot of this stuff is viewed as sensitive, and -- or whether the right approach is to allow academics to be able to play inside networks for a while. Maybe we should make every academic go do a summer sabbatical in a NOC and learn how it's really done, and they'd probably come back different people.

MR. ELLIOT: I think it's a fantastically good idea. You know, my impression is that people have a very poor understanding of how today's networks work because of the total lack of transparency. Even the operators don't really get the whole picture of what's going on, let alone academic researchers, and I think any steps in that direction will have a very high payoff because that lack of understanding is -- you know, this is a very important system and not to understand it is really dreadful.

MR. CLARK: I've had several occasions when I have discovered that a network operator didn't quite know how the network was working.

MR. ATKINS: Likewise. I've experienced that, too.

MR. SICKER: So, I'd like to open it up to the audience. We have nine minutes left.

Mike.

MR. NELSON: I'm Mike Nelson with the Communications Culture and Technology Program at Georgetown, and I'm very glad that David started his comments by saying we really needed to have a broad look at what is involved in network research, and yet I note that when you look across the panel here we have a lot of network engineers and we don't have a lot of people looking at some of the issues that you flagged as being really important, like why don't people trust the internet and why are some of our policies actually getting in the way of more rapid investment. So, I hope in your chapter you will look at some of these issues. I can give my four or five obvious questions where we need to have some more research. One of them is one you touched on, which was why can't we get engineers in different companies to work with academia? Well one answer is intellectual property laws that require two lawyers to be in the room any time two engineers talk to each other.

Another issue that's related to network development is copyright, and we have some copyright policies that are making it harder for the remix culture to take off and drive demand for more networking capacity. We have policy questions, like what happened when the FCC decided that voice over IP companies have to provide 911 service. Well, that's an economics question;

that's not a network engineering question, but it had a huge impact on that sector. So, maybe you're going to have another panel. Maybe you're going to get people who are more expert than the people part of the puzzle, but I hope in this discussion and the follow-up will have a chance to delve into that and particularly perhaps the most difficult question -- figure out how we can get people who have the understanding of the network like you do who also understand some of the social constructs and the psychology and the sociology and the policy and the economics that is actually standing in the way of the technologies that you're developing.

MR. SICKER: So, I can tell you we had a list of probably 40 potential participants, and we couldn't invite them all. That was the one issue. The other thing I'd say is I'll turn to you, Mike, and say submit us comments. There's going to be a public notice and I would love to have input on these issues. I think they're critical, and they have been brought up here and we do have some people who focus on that in their research. Dan, for example.

MR. ATKINS: Yeah, I wanted to say -- I tried to say some of that in my remarks. If I failed, I apologize, and I'm not a network engineer, so if that makes you feel better. And of course the School of Information, which you know a little bit about, was founded exactly on the premise that you've just asserted. So, I'll do my best to do better to get this into the report.

MR. NELSON: I don't know if you are multidisciplinary and multicultural, but there still is definitely a bias here towards the lower part of the stack.

MR. SICKER: I'll take you're your word for that.

MR. NELSON: Well, again, I'm happy that the record is --

MR. ATKINS: No, seriously, I was quite encouraged. There are people here who are well known at the lower end of the stack who are agreeing with you I think.

MR. NELSON: I'm very encouraged by that. I just think at the end of the day we need to make very sure that we understand that technology races ahead, people struggle to catch up, and policy is somewhere back here. And if we have the world's best technology and we don't have any understanding of how the policy and the psychology and the economics affect the deployment, we haven't done our job.

MR. ZNATI: Actually --

MR. NELSON: I will write that part of the report.

MR. ZNATI: I really want to emphasize how right you are in your thinking in what you're proposing right now and tell you what NSF is trying to do. I think that that's really what I refer to in my brief comment about socio-technical systems where you don't look at the technology in isolations anymore, and you probably heard by now that this FIND initiative -- the Future Internet Design -- and the program was structured to have multi-phases before -- actually, we tried to think about experimenting with one architecture that will embed not only the technical -- to address not only the technical challenges but also address the economical issues, the policy, and so on so forth. And we are -- we have held a summit recently about how many -- about a month ago, David, or so?

MR. CLARK: Yeah.

MR. ZNATI: About a month ago, and the summit was really open, and we sent an open invitation to everyone, not only to the people who are really focused on the technical aspect of the network, to come together and be able to talk to each other. So, we are looking for lawyers to be there, and we had at least one, if I remember, lawyer that was there. We were looking for people from -- economists, so on so forth, to come together, and in many cases not necessarily forget about what they have done in the past but be open-minded in the sense that they -- how, what type of architecture has to be in place in order for us to build the type of system that you were just talking about, whereby policy is not going to be lagging behind technology or technology is going to be impediment to be able to enable the type of economic and social issues and benefits that we're looking forward to. And fortunately, most of -- the majority of the audience was networking and technology people. We had few from -- economists and policymakers and so on to attend, and the DC -- Dear Colleague -- letter will be released shortly by NSF, and the DC -- Dear Colleague -- letter will ask -- will actually encourage people from -- that go beyond, you know, designing the technology to actually participate and team up with other researchers in order for this architecture to be built on sound ground. So, just keep that in --

MR. NELSON: And my last comment is I was here at the FCC about 15 years ago with Stagg and others, and it was almost that long ago, Stagg -- and at the time it seemed that the technologists like me and Stagg were outnumbered

10 to 1 by the economists, and the economists were outnumbered 10 to 1 by the lawyers, and so it seems that FCC strength would be in determining what kind of legal policy and economic research is needed rather than what kind of fundamental --

MR. CLARK: FCC staffers should apply for NSF grants.

MR. NELSON: That works. Let me add that in addition to the research program recommendations that Doug's leading, we also have an effort on future architectures and policy issues related to those architectures that Dave Eisenberg is leading, and so we'd welcome input there. We think the adoption effort, which is clearly a major problem -- we have a solution, but then the policy to keep up with that we don't know the -- the solution to the adoption effort is we're going to require a teenager in every home.

MR. SICKER: Are there more questions from the audience?

MR. GARR: Just underline one thing on adoption. We have commissioned the first, we think, field survey of nonadopters, so when most market research has been done on use of internet, you know, all the Pew work, you know, they ask a hundred questions, 80 are targeted towards all of us that use it and there's a few questions for folks who aren't. We're actually doing an entire survey only on the people not using it. So, that's one thing that we need to do, and recognize that those are very -- there's a lot of tricky questions with the rest of the population that isn't using it. It's not simple, and we need to treat it seriously. I think that's the -- you know, we agree the royal suggestion here, and the data's actually coming back.

Ellen, sitting over here, could probably tell us more about it, but we won't put her on the spot yet.

MS. SATTERWHITE: Wednesday afternoon.

MR. GARR: Wednesday afternoon, all right. Still on her computer, so.

MR. ATKINS: (Inaudible)

MR. GARR: No, you know, Dan, it's

funny, it's exactly right. We're actually also doing field focus groups, because the -- there are serious methodological problems in trying to do this work, and we're absolutely giving our best effort, and that joke's been floating the FCC a little bit, and I appreciate you bringing it up.

MR. SICKER: So, rather than going around, if there are closing comments by anyone, great, and if not it's time for us to have our break, okay? So we will be coming back at 1 o'clock.

MR. DOSHI: Can I just mention --

MR. SICKER: Oh, please.

MR. DOSHI: -- a couple of times today there's a public notice coming out on -- actually, it's out. It was out on the 18th.

MR. SICKER: Okay, great.

MS. BAKER: All right, so as Victor takes his seat, I think we're going to get started. We're not quite sure what the protocol is, so I'm going to start.

I have a couple of opening thoughts that

I kind of want to go over just to kind of color what we're going to talk about this afternoon. First of all, thank you guys so much for being here. I just -- I'm so deeply grateful to you. You're all such well-regarded participants, and really you're responsible in large part for the creation of the networking technologies that we now enjoy and rely upon, so thank you all for what you've done and what you're going to be doing, and we're going to have a really interesting discussion this afternoon.

We often talk about investment and innovation, and really as a practical matter, what we're talking about is your work and your efforts, so whether it's from PCS to LTE or DSL to DOCSIS 3.0 I thank you again for what you've brought us and what you're working on. Our mission is really to develop what you've created for the benefit of consumers around the country.

So, this workshop is just a really important piece of the puzzle as we move forward with a national broadband plan. The ability of the U.S. consumers to benefit from a world-class broadband echo system in 2020 and beyond is dependent upon a strong research foundation. This afternoon I hope we can all walk away with a better understand of where we are succeeding as a nation and what we can do better to improve R&D in deployment.

At its core, what we are asking you to discuss today: What do you need to create a better broadband network for consumers? Are the commercial and investment sectors providing the resources that are necessary to innovate? What is the proper governmental role to foster your research? Is it direct funding, providing incentives to innovate, or just getting out if our policies or approach undermined your efforts to develop new networks, radios, and technologies. Are there particular areas of research that require a greater governmental role? Scary.

We also need to understand that research is multi-dimensional, and the next big innovation for broadband may come from someone's garage, or it may come from a major conglomerate. So, how do the many forms of the ongoing broadband research inform this discussion really -- in other words, how do we set up a national policy to make sure seed money and the venture capital gets to the guy in the garage while encouraging and facilitating a more centralized and focused research effectively at Bell Labs 2.0?

So, there are no correct answers, and we want your ideas and your thoughts, and we want your -- we want to really give you the opportunity to be bold here.

As part of this discussion, we would also like your perspectives on what is happening globally. We have long been a worldwide leader in networking and new technologies, but as you watch the trends in international government and commercial investment, what do we need to do to stay ahead? Are there recommendations that we can make to better position our industry partners to continue to succeed in the global marketplace?

And, lastly, we want to explore how we

can ensure you remain researchers and not full-time fundraisers. We hope to understand the limitations of the current funding processes, both commercial and government, and to seek input on how changes in these processes may help researchers be better able to succeed in creating the breakthroughs that have led to the current internet.

So, we have a lot to discuss, and I look forward to engaging in discussion of these important issues.

Should I introduce the -- would you like to introduce?

MR. SICKER: I'd be happy to. First I'm going to check to see if --

MS. BAKER: May I introduce Doug Sicker. He's the senior advisor with the National Broadband Plan.

MR. SICKER: Thank you. So, I want to first check to see if David Farber's on. He might not be on yet. We're going to try to have Dave join us through a dial-in.

Going down the line, we have Victor Bahl, who runs a networking research at Microsoft Research; David Borth, who's one of the CTOs at Motorola; Adam Drobot from -- a CTO at Telcordia; Dick Green, who we have mistakenly down here as the current president and CEO of Cablelabs, but it's actually the former. Dick lives in my neighborhood, and I see him regularly, and he's now involved with the university through the Silicon Flatirons Program, which I'm also involved with. Next we have Mark Levine from Core Capital, which is a venture capital group, and unfortunately also not on here is -- Marcus is not on the --

MR. NEWMAN: Should have been corrected.

MR. SICKER: Okay, and Marcus is joining us from Alcatel, Bel Labs, right?

MR. WELDON: Yup.

MR. SICKER: Great. So, why don't we just start in the same order as we have done before.

Victor, do you want to start things off?

MR. BAHL: Sure.

MR. SICKER: And as the same format, we'll go about 10 minutes with everybody and then open it up to a discussion.

MR. BAHL: That's me.

MR. BORTH: Do you have the clicker?

MR. BAHL: Thanks (inaudible) down here.

MR. BORTH: Oh, okay.

MR. BAHL: I got it. Okay, thank you very much for inviting me and for having us over here. I'm quite impressed by the panel here and also the previous panel that I had the privilege of listening to, or at least had half of it I was listening to.

So, let me just sort of do a level set here and sort of to say some of the obvious things that people already know about, and the obvious things are, of course, that, you know, this is -- the internet penetration is really important, and the amount of applications and the data that is flowing is getting to be very, very -- yeah, we have the right slide, okay. So, there are a lot of applications that you already know about, like social networking, multimedia downloads, gaming, 2D conferencing, etc., that you see today, but we sort of have to sort of see well today to see can we come up with things that can make sure that, you know, we have future proof of policies and funding and network, because one of the objectives is to stay ahead.

And so I wanted to give you a little bit of preview of some of the stuff that we are working on at Mike's research, and that has to do with, for example, and mostly we do conferencing, with requires multiple cameras (inaudible) here. And if we were to succeed, what would that look like would be essentially that you would be here but not really here, right? Right? People have -- you'll have that sort of experience. 3D telemedicine, natural gesture computing, and collaborative development, remote health monitoring, virtual immersive classrooms, augmented reality -- these are all applications that require more and more data, right, and so more and more data between machines, more and more data machines and humans, which are geographically separated. So, we definitely need a lot more bandwidth than we have today. Today's networks cannot handle any of this stuff that I mentioned here, and this is definitely the way to move forward.

