Minutes

Department of Energy and National Science Foundation
Nuclear Science Advisory Committee
Hilton Hotel, Washington North/Gaithersburg, Maryland
September 21, 2012

**Members Participating:**

Donald Geesaman, Chair
Jeffrey Binder
Jeffery Blackmon
Alexandra Gade
Susan Gardner
Joshua Klein
Zheng-Tian Lu
Robert McKeown
Curtis Meyer
Jamie Nagle
Kenneth Nash
Allena Opper
Jorge Piekarewicz
Julia Velkovska
Raju Venugopalan

**Members Absent:**

Robert Atcher
Peter Jacobs
David Kaplan
Karlheinz Langanke

**Others Participating:**

William F. Brinkman
Denise Caldwell
Gail Dodge
Jehanne Gillo
Timothy J. Hallman
Bradley Keister

**Presenters in Order of Appearance:**

W. F. Brinkman
Timothy J. Hallman
Bradley Keister
Donald Geesaman
Stuart Freedman
Robert Tribble
Michal Herman

About 30 others were in attendance during the course of the meeting. The below summary includes excerpts from the participants at the meeting.

**Morning Session**

**Donald Geesaman:** Good morning. I want to welcome you all to today's NSAC meeting. I also want to remind you, as you can see from this very professional staff around the room, that this meeting is being webcast. If you want to see your picture, you can go to the NSAC website and find the meeting and click on the webcast. It's also possible for the people out in the ether to submit questions. So we may get some evidence that people are actually watching this webcast in the form of questions. They'll be delivered to me, and if we have time I will bring them up, representing the questions. So as usual, we'll start by going around the table, and having the committee introduce ourselves, then we'll ask the audience to introduce themselves. (Members and audience introduced themselves.)

All right, thank you all for coming. Without further ado, I think we'll move into our program. So the first speaker is Bill Brinkman, the Director of the Office of Science.

**W.F. Brinkman:** Good morning. It's a tricky time for me to give a talk here because we've asked you to do several different things, and I certainly don't want to preempt in any way or form or try to put my views into your heads. …So I can't say a lot about nuclear physics this morning. I expect to talk a little bit about where we are on budget and a little bit about what's happening all over the world.

Washington is a strange place I've discovered. I've never sat through an election period. What happens is about the end of August the town goes dead, and everybody disappears and goes out on the elections trail, then comes back in and votes on continuing resolutions. They do that in one day and get out of town again.

So right now our appropriations staff is all heading to Europe to visit CERN and Catarash and see what's going on at these various places internationally. So it's kind of a funny time, but it gives us a time to think about what we're doing.

We are starting the 2014 budget process with OMB this week. Nothing on that will be settled until after the election. What's happened is that the Senate and House have marked up the 13 budget, and really basically, the two marks are not so different from what we've had in 12. …The House is a little bit down, maybe 40 million down; and…the Senate is a little bit up. So we aren't moving much either way, and we'll probably end up compromising. It will be somewhere right about where it was in 12.

There was a lot of controversy of course surrounding your nuclear physics budget this year. The President's request was low and both the House and Senate came up from there, and made it somewhat better. …That had a lot to do with debates over FRIB and RHIC and what we were going to do. That's something I think we really can't talk about today.

It's an important time for you guys to get your input into this whole subject. And before I talk about some other things, I think I should ask if there are any questions about the general budget because I'd like to tell you a little bit about the department's goal ever since I've been here. Steve Chu and I both came here because we wanted to drive the energy issue in climate. We thought that that's the most important thing that the world has faced in a very, very long time. And we worked on it, and I'll tell you about what I see happening. There is some good news in that arena.

So if you have questions about where you guys are with nuclear physics, I'll answer them now. Any issues?

**Donald Geesaman:** Why don't I start. One of the difficulties that we face in responding to the charge is the change of direction seems to be quite abrupt when we're in the middle of projects.

**W.F. Brinkman:** You mean the fact that we started out saying we're going to double, and all of a sudden it's flat. It's been flat now for three years at this point. 2009 was a big increase, then it went flat and never increased again from then on. So it's been pretty flat even though there was all this promise of a doubling budget.

**Donald Geesaman:** So I guess you alluded to the Office of Science budget going in one direction but there have certainly been winners and losers within the Office of Science. So you alluded to that in terms of your direction towards solving the energy problems, which are certainly very important. The problem of course, as always, is how does one take into account the basic research that's needed to address the challenges of the future? And how you weigh that over the shorter term gains.

**W.F. Brinkman:** That's a very interesting question of balance that we have worried about a lot. In fact, both Steve and I feel things may have swung a little too far in the energy direction, and that we want to readdress that issue to some extent. And we will try to do that. There's a lot of different pressures involved in the budget. Obama's promise was while he talks about energy he also talks about scientific research. So he talks about both of those things, and some people tend to forget that. And we don't want to forget that. We're very proud of what's happened in the past -- the Higgs particle was really an exciting thing. I'm sometimes amazed at how popular that subject is again. Why it's so interesting from a science point of view is not easy to express. But yet, it's a very, very popular thing. Joe Incandella was elected to be a spokesperson or leader of CMS, and he's American- we're not even members of CERN, yet he's elected. We really appreciate that. We had Joe come and talk at the DOE, and the auditorium was essentially packed. …That talk has been put on a couple hundred websites. It's fascinating to see that kind of thing happen; it's great, in fact.

Much less activity is centered around neutrinos, and $theta_{13}$ - that's a very important issue, too. We're hoping that NOVA will actually give us the ordering of the masses; that's something that needs to be understood for the CP violation phase. If we got those two it would be really impressive. …We see it as important that we think hard about how to keep the United States in the position of being at the forefront of these fields in nuclear physics and particle physics, and …we really don't want to lose that.

…CERN is a big deal these days; it's a $1.2 billion per year facility. It's got a lot of money so it can do a lot of things. Japan really is doing everything it can to see to it that if an ILC is built, it's going to be in Japan. We really had to think where we're going, and this is where we get into LBNE and all this other stuff. How RHIC is going to play out is something that you guys have to think about, because it plays into this whole business of what kind of internationally attractive facilities we have. And that's a very, very important concept we need to think through.

**Joshua Klein:** There's a real concern in the community that while research into energy is important, a large part of the Office of Science's mission should be directed toward fundamental research. And the example that one could imagine is, if we asked Rutherford to spend his time on better steam technology that would have been a big mistake. You don't know where these things are going to lead. So there's a concern that siphoning money from basic research into something that seems more directed is really not the right way to go for the Office of Science.

**W.F. Brinkman:** We have to be careful about that... The thing about it is I believe that if we're right about our climate predictions, we are heading in a direction which is tremendously serious. It's not a joking matter; we may leave for our grandchildren the world's worst mess you've ever seen in your life. So while I have a lot of sympathy- I worked in basic research- I worked on esoteric things like superfluid helium 3. But I also believe we're facing this tremendous problem. I'll show you some view graphs and talk about that; I think we've done pretty well on that subject.

**Joshua Klein:** So I wonder if you have any insights you could share on budget sequestration, and how likely that would happen and how the office would deal with it?

**W.F. Brinkman:** Funny thing about budget sequestration is nobody wants it… And people believe that something will be done when the lame duck Congress comes back. It's a pretty scary thing. All you have to think about - for us it's 8 percent, it's $400 million, so try to start thinking about wacking $400 million out of the Office of Science budget and you've really got a challenge. You clearly have to shut some things down. And you have huge layoffs in laboratories and stuff. So it's not a fun thing to think about. I don't know what's going to happen in the end. But that's a dangerous situation, no question about it.

I look at what's going to happen when the election is over... First of all, the 13 budget has to be settled and sequestration has to be settled; Bush tax cuts have to be settled. On top of that is that the debt ceiling is going to have to be raised in February. So there is a lot for Congress to do once election is over.

**Robert McKeown:** Coming back to the international question, you mentioned CERN and other countries building new facilities. Presumably we all agree, in all of the fields and the Office of Science, it's important to have facilities here in the U.S. Do you see potential options for international investment in the U.S. in a more global project in the U.S. as a possible scenario?

**W.F. Brinkman:** This is one of the things that I must say I have a hard time thinking through, in the sense that the way we fund things in the United States does not yield easily to creating international collaborations in the country. And I don't know exactly what to do about that. But we have this kind of catch 22, in the sense that we're told by other countries - when you decide you really want to do that let us know and we'll come. How do you get started without having some commitment from them. One of the things that we would of course like to internationalize is LBNE; we haven't been able to do that so far. But we have some talks going on with specific countries on that.

The biggest project going right now is clearly ITER and we did sign up for it. It's a huge project and it's going to cost us a fortune for it. And we seem to be committed to it.

**Donald Geesaman:** I think the question Bob raised as you addressed is extremely important. In going forward we can point to several opportunities in the past, in nuclear physics, where we chose the avenue of investing in an international facility, and in the end we weren't able to do it, and the international facility failed. So, you know, of course there's been OECD studies of this, but I don't understand -- you hit the nail on the head. I don't understand a good mechanism in the U.S. to get the confidence of international partners with a year-by-year budget cycle. And we've had that reaction already, in the presentations to the Tribble Subcommittee where we have people from other countries who have made major investments in RHIC and Jefferson lab, and are just aghast that, out of their control, they may not be able to realize any return on those investments.

Okay, why don't we let Bill finish his talk, and if there are more time for questions at the end --

**W.F. Brinkman:** I'll just remind you what the problem is. We have been increasing the temperature of the earth over a fairly long period of time now. And if you look at this plot, you'd say it's probably already gone up by a degree or so… You know, you never know about individual events but the melting of the arctic ice this year was something else in a big-time way. So it's a little scary.

And this of course leads to $CO_2$- where there are encouraging things happening. I put this up here, because it's the first time that the EIA, which is our Energy Information Agency, has actually projected that in 2035 we will actually be putting less CO2 into the atmosphere in the United States than we do today. We're making a little progress, folks. So it encourages me a little bit.

The other interesting feature is this business with natural gas. You know, one of the things that's been true is that coal traditionally has been 50 percent of our electric supply. Today it is 33 percent and so is natural gas. In a very short period of time because of fracking, something the DOE worked on 20, 30 years ago, because of fracking natural gas has become so cheap that electricity companies are building natural gas plants and using natural gas as much as possible. So they've actually decreased the amount of coal they're using for that reason. Not for any ecological reason, but still, hey, it allows us to gain a little bit, right? So that is encouraging, in my opinion.

So a little bit here and a little bit there. But we can't forget the models predict very clearly that the place we're going to see global warming big-time is in the arctic. And this shows, the bigger the red dot the more global warming there is going to be. Over Siberia you see that it's really big. A lot of warming there. At least this indicates that there's some real issues with what's going to happen in the arctic, and that's where some of the first effects are going to be.

This is from a insurance company in Switzerland and they're very interested in measuring catastrophic events in the world. They have this plot of major catastrophic events and if you look at it from 1980 on to 2010 you can see that there's this gradually increasing trend. Which I think is one indicator that things are happening in the direction that we don't really like. And we'd like to do something about that. So there is plenty of indication that we are walking into real trouble. And we are going to have to do some remediation.

I believe there is some reason for optimism. This is a Tesla. This car has 85 kilowatt hours of battery. So it can go 300 miles without recharging, which has always been our goal. It has done a whole host of really neat things. It has taken the batteries and made them a layer on the bottom of the car. The batteries really don't interfere with the space of the car. In fact this car has an enormous amount of space for its size. It's a four-seater, plenty of room in there, but it also has a trunk in front and trunk in the back, because the electric motor is very, very small. So it's a very interesting car - it goes from zero to 60 in 4.4 seconds. That's very encouraging. The trouble with this car which I haven't told you is it's $75,000, so it's not cheap.

People are giving Obama a hard time on his goal of a million hybrids by 2015. The data is interesting; in 2012 it looks like we're heading towards 3 percent of the car market as hybrids. And 3 percent of 13 million is 390,000 cars , so his goal of a million cars is looking very realistic at this stage. So it's very encouraging, in my opinion, and hopefully we will continue down that path. Good hybrids get 40 miles to the gallon. My car currently gets 20 on average. So a nice factor of 2 is not a bad deal- it's certainly in the right direction.

The Office of Science is doing a lot to try to help out on the energy problem. We created a whole set of 46 Energy Frontier Research Centers and that are having a very big impact. People that have gotten into these things found that they're doing research in a very different way than they've ever done in a university, and that's because every one of them has these really applied goals in the end. They're trying to do research, basic research that will to try to solve some of the goals -- batteries and sequestration and capture and all these different issues, really hard issues that we face. I think that's been a very successful program. We've created some hubs. This hub is fuel from sunlight. People have been trying to figure out how to imitate photosynthesis with inorganics and not use bio approaches for the synthesis. It is really a major thrust to try to figure out how to convert sunlight and water and $CO_2$ into a biofuel, inorganically. I don't know if it's going to work. This is pretty research-y type stuff, because we don't know how to do it at all. There are several other hubs. There's a hub of nuclear power, nuclear reactor simulation hub, which is at Oak Ridge. We're actually finally learning things about light water reactors. I thought that was a mature subject but apparently there are still things to be learned.