Now, in terms of wireless use, many of you already know these numbers have been cited again and again, but it's on the upswing, right? There's lots and lots of people using their Smartphones now. Lots and lots of people accessing their networks or laptop. I was sitting there using WiFi and connected to my corporate network and doing some work as well. And the 3G latencies and the bandwidths that we have today are just not going to cut it again. We already had anecdotal evidence that recently there was this meeting where the iPhone users completely brought down a 3G network and it just became nonusable. And this is today in 2009. Imagine what we need in 2020 going forward.

So, may I say a couple things about what is broadband definition? What is on the table these days? What have people have suggested. So, there are many proposals. A few that I know of, you know, for example, are listed above, and you can see that people have done their own analysis and sort of come up with recommendations for the FCC to consider in terms of what makes sense to them. Example -- the IEEE USA. You know, the announcers that (inaudible) suggested that at least 20 megabits per second for 90 percent of the people should be there within five years.

Mike also has proposal. The baseline proposal is that about a hundred megabits per second should go to the Anchorage Institutes. The Anchorage Institutes are defined as schools, libraries, hospitals, community places, which is sort of the crux of where, you know, a lot of vulnerability is there. And from these Anchorage Institutes you can actually build out your network more. So, I believe you would like a lot more, but we believe that this is quite durable and is useful. But everybody in the university agrees that these definitions have to be revisited again and again.

So, just quickly, you know, what have we been doing from a research side to enable broadband access? We had a pretty extensive mesh networking program, and I believe a lot of the stuff that happened in community networking or blanket citywide coverages sort of came about because of that work, and what we did there was we not only did the research but built up these academic kits which we passed on to universities. There were about -- I believe I list here about 700-plus universities using those academic kits. We also funded a lot of this work, which then ended up in (inaudible) connectivity. We also have been supporting Internet2. We now have a big program around software-defined radios, because we definitely believe that's the future the way everything is moving and there we're going to get rid of the hardware as well as the software. We have the white space networking project called

KNOWS. We built out one of the first white space networks, which is now operational under Ed McCampus. With us is his blessing by getting their license, etc., and we regularly support a lot of conferences and workshops and academic summits around this area, so the point being that we've sort of been thinking about this a lot and have quite a bit (inaudible).

Let me put on my researcher hat and tell you what I think from my own experiences -- I'm going to different, you know, conferences as well as talking to researchers as well as publishing papers one of the ways it pinpoints. So, I think we -- in research we lack -- such as lack data, and data is really important. Let me give you an idea of hat I mean by that. There was a feel of cooperative caching where people thought about caching being very, very good for networking, and it turned out that this one person who now actually works in my group, the (inaudible) thesis and demonstrated that caching after a certain limit doesn't actually help. So, building these large caches doesn't actually help at all, and he did it to all these -- by looking at the data from different organizations. And that piece of work pretty much killed the field in the sense that a lot of the dollars were no longer spent, and we didn't go down the wrong path. So, in order to do the right kind of research, we definitely need a lot of data.

The other thing we need is access to network stacks. Now, network stacks access -- for example, in case of these production networks that you have in 3G and 4G -- we don't have access to those network stacks, and we don't have so we can't be creative. One of the greatest things that WiFi did for us or unlicensed networking did for us was made these stacks available to the research community, and from there they were able to do things like OFDM and MIMO, and they were able to build these things into the system and they took off from there.

Similarly, this lack of access to the (inaudible) things, like network crowding, routers, etc. Similarly, other pinpoints include very limited testbeds. The experimental work is just not there. There's a lot of good work going on, but the point solutions that come out of academia are not completely transferable into real products, so -- because there's not enough of that going on. And there's a heavy dependence on hardware for industry. I mean, I think about what has happened in the software-defined radios or cognitive radios, and this is really limited by what has been produced by industry, and I think we need to think about how do we make sure that the academics are not encumbered by that.

The other issue is that how many jobs -- the networking jobs or the number of jobs that are now available in networking are also going down. That is also something that we have to be worried about going up in the future, because a lot of the -- our emphasis now is on bicomputing, bioinfomatics, and things like that, which are really important but networking community has done a very poor job of actually marketing them so doesn't say how much impact they've had on the community.

And then there are fewer and fewer Grand Vision projects that I see off (inaudible) the CMU project, the 100x100, which was a hundred million homes, a hundred megabits per second, with one instance of a good project that was funded but not happening anymore.

Now, moving forward, so what recommendations in the little time that I have. So, I think that federal agencies and FCC have to work together. I see this that researchers are not necessarily very cognizant of the policy implications on their work and vice versa, and I think so. There has to be better synergy between the two organizations.

Now, we need to sort of foster the entry of new broadband providers. One of the things I talked about earlier was lack of data. I think with more competition, there's going to be more innovation, and if we can allow the researchers to experiment on some of these production networks, then that is great, and during that monitoring and trace gathering, etc., and stack, etc.

We must finalize these rules white spaces. This is an amazing area for research, because it allows us to do opportunistic networking, and it allows us to sort of think about, you know, how we can take and better use the spectrum, which is essential for all the applications we are building.

And along the same line, I have a suggestion, which is that we should probably consider the FCC working with (inaudible) should probably consider creating a national spectrum telescope. Imagine a sort of a table lookup or something which tells you all the spectrum uses in the entire country at all given times. And this would then help sort of said policies and rules around what more spectrum to go after because we're suddenly are going to need it. No matter how much innovations we do, (inaudible) limits is going to kill us otherwise.

The other recommendations -- and quickly because of lack of time here -- is essentially we must encourage researchers to build reusable platforms, and that should sort of take care of not having to worry so much about industry with the hardware, etc. The MSR Mesh Kit was one example of that, and so our platform (inaudible) is another example of that. Rice has a thing called WRAP. (Inaudible) research in DSA and cognitive wireless networking. This cognitive wireless networking can be disruptive technology, and this can really completely push us into the leading position over all the other European colleagues and Asian colleagues as well.

Another idea is to consider a national -- a network (inaudible) data repository. The government, for example, is a large organization, has got lots of networks, lots of data. You can consider anomyzing some of the data and providing it to the networking researchers who can then look at it. To give you an example, if you think of the data and you see, you know, whether it's -- what -- in a recent conference one of the researchers said the dominant data is SP2P. The other guy got up and said no, it's SR2P. And they were debating that, and they were both going after different sources of data, but the interesting part is that the network design truly depends on what sort of way the network has been used, and so if you want to do a real good broadband connectivity you must think about that.

And then keep in mind that good research takes time, and so we have to somehow figure out ways to do longer-term more Blue Skin type of research, and it's not going to just happen in the next year or two years but we have to put all the things in place so that we have that.

And at that point, I'll hand it over to you.

MR. SICKER: Thank you. David Borth.

MR. BORTH: Thank you very much. I'd like to first begin by thanking the FCC and Doug for organizing this session, as well as Meredith for participating in this session.

I'm going to talk about a number of

topics this afternoon. Some of these will reinforce what Victor just said about long-term research, but I'm going to begin with my first slide, which is some trends that we've observed over a period of time, and these are not predictive of what the future will be, but they do indicate what we've observed and what possibly you can make of this. But I want to also use these slides to emphasize what Motorola Research's role has been in developing these technologies. And the answer is it's taken a long time to get here.

So, the first slide it talks about 25 years of wire band with trends, and this shows the rise in data rates over wired and now cable networks and fiber networks starting in 1982 and going from, really, dial-up 300 bit- per-second modems up to -- we got to 56 kilobit-per-second modems and stopped there with that technology, then moved into the era of cable and DSL and XDSL modems, and we gradually brought up the speed considerably there. If you follow the trend line, it says by 2016 we should have around 288 megabits per second. That's just based on the trend line. There's no real data to support that we will actually get there. But the next slide will show that there's other ways of approaching this through fiber networks.

The point to be made here is there's lots of technology that we're involved in creating this type of future for the wired network, and it involves things like integrated circuit design, combinations of coding and modulation, and then full-sale deployment of these types of systems at scale, because that was one of the issues that was raised earlier this morning. It's important that these systems are brought out in these time frames.

Let me go to a next example, which is one for a wireless broadband, so wireless broadband has observed a similar sort of trend. It's gone from fairly low data rates when wireless began in 1983 with the AM System in the U.S. Essentially, there was no data network at that point in time. And then it progressed from that point on. And what this shows is an overlay of cable plus wireless networks. The cable line is on the bottom, and it -- what it tends to indicate is -- and in fact cable -- excuse me, wireless networks have grown at fairly high data rates also. In the upper right-hand corner are some penetration rates of wireless broad and wire line broadband. In 2006, we figured that about 2 percent of the population was a team of broadband through the wireless. We think that will grow to about 18 percent by 2010. So, this is a significant growth potential in the wireless world. We see this as anecdotal information today that's presented to Motorola from various points in time where users come up to us and indicate they actually use wireless methods to access broadband.

Again, a significant amount of effort was required to get here. And I want to talk about that just in a moment.

So, I'm going to look particularly at Motorola's fourth-generation wireless journey.

It's a long path. It starts in 1995 when we first started asking the question what is 4G? And in 1995, 3G was being discussed. It didn't come out until several years later. But we started looking at what were the elements necessary to bring fourth-generation wireless to the public. And there was lots of technology required. But there was also spectrum required in this process, and even back then we were looking at the MMDS band at 2.5 gigahertz, which was a convoluted way of getting there, because it was mixed up with the ITFS spectrum at the time.

So, we started looking at 2.5 gigahertz as a way of getting there. We were looking at propagation along the way. We also developed all sorts of new modulation methods or FDM technology, new coding methods -- well, if turbo coding and low-density parity-check coding methods -- and putting it all together into a system.

If you follow this timeline shown at the bottom of the chart here, in 2000 and 2001 we're conducting field tests, and finally around 2005

802.16e, which has become WiMAX, came out and Motorola announced products in 2007 for WiMAX in 2009 for LTE. This is a long time frame. It's some 14 years required to progress along this timeline, so the issue that Victor just brought up is -- really long- range research is required to get to some of these end goals.

Same sort of thing happened in the -- our efforts in Passive Optical Networking. We started the efforts in PON, or Passive Optical Networks, in 2002 and 2001, looking at deployments for providing high-speed data to the home user. For those that aren't familiar with the technology, GPON provides 2.4 gigabits per second, a dial link path at 1.2 gigabits per second, the uplink path to multiple users, so the average user gets around the order of 75 megabits dial link and 37.5 megabits per second uplink. Again, there was a long effort required. Along the way we had some initial deployments as terms of trial networks combined with a number of our partners.

Verizon announced the FIOS system

deployments in 2005, and by 2009 there's some million users are using optical network terminals in their homes today -- again, not as long as 14 years but still a considerable amount of time prior to past year.

So, what's the issue here? Well, the issue that we have is that there are some troubling trends in the technology and innovation with the U.S., and this is not all doomsday, but it is an indication of what has happened, especially in recent times.

This chart comes from the Council on Competitiveness, and it actually appeared in a report from the Brookings Institution from last year, and it indicates some trends that observed in terms of United States share of worldwide innovation indicators. On the far left side it shows two bar charts. Both of these are based on data taken from 1986 -- or '85 in some cases -- and 2002 or 2003, depending on when the data actually became available, and it shows that -- and in many cases we've fallen behind in terms of domestic R&D investment, in terms of new U.S. Patents, scientific publications, scientific researchers, and so on.

I want to underscore one of the items that are shown here, and that's Bachelor's Degree in Science and Engineering, because this is one that was brought home last month at the National Academy of Engineering meeting. At that point in time, Dr. Charles Vest underscored the trends that are observed here. In the early 1980s -- Dr. Charles Vest is the president of NAE right now -- he observed that in the 1980s Japan, China, and the U.S. all had about the same number of degrees that are granted for Bachelor-level degrees in engineering -- about 75,000 altogether. In 2002, the U.S. production of degrees drops to about 60,000 from 75,000 per year while Japan grew to 100,000 and China grew to about 250,000 per year. In terms of percentages, that meant that there were about 20 percent of the first degrees were granted in the engineering fields in Asia, about 12 percent in Europe, and only 41/2 percent in the

U.S. So, this is perhaps a troubling trend that may, as we've found over the years, attract somewhat the growth of engineering fields, and it may come back again, but it is an indication of what has happened recently.

Another point that I'd like to make is that investment in fundamental and applied research is critical. This, again, comes from the Brookings report that I just referenced, and the reference is given at the bottom of the chart. It shows that private finance of R&D is shifting away from risky or early-stage activities. This shows the change from 1991 to 2003 and shows that basic research fell about 2 percent; applied research fell about 4 percent; whereas development actually increased from the baseline up to about 71/2 percent over this period of time. It tends to indicate that research is going away from the riskier elements and taking more of a line into the development aspects, so shifting from R&D to small r/big D type of things.