Another hub is on building efficiency. Those are in the science applied energy offices. We're coming up with two new hubs. The Office of Science created three biofuel centers. One is at Berkeley, one is at Oak Ridge, one is at University of Wisconsin. These guys have done a lot of really first-rate work at trying to understand the process of breaking down plants and trying to figure out how to get rid of the ligand so you can get the cellulose, and given the cellulose how to effectively process cellulose. It's a very challenging subject. They have made a lot of progress. They've been able to modify microbes and that helped process the cellulose. They also figured out how to change the genetic code of the plant so the ligand isn't such a strong chemical bond. They have weakened it so you can get it off more easily.

We are going to have the battery hub pretty soon, and then the critical materials hub. So my view is we've done a lot to work the energy issues here. I personally think at this stage we should be using what we've got and trying to see to it that we work hard at propagating in new directions in solar and wind.

The whole solar industry has gone into a strange loop. What happened was that at the beginning of this year everybody predicted that there would be about 8 Gigawatts of installations, but what happened is that the industry built a factory capability four times that big. The whole business basically crashed. So solar cells are very cheap these days, but the companies are struggling to stay in business, and there clearly will be a shake-out. This happened in the dot-com era too. I hope that the solar market will be like the dot-com market. The dot-com market was always doubling every year, so there was tremendous overbuild by 2000. Some of these carriers were only using 5 percent of their total capability to transfer information, and so it took five years with the doubling before you finally worked all that out. Now that whole industry is going along just fine. Everybody understands it's only a factor of 2 every year, not a factor of 10, which is what some people back in the 90's were trying to claim. I think solar energy is kind of in that space. We'll see how it goes.

In any case, my view is that we're doing enough, we're doing what I think is some very good things- to try to use basic research to work on energy problems, and so I'm pretty content with where we are at this stage. And that's really all I wanted to really say about that situation. That's where we are. Questions.

**Joshua Klein:** I'm curious what you see is the reasonable horizon for return on the research that the Office of Science invests in. So for example, light water reactors is something that has a medium scale horizon, and also would be very interesting to Exelon, for example, to be funding. So I'm curious, on decisions on funding it would seem to me for Science to look at the longest term and let private industry fund shorter --

**W.F. Brinkman:** I don't see us funding light water reactors. That's out, as far as I'm concerned. This is something I debate in my mind a lot. I think it's not so much in your space; your space is really basic research to a very large extent, at least in the nuclear physics world. Where this issue really becomes very big is in our biological and environmental research, and in our BES research. And it's very complicated- some people are coining the phrase goal-driven basic research. Goal-driven basic research is applied research. But I used to claim at Bell Laboratories there were people doing applied things and people doing basic things, and sometimes they were the same thing, just the goal is different. You have to be careful about that. It's an interesting thing that's happening that I think is rather fascinating with the synchrotrons and with our high performance computing. We have set them up in such a way that a corporation can come in and use them in a very easy fashion. They don't have to write, create it, any of that. They send a couple page proposal into a proposal review committee, and they get time.

**Jamie Nagle:** When the Department of Homeland Security was created they had enormous resources to put into a problem, but in my opinion they lost site of a metric, which is making things safer, and tying those projects together. Given that even if the United States cut its emissions by 50 percent today it would have virtually no impact on global climate change projections, how does one make a metric for funding these different efforts? What does one expect for a long-term scenario based on this research that defines success?

**W.F. Brinkman:** This is certainly not a trivial problem to figure out on a national -- international scale. The only thing I can say is the United States to this day is almost a factor of 10 more energy per person use. So we could do a lot to just reduce that. How it's going to work out in the international arena, I do not know. I think it's got to get a lot hotter before people are going to pay a lot of attention to it. But -- it will.

**Susan Gardner:** I can't help noticing that many of the hubs, et cetera, are concerned with the greening of the transportation fleet, which is important to realizing energy independence and important to reaching that goal. But ultimately it seems to me that we'd want to invest in the greening of energy for American industry as well. And one of the few items I saw that was relevant to that was the simulation of light water reactors. And I wonder if one could think of those kind of issues more broadly. Why not create a hub, for example, to study accelerator driven reactors, say. That would be very interesting. Could you comment?

**W.F. Brinkman:** That's interesting that you mention that. I've had an interest in accelerator driven reactors, driven systems, and what we could do with them, couldn't do with them. I had a study group work on it one summer. And so far the problem is that there has not been a well-defined market. I mean, you know, most of the kinds of things that people think about in accelerator driven systems is getting rid of the minor actinides or some other type of by-product nuclei. For example, one of the things I wondered about is- Hanford has this whole technecium problem out there. The question is - would you build an accelerator driven system to radiate that and change its composition. And the problem we keep running into is, well, if you look at these problems you can do them with conventional reactor; we know how to do that and it's not a challenge at all. But I haven't been able to find an application that people believe in for accelerator-driven systems.

**Jeff Blackmon:** There is one issue which is a nonproliferation issue and safety issue that an accelerator-driven system might solve. So I think there are real national security issues that argue that something should be --

**W.F. Brinkman:** There is the fact that, it's not a reactor, right? So it's better from that point of view. From a nonproliferation point of view it has advantages, but so far, I haven't been able to figure out how to sell one.

**Donald Geesaman:** I think in interest of time we're going to have to end it here. Thank you very much for the presentation, and also for the extended question and answer. Normally I would take questions from the audience if we had time, but we want to keep on the schedule.

**Timothy Hallman:** From the perspective of the Office of Nuclear Physics, this is the slide we show at the time of the rollout of the 2013 budget. Our mission is discovering, exploring, and understanding all forms of nuclear matter, and the scientific challenges that we take on in order to address that. You see those listed. Some of the features of the 2013 budget are that we continue to operate and do research at our three national science user facilities: RHIC, CEBAF and ATLAS. We continue to construct the upgrade at the 12 GeV CEBAF accelerator to provide new research capability for our community, continue to prepare for the construction of a facility for rare isotope beams and do research development and production of stable and radioactive isotopes. In addition we will discuss there is a new strategic planning activity that's going on under the auspices of NSAC. So this is the FY 2013 congressional request for nuclear physics. As you know, we don't have an appropriation yet for FY 2013 so we're under a continuing resolution.

We always have to plan to the most prudent budget that's been discussed, and in that case it's this budget. So you see the breakout across the different sub-programs in nuclear physics. Of the 13 requests, and the changes that reflects relative to FY 2012. You see that basically across all sub-fields except for medium energy, where it's influenced by some things we can talk about, it's basically reductions in all areas, and those reductions are in all cases, significant. That of course brings us to what NSAC is doing for us. The impacts are significant enough in nuclear science that we've asked our community to give us guidance on how we would go about implementing the 2007 long range plan in the instance of these constrained budgets. So this is a charge that was presented some time ago to NSAC, upon which the Tribble panel, which you'll hear more about today, is currently engaged.

I would point out that this is in fact an implementation plan for the vision of the 2007 long-range plan and is not a new long-range plan. That presumably at some point will be the appropriate thing to do once we understand what situation we're in at the moment.

So just to review, this is the status of running hours at NP user facilities, back to 2001. The peak in 2003 was about 28,000 hours that NP user facilities were providing to the research community to allow our scientists in the United States to do things they couldn't to otherwise. And then you see that in FY 2013 we're at a historic low, a little bit over 5,000 hours, perhaps 6,000 hours. Part of that is funding-driven, part of it is because there are planned downs at Jefferson lab, although we should point out the initial planning was that CEBAF would be down for construction for a year, and because of budget issues, that has been extended to 18 months.

RHIC running is constrained; even at ATLAS there are some constraints because there's a CARIBU upgrade. Clearly this is something we're looking at and concerned with, we understand that operating our national user facilities is one of the most important things we do in addition to supporting research at universities to really allow U.S. scientists and nuclear science to do things that are forefront and leading in the world.

You'll see that the House mark for nuclear physics would provide 21 million above the FY 2013 request. That was allocated -- 18 million for FRIB, “to continue activities leading toward the approval of construction, and 3 million for RHIC, ” to support a stand-alone run of approximately 15 weeks in FY 2013.

The Senate language provided an increase of about 13 million above the 2013 request, 8 million was for FRIB to “complete design and engineering work” and if the Office of Science approves the performance of baseline site preparation activities, and an increase of $5 million for RHIC to maintain 20 weeks of operation.

Both of those reports included language calling out and supporting NSAC's work, and its charge to review scientific priorities of the nuclear physics program within constrained budgets. So at the congressional level they are aware and watching and waiting to see what NSAC is going to provide in terms of guidance concerning implementation of the 2007 long range plan.

So in our office, we are developing the management priorities going forward, and how we're trying to address the situation; it is our job to establish short and long-term programmatic priorities in the face of significant fiscal uncertainty. Maintaining scientific productivity with reduced facility operations. Managing construction funding profiles and addressing the impact of the directed change in the 12 GeV CEBAF funding profile in 2012. You may recall that initially according to the baseline for the 12 GeV upgrade construction they planned to have 66 million in 2012. What was appropriated was 50 million, so there are impacts. Part of our task will be addressing those impacts going forward.

We of course have another priority of optimizing core national laboratory and university research within constrained budgets. The D&D planning activities of HRIBF and transitioning of its essential staff is another priority. Nurturing of the nuclear structure and astrophysics community prior to FRIB, which is the tool for the future is another- but it won't be ready probably until the end of this decade. And meeting the stable and radioisotope needs of the nation, which is a constant ongoing activity which is very intensive. So this in some sense is our task list within the office, things we keep our eye on as we try to move forward.

ATLAS is of course a workhorse for the foreseeable future in the subfield of nuclear structure and nuclear astrophysics until FRIB comes online. Once FRIB comes online it will serve as a unique premier stable beam facility continuing in partnership with FRIB. Our new investment there of course is CARIBU, which is using a californium source collecting fission fragments in a gas catcher then reaccelerating them for research, with particular emphasis on neutron-rich isotopes for study.

Recent news here is that the new source, which is about 500 millicuries of californium - has just been installed. If any of you ever dealt with a 500 millicurie californium source, that's no minor feat, and we very much appreciate Argonne National Laboratories operations folks for stepping up to the plate and really helping to make that go as smoothly as possible and as quickly as possible. So that source is in place and being commissioned. These are Couomb excitation peaks from a cesium beam that’s coming from reaccelerated cesium from the californium source - so that's a great step forward. We're looking forward to a robust user program using CARIBU beams in the not very distant future.

ATLAS's role and goals - it provides beams and facilities enabling world-leading research to address issues in nuclear structure, low energy tests of the standard model, nuclear astrophysics, and applications of low energy nuclear physics. Any stable beam from proton to uranium, some in-flight radioactive beams and low energy and reaccelerated CARIBU beams. And of course they're developing instrumentation to make that all work with a view to the fact that they will continue as the premier stable beam facility in the future, in the era of FRIB.

The main scientific questions that they're focusing on in nuclear astrophysics are familiar to this audience, certainly the rp, alpha-p and neutrino-p process, the r process path, sub-barrier fusion hindrance, breakout from the CNO cycle, other things. And you see some of the tools that they've been working on, or they use, rather, in-flight radioactive beams, HELIOS, the PENNING trap mass spectrometer. They're working very diligently to position themselves to serve the structure and astrophysics community and nuclear science as well as it can in this era leading up through the beginning of FRIB in the future.

Before the 500 millicurie source was there it was a little bit difficult to tune and try to reaccelerate beams, but they still were able to make some progress scientifically. So these are showing you those isotopes for which the mass was measured in the Candian Penning trap and below what it's showing you is that the comparison of the masses that were measured for those isotopes relative to the predictions of the best sort of theory going, if all the masses were as predicted, they would all lie along the line at zero. But as you see, they're all basically systematically less bound than predicted. Of course the uncertainty which is indicated by the envelope of the two lines of the theory in each cell is also large. But still significant that as a systematic trend, they're all less bound than predicted. So that's a nice scientific result that happened before the 500 millicurie source was in place.

Moving to Jefferson lab- sometimes people think of Jefferson lab as mainly an electron scattering facility. That's the tool they use, but the science they do touches many other aspects of nuclear physics. And that's what's represented here on this slide. So I'll speak in a moment about PREX; that's an experiment which has implications for the structure of nuclei and neutron skins in heavy nuclei. They certainly are interested in understanding how from a soup of quark and gluons how hadronization takes place, which is relevant for RHIC. They're doing experiments in fundamental forces and symmetries; I would say that's an investment area in Jefferson lab where interest is increasing. Certainly strong theory efforts on quark confinement, and strong technical capabilities in things like pioneering the use of graphical processing units for high speed LSQCD calculations. And of course it goes without saying a great expertise in the worldwide view, I would say in accelerator science and technology. All of those things are features of the capabilities at Jefferson lab.