Now just to also bring this out, the

National Academies' broad report that came out in 2006 -- it was co- edited by Bob Lucky and John Eisenberg. Bob Lucky was formerly head of the TAC for the FCC, so I bring this forward. In this report, they indicate that long-term fundamental research aimed at breakthroughs is declined in favor of short-term, incremental, and evolutionary projects who purpose is enable improvements in existing products and services.

So, that concludes my formal remarks. I'm open to questions during the Q&A. I can comment on a number of areas also with respect to our own involvement in the European framework programs that we've participated in for the last 10 years.

MR. SICKER: I particularly would like to hear a little bit more about that latter, but we'll wait.

MR. BORTH: Sure.

MR. SICKER: Adam Drobot?

MR. DROBOT: Okay, so first of all let me thank you for the invitation to speak today.

What I'd like to do is say a few things in my role at Telecordia, which is a research organization that has its heritage back -- it goes back to the Bell Labs system and then I think in closing say a little bit about my role at TIA, where I run the Research Division that in fact, you know, took a look at this issue of the last three to four years. I think we have put together a number of white papers, and I think those are available essentially, okay?

So, to go through the prepared remarks, what I'd like to do is first of all set the stage -- why I think what we're doing here today is important; talk about some of the areas of research; talk about something I've labeled refacturing, because I think if I look towards the future, I think the way broadband is being used will change very dramatically essentially -- I think it will change our lives in some fundamental ways; and then do a little bit of a summary essentially.

So, let me sort of start by setting the

stage, and, yeah, the big picture, not all inclusive but (inaudible) chunk things. I would say if you are running a large organization, whether it's a Google, whether it's a service provider, whether it's a cable operator, wireless operator, you can look at your capital deployment, and that involves the goods that actually got into the field; the labor that goes along with them; then the cost of operations, the applications and services that you build on top of that and then sort of the hard work in the marketplace, and how do you get adoption penetration; and, as I'll talk later about refacturing, how do you get some deep usage of all of this infrastructure that really makes a difference go the nation.

The second point I'd like to make as part of this big picture is that the issues are complex and, you know, sometimes the processes we try to go through, try to reduce things to a very simple sound bite -- to be a little inflammatory, I might say neutrality is one of those sound bites. It means a lot of things to many different people. But, you know, I have a feeling this dialog really has to look at the basics. Again that is what's coming from technology? What are we doing new in business models? What are the demographics of this country in the world? Is our geography that different from the rest of the world? What clusters have we actually formed around telecommunications in broadband? Are those clusters whole, or do they need some repair at this point? How do we deal with legacy? I'd say the accelerating time scales in which technology is deployed, the investment climate -- you know, all of those things matter and, you know, somehow have to be dealt with holistically.

I'd say the next part of the big picture is really performance, okay, and performance matters, okay? Whether it's speed, the amount of computing I can do, the amount of storage I can have, the quality of my interfaces, the software that I'm using, the experience that I have as a user -- all of this matters, I would say, in terms of two things, you know, sort of the raw numbers

-- but it's not a raw number that is going to stay the same; it's going to change over time, and it's very important to take that into consideration. And the next thing is not only is it going to change over time, we have an expectation that the basic ingredients with which we grow the broadband world, okay, will actually continue to come down in cost. So, there is, you know, for a user, how much of a bang for the buck am I actually going to get out of this? How many new things can I do? How can I get them at a reasonable cost essentially? And I think the underlying research and a lot that we do, well, again, has a lot to do with that.

Let's say the next item is that the economic impact we've seen from information technology over the last two decades has been really profound. It's sort of -- you know, I would say something like 40 percent of the improvement in productivity is attributed to IT. As a technologist, I would say when you start looking at broadband what it means for everything from consumers, small and medium businesses enterprises and then the addition of mobility to that, I think we will actually see the next revolution in what productivity actually means, okay? And so for -- you know, as a motivation, again for the nation, you know, leading in this area is not just inventing those things, but it's being fast in deploying them, making sure they're part of our economic systems and that we are part of that next revolution productivity. Again, the fast pace of change, accommodation of our business and operational processes, the regulatory approach so that we can in fact accommodate the future has to be part of this, and, you know, long-term research and sort of looking at what the options are I believe is an important part of it.

Lastly, we have an explosion of options. You know, the number of new things, new combinations of what you can do better and faster are sort of incredible. Again, how do you harness all of that, and, you know, sort of the goals of leadership and high value, not just for this nation but for the world.

So, I think in terms of, you know, setting the stage is if we really don't think of broadband as something that we start deploying and continue to improve over time, then we will have missed the mark essentially, okay?

So, let me take a look at, you know, the research part of this and its importance.

And the first thing I'd like to do is deal with physical systems. I think Norm Augustine wrote a report called Above the Rising Storm essentially. Came out of the National Academy, and what he pointed out is that the investment in physical systems is considerably down over time essentially, okay? And, you know, if I look at one aspect of broadband -- the fiber world -- you know, what I hear from my friends in the service companies is that their core network traffic is still rising around 30 to 40 percent year over year per subscriber. When we've gone through an economic lull, you can sort of afford to get away with deploying what's on the shelf, okay. If we come out of the recession, which I expect we will, okay, then you start running into a real problem, because my belief is the man will rise sharply when that happens, okay? And if the cost of the goods that go into the core doesn't come down in cost, okay, and stay on its exponential curve, we actually will have a problem, okay? And, you know, that doesn't happen without investment. In this particular case I would say, with the bubble around 2000 or so, a lot of the companies that were in the marketplace I would say cut their basic research, their long-term investments, to a considerable degree, okay?

If you ask for a comparison with what is happening in Japan, what is happening in China and in Europe, okay, I would say those investments continued maybe at a somewhat slower pace but a much greater than what we do at this point in time, okay?

And that has a lot to do with leadership in the future essentially. If I were to look at wireless systems, okay, whether it is software-defined radios, cognitive radios, the use of MIMO technology, which shows the promise of extracting a lot more in terms of bits per hertz out of the spectrum -- I think in laboratory experiments people have achieved numbers over 20 bits per hertz as opposed to the 2 or 3 we find in commonly deployed systems today, okay? Those things become possible if the research is done, if the components are built. And these are really long-term issues, which today find it very hard to attract funding essentially, okay?

If I were to look outside the physical systems -- you know, we have software; we have operating systems. We have had many stabs at this, whether it's systems for routers, whether it's for general-purpose software and PCs, which is on the use end, I would say those are broke in today's world. We're patching them. It costs us a lot to do that. I don't see a concerted investment being made in those areas that we should be doing today essentially, okay?

Security, assurance, privacy, trust -- sort of as a clump, okay? All of those things are very related to each other. Again, lots of promise in that space, but the critical level of investment, okay, that'll actually translate into something where what we build as an infrastructure is usable again I would say begs for investment in today's world. The things that I see being done is rehashing of a lot of stuff, a lot of investment in product at this point, okay, but the flow of new investments and new ideas in those areas is very hard to come by essentially.

You know, we invented something called the internet IP protocol, and whether it was something we stumbled, there was a piece of magic that happens there, and that piece of magic -- you know, if you sort of parse it, you can get very technical about it, but there's one very simple aspect to it. It delivered so much that it became probably one of the longest lived protocols that we have in some sense, okay? And when you have long-lived protocols, okay, you can put investment on top of them. You can build things with them, okay? I don't believe that what we built was the last word in that area, okay? It's worthwhile having an investment, and there are many aspects, okay, of, you know IPV4, IPV6 in that succession, okay, that need to be done. An example of that would be how do I have a fabric underneath that with switching, okay, that promotes efficiency, allows you to do a lot of things that we can't do today and really works a lot better with mobility than even IPV6 does at that point, okay?

So, I can go through a whole list of these things.

Let me very quickly say you can add interoperability, managed services, all the -ilities from scalability to robustness, I would say all of those again begging for resources at this point in time, okay?

Now, if I look at refacturing, what I would say is before the national 4G networks, 3G networks, things of that sort, every application had its own, okay? Had its own network. Today, I think we can do things in common as systems and common networks, okay, and going in that direction, though, again would have a lot of value. Building core systems so you can support every application from health care to telematics to banking, finance, all of that, everything running of the core in a secure way, again lots of value.

So, I think I'm, if I notice, out of time at this point, okay? I think what I'll -- was going to cover is how can one do this institutionally, but that's for later.

MR. SICKER: Can I ask you to maybe when we have questions --

MR. DROBOT: Yep.

MR. SICKER: -- bring that in and at least put it on the record --

MR. DROBOT: Will do.

MR. SICKER: -- so that we will have your notes.

MR. DROBOT: Thank you.

MR. SICKER: Thank you. Unfortunately,

Dr. Farber can't join us today.

His wife is ill, and he's in the middle of transporting her from hospital to another, so our best go to Dave and to Gigi.

We'll now turn to Dick Green from Cablelabs and the University of Colorado.

MR. GREEN: Thank you very much, Doug. And thank you, Commissioner Baker for putting this panel together. This is one of the topics of course near and dear to my heart.

For 21 years I was president and CEO of Cablelabs, the cable industry's research and development consortium, and before that I was the CTO for PBS and earlier served as the director of the CBS advanced TV laboratory. For the majority of my career I've been involved in research and development activities. Those activities have included experience in government, academic and industry laboratories. My comments today are based on my experience in several industry laboratories and do not necessarily represent the specific views of any one of those industries that

I've been a part of. And having good fortune to participate in several major developments in progressive technology, these include the development of digital and high-definition television, the telcom revolution, which resulted in facilities-based competition in data and telephone service, as well as the emergence of the internet.

However, like many of my colleagues, I've been concerned about the general decline of research activities in the United States. Also, like many of my colleagues, I'm concerned about the level of sponsored research in advanced internet topics.

I want to use my time today to offer two possible suggestions on indirect measures the government can take to influence innovation and to create more incentive for industry to venture into new research. I hope that these suggestions can be useful as recommendations to Congress, and I hope that these ideas can enable improved models for the U.S. to assert leadership in broadband networking research and development.

Neither of these concepts is new. They've been around for a long time and are presently being implemented in limited ways. There's ample evidence that they work. What I'm suggesting is that these ideas could be expanded and utilized in broader ways. The concepts may serve as proven policy which could be carried out on an expanded scale.

For the last 21 years, I've had the good fortune to be part of a research laboratory that has had some success in advancing the capability of the internet, specifically, the delivery of data over the last mile into people's homes. Although the achievements of the lab could be attributed to numerous factors, an important precursor was the National Cooperative Research Act of 1984.

Before I proceed with that thought, I hope you'll me a moment to outline the contribution that I believe has resulted from the work of this laboratory. The specific example that I'm referring to is the development of the data transmission over a cable system.

The basic technology building blocks for cable internet access service are contained in the Data Over Cable Serviced Interface Specification -- DOCSIS. DOCSIS is a unified standard developed by Cablelabs and its partners beginning in 1995. The most version is DOCSIS 3.0, which was designed to significantly increase transmission speeds. Increased speeds are needed to move growing consumer demand for all kinds of applications, including internet video, teleconferencing, and new applications in health, education, and other fields. DOCSIS 3 can support cutting edge speeds today and even faster speeds in the future. Currently, for instance, DOCSIS 3 delivers up to 160 megabits downstream. Upstream channels deliver a maximum of 120 megabits. New modems and head-ins are now being developed for commercial release next year in 2010 and (inaudible) maximum download throughput at more than 300 megabits.

I hope you will agree that this

development has been important in improving last-mile capability as well increasing internet access to a large part of the population. The capability of delivering data over cabled networks was a development of a collaborative, nonprofit research consortium. As I have mentioned, Cablelabs was incorporated under the National Cooperative Research Act. The Act reduces the potential antitrust liabilities for various types of joint ventures involved in research and development. It has the effect of encouraging the formation and operation of this kind of joint venture.

The NCRA was modified in 1993 and again in 2004 to include standards activities and currently has two major technology policy goals: first, to increase the number of joint R&D and production ventures entered into by U.S. Firms and, second, to increase competitiveness of the United States in key technology areas of research and development and production.

Collaborative R&D agreements have often

been considered an important policy tool to stimulate innovation. They can reduce costs by achieving economies of scale, eliminating duplicate R&D efforts, or by encouraging synergies.

In the case of the cable modem, it was possible to develop a common specification that provided a uniform approach to transmission of data over cable systems. The specification was submitted to the ITU and was approved as a worldwide standard. This created worldwide scale economics for manufacturers and cable operator alike. It also led to private investment in the nation's infrastructure, and it increased competition.

To bring services to Americans using the DOCSIS technology, cable companies have invested over $145 billion in private capital since 1996. The investment build fiber- rich, two-way interactive networks throughout the country. Although cable led the way into broadband in America, there are now multiple broadband platforms as a result of hundreds of billions of dollars of investment by competing providers like telephone companies and wireless and satellite providers. Content and application providers in turn have been able to utilize these platforms to create multibillion dollar American businesses. I believe that this is an example that may illustrate how from one simple collaborative idea it involved into creating a significant social good. So, I would submit for the purposes of this discussion that congressional encouraging collaborative research is certainly a very useful tool to foster development and create efforts that can strengthen U.S. Leadership and internet technology. I believe that the Cooperative Research Act have shown that these advantages are real and increased emphasis on collaborative research is a worthwhile policy objective.

I'd like to offer a second suggestion based on a concept exemplified by a program in the Department of Defense. I'm thinking about the IR&D program. IR&D is shorthand for Independent

Research and Development. It's a way for the government to stimulate R&D by offsetting some of the costs of industry research projects. In this model, IR&D costs are incurred by a company on its own in conducting basic research, applied research and development. In order to be charged to a government contract, IR&D must be a potential interest to the government and must fall within certain defined areas. Contractors can recover a significant percentage of their approved research costs as indirect expenses under the government contract. Therefore, the government pays its share of the company's IR&D and the price it pays for products and services. This augments the company's expenditures for R&D, and it allows additional company spending to explore advanced concepts and create new ideas. It permits the company to pursue technology advances in areas where the firm's capabilities are the strongest. IR&D benefits the country by providing new products and technologies and contributes to industry competitiveness and a stronger infrastructure. I'm suggesting that whenever the government purchases information technology, service, or equipment it may be useful to attach an IR&D incentive program to these contracts. Companies with expertise and capability in the IT space could then pursue independent research on topics deemed important to network development. The cost of such research could then be indirect expense to contracts. This mechanisms would encourage private company spending to explore advance concepts and pursue IT technology projects that are inherently high risk.

Briefly, two other DoD efforts are worth mentioning. The federal funding in these cases is provided directly to the research entity. The Small Business Research Program, SBIR, provides up to $850,000 in early- stage R&D funding directly to small technology companies or individual entrepreneurs.

The second program, the Small Business Technology Program, STTR, provides up to $650,000 in early R&D funding directly to small companies working cooperatively at Universities and other institutions.

The funding levels that I've quoted here are just from the DoD program. Ten other federal research agencies also participate in these small business research efforts.

To summarize, the principal suggestions are (1) an increased emphasis on support for collaborative research in industry and (2) the use of government contracting mechanisms to support independent IT research. I hope that these two suggestions will assist the Commission in formulating and implementing the national broadband plan. Those of us involved in the R&D community look forward to working with you to bring improved broadband to more Americans so that Americans can be informed, connected, and benefited by everything broadband has to offer.

MR. SICKER: Thank you, Dick.

MR. GREEN: Thanks.

MR. SICKER: And we'll continue on. I did want to mention I think we're going to have some interesting additions here that we haven't really talked about in the earlier part of the panel. I mean, we -- you know, the earlier session was really focused on the academic interests, and I think really understanding how industry funds research is going to be an important part of this chapter and something that I personally am going to need a lot of input from you all to be able to write properly, so I again will put out the notion that please let me come and bother you over the next few weeks for additional input.

Okay, Mark Levine.

MR. LEVINE: Commissioner, thank you for your leadership here today.

Doug, Stagg, and Rashmi, I look forward to working with you as you pull the report together.

Thank you for the opportunity to speak to you this afternoon. Like the others, I laud the goals and objectives of the task force and the open spirit in which you are approaching this whole process.

My name is Mark Levine, and I'm a partner with Core Capital Partners. We're a venture capital firm located in Washington, D.C. We have $350 million under management, and in the past 10 years we've invested in 45 companies, and I dare say that there isn't one of our companies that would be existing today if we were in a 1200 baud world. So, that's one thing we can just put right on the table (inaudible) happen.

But, most importantly, we're very bullish about the future of the mobile internet. Our investment pieces are built around the future of the mobile internet, and the investment opportunities we see in the next 10 years as a result of the growth and adoption of the mobile internet by consumers and enterprises. And we feel that federal broadband policy will be one of the catalysts for economic growth in the entire United States. Fourteen percent of the new jobs in the economy are created by startup businesses, and VC investment plays a major role in not only a majority of those jobs but also the highest scale jobs, the jobs they create more, and that's one of the ways we're leveraging below graduation rate in engineers today hopefully with our dollars to put it out there in airways.

The state of the broadband industry generally affects the engine of VC-backed economic growth, and intuitively this seems applicable to biotech and other areas of investment. As for IP, the ability to move data is integral to all areas of innovation. We believe that broadband IP, however, represents the greatest growth opportunity because by its very nature it is forever creating new investment opportunities and thus driving economic growth. This IP growth is driven by the fact that IP is iterative and compounding. It is iterative in that it builds refinements on the previous application, and it is compounding in that one application leads to another. Yet for this iterative and compounding capability to exist is a derivative of the ability to move the data in the first place, the area over which it can be moved, and the speed of the movement.

We invest in new companies that move iterative and compounding applications around. While some of this can be done inside a computing center, the broader economic success is going to come from the broader application of the new idea, and that requires broadband distribution.

So, I want to give you a very short overview of some of our investments -- I won't go through all 45 -- that operate on both policies we have today and the landscape that will be affected by the work of this task force.

Swat drive is one of our investments, and it uses Cloud Storage Computing to store over wireless -- for wireless carriers and their customers who want remote access to their data backup, their video, their music, their picture, and their data files. In one of our investments in our first fund, if you just understand what the effect of policy that starts here in this building -- number portability alone was responsible for driving over $300 million in sales by Infonic as consumer -- and it was orderly growth, because it was -- as consumers came off their contracts, the ability to switch in turn -- that drove $300 million worth of revenue and probably 400 jobs in a venture-backed startup.

Trusted (inaudible) is one of our investments, and they're a leader in securing wireless devices.

One of our investments in our second fund is Round Box, and they're a leading provider of one too many broadcast services for wireless networks. Their multi- bearer technology includes exclusive availability of the TV guide -- electronic service guide. And Round Box technology is specially suited for wireless broadcast of content across the ATSC and 700 MHz spectrum, and I know that's a big area of looking at where the broadcasters will come out with that.

ULI -- Update Logic -- and we've got a partnership with Cablelabs that's been very, very successful, and it's one of the first times you brought in an outside party like ULI. They provide over the air and through the cable system updates to high definition TVs, which are nothing more than computers and how else can we access them for software patches and for updates. In fact, one leading consumer electronic manufacturer said nah, we don't need to deliver upgrades, and within months of launching a product found they had 10,000 TVs that wouldn't turn off, so they needed to get to them and now all six major consumer electronic manufacturers are working with ULI to reach their HD TVs.

Inlet is one of our investments, and they have a HD encoding and decoding technology to stream video over the air and over the internet from any source to any device, and it's the first of its kind.

Twisted Pair provides a crucial element in our national security and homeland preparedness. They have an IP bridge that allows radios operating on different frequencies to communicate with each other in a controlled manner and think about 9/11 and what happened when police and fire could not communicate. Twisted Pair systems are aimed at that problem.

Bridgewave is a point-to-point wireless broadband technology that's being used by enterprises, campuses, and service providers to deal with the volume of bandwidth that's needed just to backhaul the increased data usage and the explosion of data across networks and also for the rapid deployment of new networks and extensions of existing services. And this is a small startup company that just received a $14 million contract last year from a major waterway wireless provider so that they can use the technology to enter new markets.

So, this is just a short list of companies in our portfolio affected by our national broadband policy. There are thousands more companies in portfolios at other VC firms and hundreds of thousands of jobs created by our investments. We are deeply invested in the future of broadband and we feel the United States is on the cusp of an explosion of new services and applications that will not only serve consumers but help our economy into a new area of competition, job creation, innovation, and world leadership. And unlike in other economic debates, this one is uniquely American. For example, in the health care debate, many multinational corporations are sitting on the sidelines, because their excuse is they're looking at the global landscape and they're looking at global competitiveness. But here in the United States, we're focused solely on how we make American industry more competitive. So, among the issues we'll be considering on the task force -- and we can now direct our concerns and comments to two prime issues, and that's spectrum and competitiveness.

Spectrum must be made available in a manner that fosters innovation and competitiveness for new services and application, and competition is the key as it is a bedrock component for investment and risk taking. The policies must have provide for consistent availability of high-quality, high-bandwidth spectrum. Network operators must be able to reach all potential subscribers in order to roll out new services and applications. As an example of this, rural hospitals and small business in rural areas need to have the same access to broadband as their urban counterparts before we can really take advantage of the potential in electronic health records, disease management technologies, and all the derivatives that come from that, and wireless business applications before they become ubiquitous.

And the need for spectrum is acute. In the past year alone, AT&T has seen a 5000 percent increase in the demand for data usage due to the iPhone, and imagine the widespread adoption of the mobile internet by Enterprise will have over the next 10 years. I'm sure a sure a similar effect.

In the area of public safety and communications, the task force should emphasize the use of technologies that promote spectrum sharing rather than setting aside valuable spectrum for single-purpose use, and in all areas of consideration, first consideration should be given to new technologies to help us maximize the use of available spectrum, including broadcast technologies that extend the reach of today's networks.

And I want to echo a comment you made on the SBIR, Dick. Many years ago -- in fact, in 1982 -- I was staff director of the subcommittee on Capital Hill where the SBIR bill originated, and it would be great if the recommendations brought some of the collaborative discussion that you brought up earlier and maybe the FCC could take a roll in working with other agencies -- primarily the Department of Defense -- in funding small businesses for spectrum sharing, spectrum extending technologies.

So, thank you very much.

MR. SICKER: Thank you. The whole issue of SBIR is quite interesting.

I've had some conversations with a

number of different funding agencies, and the great success early on that it met with kind of allowed it to continue kind of unchanged for years. Now the question is, is SBIR an effective funding tool. And there's a lot of people who aren't so sure about that, and probably it's worth visiting how that might change and evolve to meet the needs. So our next speaker is Marcus Weldon, CTO of Bell Labs.

MR. WELDON: Thank you very much. So I'm going to have unprepared comments. Seriously though, I have the corporate CTO role for Alcatel-Lucent, which makes me inclined to sell products. But then I have the Bell Labs role, which makes me disinclined to sell products. And so I'm going to let you talk from the Bell Labs side of my mouth, which will mean that I'm not going to push any particular agenda but try and talk openly about what Bell Labs is and has been.

Bell Labs keeps coming up as this -- of virtue and indeed it's a very virtuous place. I joined it in '95, which is an interesting period of time because I was actually AT&T in '95 for one year and '97 was lucent and '99 was the bubble and things have been different since then. But I've got a little bit of what I've seen there.

When I joined Bell Labs I joined from Harvard and it was the only place in the world I wanted to work. It was absolutely clear to me when I read the papers coming out of Bell Labs this was the only place that was worthy of doing the research I wanted to do. I had an inflated self opinion at the time because -- but it clearly was -- and when I got there it really was that place.

So '95; it was a remarkable place full of incredibly bright people and an incredibly competitive intellectual culture. And just to calibrate you, in '95 there was still 150 people working in physical science, which meant basically physics. These people generally did not produce devices; they discovered phenomena that lead to devices.

And just to remind you of what those

things were, they were the transistor, the laser, cellular communications, the latest Nobel Prize in CCD, which was the seventh Nobel Prize for Bell Labs in the last 50 years; 11 Nobel Prize winners, quantum hall effect, fractional quantum hall effect.

All of these things happened in Bell Labs. It was a remarkable, remarkable place. So -- and that continued, by the way, through '97, '99, and the bubble. There was in fact an increase in the physics department and it still was a hot bed of places for new researchers to come.

Around '99, 2,000, when the bubble happened, of course there was a reassessment of all of that and as a result there was that diminution of fundamental research that everyone has been talking about. So though Bell Labs is still funded at a very high level, it's around about a percent of Alcatel-Lucent's funding, which is still on the high side for fundamental research.

It is more applied research than it used to be. That physics division now has been renamed, enabling physical technologies. But notice, physical technology is not physics. And so that is part of the transformation that has happened and it is a more applied place than it was even 10 years ago and even, I would argue, seven or eight years ago.

So why? Well fundamentally, the first thing to go when economic times are hard and clearly lucent in Alcatel- Lucent -- had some financial difficulties in terms of it's in an increasingly competitive marketplace. Why is that increasingly competitive? Well, to be frank, the Chinese vendors are very hard to compete with in terms of the cost points they set in the market and they increasingly are penetrating all markets, particularly in Asia but Europe and now, North America.

So that really does have an impact in the profitability of Western companies. And as a result of that, there's a -- in the funding level.

If your top line goes down in Europe, percent of the top line, then your revenue base that you fund corporate research with goes down. And so over time it made sense to move away from fundamental research and it frankly was because of market pressures and market share decreasing.