This is the highlight I mentioned for the PREX experiment. By studying parity violating asymmetry in electron scattering they've observed the neutron radius is greater than the proton radius for lead nuclei, and you see where the data lands compared to two predictions, one of a relativistic mean string theory and one to a nonrelativistic Skyrme model prediction, you see actually the difference is fairly significant, about .3 Fermi. They will do the measurements again and reduce the errors, but already this provides a model independent confirmation that there is a neutron skin on such nuclei, and that has implications for things like the structure of neutron stars and their evolution. This is a very nice result coming from Jefferson lab.

Of course, one of the exciting things about the upgrade that's planned is the construction of a new hall and the tagged photon beam that will be there in Hall D. This shows you the experimental apparatus. This really promises in many people's view a new NP science watershed, particularly the ability to compare the studies of exotic mesons from this detector with the predictions of lattice QCD. And both of those are strengths at Jefferson lab. So I think this is really going to be a real watershed for nuclear science in understanding the strong force.

So the 21st century science questions being addressed at Jefferson lab, the role of gluonic excitations and spectroscopy of light mesons, that's what I just talked about at hall D. Can these excitations elucidate the origin of quark confinement? Where is the missing spin in the nucleon, is there a significant contribution from valence quark orbital angle momentum? Can we reveal a novel landscape of nucleon substructure through measurements of new multidimensional distribution functions, basically trying to head towards a Wigner distribution with a much more complete description of nuclei at all scales? What is the relation between short range nucleon-nucleon correlations, and the partonic structure of nuclei, and can we discover evidence for physics beyond the standard model of particle physics? So the status is that the Office of Science is in the final phases, within a couple years of completing a major upgrade of the CEBAF accelerator that will provide forefront capability and the unique capability in the world to address this science.

Going to the future of nuclear structure and nuclear astrophysics of course, the future of those subfields in the United States is critically dependent on the construction of the facility for rare isotope beams. And it will have a capability second to none in the world. It will increase the number of isotopes with known properties from about 2,000, over the last century, to about 5,000. It will provide world leading capability for research on nuclear structure, nuclear astrophysics, and fundamental symmetries. This design is well along, and this preparation under the leadership of Thomas Glasmacher has gone extremely well. This project is well poised once we understand what the funding will be to move forward.

One of the things that was approved in the Office of Science in the interest of making sure that the project is as positioned as it can be to move forward, once a decision is made, was to allow some earth retention work to take place, and the driving of piles to basically allow the excavation to proceed once a decision is made. So that work is ongoing at this site.

Seen here are the isotopes that are estimated to exist; there was a nice Nature article that came out that had a prediction which was an outcome of a SciDAC award. You see the isotopes that will be new from FRIB in green, and basically the things we know now in dark blue. And basically you see we're increasing our knowledge in this area of physics very significantly.

The 21st century science questions that will be addressed- how did visible matter come into being, how does it evolve, how does subatomic matter organize itself, and what phenomena emerge. Are the fundamental interactions that are basic to the structure of matter fully understood, and basically how can we use this knowledge to benefit our society and mankind.

This is a very important agenda. The FRIB science is going to be transformational. And you see on the left some of the areas again where it will be transformational. I won't go through that in the interest of time, but let me point out that the 2012 decadal study which you will hear about later today from Stuart Freedman also concluded that this science will be transformational.

FRIB will enable experiments in uncharted territory at the limits of nuclear stability. It will provide new isotopes for research related to societal applications, address longstanding questions about the astrophysical origin of the elements and the fundamental symmetries of nature. So again, a very leading capability that is critical to the future of this subfield.

Moving to the Relativistic Heavy Ion Collider, the research focus there is to elucidate the fundamental properties of the perfect liquid discovered in gold-gold collisions, to determine contributions to the proton spin from gluons, quarks and anti-quarks, and to address other scientific targets of discovery. This highlight is a little bit from the past but it's certainly iconic, it's a discovery of anti-helium 4 which everyone suspected was there but no one could possibly measure until the capabilities made available by RHIC, and detectors at RHIC actually did the tour de force to prove the anti-helium 4 matched the expectation based on empiricism and theory.

So this is a brief highlight from RHIC. One of the things that's been a real excitement there is advances in machine capability based on new progress in accelerator science and in particular stochastic cooling. Here you see for the first time the acceleration of uranium beams at RHIC. Here you see in red what would happen if you have no stochastic cooling of the beams, so the beams can blow up if you don't cool them. You see in blue what happens if you do stochastic cooling and in longitudinal, vertical, and horizontal. And basically the difference is night and day, if you're dealing with the red curve, you have real difficulty doing your science. If you're dealing with the blue curve you're cooking with gas. So that was a real important advance.

The other thing that came on line, which occasioned the uranium run was the beginning of operation of the electron beam ion source, EBIS; that was another very important technical event.

The provision of these as often happens when you have a new capability, you get new insights, so one of the important things that's already hinted by the analysis of uranium collisions from the 2012 run is that there's little flow related background to the chiral magnetic effect which is a charged separation which comes perhaps from a local parity violation that's observed in high energy gold-gold collisions. So this lends further support for an interpretation of that event EDM effect. This is something seen in a single event, not averaged over many events. The interpretation of that in terms of hot matter manifestations of excited QCD vacuum fluctuations which are called sphalerons, analogous to the electroweak sphalerons speculated as the source of the baryonic asymmetry of the universe.

The short summary of this, is it's a very interesting observation that's been seen. The experimenters are trying to understand if their interpretation in terms of vacuum fluctuations is correct, and the uranium collisions that were provided by this new capability have put another very important piece of information on the table in sort of lending support to the idea that the effect is real and important and not just an artifact of some kind of experimental background.

Going forward, the science program for RHIC is to determine with precision the detailed properties of this matter, further explore and develop intellectual connections and broader impacts and other subfields, and a search for new discovery such as the postulated critical point in the phase diagram of QCD, and further unfold the spin structure of the nucleon. Those are fairly broad overarching topics. I assure you there's a specific list of things they have in mind specific measurements they want to do which I will not go through because of time but there's a very well developed agenda including what beam species over time they wish to run in carrying out this program of science.

Moving to fundamental symmetries with neutrons and neutrinos, there are selected NP science targets of opportunity with the potential for high impact system using neutrons and neutrinos. These experiments could take on even greater significance going forward based on results of accelerator research in the next few years. It may be that some of these experiments at the precision frontier are best approach to understand what's happened in the very earliest moments of the universe when the energy scales were extremely high.

To the extent there are resources to pursue them and they're complementary to HEP because we're very careful not to overlap with HEP, we will look at pursuing such opportunities based on whatever proposals come our way. There is ongoing neutron EDM R&D underway; our view is that the science goal continues to be very strongly motivated and the R&D continues. We're going to look at the progress after about two years to see whether or not it warrants proceeding with a full experiment, if that's financially feasible. Of course neutrino double beta decay experiments - we have a number of first phases going on; people here are very familiar with that. Ultimately these point towards the need for a tonne scale experiment which would be costly, and so eventually in the Office of Science there will be a down select to the best technology. What that will be, and where it will be of course will be decided on the best value for the science.

At the moment we have a demonstrator project going on at the Sanford underground research facility SURF; you see pictures of the tremendous progress. If you look at the lower right picture, a year ago those people would have been wearing mud boots and miner's helmets. Now they're wearing clean suits in a clean room, so the progress there has just been outstanding. To the extent that in nuclear physics, the Marjorana demonstrator has gone underground and those people are electroforming copper for a cryostat. In high energy physics the LUX experiment has gone underground, and they're almost ready to begin operation to search for dark matter candidates. And they will have the best limit on that for at least a little while.

Nuclear theory spans the entire portfolio and is essential to everything we do. It poses scientific questions that need to be answered, it helps us make the case for and guides the design of new facilities, provides a framework for understanding the measurements we make. One of the things that was new in our office three years ago, or about three years ago, was the establishment of topical collaborations, sort of hubs if you will, of theorists, focused on solving a particular problem in nuclear theory, with a finite amount of resources and a finite time scale to do it. Those have been going well. We're in fact in the middle of a mid-term review of those, which has been going on for the last couple of days. So I'm interested to hear about the outcome and what the review committee thinks about those.

The conclusion, which goes without saying, is maintaining adequate support for robust nuclear theory effort is essential because it essentially underpins everything we do.

The other very important activity in our office is isotope production- providing isotopes and radioisotopes in short supply. It's fair to say this is a very labor-intensive activity which is very important for the nation. Many things that are critical would not be provided except for this program. You see some of the ones that we produce on the left, and some of the companies that use them. And so this is a very important aspect of what we do in our office.

We will always be open to supporting important applications. This one concerns atom trap trace analysis and how that's used to date ancient aquifers. This is a technique that was developed at Argonne National Laboratory, although to keep Mont in his seat I have to point out there's also been cooperation with Jefferson lab which should make it easier to use more manageable samples in order to carrying out the dating activity.

So the conclusion is of course we're all waiting to see what the NSAC subpanel will suggest as far as guidance. You'll hear sort of the status later in the day about where they are.

The future of nuclear science in the United States, at least as far as the support that comes from our office, may not be quite as we envisioned in the 2007 long-range plan depending on how budgets go, but it certainly remains rich with science opportunities.

The United States continues to provide resources for world leadership and discovery science in nuclear physics, tools necessary for scientific and technical advances and strategic investments in tools and research, to provide U.S. scientists with premier research capabilities. And there's no question that nuclear science is going to continue to be an important part of the U.S. science investment in the Office of Science to create new knowledge and technology innovation. I think we have some challenges, but we also appreciate where it counts with the people who make decisions.

**Joshua Klein:** In your language about fundamental symmetries and neutrinos, there seemed to be an implication that this is in large part the domain of high energy physics, and therefore only if things are complementary and only if they're not too expensive ONP would pursue them. But the same argument is actually made from the Office of High Energy Physics, particularly for neutrinos that nuclear physics is doing that, for example neutrinos double beta decay, so we shouldn't fund this. So there's a real potential for some of this compelling physics to fall through the cracks. To what extent is there communication between the offices to ensure that doesn't really happen?

**Timothy Hallman:** I would say there's a very good communication between high energy physics and nuclear physics. We talk to them often. There's a great emphasis in both our offices of being sure that there's no perception with the appropriators that we're somehow duplicating or overlapping because that could be very dangerous for both offices. I think this presents a challenge because in some cases we should collaborate on these things, and both be involved, even though one office might have leadership. It's a little bit difficult for me to imagine that there could be something very important that would fall through the cracks that somehow we wouldn't be talking to each other, but that's not to say there's not this issue of figuring out how to navigate those waters.

An example of where we have made an agreement along these lines is that there's an agreement in the Office of Science and with the Office of High Energy Physics that nuclear physics will be the steward for neutrino-less double beta decay experiments going forward. So even though for example at the moment there is R&D going on for EXO out of high energy physics, in the future when we get to the stage of wanting to build a tonne scale experiment, it will be our responsibility to be the steward of all the possibilities and to bring those together, make the best investment for the nation in terms of location and technique

**Jeff Blackmon:** I wonder if you could say a word about a couple things you didn't mention. One is a possible review of the university programs and the other is I wonder if you could say a bit more about your thoughts about the D&D and Oak Ridge and the time scale and how that will proceed.

**Timothy Hallman:** You'll notice on one of those slides it talked about optimizing core research within constrained budget. So the subtitle there is that we are planning to have a comparative review in the different subfields, but across the entire program of the research efforts at laboratories and universities. We envision the time scale for that to be spring of 2013. It's not unique in the history of nuclear physics, something similar to this was done I believe in 2003 when Peter Paul chaired a panel to do it. But the strategy here is that going forward we should really try to ensure the strongest groups within nuclear physics have the resources they need to do a good job. It shouldn't be the case that we sort of peanut butter the pain across all groups so that everybody is not able to do what they need to do. We should have a better strategy than that. There is a planning activity that's ongoing for D&D at the HRIBF facility which is being managed in the Office of Facilities and Projects under Jehanne Simon-Gillo.

**Zheng-Tian Lu:** So on the RHIC slide you showed the cooling, and the increase of luminosity by a factor of 6, the blue curve versus the red curve. Do I understand correctly that all the science RHIC has achieved so far was done with the machine with the red curve and moving forward that RHIC will operate with a machine in the blue curve?

**Timothy Hallman:** Much of the science up to this point was done before the benefit of stochastic cooling, but the stochastic cooling activity has actually been ongoing for a number of years. They've been gradually over years making advances over the last few years - one year horizontal, one year longitudinal. Going forward, though, they will have the benefit of that capability to cool the beams with longitudinal, horizontal and vertical. So that's a very important thing. I was told anecdotally by Steve Vigdor, who is the AD, that when they started the 2012 run it was very difficult for the experimenters to do anything when they were dealing with the red curve, right? And all of a sudden they turned on the stochastic cooling and all the complaints went away. Because basically everybody was getting all the data they needed during each fill. So it makes a huge difference to the science to have that capability in place.

**Donald Geesaman:** Okay, thanks, Tim, we'll move on to the National Science Foundation.

**Bradley Keister:** Good morning, I'm going to give a fairly brief overview. This will be an update since our March meeting of NSAC. The first slide gives an overview of areas that we normally think of as nuclear physics, though they're not all supported within the nuclear physics program inside NSF; there are sometimes other programs involved. There's a strong overlap with the fields that Tim has already discussed. I should say at the beginning that NSF is a stakeholder in the latest charge to NSAC. We have significant play in those areas with strong university groups and individuals, so the outcome of the NSAC deliberations is of great importance and significance for us as well. So I want to emphasize that.

The pie chart is very similar to what it was last year (FY11), we're just about finished so we have the FY12 numbers now. The numbers are less than last year, and that reflects the fact that many of the investigator programs in the physics division are down about two to three percent compared to where they were in 2011. And the rest of the elements are flat.

So here's a status of the 2013 request, and congressional action so far. At NSF essentially all the research areas except for education are rolled up in a single number – Research and Related Activities (R&RA), and that's what Congress appropriates. What I'll show you in a moment is a breakdown of the requests, so you can see how that breakdown feeds into the single R&RA number. After an appropriation by Congress, we're back to this top-level number. You can see the comparison between FY2011, FY2012, and the FY2013 request. The House and the Senate have both put marks on that, and those are not at the level of the President's request, but they're up from 2012.

We're about to enter a continuing resolution for six months, and it's your guess as to what significance these numbers have when Congress eventually gets to the point of making an appropriation. Here's a breakdown I presented in March. Under R&RA are the request budgets for each NSF directorate. Among the directorate budget changes from the previous year differ by a few percentage points, with the exception of Computer and Information Science and Engineering, up about 9 percent. Within the Mathematical and Physical Sciences (MPS) Directorate there are five divisions. Again you can see differences of a few percentage points across the divisions when compared to FY12. These differences partially reflect the extent to which divisions map onto NSF priorities.

Now the Physics Division request. The yellow bands here refer to areas that overlap with the nuclear physics community. The first is research, that covers all the investigator awards within the total; it's an increase of about 1 percent. The NSCL, which is an NP facility, is flat. At this level NSCL can support a vigorous research program, but cannot operate its new world-class ReA3 accelerator. And we're aware of this and it's an issue. So in addition to investigator programs going down this flat budget here has its own set of implications.

I often talk about a joint program with a domestic nuclear detection office at Homeland Security, in FY 12 that process was cancelled because DHS did not have the funds to launch it. The money comes entirely from the Department of Homeland Security. NSF manages the review process in the first year of each award, and DHS manages the award after that. There are some nuclear physics grantees who have been able to participate in this initiative. Second, the major research instrumentation (MRI) program in which nuclear physics does quite well has been finished this year. There were two awards to universities that were engaged in activities at Jefferson lab, one associated with the upgrade, the other a new, more precise measurement of the proton radius. And another award went to Michigan State for its stopped beam activities. The total was about $1 million which, given the MRI funds available to the Physics Division had, is doing very well for this community. The division had less money for MRI than it had in the past years. If you look back at the previous presentations, you'll see this number is also less than previous years. My final slide concerning personnel is rather busy this year. There's a lot of transition right now at NSF. Within the program itself, Kyungseon Joo finished his two-year appointment. We've been delighted to have him with us, he has now moved on. About a month ago Gail Dodge joined us from ODU, and we're very happy to have her on board, she's coming up to speed very rapidly, and is here at this meeting. Ed Seidel, who was the Assistant Director for Mathematical Physical Science finished his IPA appointment; there's now a search underway for a new AD. In the interim, Celeste Rohlfing who has been the Deputy Assistant Director is now acting as Assistant Director. Finally, in the Physics Division you may be aware that our division director Joe Dehmer has now been made a Senior Advisor for Strategic Planning, reporting to the NSF director. This is not a detailee – he has left the division. There will be a search fairly soon for another director, and in the meantime Denise Caldwell, who is the current Deputy, has become Acting Division Director. Denise is here today - she hasn't come with prepared remarks but she'll be here for the duration of the meeting in case you want to chat with her,.

Denise comes from AMO physics, and handled that program for many years before she became Deputy Division Director. She was also involved with the Physics Frontier Centers from inception, and still maintains an active role with the Centers as Deputy Division Director.

**Donald Geesaman:** We're very happy to have Denise Caldwell with us today, and to be able to show off what NSAC does.

We've seen the Assistant Director position, Seidel's position, seems to rotate every four or five years.

**Bradley Keister:** It's typically an IPA appointment.

**Donald Geesaman:** Joe Dehmer was in the Physics Division Director position for quite some time.

**Bradley Keister:** 14 years.

**Donald Geesaman:** Should we expect that the Physics Division Director would be a longer term appointment or --

**Bradley Keister:** I think the most likely thing if you look around the NSF, it's most likely to be a rotator. The idea of a permanent appointment is not excluded, but there's a very small number of people in the building with a permanent appointment, and I think that number may actually be zero now.

**Denise Caldwell:** Let me just say a couple of words. I did work very closely with Joe. Within the division, we worked together since 2006 when I started as the deputy, and so I would anticipate that at least while I'm acting, the activities of the Physics Division will continue as they would have continued under Joe. That's certainly my intention. Joe and I discussed priorities, we talked about plans for the division, we set the course for the division working together and we'd like to keep that on course. I think it will be very important in the upcoming division director search that the community be very active. A search committee will be given the task of - I guess you might call it twisting arms to get people to apply, to make sure we have a very strong list of candidates when the application process starts. It has not started yet, we're in the process of preparing all the paperwork. But involvement with the community I think would be very important.

I'd also like to say something else about something that Brad mentioned, and that's the overlap with priority areas, and also a reference he made to other funding sources within the NSF. He mentioned the MRI program, and the Physics Division really benefits greatly from the MRI program, because that's where we get a lot of the instrumentation that we're not able to afford otherwise. Now we have set up a mechanism for funding within the division for instrumentation that costs above what a normal research grant can afford. Proposals are reviewed within the scientific program, so it has to be the very best science in the program, and if it was needed to make that science happen, then there could be an additional request made to the division, which is allocated for providing the necessary instrumentation. We do try to look to make sure that you have what you need, particularly in the way of instrumentation. But there are other activities within NSF, priority areas that come out within the budget. Now, Dr. Brinkman talks a lot about energy, and it's certainly true that energy and sustainability is something that is of considerable interest to the present director of the National Science Foundation. And there is no doubt that he'll be investing money in that area, particularly if there's any request -- any increase in the budget over the base. However there are other areas. One notable area is in computational and data enabled science and engineering. The request number for the Computer and Information Science and Engineering (CISE) Directorate contains a portion reserved for "big data," which is of great interest to the administration. But the Physics Division also has a considerable investment in computation. And as activities are announced that may not necessarily be in nuclear physics per se, but the community has the skills and interest to engage in these activities, then they should certainly apply. Our physics at the information frontier contains computational physics, and we do fund nuclear activities within computational physics.

So I would encourage you to think a little bit outside the box, that it doesn't all have to be nuclear physics program per se, but to make connections to other areas of physics and other areas of science. And take advantage, and bring your knowledge, and specific capabilities, to the table where they can make a real intellectual contribution, and be open to doing that. And I'm happy to try to answer any questions anybody has.

**Donald Geesaman:** Thank you very much. Any more questions for NSF?

**Allena Opper:** I wanted to follow up on Bob's comments about IPAs at NSF, because it affects strategic planning. So there's a trend for more and more NSF scientists to be IPAs, so is that coming from the Science Board or -- what?

**Denise Caldwell:** I think it's really difficult to say. In any organization like the NSF, the top management makes the decision as to how they're going to manage the organization. And the types of people they're going to put in various levels of management. At present -- and it has been this way I would say for what, Brad, about the past 10 years?

I should say that I think the trend is not at the program director level. I think that's been about 50-50 permanent and rotator whether it's IPA or visiting scientist. The real trend has been at the division director level and at the AD level where it's almost 100 percent now rotators, that's over the past10 years. So it's sort of like being at the university with all your deans on a temporary rotating position coming from the outside. At the next level down are your faculty chairs. Faculty chairs are like division directors. And so if you look across the NSF, the MPS Directorate currently is looking for three division directors. There's a search for division director for chemistry, materials research, and now a search for division director for physics. The math division director has two more years, I think. And astronomy is on a temporary appointment. And that's pretty true across the foundation. So I think you have to conclude that this is the way that management has decided to run NSF.

**Jeff Blackmon:** So I'd like to make one comment on that since I was on the committee of visitors, this was a point that was raised in the physics division was how -- what a good job we thought Joe was doing, and how having long-term leadership was important, and we raised the concern about this fact. And I see how that was handled.

**Donald Geesaman:** So before we start again with the regular program, DOE and NSF would like to make a thank you announcement. There's someone who has served, and is rotating off, and we want to recognize that service. We understand for all of you, this is, you know, an extra burden and we really appreciate the work that you do for us. So let me hand it over to Brad.

**Bradley Keister:** I'm going to read the citation, and this involves someone for which we bear some of the responsibility for an early transition. The Department of Energy and the National Science Foundation express their appreciation to you, Gail Dodge, for the significant contributions you have made while serving on the Nuclear Science Advisory Committee during 2010 through 2012.

**Timothy Hallman:** Okay, sorry for the brief interlude here. Basically, Don will comment in a few moments also, but this part of the presentation is about a committee of visitors. And so the Office of Science, Dr. Brinkman, has charged NSAC with carrying out of a Committee of Visitors exercise for our office, the Office of Nuclear Physics in the DOE.

The NSF has used Committee of Visitors exercises for quite some time. The DOE began using this as a tool in about 2002. And so basically, it's an opportunity for our office to get feedback from people who come from the community on how we're doing business, whether we're being effective, the outcomes of our work, and the quality, and so some of those things are reflected in the charge here. The panel should assess the operations of the office programs during the fiscal year 2010 to 2012. They can examine files from this period for all actions administered by the program under review, including funding at national laboratories, universities and other activities handled by the program. They should consider things like the efficacy and quality of the processes used to solicit, review, recommend, monitor and document applications, proposals, award actions, and the quality of the resulting portfolio, including its breadth and depth, and its national and international standing.

The status is that NSAC has received this charge. Don will tell you more about what he's done since then, in terms of carrying out the charge. And as well as when the time for the charge will be, which is next January, he will say in detail. And I think that's essentially what I wanted to convey in the file that apparently didn't make it. But the only other comment I think I wanted to make is that this is a well-defined process in the Office of Science, the report comes from the director, it goes back to the director, and it's posted on the website for the Office of Science, and along with an action plan, if you will, from the Associate Director on how the office plans to try and address things that have been identified or commented by the COV. So actually, the last one took place in 2010, and what was on one of my slides, but you can find it easily if you go to our website and look under NSAC reports and charges, is the last COV. And what you will also find there is the action plan that I provided at that time on how we were going to sort of address the comments from the COV. The other thing that's a requirement is that prior to this upcoming COV I will be sending to the COV Committee and Don a report card, if you will, on how well we've done on the action plan that we identified. And so those are all steps that happen as part of this process. So I think Don will tell you a bit more detail about, you know, the logistics and the committee, but I can take questions if there are any. I should say this is all part of the Office of Science's interest in making sure that it does a good job in carrying out its mission and serving the community well.

**Donald Geesaman:** Okay, so this is I think the fourth time we've gone through this exercise, so are there any questions from NSAC about what the intent of the charge is? And then I'll address how we plan to address the charge, and then you can ask me and Tim more questions.

**Raju Venugopalan:** Is the report from last time available?

**Timothy Hallman:** The report is on the Office of Science web, the report of the committee is there as well as my response to the report, which is required by my director on our action plan of steps we're going to take in response to the COV. And what additionally will be put there, before the COV actually takes place, is another statement from me on the progress we've made towards the goals, and where there's still work to do.

**Donald Geesaman:** Okay, so there's two things that we should discuss. When we received this charge in July, I sent it to all the NSAC members, I asked them for suggestions for committee membership. The most important thing was to identify a chair because that helped set the time scale of when the committee would meet, and John Harris, who served on the 2010 Committee of Visitors has agreed to be the chair. And the charge suggests that we should have a site visit in early January, and based on his schedule, it's going to be during the week of January 7th to the 11th. So with those dates in mind, with your suggestions, with discussions with Tim and with John we started putting a committee together so we laid out a strawman committee. So far, these people have all agreed to serve.