So that's what happened. And as a result Bell Labs now is a more applied place. And the way we approach innovation is by funding small internal ventures so we actually borrow from the venture model, to have Alcatel- Lucent ventures, which come out of Bell Labs, as well as larger what we call grand challenges, which are where we really take a bet on something. So it's a different culture now.

Grand challenges and venture, internal ventures are the ways that Bell Labs actually puts a bigger bet on things in order to make better use of that small chunk of money. So -- so that's where I came from.

It's still an incredibly innovative place. But the mention was made of how do we do

Bell Labs 2.0. I think we need to look at many of the comments made around how an increasingly competitive marketplace, where the revenue base is diminishing, we can still do that sort or level of fundamental breakthrough research, competing with the Asia Pac companies who are doing it with a fraction of the -- count cost. So that -- makes it very difficult to sustain all of the innovation that we used to be able to sustain given that our -- count costs are necessarily high.

So what could we do? I agree with the proposal that having collaborative research enterprises across companies is certainly one way in which this process can be reversed somewhat.

So if you're looking at the idea of that 100, 150 researchers in physics that used to be in Bell Labs, there's no reason why that can't be reconstituted as a collection of grand challenge researchers working on the big topics of importance to the U.S. And so that I think is a very important recommendation.

I think also allowing for innovative

differentiating technologies in the U.S. marketplace. I'm not arguing for protectionism but I'm arguing for technologies that allow innovation. So I do agree that one troubling area with the net neutrality regulation is in a sense -- arguing for the lowest common denominator platform, as opposed to a truly innovative platform that allows differentiated services -- maximum degree of innovation.

Our argument is allow for a platform that has a maximum degree of innovation. The innovation changes its place, and position, and time, and space and so you need to support maximally differentiated innovative platforms, some of which will support high speed internet type connectivity as we know it today and some of it will support what we call the manage services aspect.

When we believe that at least having those sorts of innovative platforms allows U.S. companies to have a head start perhaps in the technological realm and compete more favorably perhaps against the local -- who frankly are impacting our ability to then fund R&D.

So a couple of things; innovative platform support, large scale collaborative projects are good ideas to keep the U.S. more competitive in the marketplace. Specifically, and I'll make some comments about technologies where I do think there should be investment, I think if you set the bar at 100 megabits per second per subscriber, which is a worthy goal and in fact one has already been met by European companies. In France, for example, free France Telecom both offer 100 megabit per second nominal rate to subscribers and yet the U.S. is far behind that.

So if the U.S. wants to lead, it has to set a bar of 100 megabits per second to the population. That's a very aggressive goal requiring many different technologies and I think -- we had grand challenges projects around those sorts of goals. So for example, how to do it over copper, how to do it over wireless technologies whether it be fixed or wireless with mobility, and how to do -- technologies in a way that is both innovative and lowest total cost of ownership.

So if you have grand challenge projects around those ideas, then for example, out of that will come the necessary evolution and DSL 5 technology, cable modem technology, perhaps its spectrum or its modulation schemes for cable modems.

Out of that will come network MIMO, for example, and wireless technology or enhanced self optimizing networks and small cell technologies that will all come out of that, as well as low cost WDM optics, low cost -- optics will all come out of those initiatives to make those deployments more and more affordable.

Mobil home networking technologies will also be one of the other offshoots of that. So -- so I think if we set a large grand challenge goal around how to get 100 megabits per second connectivity to every subscriber, which is essentially just matching some of the European countries, maybe it's collaborative enterprises with support at a regulatory level for innovative approaches to allowing differentiated services, then I think we have the beginnings of a recovery that would rival the kind of -- that Bell Labs wondered of in terms of its ability to impact and change the U.S. economy and culture.

So overall I think that's essentially my summary remarks. I think that, you know, to recover Bell Labs we need to reinvest at a federal funding level, as well as, to encourage grand challenge type collaborative projects. And we need to set a goal that is clearly around the Broadband yard stick and any application that drives those will naturally be a by-product of that.

So it'll lead to massive degrees of innovation and regrowth in the economy, and one that rivals Europe and Asia Pac and allows us to compete more effectively with low cost commodity type equipment vendors, which essentially drives companies like Alcatel-Lucent to minimize their investment in research when they compete in those markets. Okay; well, with that I'm out of time and I'll be happy to entertain any questions.

MR. SICKER: Thank you. Actually I will start with one. I'm trying to think how to even put this together. So given the -- and we all know that Bell Labs has lost a lot of its kind of hardcore basic research over the years, how long out does that take before it really hit in terms of the products and the discoveries and -- have you done studies like that? Can you share any of the --

MR. WELDON: Yeah; it's a very good question. I think it's decades, right. I mean we just won a Nobel Prize for CCD, which was 1956, '52. So we wouldn't -- it'll take 50 years to know that we haven't -- that we caused a real manifest corruption in that process, right, to be honest.

One thing pointed out by that Nobel Prize winner was Nobel Prize winners are being awarded further -- physics are going further back in time. So at some point there must -- they'll run out of people who are still alive and they'll come back and award Nobel Prizes in newer technologies.

But it really is a long time frame thing -- apart. You're talking about 30 to 50 years before you know whether you've actually, at least at the Nobel Prize recognition level, know whether you've impact your degree of innovation. Of course, I think technology availability in the marketplace of CCD was out there probably within 20 years and is now in every device from digital camera to a video camera, et cetera, projecting screen.

So I think it's a 20 year perhaps until you know the success the marketplace -- 50 year until you know if you're still leading Nobel Prize level innovation in your culture. So it's a long time.

MR. LEVINE: My industry complains about a lot --

MR. SICKER: Dick.

MR. GREEN: Well, I think it varies

quite a lot. I think the average time for ideas -- to get to the market was on the six, seven year category, but those were developments. They weren't basic research. You know, some developments like high definition took a very long time, almost 25 to 30 years, before it really became dominant in the marketplace. And so I think everyone's different. I, you know, was involved in research -- shortly out of graduate school and these were more DOD kinds of things and they were in the 15, 20 year period and they were laser kind of fundamental kinds of things. So it depends.

MS. SICKER: You can imagine my thought is that -- I know that the -- and others are looking at this but this is one of those eras where we, you know, we're going to start feeling it. We're going to -- really start understanding the impact of the loss of that kind of basic research funding.

MR. GREEN: So we're eating our --

MR. SICKER: Right.

MR. DROBOT: I think the point is while you may be wondering about what Nobel Prizes one gets, you know, from an organization that gave you DSL, ISDN, quite a few things, Sonet, ATM, things along those lines, I know longer see the kind of investments that made those things possible.

And if I were to make a case, corporate case, that years from now I'm going to have to -- bust your product, bring this to market, I just do not see the investment -- very possible. And I think most of my colleagues would share that across the table.

MR. WELDON: A comment on that. I mean I think we struggle but we do still do some of it. So there's a vestige of it and DSL technology for example, there is a thing called vectoring that has been five to seven years worth of investment that will probably come to market in two years. So we can still do 10 year level research but it really has to be probably tied to an existing technology, otherwise it falls into the --

MR. DROBOT: So it's an improvement.

MR. WELDON: -- it's an improvement but --

MR. DROBOT: --

MR. WELDON: -- it is a -- the other things we call grand challenges and those are whole new spaces. And it's difficult to find anything in between those, which is a lot that's falling through the cracks, I think, which are things that are really good ideas, but they're not quite big enough to be game changing. And then things that are evolutions of current technologies, we can bet on for five to ten years still because we see the --

MR. SICKER: I think Stagg had a comment.

MR. NEWMAN: Well, really trying to in fact get deeper into that, David gave a lot of the indications of where the inputs are down in number of PhD's produced, number of funding this and that -- are the canaries in the coal mine that says -- because the oxygen level is down, things are starting to die. You know, the one example, for example, Cisco I know just awarded best inventions around the world in the internet space and there were no Americans on that list. So maybe, particularly from Mark, who is a venture capitalist, Cisco is not on the panel, or Microsoft, or Motorola; are you now going overseas to find that next invention you're going to product -- or the next company you're going to fund or not?

MR. SICKER: I wonder if the canary in the coal mine is that the FCC is asking about research recommendations.

MS. BAKER: I just want to comment, echo, that I think that's a really good question that was sort of leading into the next, which is, you know, what color is your choice as to where to perform the research?

MR. DROBOT: So let me maybe give you the following answer. It's not a comfortable one. We did, from my organization, a survey of sponsored research programs around the world and compiled a little book that first of all has what

I call economic development agencies.

You know, 25 of them in Europe, a lot of them in Asia, Singapore, Malaysia, the Chinese have one, Taiwan has one, and each of those provides considerable subsidies for placing research activities in their territories. Again, if I look around the table, I know Microsoft, Motorola, Lucent, you know, Bell Lab, all take advantage of this today.

The second thing you have are national programs. There is the 843 Program in China and that's not just an incentive, it's direct funding for research activity. Seventh Framework Program in Europe, which has I mean an incredible amount of dollars associated with it; it's -- I think 10.3 billion euros.

You have similar programs in Korea, again, Taiwan, really around the world. And what you're starting to see is that of the 5,000 PhD's that we actually produce annually, nudges the 60,000 at the Bachelor's level. Around 70 percent of those are not U.S. citizens.

They are now going back home, they're practicing, and they can find better streams of funding under home territories than they can in the United States. And so to -- you know, we're -- I have a laboratory in Poland, I have one in Taiwan for exactly those reasons.

MR. SICKER: It has been my experience -- two of my --

MR. DROBOT: And one more -- you know, if you actually want to experiment with 4G networks and advance services, you've got to go to places where they're fully -- they also have the laboratories for doing that.

MR. SICKER: Victor.

MR. BAHL: Yeah; I have a couple of comments regarding Bell Labs 2.0 and -- time to market, et cetera. So I think, you know, MSR is much younger than Bell Labs and I used to work for DEC Research -- Research, no longer here. But so -- there were a lot of lessons that we learned in the process of seeing -- PARC go away -- go away.

And part of the lessons sort of resulted in the present culture research where we recognize, you know, we're not dumb people, we sort of recognize the fact that there's a love hate relationship between products and research. So when the going is good, you know, products -- this is great, you know, this is going to keep the future alive. When the going gets tough, it's mostly like what are we paying them for, right.

So I think the trick there is now to balance your portfolio of research projects. One of the first things that I sort of tell -- is don't get frustrated when your products don't get transferred because it does take, as you said, seven to eight years in our case, for something to go and that is -- that is -- something not very fundamental. You know, but something more fundamental will take much longer time.

Now regarding sort of the where do you go for research problems and things like -- or you know -- things that you asked for, it is true, we have labs in India, we have labs in Beijing, we have labs in Cambridge, and U.K. So we have sort of good experience with that.

It's not that important where the sort of the idea comes from. I do realize -- I mean I do, just going there and talk to individuals, I do think, on a positive note, that because research is one of those things that requires maturity, sense of maturity, sense of -- with the right investment, the talent is still here, I mean in terms of what the right problems are.

I was mentioning this earlier at lunch that it was in January I was in China and one of the workshops, which was called Teaching the Teachers, and they were all faculty members there and they were asking us questions about how do you select your research problems, what do you do. And we found that the money coming in to those people was far more than anything that the -- in the United States.

I mean I don't remember the numbers but it was -- three times. But the problems that they were going after were not as good. But the interesting thing is that they will -- they're also sharp and they will understand these things and then they will get on to the big problems. And you can see that in terms of how many papers are being -- by China or India into all of these conferences that we look at.

So it's not bad yet but definitely the governments across the world are doing all of the right things, putting all of the right structures in place, the policies in place, to make sure that they come out ahead. And if the U.S. sort of lags and doesn't -- doesn't sort of internalize that at this point and doesn't go back and reinvest in the research community, which made them great, then there is a serious problem about 10 years from now, 15 years from now, that we will not be where we are today.

MR. WELDON: I'd like to echo that. Bell Labs, again, was never really America in turns out, in terms of its composition. It was probably more than 50 percent European in terms of the researchers. The difference was they stayed; so that's a very important point and as you can tell, I'm not natively an American. But the idea was that we stayed because it was a fantastic environment, fantastic innovation engine in the U.S. culture.

But I think the point made that now the idea is to go back to native culture where the funding is perhaps more secured, where there are more opportunities outside of the VC system, which I think is still quite healthy in the U.S, is the problem.

So there's actually the brain drain that used to come here is now more like a U turn and that is a misuse of, I think, U.S. educational resources and actually also compounds the problem we have, that we're talking about. So I very much agree with that.

MR. SICKER: Rashmi.

MR. DOSHI: I guess just expanding on that point a little bit further too. I mean earlier in the previous panel we heard a couple of comments about how global research -- and sort of -- and others are being conducted. How does one balance? I mean is there sort of a -- that says this is U.S. only, this is worldwide, this is global -- collaboration -- across the board or does it even make sense to talk about it in terms of what research funding really should look like? I mean -- and one of the points I haven't heard anybody comment here is what's the role of academic universities in the work that you do in terms of funding your own research versus what you would fund at universities?

MR. BORTH: So I'd like to start on that one, the last point in particular. Since we've dropped the amount -- percentage of research that we used to do internally we rely much more heavily on academic research going forward. And we've always relied on academic research to provide basic research.

They were the ones that could go out there and work on the long range problems. But we rely much more heavily on that now. Now is the funding coming from the corporation to academia? I'd say given the current recession, no, that's not the case. So that is a problem going forward.

In regard to around the world do we find centers that are better environments to do our research; we've had centers in China, we've had centers in India, we've had centers in Europe. No, it's a diversity of thought that we've observed.

In some cases they pick up certain ideas faster than we would in the U.S. Just as an example, short -- service was available at GSM from day one and yet we didn't pick it up -- adopt in the U.S. until much, much later and that's just the case. You know, but it is a way of really looking at certain trends around the world. But overall, I haven't found any particular area except for the diversity of thought.

MR. DROBOT: You know, so there are, you know, when you start looking at research and sort of peel things back, I would say that there are things that have become orphaned. Okay; so let me take a look at a couple of those.

Okay; first of all, if I take a look at

building a network in India or building a network in China, it is very different from the way it is done in the United States. The craftwork, the labor, environmental factors, you know, how do I build stuff that doesn't require air conditioning enclosures, things of that sort of -- very different; okay.

What I don't see -- the way we have disaggregated the industry essentially is, how do you get, okay, the balance of investment in things other than just that physical network itself? Because a lot of the cost we put on our consumers, okay, that's only a small fracture. And when I look at something like fiber -- to the home, the electronic cost of goods is less than 50 percent; the rest of it is labor, okay.

Okay; who is investing and making sure that we have the best labor practices, okay, the best technology, in fact, for during deployment? So if you look at a national bill, you know, how do you slice 30, 40 percent of that national bill essentially, okay? And that's unique to us essentially at that point.

So there is a whole class of problems in this. Operationally; how do you run a network? Again, you know, that's a large chunk of -- cost to consumer. How do you do that a lot more efficiently? How do you do that in an automated way?

MR. WELDON: And a related point to that. I mean of course the other way you could approach -- I agree that the U.S. is a labor intensive place because of the shear physical distances -- of course if you could develop technologies that were uniquely applicable to the U.S. in terms of long reach technologies, are highly optimized to get to a higher capacity in longer reach scenarios. And that would clearly be a good national Broadband initiative that doesn't require you to solve the labor problem in terms of the cost of labor.

Back to the research thing; we have research -- Bell Labs is about to open a Bell Labs -- as well. We have China -- Singapore --

Antwerp, and New Jersey. The issue is really the -- we see actually in India and China; is the biggest problem we have because that is essentially leakage of learning and it's not malicious and it's not even a violation of any particular -- property. It's the -- headcount through those sites that is the most troubling because you don't get that permanence of knowledge, right.

And so it's great to have new research come in with a radical thought but with -- and a little bit of history behind that; the radical thought doesn't get developed optimally. So we see a lot of -- in China and India that makes it a little more problematic.

Of course all software -- tends to move quickly to India, which also means there is a divide between physical sciences and software that happens. And so that is a bit of a troubling trend as well if we're getting a lot of -- on the software side where that is -- substantial -- the innovation is now in addition to the physical technologies.

So those are some of the issues we wrestle with, I think, as opposed to the difficulty of running a global project which isn't the biggest problem; it's the fact that the headcount is fluxing and their skill sets are very different. So that's the issue we see.

MR. DOSHI: I mean where I was leading with the question was I saw some really good proposal from David -- in terms of creating national cooperative research associations and others. And then on the other hand, we have the issue that we don't have long term fundamental research. And then the third dimension is this globalization of research activity.

How do we reconcile and -- create? Is it national research cooperative -- pointed out a whole bunch of global subsidiaries to try and leverage that? I mean how do we get our arms around some of these funding issues, management issues, and --

MR. BAHL: Let me take a shot at the

earlier questions and I'll maybe go after -- more elaborative question that you asked. So from an earlier -- the question you asked about university funding was -- sort of what research labs do. And you know, we -- there was -- about a couple of months ago or maybe a month ago that I was at and something like this was brought up there too.

And so my remarks there were the following, which is I think that academic research should not be encumbered at all by any of the constraints that research labs may potentially feel. Now -- research is one of those very few rarities today that allows us to do fundamental research. We are not dictated at all by what the product groups are doing. You know, we don't ask anybody for what projects we're working on. You know, we do whatever we think we want to do.

However, because of the sensitivities that I brought up earlier, I noticed that at least in the area that I had, which is around systems and stuff, there is a bias towards selecting projects that sort of fall in the intersection of what would constitute research and get great publication on Tier One conferences and also impact product; perhaps not now, but in about six, seven years.

Now -- and I think there are people in the lab who are doing things which are much further out, which will never see the light of day. Hopefully they will, but may not see the light of day. But in academia, I think that they don't talk to customers, they don't have to worry about operational cost, managerial -- sort of thing. Although these are important subjects, but they should be unencumbered and should think freely.

And I don't see enough of that. I sit on the NSF panels, and I review a lot of proposals, and I go to these workshops, and listen to them, and I seem to remember work being done. And so there has to be some reeducation, there has to be certain, sort of a goal, for example, as was mentioned earlier.

I think presenting a really interesting

hard to reach challenge in the context of national priorities and then funding it with the right amount would allow academics to actually get into that business of trying to reach that. Industrial -- initially react. Let me give you a very simple example of this.

So we talk about cognitive radios, we talk about wide space network and things, and the way -- the policies are written in a way, you know, we think about it and we think about -- kinds of -- the radio device, for example.

Now in academia, there has been a lot of work around cooperative sensing. They are sort of like where -- sensing the environment and sort of deciding, you know, whether there is a primary user or not user. Now they don't worry about certifications, they don't worry about, you know, what the policies are, they just do their research. And the results show that if you do cooperative sensing, for example, you can actually lower the threshold that, you know, the policies have come up with. For example, you know, you're thinking about -- you can actually do it at much lower thresholds and still get the same level of acceptance in terms of positive things.

So the way we sort of separate research, I think is the stuff that we do very well at the research lab -- do well because they have a lot of data, we can do stuff, and we fund research that we are not necessarily doing. So that's sort of a separation between university and us.

And in terms of labs, for example, an India lab, when the researchers come there, we try to -- the process in a way that they try to do the research that is relevant to the countries that they are in. That's not necessarily always happens because we don't necessarily sort of tell people to do -- research, but the -- does happen.

I mean it was mentioned; the networks in India are very different from the networks in China for example. The white concept of white spaces is not there really in China the way they've sort of handled it. In India they're still coming around. So I think we tried to sort of break it up in that manner. But ideas can float from anywhere and we encourage that in a big way.

MR. WELDON: I think -- the decoupling I don't think I agree with. I think academics working with the research labs and industry, as a collaborative enterprise -- on different aspects of the problem. But as it appears, it is one of the optimal ways to innovate, right. So the industry should not encumber nor dictate what academics do but there should not be a separation between the two; they should be part of a continuous collaboration.

And so Bell Labs does a lot of that too; we have an office of the chief scientists that is specifically responsible for academic collaboration. We don't fund them but these are collaborative projects with the universities and I think that's actually a very powerful way in which we extend our thinking beyond some of the more encumbered thinking that perhaps we've become used to. So there still is a very important avenue for us and I think it should be encouraged more.

And I think in terms of grand challenge proposals or big visions, there's no reason why this can't be extended so that researchers from outside of the U.S. can actually participate in that project, I think, and that would be ideal so you don't have to partition it to be only America researchers working on that topic because I do think there are different skill sets that need to be brought to --

MR. SICKER: Adam.

MR. DROBOT: So you know, I actually sort of wrote down -- didn't get through -- a little too slow in the prepared remarks. Really wrote down some characteristics of what successful funding would be and how you would go about doing it. And you know, I started off by actually looking at Bob Lucky's report for the National Academy, which proposed that we actually set up a national telecommunications laboratory; okay.

Myself, I'm not a fan of that for lots of reasons and I took a hard look at it and, you know, here are some of the things that I think really do matter; okay. The first one is in selecting research to be done. I think having access to data and access to real problems is an essential.

If I look at the heart of my own organization, which used to be called Bell Corp. before it was Telecordia, and service the -- I think one of the reasons it was very successful in creating technologies that had wide usage, okay, that were promulgated and had wide acceptance, is really the trusted access to data and exposure to real problems. Okay; and you know, from -- Bob Frosh used to run NASA once upon a time. He had a very wise saying and that is if you want technology transfer, you want to do real things, you have to move people around.

One of the things we did as an institution is for a new researcher, one of the first things that they did is a tour of the operations in each of the Bell Lab -- in each of the Bell Operating Companies. So they got, you know, dirt under their fingernails to actually learn what was important and what wasn't essentially; okay.

And so when I start looking at, you know, what constitutes good research, okay, it's not that you can show functionally what something is, okay, that is the easy step. Okay; it is all of the other things that you have to build around it so it becomes practical over some period of time.

Okay; that doesn't happen without research. And so what I'd like to do is actually offer an example of where we as a nation do this very successfully, okay, and that is when I look at processors, no matter where they're manufactured in the world, okay, the real core of that technology still comes from the United States.

And every time we move, let's say, the rules from nanometers to 32, so you stay on the Moore's Law Curve, okay, there are something like 20 inventions, 20 curves, that have to move at the same time. Okay; it's not incremental. Each of those requires a breakthrough; invention of new materials, new properties.

The same is true of telecommunications. You know, we have a project today from DARPA to look at one terabit per second on fiber, okay. What you find is a lot of the components that you need in real systems don't exist today. I can't build the right buffer, I can't do my processing fast enough, somebody didn't invest in the gallium arsenide at the right speed; all of that has to come together. You do that list, it's 30 or 40 things that has to happen; okay.

So one of the things that's successful, again, in this game is the creation of roadmaps, okay, and understanding how we go up the improvement curve but in a very predictable way because I think a lot of magic happens when you do that. Okay; so I think that's important.

Let's see; the next thing is I don't think we need to build another laboratory starting from scratch essentially. A model of an organization that actually balances participation from government laboratories, the commercial sector, and academia, and it's really based on the best ideas and the best science is really what ought to be at the core of it essentially.

Okay; nothing happens unless it is well resourced. One of the things in Bell Labs, one of the things with Bell Corp., we had a monopoly that essentially generated the funding that made all of this research possible. Okay; that funding basis in here today. Whether they used the universal service fund, whether you have a research title in the next telecommunications act, okay, I would say some level of funding really focused on a Broadband mission for the nation is something that's worth while setting up.

Okay; but not to create another large bureaucracy -- essentially with people coming in rotation, more like the DARPA style possibly. Okay; I think that's where we would get the best bang for the buck, okay.

MR. SICKER: Broadband research tax.

MR. DROBOT: Not a tax.

MR. WELDON: To underscore that though, I mean if we go through the Nobel Prizes again, because they -- I mean maybe there's nothing more definitional than a Nobel Prize to say fundamental. But in fact, the transistor was solving a problem which was the vacuum tube, right. CCD was actually looking for a memory device. It was a thing called magnetic bubble memory if you remember that and that wasn't and that wasn't optimal --

MR. DROBOT: -- practical purpose.

MR. WELDON: -- and so out of that came a design for something that was a storage device and then they found it propagated charge in response to lighten -- CCD et cetera, et cetera, et cetera. Nearly all of these came about by solving a practical problem on the table, which is again, back to this point that when there's a problem on the table, put some really smart people who are free thinkers around that problem and you get tremendous developments --

SPEAKER: -- collaborative.

MR. WELDON: -- go ahead.

MR. GREEN: Yeah; I wanted to strongly support that. Having the right problem to solve is 90 percent of getting there and there's just countless examples of looking at a problem and finding a new solution. In the cable industry, the problem was that all of the lasers that were available were digital lasers and for linear transmission of multi- channel, you needed to have a linear laser, right. And so solving that problem was the breakthrough that made it possible to carry all of the channels on fiber, on a cable system. And the heart of that, I think is collaborative. If the academics are too isolated, if industry is too isolated, you tend to solve problems that are -- not necessary or are ill defined.

MR. SICKER: Right.

MR. GREEN: But collaboration tends to bring in all of the elements so that you can discover really interesting problems and really challenging problems and that's when you really get the horsepower from --

MR. SICKER: Right; shortens the focus. So let me --

MR. LEVINE: -- to that also. The government belongs in that equation as well too. Not necessarily as the driver, but as part of the virtuous cycle and kind of what we're talking about here is how --

MR. DROBOT: -- some unique resources.

MR. SICKER: And ability to do it.

SPEAKER: Yeah.