We were going to add about six or seven more names, so there's certainly still time for input. One of the things we're clearly missing is accelerator expertise, accelerator operations expertise. That's not because we haven't asked people yet, but because they haven't responded. That's the first order of business, which is the membership of the committee. Second is we need to provide a letter transmitting the charge from NSAC to the Committee of Visitors, which is an NSAC subcommittee. And they, as you see here, the first committee business meeting takes place during January 2013, the results of this assessment should be documented in a report, and the report should be submitted to NSAC by March 15$^{th}$. So this is one where it doesn't say that NSAC has to accept the report and transmit it to the agencies by March 15$^{th}$ so I think we have some flexibility in the schedule in the draft charge to the subcommittee I ask them to have the report ready so that NSAC has sufficient time to review it before March 15th. But I don't think we need to specify an actual date. But if you think we should, I'm happy to change it. So I sent to NSAC a draft charge from NSAC to the Committee of Visitors, which is boldly plagiarized from previous letters of transmittal to the Chair of the Committee of Visitors. It basically just parrots the text in the charge, provide an assessment of the processes used to solicit, review, recommend and document proposal actions, and monitor active projects and programs, review laboratory and university programs. It asked for John to serve as Chair, and to report back to NSAC. So I said I've asked them to report -- to submit the report to NSAC -- the charge says to submit the report to NSAC by March 15th. The site visit will take place on that week[January 7]. We anticipate scheduling the NSAC meeting in March, and I want to make sure NSAC gets the report in sufficient time they can give it the proper deliberations. Certainly the last time in 2010 NSAC received the report, and as a result of NSAC discussions there were some changes made to the report before the final report was submitted.

The report will need to be sent to me for distribution to NSAC in sufficient time before the NSAC meeting to ensure NSAC membership has time to read and reflect on your report. And then it thanks the subcommittee, and mentions that I get to serve on this and join in the pleasure. So were there any suggestions that NSAC members had for this?

**Jorge Piekarewicz:** I have one. I'm looking at a document that you mentioned, and the first recommendation from 2010 says consistent with the recommendation of the 2007 COV it's imperative that nuclear physics immediately establish a database that can be used to track relevant proposal and grant information, which I think would be very useful for the committee, and I don't know if you want to comment. What's the status?

**Timothy Hallman:** Thank you for the question. So there was a response to that, which is also on the website, but the short summary is that there's a new software called PAMS, which stands for portfolio analysis and management system that has been in development now for several years, and it's viewed as the instrument which will speak to that recommendation and do many, many things. It's in the early phases of deployment and testing. If you speak with George Fai in our office, who is the liaison to that effort who is here today, he can give you a more detailed -- probably more detailed than you want, status of where it's at. But I think it's fair to say that within a reasonably short outlook, perhaps another year or a little bit more, it should be fully functional. And I think that recommendation will be largely satisfied. So it wasn't unique to nuclear physics that that came, all the programs have been getting that same recommendation, so there's sort of a unified solution that's been put in place. Or is being put in place.

**Donald Geesaman:** Okay, any other comment on this letter transmitting the charge to the subcommittee? Do I have a recommendation that NSAC approves this letter? --

Unidentified NSAC member: So moved.

**Donald Geesaman:** Is it seconded?

Unidentified NSAC member: Yes, seconded.

**Donald Geesaman:** Okay, all in favor raise their hand. All opposed? Okay. So I will sign this letter and transmit it to John Harris. Your question is the first of many that I think we will be asking very hard to the office in terms of the recommendations because there were a number of significant recommendations in the last one. One of the things they have acted on was the request that the office pay for the travel for NSAC committees and subcommittees, and so we've all benefited from that very directly over the past few years.

**Timothy Hallman:** Yes, just in the spirit of that comment, Don, I would say this is a very productive exercise, that we do listen to the guidance carefully and take it very seriously and appreciate, the work that the committee puts in to give us advice. So it's a serious thing, and we take it seriously.

**Donald Geesaman:** Right, and we really welcome the opportunity to state it. Again, if you have any additional suggestions for membership there are about seven more people who will be invited.

I want to remind, if there's anybody in the audience who would like to present something for public comment, if they could let me know in advance. We have a break coming up at 3:30, so if you want to let me know then. So very much we'd appreciate it.

So as we've heard this morning, we've had this lovely decadal review of nuclear physics that was led by Stuart Freedman and Ani Aprahamian, and that report is available and is a very important input to the exercise we're doing now on implementing the long range plan. So we've asked Stuart to discuss the report. And thank you for coming, Stuart.

**Stuart Freedman:** Thanks for having me. I've been asked to tell you about our report. Unfortunately it's given the name decadal review, and it has the number 2010. And as you might have noticed, it's now 2012. But I have an explanation for that.

This is a report from the National Academy, the NRC provides these reports. Now it's become traditional that many of the subfields of physics have reports like this; roughly, and you'll see what we mean by roughly, on a 10 year period.

These are used differently by the different areas of physics. You might know that for example astronomy uses this report in a large way to set their priorities. In nuclear physics, that hasn't been the case. We have a long range plan, and I'll speak to that because that's part of the charge of this committee, and that's where most of the priorities of the field are usually set.

The committee is made up of people that you probably recognize. These are experts in the various areas, diverse areas of nuclear physics. It wasn't necessary to have a lot of input beyond the committee. We had four meetings, and we focused on presentations that supplemented some of the expertise that we already had in the group.

We also had people outside of nuclear physics, Cherry Murray is a renowned condensed matter physicist and Tom Ruth is in isotopes, which is a new mission for the Department of Energy nuclear physics. And the rest of the people are from the various laboratories and universities in our field.

This is the fourth in a series of these things. The first time this was done, it captured most of the subfields in a few volumes done all at once, in 1972, and then you'll see 14 years later there was a specific volume for nuclear physics, and the following three volumes used that method so that what was here is now divided up into the subfields.

And this is the latest version; this I believe will be the official cover. You can ask questions about that if you like.

I was amused to hear my young colleague, Josh Klein, mention this guy. This is a picture of Rutherford at an age younger than Josh is now. I was pleased to hear from Josh that he didn't go into improving steam engines, but instead, this unlikely young man entered physics, showed himself to be quite good at it, and made a discovery which is the basis of the field that now survived after 100 years.

And when this report was written it was basically 100 years after Rutherford. What he did was really extraordinary. But I had trouble during this group because I wanted to write a lot about Rutherford in our report, and I was prevented from doing that by the younger members of the committee. I'm sorry that Josh wasn't with us, and so that's unfortunately taken out.

The report is filled with lots of words, this is a collection of them. The size indicates the number of times it's been used. It's not surprising that nuclear is prominent, but if you look closely there's words like quarks, QCD, gluons, which represent how this field has changed and evolved in time.

This is our statement of task. The NRC takes this very seriously. You can read it for yourself, but I can paraphrase it. The first part of it is to assess the long range plan that was in 2007. We began our work at the end of 2010. So we looked at the long-range plan in the context of it having existed for awhile. And evaluated whether it still made sense in the context of the time we were looking at it.

The second phase is to look broadly and more globally to see how nuclear physics is fitting into the international framework and also, to see if we could find any issues that should be addressed in looking forward in the field.

Later I'll talk a little bit about the budget, that's obviously something that this committee is concerned about. But I should say at the time we received our statement of task, we weren't asked to look at the budget. At that time the budget looked like it would sustain most of the recommendations that had been addressed in the long-range plan.

This is the long-range plan of course, and I don't have to tell you it started seven years after the first NRC study of nuclear physics. And it's continued about every five years since then.

This is a very different process; it's a bottoms-up process involving pretty much the whole community. And again, our committee was very impressed with the method used here in establishing priorities in nuclear physics. We think it serves the field well.

There is always some concern because NSAC advises the funding agencies, that they may influence what this group -- these reports say. But we found none of that, we think that this really represents the community.

I don't have to tell you that presently there's four national user facilities in nuclear physics. Approximately 40 percent of the users are from foreign countries. And while these are national facilities, it's obvious that there's an international context for what they do. And it's an electron accelerator, CEBAF; heavy ion collider, RHIC; the stable ion, heavy ion machine, ATLAS; and NSCL, higher energy heavy ion machine.

This is a transparency I lifted from Tim. I think it makes a clear impact. We -- Tim uses his to make an argument that facilities in nuclear physics come, and when they've been used and we see that the value of keeping them running is not there any longer, we move on to other facilities.

If you look back in, oh, around the year 2000, there's one, two, three, four, five, six, seven, eight operating facilities. If we look now, there are four, including the -- this is obviously done on Tim's plot, the NSF supported laboratory at MSU.

The point is that decisions are made, facilities come and go, and basically these decisions are made by a process that normally involves the whole community.

This is to indicate some opportunities that we're not taking, so the point is that we find that decisions are being made predominantly for these larger facilities.

This is a summary of what the four existing national facilities, national user facilities, do. You can see they have a large user base, 40 percent foreign, and these are all for the most part right now very productive laboratories, some of them are getting upgraded, as we'll see.

Now unfortunately this gets very busy because I've tried to update it. It's from Nagamiya originally, who advised us about the foreign contacts of major accelerators in the world, and you see that the United States provides complementary facilities to complement what's going on in the rest of the world. And this isn't complete, I see I left out ATLAS.

But a major electron machine, heavy ion collider, one of the only two such facilities that are operating. And it's also clear, in looking at the demographics U.S. scientists use these international facilities to complement their research programs, and foreign users come to use ours. Seems like a healthy situation.

This is unfortunately not complete, too, it doesn't do justice to NSF, but it shows the university laboratories and funded groups in addition to the larger facilities. There are still in nuclear physics a number of small universities that have in-house facilities.

At one time, and looking around the room obviously there's enough gray here to make me realize people can remember when it was very common for U.S. nuclear physics laboratories to have facilities at their universities.

And that's no longer the case. But there are still some facilities, this is a list of some of them, at universities. Recently they've formed a consortium to try to cooperate for running these. We found, and you'll see in our recommendations, that this is very healthy for the field and should be encouraged. That this provides students and researchers at those universities with infrastructures that are very important for the health of the field.

And also, it's important for the health of the larger facilities, because it allows these smaller groups to have the facilities in-house for building equipment, substantial equipment for the laboratories.

And of course, it also supports the feature by providing a place for students to get engaged in nuclear physics. It's very good for recruiting, obviously. One of the problems we have in this field is when we move the facilities out of the universities, people have to go to other places to do their research. For students, it's a difficulty in recruiting.

And we see that some of our sister fields like atomic physics benefits a lot by having laboratories in their departments.

This is the layout of chapter two, which addresses the science topics of nuclear physics. And this chapter, if you look at the previous reports, has changed a lot. The emphasis on what is important now is not the same as it was.

The structure of the atomic nucleus is still an important aspect of nuclear physics, and by this we mean nuclear structure, the description of the nucleus is a collection predominantly of neutrons and protons. It's what most of our colleagues may think that nuclear physics is all about.

But it's actually about a lot more. It has a large component of nuclear astrophysics. This builds a lot on what can be done with this description of nuclei. So this is a great user of the knowledge that comes out of this particular direction.

But in addition, QCD is now an intimate part of nuclear physics, and studies predominantly at the heavy ion collider are directed toward understanding the nature of matter at extreme conditions. And we'll see that, as you know, great discoveries have been made in this area, and now looking at matter as a collection of quarks and gluons, and not nucleons.

The core structure of the nucleon is another aspect which has emerged and has a great deal of activity. That's for understanding QCD. We have ironically a theory for the description of nuclear matter which no one seriously disbelieves. On the other hand, the theory, it turns out, is so complicated it's hard to extract the physics. That's in some sense depressing, on the other hand it's very interesting and a miraculous challenge. For understanding really the nature of the matter that's most important to us, it's what we're made of and what we buy and sell at the convenience store.

Fundamental symmetries is an area of nuclear physics that borders on other fields in large part with particle physics, but also cosmology. And this is an area which really gets a bang for the buck by applying the techniques of nuclear physics to this particular area, and there's been great advances in this. There are also applications, and we look a lot at the applications.

This is a -- I'm not sure if that's the politically incorrect word -- curiosity-driven science. Discovery science. But there are applications. Another example, that when you do this kind of research, the applications come out.

So we looked at the matrix of this was an interesting part of the study, the major accomplishments since 1999 and they're significant, but on the other hand too large a list is not appropriate. The three top items on the list is the discovery of what was thought to be the quark gluon plasma, but it's better described as a nearly perfect fluid that exists in relativistic heavy ion collisions. Presumably it's the same kind of matter that existed right after the big bang.

The precision determination of electric and magnetic form factors, these are exquisitely precise experiments which map out, in much the same way that Rutherford tried to do with the atomic nucleus as part of the atom, the parts of the nucleon. And these, what we're learning with precision, are things that need to be explained by QCD.

Finally, and this is not an ordered list, I find this is a transformational discovery, the final resolution of the solar neutrino problem. And that's associated of course with neutrino masses and mixing. This is a plot you've all seen before, it measures how perfect a fluid is. I had one version of this, I'm not sure if Bill did it, but it had beer on the plot but I forgot where it goes. Maybe you know, Tim. An imperfection index. So a perfect fluid, something that shows no viscosity, would be down here.

There seems to be a limit on how perfect it could be, and what RHIC is discovering is a kind of matter that nearly meets this limit. This is a quantum limit. This is a very interesting substance. It's a liquid, it defies description in terms of particles. It's really a quantum phenomena. It's not unique in the sense that there are other types of matter that exhibits these kind of properties, and cold atoms are one of the examples of this. But this is revitalizing interest in this kind of a liquid, and it's kind of ironic that it comes from studying the substance that existed at the big bang.

This is data from JLab. Unfortunately, I didn't take the nice plot that I saw that Bob plotted. We used this one, which is a little confusing. But what it's saying is before 2000 it was thought that the magnetic and electric form factors were the same. And better measurement showed that there's a great difference between the magnetic and electric structure of the nucleon. And again, that's an important fact of reality that needs to be accounted for, QCD.

And again, the solar neutrino problem was solved. This ends a quest that began in the 1930s for understanding what is the mechanism that powers the sun. The idea was it was nuclear reactions. We now know that it's true, the sun is powered by nuclear reactions. In the course of finding that out, we discovered that the neutrino is not as simple as we had thought. They're not massless, they have mass, and they mix in a way that's very similar to the way the quarks mix with each other.

This are results from SNO and KAMland, which puts precise measurements of the difference in the mass squares of the neutrino mass eigen states, as well as numbers that correspond to the mixing. And it was really these investigations which solved the long standing solar neutrino problem, the observation back in the '60s that the number of neutrinos coming from the sun didn't agree with the theory of the mechanism for the sun's heat. It comes from nuclear reaction.

And this is a plot I like, showing basically if you sat in the rest frame of a neutrino, this would be a proxy for the rest frame, upper time, and what you're watching is a neutrino oscillating between up here at 1 it's an electron antineutrino, and down here it's not an electron antineutrino, and you're seeing it oscillate in time. Which is what you mean by neutrino oscillations. And here you can see it, as close as we can now do it as it would look if you were just sitting here watching a neutrino.

This is a major accomplishment of nuclear astrophysics, and this really is transformational. We understand the sun to a few percent, but that's extraordinary.

We know this is the basic reaction that powers the sun, and now we're looking for smaller components, other nuclear chains, that also provide some energy from the sun. And of course, you've seen this probably many times, that's a picture of the sun as it would look if you could see neutrinos.

Progress in nuclear structure and nuclear astrophysics increases, this is in some sense incremental. The object here is to understand the origin of the mass that surrounds us. Where did it all come from. Much of it come from supernova, astrophysical events, mapping this out involves a rigorous process of categorizing nuclear reactions and understanding the nuclear physics that goes on.

The progress has been quite impressive. We're in a much better stage now in understanding these processes.

Some of the old issues in nuclear physics are being clarified. For a long time it was expected there would be an island of stability based on the old models of nuclear structure, with some new insights. And now it seems like that is beginning to be discovered, in very precise and difficult experiments in which you deal with perhaps a few atoms after waiting for a few months.

Now, there also are applications, and this one you've seen before. This is Zheng-Tian's work. He used techniques that were originally employed by nuclear physics, to do experiments to study beta decay with radioactive nuclei. And he saw he could use it as a dating tool, it's a miraculous data tool, and by picking specific atoms here, krypton 81, he's able to go out into the field with his colleagues and determine the ages of aquifers in the Sierra desert.

This is a graph indicating I think what his next mission is to understand the flows of water, the Atlantic conveyor, is that what it's called?

This is a method that has a lot to do with the climate in the world. It apparently has about as much to do with the atmosphere, so it's an important issue. And to do this you have to do some very difficult nuclear physics to be able to detect this nucleus, which is a quarter of a million years -- quarter of a thousand? 240 --

The half life is 270 years, and the --

This process is a thousand years, is that right?

**Zheng--Tian Lu:** Yes.

**Stuart Freedman:** There's new and improved imaging techniques. Maybe you're not aware there's a revolution in neuroscience, and that we're now starting to understand how the brain actually works. And the way we understand it is by these techniques of imaging. A lot of this is MRI, early MRI was contributed by mostly by nuclear physicists, despite what high energy physicists say. It was applied mostly for finding moments of radioactive nuclei or nuclei, stable nuclei.

These techniques are teaching us how we think. There's a biological basis for that. Which I guess means how much you're enjoying this depends on what you had for breakfast. Also combining with computers various techniques of imaging, it's an absolutely amazing tool now for medical diagnosis.

And these tools still rely in a large way on the technology which is coming out of the basic science of nuclear physics.

I put this in because I think it shows something that impressed us. This is that there are capabilities in the field of nuclear physics, and this was one that emerged after the Fukujima reactor event. This shows the radioactive plume from the Daiichi plant. But what was recognized almost immediately was people carrying out sensitive double beta decay experiments with underground detectors with high capabilities for seeing very small amounts of materials could identify the fission products which could only come from a reactor all over the world. This is a measurement showing the signal from Fukushima at Seattle, but these were measured almost everywhere.

Taught us a lot about how the wind blows, and a lot about how you could diagnose, in fact, how the event occurred. You could see the pulse of this reactor failure moving around the earth several times.

Okay, unfortunately I can't write the statement of task again, but this was what we were supposed to do. Scientific rationale and objectives of nuclear physics. That was covered primarily in the science part of the report.

Then to develop a long range strategy, we did that, we think, by making some conclusions and recommendations which I'll now tell you about.

First we were instructed that we should take a look at the 2007 long range plan from a perspective of three or four years later. This was the recommendations ordered in the long range plan, four recommendations. The first one is to complete the 12 GeV upgrade of CEBAF, something that is now nearly completed. The second recommendation argued for the construction of what is called the facility for rare isotope beams. Some people say FRIB. That's actually correct. And this was, at that time, was identified as something that would increase, this capability, provide all kind of possibilities for scientific discovery in the United States.

Third recommendation had to do primarily with the underground laboratory. At the time that this recommendation was made it was thought that there would be a laboratory called DUSEL, it would be funded by the National Science Foundation. But the recommendation basically identifies the science that's done at these kinds of facilities, that this has been very important, most of the discoveries. And at the time nearly all the discoveries, important discoveries in neutrino physics, the new discoveries had come from these underground laboratories. And unfortunately, most of them are not in the United States. And this was thought to be a good move for United States science.

And the third -- fourth recommendation was an upgrade that had been considered and hadn't begun of the relativistic heavy ion collider called RHIC 2.

We discussed these recommendations for a long time, and we came to a conclusion about what we would recommend.

What we saw that made sense, the upgrade to CEBAF is nearly completed. The upgrade to RHIC, which was not expected to happen so quickly, just happened. And they managed to do this mostly out of their operating budget, and as we know, primarily because of innovations that they discovered in beam cooling that made this a remarkably more useful facility. We recognized this and at the other facilities that had been upgraded, and we recommended that a good idea, good conclusion, would be to exploit these strategic investments.

It seems very sad to upgrade a facility and then turn it off. I'm told that that's not so unusual in other areas of government, but it's still sad to see it happen.

This is RHIC. This is when it had four experiments. And this is a slide I think that comes from Tim showing the RHIC program. There's upgrades to PHENIX and STAR, mostly completed, I believe. Increase in luminosity. This involves lots of foreign users, we provide them with an agreement that they'll be able to come and research. There are also other opportunities in the world at CERN, at FAIR, and GSI. And our community also used that.

This is a picture of CEBAF, slightly poor color, and this describes in some generality what the upgrade would be. It goes to 12 GeV, introduces a new hall. This has gone very well, as far as I understand it. The project is on -- I saw nodding -- it's on track.

And I should say also this facility provides us with a lot of the technology base in nuclear physics. I think nuclear physics in the United States is not properly appreciated for their contributions to accelerator physics, and that's something we saw very clearly from this, and CEBAF deserves a lot of credit for developing this expertise. Which is now world-famous, people go there for help with their accelerators.

The rare isotope beam facility is one of our recommendations just as it was for the long range plan. This has moved through most of the process for developing it. At the time of the long range plan the site had not been selected, it has now, it will be built at MSU. And our recommendation in this case is that that project should be moved ahead, as expeditiously as possible. So that the benefits of this facility will be available to the U.S. community and to the world. And this is just a picture you've seen before about what are going on -- what's going on at MSU.

And it seems like as near as you can tell that the process is moving ahead, it's being done under a cooperative agreement, because it's at a university, but it's being carried out with the procedures that are developed for the LHC, DOE, and I think everybody is happy with that.

Now underground science in the United States- it's clear right now that the situation with DUSEL had changed significantly, and we discussed this. What we affirm here is this still maintains a very high profile. There's lots of important work done in this field. A large part of our nuclear community is engaged in this kind of work.

We suggested that there be a clear implemented targeted program of underground science. The field is a little more advanced than it was at the beginning. Many of the major discoveries now lead us into the right direction for new underground experiments. For example, the search for neutrino with double beta decay is now a clearly high priority in the next experiments to do. So it's a little bit more of an area where targeted experiments make sense.

I love to talk about this field because most of the discoveries were made by mistake. The first observation of neutrino oscillations in atmospheric neutrinos was made by people who really wanted to see proton decay. So a lot of this was unexpected.

When Ray gave his look for neutrinos from the sun, he never imagined that he would find out that neutrinos coming from the sun were completely different animals than people had suspected.

But now it's clear, or mostly clear unless there's a new surprise which is always possible, that there's well defined experiments to do finding out whether CP is violated, whether the neutrino is its own antiparticle, and so forth. And also the object of our [unintelligible] has something to say about that. But this speaks to some extent by the boundaries of our field. Nuclear physics has a lot to bring to that area of investigation.

It's basically at least we think now a nuclear process using techniques that are important to nuclear physics, and it's not unusual that experiments designed to look for, say, double beta decay will also have -- can make important contributions to the search for dark matter.

Now, that's not a scientific problem, unfortunately that's a bureaucratic problem, but we encourage the agencies to deal with that. You wouldn't want to make -- discover something you weren't funded to discover, but.

We saw nuclear physics in exascale computing, it's a new opportunity, something that nuclear physicists should take advantage of. The community is not asking for new computers, they just want to be involved in this revolution. Getting time on existing computers, establishing infrastructures that would allow them to work together with computer professionals and program professionals to really adapt this kind of power to the problems of nuclear physics.

We spent a lot of time discussing nuclear physics at universities. We see it as in some sense endangered. The number from the statistics we can get is that actually it's not declining, but many universities no longer do nuclear physics. It was very hard to get the statistics correctly. The number of Ph.D.s graduated, has stayed pretty much constant or possibly even increasing slightly, but somehow we have the feeling that the profile of nuclear physics at university deserves some encouragement and support. This is where the future of the field comes from.

One of the challenges is incorporating a strong university program at the same time that you have a strong facilities program. And the problem, of course, is the operating cost of the facilities continues to grow, and that can take away from the research program.

And this is we find a daunting pie chart where we find facilities operations I'm not sure this is the most accurate, is creeping up above the 50 percent of the activities in the field. Now, that's a challenge, these large facilities are necessary to carry out the science, but we also have to recognize that the long-term health of the field is also necessary to carry out the science.

We made a bunch of specific recommendations, eight or so of them, that didn't seem to cost very much money but are worth considering. Special fellowships, national prizes to recognize nuclear physicists. The intent of course is to set the profile so that these young people can get tenured academic positions at universities and set up programs.

These are the eight other programs. We find a lot of things that were going on are very good, the NSF, REU and CEU program, that should be continued, and enhanced if at all possible.

Competitive awards for shared research and instrumentation -- the NSF is doing a lot of that --the DOE might also benefit from doing this. Trying to put the infrastructure, the capability, of doing important experiments -- or conceiving important new detectors and experimental apparatus and putting that into the university so that it can be -- that students and post docs and faculty can participate in this development.

This is an issue that came up during our investigations of our international colleagues, and physics is a cooperative venture, it's one of the best. You get a bunch of physicists together from different countries and that all goes away, they focus on the physics. But it's also competitive. And that's part of the charm of doing it.

Getting to the answer first is part of what we do, and making the U.S. program vital means sometimes us winning the race and getting to the answer.

And to do that, we should do things to keep us nimble, to be able to change direction quickly when a new idea occurs, and often this is -- puts risk in. Most new ideas don't work. I know -- well, that's a categorist statement, certainly most of my ideas don't work. The United States needs to be innovative. We see a little concern for the processes, particularly at the Department of Energy, that seem to slow down this activity.

Now, these are very hard judgments to make, but we used to talk before I can remember that we should take some risks. When this came up, of course, the people we saw as taking the most risk were the Japanese. They had a very risky program, and they scored some big wins with it. So we'd like to be just like them. But of course, there's also big losses.

And then of course the new machine on the horizon, the only one that really came up during our course of this report, was an electron ion collider. This -- and we, in our finding, looked at this as an upgrade of one of our existing facilities. Obviously either RHIC or CEBAF, in that context it seemed to make sense, although we haven't done a evaluation of what the cost would be and what the benefits and so forth. But that seemed to make sense that that should be investigated.