MR. SICKER: So Commissioner Baker pointed out that we are accepting questions via the web and I wanted to raise one or two of them that I've received. So Brett Glass who's a Broadband service provider up in Wyoming; I'm not going to read the whole question, I mean I'll paraphrase it. But this might have been a really good question for the earlier panel. There are some rule makings going on and Stagg kick me at a distance or the Commissioner if I'm --

MR. NEWMAN: -- talk about --

MR. SICKER: Yeah; if we can't talk about this. The question is, you know, there are things such as other -- let me put it more general. There are proceedings underway and there are a number of them. And what might these mean for academic and academic research if you have some kind of restriction that's a policy implication? I mean arguably there's a lot.

So HDTV, you said it took many, many years. Well how much of it was adoption, how much was it policy, how much was it, you know, cognitive radio? I mean we have cognitive radios; we're doing great things now. But how much of it's being limited by, you know, policy or blocked by policy?

MR. GREEN: Let me -- yeah. Let me answer the HDTV question because I really know about that one. The U.S. did take a leadership role. High definition -- there were proposals in Europe and Asia which were hybrids; they weren't all digital. But the U.S. chose an all digital approach and there was a blue ribbon committee that the FCC chaired and really drove the parameters of the standard. So we were very much in the leadership role in high definition, especially because we solved the digital problem. Motorola solved the digital problem actually.

And so the policy was very good I think. But what caused the delay was technical and it was because the difference between standard television and high definition didn't become apparent until the screen sizes became larger and there was really no good way to make a large screen for in home use except projection and, you know, very few people wanted to do that.

So it hinged on a new development, which was the large screen plasma displays and LCD displays. So even though we had the policy right, I think, and we as a nation were in the leadership role, the barrier of technology got in the way. Until we solved that, it didn't become a product. So sometimes, you know, you really have to wait for the technology elements to be there to make it

-- and it was --

MS. SICKER: So I kind of --

MR. GREEN: -- we didn't know that either. We kept saying why isn't this working.

MR. SICKER: I think Brett's question is the flip of that which is can policy get in the way of innovation. And you know, researchers want to innovate and do interesting things. We go down this path and we do inventions and what if the regulatory -- what if regulation cuts us off? What's a researcher to do about that?

MR. BAHL: So I don't know if policy comes in the way of research. I want to sort of make two points and then I'll make a third sort of a separate -- the first point is I think that only recently in the last like, I don't know, three or four years, five years, since we started -- I guess, that have researchers started to realize what FCC does impacts them.

I mean, you know, whether you believe it or not, I mean it's sort of a different world, you know, they kind of do their own thing and with whatever they've got and they don't necessarily recognize that the decisions that are being made here are going to have a long lasting effect on them. So -- but -- so this is good in some sense for you to even bring this workshop together and have academicians here, as well as, you know, stuff that NSF is doing because there's been some sort of knowledge -- in fluxing to the NSF and they are having these meetings and they are also getting them -- getting -- inviting -- John was there in the last -- meeting that we were there. So that's all goodness.

And now the other point that I sort of wanted to make was that I think that in terms of policy, you know, and think about spectrum, there's sort of debate always about licensed and unlicensed and we know the pros and cons of each and we sort of, you know, we come out in the way that both are very good.

But let me say something more good about when you decide to do something unlicensed beyond the economic value that people always talk about and be able to -- a paper or a report, whatever, that talks about the economic value of unlicensed spectrum.

But another value for unlicensed spectrum is that it allows, and I said this in my presentation, it allows the networking researchers to get into the stack because it's open. And anybody can build a device, and then anybody can build software for it, and anybody can experiment on it.

You can't do that with licensed networks. And so a lot of the innovations that have happened in the last decade or so, or maybe even more, happened because WiFi existed; not because WiFi allowed you to connect, but because I, as a researcher, could get into the stack, into the Windows stack, and actually could change and tweak the parameters.

We published a paper, for example, in -- that channelization is not a good idea. You can have varying -- lengths. Now, you know, you have to wonder like what the heck, it took so many years to sort of figure that out; no, because it wasn't -- it was sort of like we didn't -- nobody had the option of actually getting in there, and trying it out, and sort of saying that you can actually build a better network. So the point being that unlicensed has this side effect, which is that it enables researchers to do a lot of good stuff.

Now, the third point that I sort of wanted to make, which maybe I didn't make a strong one because I was going so fast in my presentation, is -- the discussion has been that, you know, how much capacity -- well one other thought has been can we extract more capacity out of the spectrum. And there's a lot of work, you know, I would -- actually by training IEEE, but I do work a lot with the computer science part. But you know, like for example -- have gotten a lot of efficiency out of -- and have reached us -- got us to the -- limit.

Computer science protocols, et cetera, are not there but we get us there. But there's only a limited amount of capacity anyway. So the question is you need to have more -- get more pipe. You have to get more pipe; there's no option to that. Getting more pipe and then you have to say where am I going to get it. And so, you know, there's all of these discussions about try to get it below -- gigahertz pipes, right, because that's where the -- is good and all of the goodness happens.

So one of the suggestions that I had made -- the recommendation that I had made was that, you know, we throw out all of these results about, you know, how much spectrum is being used and it's kind of based on going back to one of those sources of data that we have and we sort of just -- I think the research community can help by creating sort of what I call -- national spectrum telescope, which is sort of a real -- database of spectrum users across the country so that you, you know, the government, or anybody at any given point can actually see what's being used and is it actually being used because I bet you when you sort of start seeing that you will see lots of areas where you can actually go in there and say hey, we need to really look at these policies. We need to really look at what's going on here.

MR. SICKER: I wanted to add -- Brett followed up with another question that ties into two of the points that you made asking, you know, what the -- a lot of the spectrum being auctioned, there's less and less available. And what does this mean particularly for innovation for entrepreneurs and researchers and should this be considered as part of this research agenda here looking at how do we make the spectrum available?

And Rashmi -- and we were talking about that earlier, sort of looking at experimental licenses or other such things, or does it need to be more than that. Does it need to be bands, does it need to be at a larger scale, and how can we -- one justify that and move it ahead?

A lot of these things are hard to, you know, 2.4 didn't happen for the reasons that it's so successfully used for now. It's a very second order, right, third order. And it's been wonderful, right, but it wasn't the FCC's insight that -- for that. It was built around it. It was available and then cool things happened.

MR. DOSHI: I guess just to add to that just as a reminder, in fact, both in our wireless innovation and -- and also in the research there are proposals -- looking for inputs, concrete inputs in terms of what additional things we can do to create experimental test -- beyond what we have right now.

I think it's part of -- encourage. And again, we seem to be talking quite a lot about wireless. I don't know if there are similar concepts for wired that one ought to consider or at least, I think --

MR. DROBOT: There ought to be.

MR. DOSHI: -- and the question is what are they? I mean we're not clever enough to figure that out, perhaps there are things that perhaps could be proposed in terms of --

SPEAKER: --

MR. SICKER: One of the things I was going to mention before is that, I mean, Victor's concept of a telescope, actually understanding, actually having a good database and really being able to know what's available, you could then do some more serious experimental licenses. You should -- you could be able to get the bands, the scope, the coverage.

MS. BAKER: Or secondary markets.

MR. SICKER: That or -- well yeah. That started when I was here.

MR. DROBOT: You could take one other view of this whole problem and that is if I look at the use of spectrum and you start looking at what is it that people actually use it for. A lot of us will make a wireless call from our house and maybe the architecture of what we do should have -- things of that sort.

So you use the spectrum for what it really ought to be used for and that's mobility. Okay; there's a lot of ways of relieving the use of spectrum and I think there's a lot of ways of, I would say, using MIMO technology to in fact increase the efficiency with which we use spectrum today.

MR. SICKER: Marcus; had a comment.

MR. WELDON: Yeah; a couple of things. Yes, there are things -- but I'll finish the wireless topic and -- yeah; I think one of the things that clearly could be part of say 100 megabit per second wireless or maybe it has to be more than that depending on how you count LTE, whether it's 100 megabit per second wireless, is actually intelligent usage of hybrid or diverse networks so that you're actually -- if a user has a femtocell then allow for some kind of mandate to drive the traffic that.

If they're in the presence of a WiFi hotspot, allow for a mandate to drive the traffic that way. So clearly spectrum is good; more spectrum is good. But there should also be intelligence in how to offload that traffic into other networks, whether that's a previous generation of wireless technology or even commercial WiFi hotspots.

So that's a way in which spectrum gets reused optimally, allowing -- devices to support that sort of mode of being driven to connect to the network that is the one that is optimal from the spectrum usage standpoint; for the service that they're looking to get at that point in time would actually be a very valuable way of optimizing the spectrum that is already deployed. So you know, some kind of mandate in that direction or research in that direction even would be a good thing to do as well.

MR. SICKER: I do think -- I think it's important to say that I -- we talked a lot about spectrum and people who would look at my CV would think I'm a wireless guy. I'm just a networking guy. I don't want -- I don't want to get off of the importance of understanding the wired network, fiber, copper, and everything else. I think it's a -- I mean it is hybrid. I think we're going to look at a hybrid future and --

SPEAKER: Right; so --

MR. SICKER: -- we can't assume that spectrum solves all of our problems.

MR. DROBOT: Doug, again, you know, sort of beating a dead horse maybe, when I look at this whole spectrum issue, you know, we find in actual operations the service provider will worry more about -- a stable network than worry about efficiency at times.

MR. SICKER: Mm-hmm; right.

MR. DROBOT: We find retuning you can recover 20, 30 percent additional capacity out of the network. Okay; if you match the backhaul properly, things of that -- these areas again, not well researched today. Access to that kind of data, whether it's the academic community or other researchers, is where the operational side of this really makes a tremendous impact. And innovations there can be as big as dealing with the physical spectrum itself essentially.

MR. GREEN: I certainly agree. I think the future is hybrid networks and you're certainly in a position, and of course the commission has done this already, set some spectrum aside for research in order to get people to use it though, coupling it with some kind of mandate that the priorities ought to be for experimentation hybrid networks.

Also -- helpful I think, as was talked about this morning, that that be a national set aside so that researchers in various parts of the country can collaborate using it.

But it would -- it would establish a platform supported by the FCC for experimentation and development because probably the greatest need, I would think, for development, not necessarily research, but development is in that area. Hybrid networks, efficient use of spectrum by using the capabilities that are inherent in a hybrid architecture.

MR. SICKER: I don't think we're going to turn into pumpkins if we run a little over. Are there any questions from the audience?

MR. GREEN: Are you getting a lot of questions on the internet?

MR. SICKER: I have just gotten two so far.

MR. GREEN: Just two.

MR. SICKER: I think.

MR. WELDON: I will -- I will comment on the wired side.

MR. SICKER: Please.

MR. WELDON: Because we have to give some credit to the wired network. Yeah; I mean to answer the optimization question on the wired side, you could argue that PON is such a high band with technology that perhaps no further optimization required and maybe that's reasonable for the time being.

But DSL for one, and I think HFC too, there's definitely optimization techniques being applied to -- cancellation that mimic what is done in a wireless domain. Clearly that's another area where if there were more, again, grand thinking big challenge stuff there might even be a new DSL -- in the end or deployment rules.

So that's where it could even be some

kind of regulation or recommendation for deployment rules. There is potential to take DSL from a 30 megabit per second service to 160 megabit per second service. So there's -- there's a kind of -- you could get there that shouldn't be ignored. And then back to the coupling between wire line and wireless, network MIMO is enabled by synchronous transmission over neighboring cells.

If that was backhauled through a wire line element, whether it be an aggregation switch where that was configured and was one of the parts of the initiative that a wire line element backhauling cells to do network MIMO. There are things there that, again, are very experimental, but could be where wired plays a significant role and there's innovation there; yeah.

MR. DROBOT: Absolutely.

MR. BAHL: Let me just quickly say something that's food for thought for you and that's in the wired space, since that's what you were sort of really wanting -- so I think the issues, as Doug sort of mapped from the spectrum to the wired, is actually the correct issue which is just the transparency is -- I mean for network -- don't have that.

So in -- design, you know, sort of -- FCC needs to know what is really possible before policies are made or whatever. Right; and in some sense the more -- the access to network traces actually does help design networks better. I gave an example of P2P -- and these networks are differently designed. And so I think whatever you guys can do in thinking about -- I mean I had mentioned, you know, the government being a large organization, has a lot of the internet works and there's a lot of traffic that can be made and can be -- that can be anatomized and provided to the research community to look at and carefully see what are people using it for and how they are using it.

That would actually fall back into the designs of routing protocols, routers, and switches, and all of the other stuff, which is then going to enable a more spread of your

Broadband.

MR. DROBOT: So one thing, again, I haven't heard in this discussion here is, you know, IP as a protocol has been very successful. The way the network actually runs today, you still have two layers underneath it and there is a lot of research that says what we ought to do is really go to the direct IP protocol of some sort for running everything; number one.