These are the people who -- the supporters of this were asked to talk to us, and they made arguments about why this is really the next place to go, in QCD. And what we recommend is that there should be some investment in this to see if this pans out. If the scientific case develops, then it probably should be considered.

And this is, I think, the most current two graphs of the major facilities that are angling for this in their future.

Now, this is one of Tim's plots. And during the course of this I'd like to say that while we were doing our deliberations the ground fell away. And at about this point there were discussions about whether or not the nuclear physics program would be sustainable with the budget profile that was looking like this, instead of like this. And I must say that this delayed our report by a great amount.

We were not charged to consider these things. And in the end, we decided that it's really not our purview to do that.

And I'm pleased to see that NSAC was allowed to form a subcommittee that will look more carefully into this. But this certainly changes the framework.

From our point of view, with the ground rules we were given, the program that was projected made sense. It was affordable. There was a little concern, and still is, about managing the balance between universities and laboratories. And in terms of what we thought was the important thing to address, that needed to be addressed before damage was done to the profile. But pretty much the number of major facilities had been turned down to four, they were all operating well, they had good programs. The universities were struggling a bit, but it looked like without a lot of extra effort or rebalancing, things could be fixed.

But this of course is -- if real, requires a much more major consequence.

And that's where I leave it to NSAC. Thank you.

**Donald Geesaman:** Okay, thank you very much, Stuart. I certainly want to compliment the job that that your committee did, I think the exposition of the science in your report is really outstanding, and it really represents the field very well.

So are there questions from NSAC?

**Raju Venugopalan:** So I read parts of the report, I can't claim I read all of it, and it really read well and I congratulate you on that. So I have a question with regard to the relation to the past long range plan. So it seemed like you endorsed, you know, very significant aspects of the long range plan. Were this any elements that were at significant variance with respect to that?

**Stuart Freedman:** I think what we said about the underground laboratory was of course different than in the long range plan.

The underground laboratory at the time of the long range plan, I don't think nuclear physics thought they would manage and support the lab, it was just too big.

What will come out of the underground laboratory now is very much in question, and what we tried to highlight was the science, and I think, I mean, there's various ways you could be concerned. If the facility becomes the center piece and that becomes more important than the science and prevents good experiments being done at other underground laboratories, that wouldn't be a good outcome. So how that plays out is important.

What we endorse is that science, it's right at a time in its evolution where there's probably some more discoveries to be made that will be important.

And it may be I gave the case that I know what we're doing now in neutrino physics and that may not be the case, maybe we're missing something completely.

**Allena Opper:** Like Raju, I haven't read the report fully. Could you summarize the main points of what you discussed on measurements of fundamental symmetry that take place above ground?

**Stuart Freedman:** Yes, we talked about all the ongoing programs in that area, I didn't highlight that.

There are results coming out. And what is interesting is that this community is embarked on some of the most important problems. The electric dipole moment, for example, with several approaches. And this is a transformational discovery.

There's good reason to believe the electric dipole moment is bigger than the standard model predicts, and the people who are carrying the ball on this pretty much in the United States is this community, both with neutrons and more exotic atoms. To try and discover this.

This is a discovery that it may turn out, sadly, like the Higgs, that it's just the standard model. But that would be important to know, as well. And we have another reason to understand why we're all able to sit in the room simultaneously.

**Jorge Piekarewicz:** So you started commenting on having international colleagues in the committee. So how do you see nuclear physics evolving in other countries relative to where we're going now? What can we learn about that?

**Stuart Freedman:** We discussed this, and we have no recommendations on it. There are some clear observations. A lot of the work that's done, in particular in certain fields, is done abroad. Using other people's facilities, I think we'll probably see more of that when J-PARC gets running in Japan that's kind of a unique facility there's lots of interest in the United States of doing experiments at J-PARC.

That's when we behave as users in foreign facilities, and I think, that method is working out okay.

When we're involved more at something that's not at an accelerator, not an established facility, then, for example, underground science, where we have to come and help build some large apparatus, there are glitches in the way we behave on the international scene. And quite simply, one of the problems is the way we do business. We do it year-to-year. And so it's hard to make long-term commitments from our side.

And that puts us at a disadvantage in foreign collaboration. This is not unknown. We've discussed this, but nobody was able to come up with a way to fix this, other than to change the way the Congress operates. But that didn't seem possible.

I think as time goes on there will be more, not less, international collaboration. That's the way science is going. Some of these experiments are just too expensive for one country. And in fact, they require too much expertise for one country, too. So we'll be doing more and more of this kind of work internationally.

And also, just on a day-to-day basis, it's as easy to collaborate with somebody in a foreign country as it is with a guy down the hall. The only difference is you'll have to manage your chat, if they're sleeping. But that can be easily fixed, so it's -- because of technology, this field is truly international. It's something we need to pay attention to.

And being better partners in experiments is something we need to work on under the constraints that we have, because of the way we operate.

**Robert McKeown:** So Stuart, one of the ways that this report is different from a long range plan is the audience. It gets disseminated to a rather different set of people. I know you briefed PCAST, for example. And I don't know where else, you've probably done similar things to other bodies. I'm just wondering, can you give us any perspective on the reactions you're getting from outside the field?

**Stuart Freedman:** Okay, the Academy believes that this is an important thing. And I asked those kind of questions at the Academy. You might ask why we need this. We have a long-range plan, and that attention is paid to these reports, because it's felt that it's an arm's length evaluation of the field, even though there's people involved in the field that -- and mainly because, no offense to the agencies, because the agencies have nothing to do with the process.

There is a perception that all the agency wants to do is build big things. And that all of this science stuff is just to promote these big things. And this mitigates that to some extent. It's supposed to be basically at least the community clearly not coerced in any way.

And what we find is that the conclusions are pretty much the same as the long range plan, and I think that's one of the reasons that it's a healthy process in the long range plan. But I don't think there's general appreciation for what goes on in nuclear physics, which is in many ways I think much better than the way priorities are set in other fields.

**Jeff Blackmon:** Sort of along those lines, there's also a nice video that Ani put together, and I guess maybe following up on that, are there ways to disseminate this? Do you want to comment on that effort?

**Stuart Freedman:** The video is available. If you clicked Youtube, the heart of the matter and you can play it on your computer. It's a very high quality piece. I'll confess that I was not for it. Ani pushed this through, she was right. It served the purpose it was intended, and I guess Konrad had obviously a lot to do with this. He identified this guy who made this. What I've learned from this is if you're going to do it, you need to do it right. The academy makes lots of these kind of things, and boy, are they awful. It was very difficult to get them to go along with this very professional thing.

Now, it was not cheap, compared to what the academy did, it seemed very expensive. But in the end, it was obviously worth it, and it has had a lot of impact. I encourage you to go look at it, it's a very high quality stuff. It's what you see on Nova or some of these very well produced science shows.

**Zheng-Tian Lu:** Stuart I really like this recommendation of establishing various fellowships to identify and encourage the best people. I know AMO the NRC fellowship is very successful. But it does have limitations; it's only for U.S. citizens. I hope the new fellowship programs will be open to international students and post doc as well. I share your view that our science is getting more international, and also we want to recruit the best people, not just from the U.S., but from the world.

**Stuart Freedman:** We have a great scientific enterprise in the United States, but in fact it began because of people leaving Europe because of the war, and that's where the U.S. science was established. There's a nice figure on the back of the latest or the one before, Scientific America, so in the distribution of Nobel prizes among the world, and including the United States.

And you see that after the war, everything is in the United States. So we're a country of immigrants. The scientific enterprise is made mostly because of people recognizing for a long time that this is the best place to get an education in science, still is. We should make sure it stays that way. And we should encourage people to come here and do their scientific work. Why not.

**Donald Geesaman:** So I guess I have one question, which you addressed in your presentation, but it of course relates to how we take the work that you have done and move forward to the work that Bob is doing. And that's how do we understand the recommendation about relative funds for operating facilities, and research.

Now, if there is -- if we are building a new facility -- we are building a facility, and that will clearly require increase in operating funds. So one way to take that recommendation is are you pressing back that maybe we are building too many facilities, and the facilities we have in fact are underutilized. So that's a concern in terms of operating funds, you'd like to have more.

Now, there certainly is pressure to have the people to use those facilities, and there's also the pressure to have the free energy to invest in new pieces of equipment, either for the facilities or stand-alone experiments. So how are we to interpret your recommendation?

**Stuart Freedman:** I think you should form a committee of brilliant scientists to sit down and figure out how you fix that knotty problem. One way to fix it is to make a better case that this field deserves an influx of money. And that the loss, because of this, will be substantial.

And it seems to me we've made the case that that's true, but somehow people aren't listening. And I think it's a shame. You can see in our recommendations we're left with -- if you really have to get rid of something, you're left with harming universities, shutting down facilities that have just been upgraded and have enormous potential. It's all of the choices are not good.

I think it depends a little bit on where you see the future of the field, about how you would address this. But it's obviously an incredibly difficult and challenging exercise. And very important, I think -- I haven't seen in the past where nuclear physics is at a crossroads, as it is now. As far as I can see, it did everything right, and now it faces a serious problem.

I don't know why, frankly, it was swept up in this, that's just my personal opinion. I think it's healthy. And justifies the projections of what the funds would be required to keep it that way. So I dodged your question.

**Jamie Nagle:** I was just curious since you mentioned how different countries operate, somehow sitting on this committee, I'm given the distinct impression that if we want to have a strong nuclear physics community we have to live with a planet that's much hotter, there's competition. I'm curious, in Japan, or Germany, where actually they seem to be maybe even more heavily invested in alternative energies in energy science, where they have a similar problem or competition, or it's not viewed that way. Whether that's something useful for us.

**Stuart Freedman:** Well, I've been around awhile, so I can get a picture of how these things go. The Japanese are operating on a system that existed for some time in the United States. Mostly in the 50s after the war. It's a basis of trust. We scientists could go to Congress and argue that they want to do this, and they were just trusted. Good stuff will come out of this. And that's still pretty much the way it works in Japan. Japanese scientists get some renown. They make proposals and, okay, go do it.

And they're generally funded -- well, they're always funded, for five years. So no matter what the project is, it has to be five years. At least that's continuing. It's changing now because things are taking a little longer, so they get two 5-year grants. But it's basically a system which is designed because they trust. And deliveries have been good.

Germany had a similar system of trusting scientists. The German academics is what we sort of emulate. German professors were next to God, and so the government would support their facilities.

Now they have financial issues much like the United States. The energy issue in Germany I'd love to talk to you about. That's not turning out so well, as it seems from the outside. And the Germans aren't very happy with how that's coming through. They certainly have a lot of interest in green and so forth, but they also manage to affect the government in getting support for their activities.

**Julia Velkovska:** I had a question about the statement about the prospect of being nimble and competitive and being able to initiate small scale projects. I haven't read the report, so I didn't understand, what time scale projects were you thinking of, and how would that play against having larger facilities, that you can act on a short time scale to cover particular needs.

**Stuart Freedman:** Obviously nuclear physics is managing very large projects, like the CEBAF, upgrading MSU, and there are procedures for doing this.

It's a question of how far down on the list of, say, MIEs that you put those kind of restrictions. It's good to monitor these things, but at some point it's more efficient not to require all these high-level issues of project management on a very small project. It's costly, and it's not clear that it benefits much in the end.

A lot of these small projects are done by -- by scientists who are not project scientists, so they can't respond to -- easily respond to these requirements that are made. It just requires -- I mean, it's not that they shouldn't be managed but it should be required -- management of a slightly different style, where the high level of management required on a very big project, where you have to make sure that there's no major disasters which could be significant, because of the money involved, shouldn't be applied to smaller ones, should be tailored more appropriately for the size of the project.

**Joshua Klein:** I just wanted to make a suggestion. People at labs have LDRD as a possibility to do exactly what you're talking about. Universities do not have anything like that, and something that played that role is exactly what?

**Stuart Freedman:** It's a problem in general with the balance between universities and national laboratories. You put all the resources into the national laboratories that you have to go to them to do almost anything. And as our field evolved, that changed. There's a school in Pennsylvania, for example, that has always been very good with electronics. And for decades they brought high quality electronics to experiments. And I don't know what your infrastructure is right now, but I would imagine it's very stressed. There are not too many universities left that have in-house these kinds of capabilities.

For nuclear physics it was because very often you had an accelerator, and that accelerator required technical people to support it. And they didn't run the accelerator all the time, they could also help the scientists and students build equipment. And we've lost that, and that's a problem.

**Donald Geesaman:** Okay, thank you, Stuart. While Bob is setting up, we actually did get one question from the web-verse, but it was actually a scientific question. That -- so I thought it was more important that we get these NSAC questions that dealt with essentially the process and planning issues. So I apologize to Mr. Eliason.

If you would like to answer about the theory of fluid medium responsible for charged particle interactions, and what additional theory and experiments might be necessary to enable serious nuclear science reconsideration of a fluid medium for particle interactions and wave propagations. I'll give you a few seconds.