Number two, while we may have 4G networks, some of us believe that 5G and 6G are breathing down our necks and they're about a totally different topic. Okay; and that is how do I bring computing, storage, into the picture because again, that takes capital, that takes deployment, it's what gives the user an experience essentially. Okay; and you can think of 5G maybe as, you know, how do I get what I need when I actually need it.

Okay; how does my information arrive just in time for what I need? And maybe 6G's, how does the system, and the artificial intelligence, and the other things that we all spent time on, okay, actually anticipate what our needs are going to be.

And that gets into the whole world of how do I look after the electric grid, how do I look after automobiles, how do I, you know, sort of provide a world in which it is safe to put a lot of applications, okay, that really -- the network, isn't just the transmission part, it's the -- essentially.

SPEAKER: So --

MR. SICKER: Commission Baker has to leave at some point --

MR. DROBOT: Yeah; sorry about that.

MR. SICKER: -- I don't know --

MS. BAKER: I am the one that does turn into a pumpkin actually.

MR. DROBOT: Okay.

MR. SICKER: I don't know if she has questions.

MS. BAKER: But let's have a -- I think we should have a couple -- I want to hear a couple final comments. I think the discussion has really been terrific. It's been all that we had hoped for with the expertise that is, you know, assembled here at the table.

I think some of the suggestions really have given us some paths to go down, whether it's more cooperative research, whether it's more unanimity between government research, academia, and private sector.

I really think some of the suggestions here have been really terrific. But maybe if, at least for my benefit, if we can maybe have some last comments from the table and then if you want to continue question answer after that then that's great. How does that sound?

MR. SICKER: Sure.

MR. WELDON: I have a -- my last comment would be a new comment. So one thing we didn't talk about but in the spirit of white space or whatever, one thing we're increasingly seeing is the need to open network API's. So the point about 6G, where actually that's more like a web

3.0 applied to the network, meaning that the concepts are that the network -- the web figures out what it is semantically you're looking for.

But in order to deliver that efficiently, it actually requires you to control the network probably as well; meaning that -- but not in a close way, meaning the network -- an API, that the application can then invoke the services it needs and then terminate those services it needs.

And that's a more organic network, which I think is a very real thing, but it is something again, constructive that could be done from a regulatory point of view; is mandating or recommending network API's be opened up, that application developers can innovate on to take you to the next generation. And that includes a little bit of the concepts of white space. So that was my last remark, which is a new one so it doesn't summarize anything. But, go ahead.

MR. SICKER: You will be happy to hear that about an hour of meeting with NSF, some --

about a month ago, focused right on that -- on that topic.

MR. WELDON: Great.

MR. LEVINE: You know, on the macro end, clearly collaborative research and not reinventing the -- there's a laboratory structure in place; there's other organizations within the government that are -- that are looking at the same issues that have mission critical problems and there should be ways to leverage that over the next decade, certainly as we build out our Broadband system.

But on the micro level, as long as we have a free, and open, and competitive environment where tiered pricing is involved, where tiered levels of service is involved, where people are free to innovate and provide value, then you'll see entrepreneurs step u and fill the void and there will be capital for them in the market; sure.

MR. GREEN: This is a very exciting time. I mean think of all of the really interesting problems that we've just covered lightly here today that would be interesting to work on and -- solutions. And I think the FCC is in a position especially with the Broadband study to help prioritize some of those to focus on the ones that are -- that have the most importance in terms of policy.

And I do think that we can do a lot better job of research than we're doing and I think by emphasizing, obviously collaborative associations within industries between government and industry and between the government labs and industry and academia. There's a lot of opportunity there for future development and a much better way of getting our research.

I -- one of the issues here which I'll just raise, this is not probably -- summary; it's not -- used to be a glamorous job to go into research and I don't think it is anymore. And I don't know what you can do about that. But I think it was mentioned in the earlier panel, when you get -- after you get a newly admitted PhD, you're first thought is -- to work for a company or start a company. You don't really think about spending time in research or staying in the university as a junior faculty member; it's kind of a -- it's kind of a rough role.

There's something that we could do to make research a higher priority, to give it a little more push. I think that would be great. I have no idea what that is but I'll leave that for the Broadband team to come up with a solution; Doug.

MR. SICKER: Okay; make research glamorous. John, you take that one on home.

MR. GREEN: Well more glamorous --

MR. SICKER: Adam.

MR. DROBOT: I think we live in very exciting times. I think, you know, for this country to have a leadership in telecommunications and the future, I see research as one of the essentials on the agenda. I don't think it's possible to get there without -- in the process. So I hope the FCC takes a hard look at this and, you know, really ends up supporting how we go forward.

MR. BORTH: I think it's very interesting first of all that the FCC held this session because it's kind of unique. You don't see a lot of other agencies holding sessions of this nature to say what could we do to further on the whole industry as a whole, but also the direction of Broadband in this particular case. So I applaud you for actually holding this session, both the morning session, as well as this session.

I think the directives from this session were somewhat clear at least to me. One is we do have to have grand challenges. I think that's very important to try -- as Adam brought up the concept of having roadmaps. Roadmaps are a very excellent device for driving technologies and furthermore we need additional funding and collaborative research. And just on that last item, we could pick up various models for collaborative research from various places.

I mentioned when I gave my opening presentation that we participated, Motorola participated, in the EU Framework Programs and that is perhaps not the best way of spending your money. We had this conversation at lunch with a couple of us in the sense that there was a lot of money involved but did it really drive fundamental research; definitely not. It was not basic research; it was applied research and politics played a very significant role throughout that whole process.

So there are ways -- there are probably lessons learned that should be entered into that phrase. But I think a collaboration is very important between industry, academia, the national labs, and the federal government in that regard.

MR. BAHL: Okay; about making research glamorous. I think there is a way. And I think the way is that if we can sort of like, you know, if you can educate the public about the stuff we've done, which is create -- has had large societal impacts than it does become kind of interesting and sexy.

But anyway, I think -- first of all, I want to thank you. This is great. I -- it was educative for me as well listening to everybody here and the fact -- the openness of the process is very, very good and heartening. I would say, I think in terms of grand challenges, we should move forward with things like 100 megabits, at least to the anchor institutes. I think that's a good goal to have that is doable and that's going to bring us -- make it competitive.

I think you should keep pushing on the white spaces stuff and make the ruling happen and let it, you know, and open it up and the innovations will start to happen and that's great. I think -- it is true, researchers are very motivated by funding. You know that, you know that -- you know where there's money, they'll go.

And so if you can bring money to bare on the is problem, I think -- you know, and then start funding a lot of research, it's going to help; it's going to help quite a bit. I do -- I mean I agree with everybody else about the collaborative stuff.

I also want to say one thing; that in terms of all of this collaborative, you know, they do exist, geniuses amongst us; well, you know, maybe you, you know, since you were so -- but so the little guys are as important sometimes to -- they'll find things that, you know, collaborations may not be able to find. But on that note, thank you very much for giving us this opportunity.

MR. SICKER: Thank you. We still have some questions from the audience. The Commissioner might have to leave.

MS. BAKER: --

MR. SICKER: Thank you for joining us.

MS. BAKER: Thanks guys; great to see you.

SPEAKER: Thank you.

MR. SICKER: I'll ask you to stay a little bit longer; Mike.

MR. NELSON: Michael Nelson with Georgetown University, Communications Culture and

Technology Program. Adam started to answer my question so let me see if I can get a few more answers to my question which is about the issues I work on. I tend to work on what we're going to use the network for, and particularly, I spent a lot of time with the future of computing, cloud computing the internet of things.

And I'd just like to know if you've seen any particularly good roadmaps developing to help us understand what we're going to need to do to the network to support these 5G, 6G networks. We're going to have 500 billion devices connected to the net. We're going to have people doing 50 percent of all of their computing out there in the network and yet I haven't seen any good roadmaps that indicate where the bottlenecks are going to be in the network, whether we're actually going to be able to support this fundamentally different way of doing computing and of using the network. So this is -- this is really a specific question about where are the bottlenecks going to develop and then a broader question about how do we start addressing those questions and laying out a research agenda.

MR. DROBOT: So let me make a -- comment the following way. If you look at the way we have built networks so far, okay, they're really quite higher -- you have a core, you have something regionally, you have metro area, you have access lines, okay. And I don't care whether it's cable; they're all built the same way. Okay; and there's something in nature that tells you things not to be built this way. So that's one part of it.

The second part of it is we used to have this debate is -- intelligence going to be in the network, is the intelligence going to be at the edge. And as the cost of intelligence got lower and lower, you find it's everywhere. It's at -- all of that.

So when you step back and you actually look at the way people design stuff, okay, and you know, we're at the -- we still, okay, as a discipline -- solutions. Okay; the mathematics and the understanding of networks, usage, how it all comes together, is still very fragile and in its infancy.

And so what you find is we create bottlenecks; stuff isn't balanced. If the core pipes aren't big enough, you know, you can't build stuff on the edge; okay. If you build too much on the edge you're paying for a resource that's not revenue for you; okay. And so there's a lot of stuff of that -- that we haven't discovered at this point.

MR. BORTH: I don't think we know the applications either. I mean we're using today's applications that we found out, you know, we came up with things like ADSL and, you know, they're asymmetric type applications yet we found out -- all of a sudden it went -- it was fully symmetrical or it went the other way. And we don't know if it is. About a week and a half ago there was a meeting up in Georgetown on -- by the FCC on EMS applications for Broadband and there was a pretty astute gentleman that noted that Broadband to public safety was like creating a basement. If you build a basement you'll fill it up and I think that's the case here as we go about building it and deploying Broadband we'll find new applications that -- it's a little hard to predict right now what the future will be.

MR. NELSON: But there is a fundamental difference -- new architecture, new applications --

SPEAKER: You're going to want to talk to 10 or 15 or 50 things at once.

SPEAKER: Yeah.

SPEAKER: It's not like --

MR. DROBOT: And mobile.

SPEAKER: Exactly.

MR. DROBOT: And move that whole session at the same time without breaking it three times, like on the -- today.

SPEAKER: Right; yeah.

SPEAKER: Move it at 100 miles an hour while you're at it.

SPEAKER: Yeah.

MR. WELDON: It's a very good question;

distributed cloud computing with many endpoints being the cluster that you might need to talk to as opposed to just cloud being it's not in your home or -- that is a whole new -- in fact it happens to be one of the grand challenges in Bell Labs working on that is distributed cloud computing to figure out exactly how you can extenuate virtual machines and move your application with low delay, high availability kind of -- characteristics as opposed to net characteristics. It's a very interesting question.

And I think until you figure out what you can do there you don't know the network architecture. The two are tightly coupled of course in that if there's a limit to how you can -- and create resources dynamically, given current computing technologies, then that dictates the architecture that you realistically can deploy.

You might have to cluster right at the edge and have those be the resources that you most utilize and that it has something, you know, again, somewhat -- as the secondary resource that you utilize. This is -- again that the network provider owns some -- computing resources, if not, if they're highly distributed over the web, then of course you've got many clusters that are -- but they may not be the most efficient way to do high -- low delay, high availability applications.

And it's going to be a whole set of different things depending on the needs of the application; how you extenuate it will change and so the network design will change. So it's a free space that needs much research I think.

MR. GREEN: No shortage of questions. It's an equation with too many unknowns.

MR. WELDON: Exactly.

MR. GREEN: -- problem. And I don't know. I kind of favor tinkering with architectures; I think we can do a lot of experimentation or even theoretical work with different kinds of architectures so that as the applications develop we would know which way to move. But I think -- and I think there's --

that's quite interesting and it has, you know, many parameters that can be changed. And we may miss it even so but at least it would give us kind of a store house of ideas that we could use to address whatever the applications bring about later on on the internet.

MR. DROBOT: You know, what -- one of the things with cloud computing is this notion that I can on demand get resources. When you look at IP, which is the fundamental integration mechanism today, it is fundamentally static. And we haven't even broken, you know, how do you take the fiber underneath that and switch it at this point. So lots of room.

MR. GREEN: Physical layers to --

MR. SICKER: Okay; so we're about 20 minutes over so we don't have to pay you guys overtime, I'm so pleased for the enthusiasm and what I will ask, again, as I keep asking, continue. Give me feedback to my public notice. We need -- James Miller and I are going to be writing this chapter together and we need input.

So I appreciate everything you can provide me. Thank you.

(Whereupon, the PROCEEDINGS were adjourned.)

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I, Carleton J. Anderson, III do hereby certify that the forgoing electronic file when originally transmitted was reduced to text at my direction; that said transcript is a true record of the proceedings therein referenced; that I am neither counsel for, related to, nor employed by any of the parties to the action in which these proceedings were taken; and, furthermore, that I am neither a relative or employee of any attorney or counsel employed by the parties hereto, nor financially or otherwise interested in the outcome of this action.

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