**Stuart Freedman:** Could you repeat the question?

**Donald Geesaman:** I'm sorry, but we have to move on. So thank you very much, Stuart.

So while Bob is setting up, we, at the last NSAC meeting in March we expected to receive a charge from the Department of Energy and DOE, but it wasn't quite ready at the time of the NSAC meeting.

And so this is actually the first time that NSAC as a body has had a chance to react to the charge and to the planned execution. So I thought it was very important that we have some time to discuss it here, and so Bob is going to present what's going on.

**Robert Tribble:** In between Stuart and Tim, much of what I'm going to say today has already been said. So you're not going to hear a lot new, if you're expecting to hear some new things.

Hopefully, I can tell you what we are doing in the process of trying to answer the charge that you received. And just to set the scale, I will re-show the plot in a slightly different form that Stuart showed at the end of his presentation. I think this is the slide actually that Tim showed at the March NSAC meeting.

Spells out very clearly what the problem is. I think in a period sort of in here, it was a little bit harder for us to know exactly what the problem was, because we had had some Recovery Act funding that came into play which kind of muddled the water a little bit in terms of what the actual budget was compared to what we needed.

This makes the problem very, very clear. And turns it into an enormous issue for us in the field.

So as a result of the President's request, there was the charge that came to NSAC, as Don said, that was a little after the meeting, about a month after the meeting. Tim has already pointed out one of the important paragraphs in this charge just to remind you. The third paragraph on this page is the one that really asks for the input from the subcommittee.

I was going to read this, Tim has already highlighted it, you have it, so I'm not going to spend the time to go through and read it. But it is very, very critical in terms of what we are supposed to be doing.

On the second page of the report, the first paragraph, also is important. And just to reiterate what Tim said this morning, this is not about a new long-range plan. This is about looking at what was put forward in 2007 in the long range plan, and trying to see how we can fit a program into what could be very constrained budgets for us.

Well, following the receipt of the letter from -Bill Brinkman and Ed Seidel, Don circulated to NSAC members a letter to send to me to chair a committee to try to respond to the charge. What we will do is put together a report that will be submitted to NSAC. NSAC then has the responsibility for accepting the report, rejecting it, suggesting changes, et cetera.

So ultimately it's the group here that will be submitting a report to the agencies, not us. We are simply reporting to you.

We put together the subcommittee by trying to consider how we could have broad coverage in the field with people that were active in the various areas that make up our community.

I think if you look through the list, you'll see that we have done that. We have members of the community that span all of the major areas of nuclear physics and include the major areas supported by NSF both in Brad's shop and also in the particle and nuclear astrophysics area as well.

We have two people on the subcommittee who are outside of our field. Adam Burrows who is not as far outside, perhaps, as George Crabtree, but still not a member of our nuclear physics community.

And at the bottom of this, I made a note about the website that we have set up for the subcommittee. It has, in addition to other bits of information, a list of the subcommittee members. It may even have their email addresses in that list.

So we had an organizational meeting in mid-May, May 15th, here in D.C. And at that meeting we heard presentations from both DOE and also NSF on budget issues, on vision, what the agencies were trying to do in terms of the longer term. And we took that information then together with a lot of discussion to try to achieve some outcomes.

And I listed four of what I would call outcomes from that meeting. It started with an outline for the program that we would have for our second meeting.

We then created a number of questions which we started at the meeting here and iterated with email to try to guide the presentations at this second meeting that we had. We discussed how we might try to structure the report.

And after hearing comments from various people in the subcommittee, I agreed that we should add a way on the subcommittee website for the community to access the subcommittee by posting comment. So that's been done, as well.

This was all advertised through a Division of Nuclear Physics announcement. We've had, to date, very little activity on posting comments, but that might speed up in the future.

Our second meeting was two weeks ago, and it may have clobbered a couple of attendees for this meeting, I see both Pete and David are missing here because of perhaps the activities that went on a couple of weeks ago.

We had set the meeting agenda up in order to try to minimize some conflicts we had with people that were making presentations, and we also tried to set it up in a way to provide some coherence among the various different areas. I'm not sure we were able to get that completely done, but that was at least the intent in how the meeting was organized.

The committee treated this meeting strictly as a fact-finding mission for them. It was not a meeting where we expected to make any strong recommendations, or even weak recommendations. We really wanted to try to understand the full activities in the field, and then have some time to try to digest that in order to come to grips with the troubling problems.

We had a lot of excellent questions asked by the committee members. And I was loathe to actually cut off that discussion, because I think the questions were really, really important, but it did create some problems for us, we had to make schedule adjustments on the fly. That's -- we lived with that.

We saw many, many examples of science, excellent science that had been accomplished since the last long-range plan. Stuart has already covered some of that.

I'm not going to talk about any of that today, just simply don't have time. But instead what I will do is focus on what I think we need to focus on in this report, which is the forward look that we have to make, and what our big issues are.

So some of this, as I said, has already been said by others, but perhaps it's okay to repeat it. I'll try to go quickly.

We started out two weeks ago today in the morning with a session on relativistic heavy ions and in each of the first four sessions, morning and afternoon sessions, we had overview talks that were given by people that were sort of chosen by the subcommittee. We wanted to do this in order to provide a broad view of what was going on in these fields, and not make them completely centric around a particular laboratory. Relativistic ions in the U.S. is dominated by RHIC, so the talk given by Bill Zajc as an overview was then followed by a series of talks that were organized by the RIHC group at Brookhaven.

As I said, lots and lots of questions, so what happened in the morning session was, well, we killed a coffee break, we didn't have a break, and we had one other casualty with time running out, the feeling by the RHIC community was that we really needed to hear Steve's wrap-up, and so we did not get a chance to hear the talk by Akiba on hard probes.

We were able to pick up some of the important points there, some of those important points referred to the Japanese contributions to RHIC. In our evening executive session, at the beginning of that.

So what did we hear from Brookhaven? Again you've heard some of this already, they gave us the broad science goals that they have for the next decade, which includes quantifying the properties of the QGP and features of the QCD phase diagram as functions of temperature and net quark density from the onset of deconfinement toward even earlier universe conditions. They would exploit new discovery potential in searches for a QCD critical point, and for the nature and influence of quantum fluctuations in initial densities in the excited QCD vacuum.

They finally said they would continue the explorations of the role of soft gluons in cold nuclear matter, and that's got to do with gluon saturation at small x and with what the gluon contribution is to the proton spin.

Throughout the morning session we heard repeatedly the complementarity that we have between the programs at RHIC and the LHC. Both are needed to explore the temperature dependence of the QGP properties, and in particular, RHIC can reach down in energy to explore the onset of the QGP.

RHIC also has, relative to the other colliders that have been used around the world, including LHC, a unique complement of probes that it has at its disposal, from polarized protons all way through uranium now as heavy ions.

A different way that this was cast for RHIC, were in a series of questions that were posed as the questions for the next decade in hot QCD. What's shown here are a list of questions that we got from Brookhaven, the facilities that are needed to address them, how they would be addressed in the various areas, and finally, how they related to questions that we had asked RHIC to address in their presentation.

So I'm not going to go through these, they're in the transparencies, available to you, if you want to take a look at them. But they span a wide range, essentially all require RHIC, but also other facilities.

Again, what we heard was that to try to answer these questions would take about a 10-year program, presumably running every year, at RHIC, using various combinations of A plus A, p plus p, p and d plus A runs, and at various energies. And in fact we got a straw -- straw man straw person profile of how that might proceed here.

In terms of the species that you might run over the next decade, what the science goals would be for those various runs, and how they would interface into the upgrades that have been going on and continue to go on both to the accelerator and to the detectors.

I said strawperson because all of you know that there's a program advisory committee at any of these large facilities that's going to dictate how the program runs, but this was one way that they could see the program evolving over the next decade.

Then beyond that, RHIC envisions itself as moving into the realm of cold QCD matter through eRHIC. And again you've heard this, Stuart showed the diagram here in his presentation.

This is the vision that RHIC has for an electron ion collider involving using electron recovery LINACs to produce the electron beam, and they make a note to point out that this actually correlates very well with some upgrade work that they want to do on the beam luminosity using electron cooling with electron recovery LINAC as well.

The afternoon session was focused on fundamental symmetries and neutrinos, again the list of presentations are shown here. One of the overview talks was given by one of the subcommittee members Michael Ramsey-Musolf, the other by Hamish Robertson, they sort of split it in two pieces, and again we had lots of questions.

What happened here was we had to start hurrying up people, we really had to hurry up there through Carston Heegers talk, and we didn't really have the session that we had scheduled at the end to get questions answered from the management, if you will, at the fundamental symmetries and neutrinos area. There's not the same kind of management as we have at other labs, and in fact we have some very important people in this community on the subcommittee. So we just raised those questions later in the closed executive session.

Michal gave us a nice overview of many of the facets of fundamental symmetries program. I picked out a slide of his showing four different components in the field that range from electric dipole moment searches through neutrinoless double beta decay to smaller experiments that are carried on and experiments that are being mounted and proposed for lepton accelerators.

Hamish talked about the neutrino program, I thought he had a very nice slide here showing the known knowns, the known unknowns, and the unknown unknowns in this field.

Some of these we've now figured out. $\text{Theta}_{13}$ which was a known unknown for a long time, is now a known. But we still have issues here like what's the mass hierarchy, what's the actual mass scale, CP violation, and what kind of particle are we dealing with, Majorana or Dirac. And the unknown unknowns, this was the purported neutrino faster than the speed of light problem, that's gone away. And perhaps an intriguing one is this suggestion now coming out of cosmology that maybe the number of sterile particles, neutrino-like particles, might be 4 rather than 3. That's probably going to be answered very, very well in the next few years from the survey that's being done now.

In addition, we have the issues still surrounding mini-BOONE analysis and LSND. We have anomalies, gallium source anomalies, a number of issues that are still hanging there that aren't understood.

The color coding here shows areas where DOE nuclear physics plays a strong role. I should point out that the particle and nuclear astrophysics program at NSF covers a lot of this territory as well, perhaps even broader than what the nuclear physics program covers.

One of the major areas where we could spend significant resources from nuclear physics is the EDM program. Again, you've heard already from Stuart some comments about the EDMs and how they're being carried out now in radioactive ion beam experiments.

But also consideration is underway for a proton EDM experiment that would be carried out in the storage ring, and of course the neutron EDM is still a major project in our community, and potentially the one that would have the biggest budget impact over the next five years. I think NSAC members know quite a bit about this program, because it's not so long ago that you had a report on the neutron physics program, so I didn't plan to go into any detail at all about that.

The other area that I thought I would just mention briefly has to do with experiments that take place at JLab in the future, when the upgrade is finished. There are three experiments that are in the queue now that would impact how we understand standard model physics. They're Moller, Q weak and SOLID. These experiments would provide information on the running of the weak coupling constants, $\sin^2\text{theta}_W$ at a variety of energies, and in particular SOLID would provide a check on what's coming on with NUTEV experiment, which is a big anomaly compared to anybody else.

In addition to what you get from these experiments here, Q weak also provides a way to really understand what the weak charges are, and it will make a huge impact on how well we know what these weak charges are as it's proposed to be carried out.

Saturday morning, if you haven't noticed we started at eight and we ended about 9:30 or 10:00 each night. Saturday morning we had Medium Energy Physics, Roy Holt gave the overview talk. Once again we killed the coffee break and killed the last talk in the session from Hugh Montgomery I think he had something like 15 slides and we allowed him to show the last of his 15 slides in that session.

You've heard some of this already, these are the 21st century science questions that were posed by JLab. What's the role of gluon excitations, where's the missing spin in the nucleon, can we reveal the novel landscape of the nucleon substructure, what's the relation between short-range nucleon, nucleon correlations in the protonic structure of nuclei, and through experiments like Moller, SOLID, Q weak, can we discover evidence for physics beyond the standard model of particle physics.

At JLab they've configured four halls to carry out this program. The new hall D, as Tim pointed out this morning, will be devoted to the gluex experiment to look for issues of confinement, studying exotic mesons. Hall B will be devoted to understanding nucleon structure through generalized parton distribution measurements. Hall C primarily involved in precision determination of valence quark properties of in nucleons and nuclei, and Hall A will be doing form factor measurements, but this is also the hall where future experiments like SOLID and MOLLER would be placed.

So again, there's a significant potential that's already been mentioned here with the opportunities to discover and study new exotic mesons, opening up a new landscape of nucleon tomography, establishing the quantitative foundation for the short distance behavior in nuclei. Providing stringent new tests of the standard model, and ultimately establishing a firm basis for higher energy studies with a future electron ion collider.

So as we've already heard JLab and RHIC, both see this as the future for QCD studies in the United States. The version of the electron ion collider from JLab is the medium energy electron ion collider, MEIC? And the concept is to use the 12 GeV beam out of CEBAF in collision with particle beams that you would get by adding a new hadron facility to JLab to provide those particle beams. The first stage of that is shown here in red. It could be extended to a larger set of rings to get up to the energies ultimately of about 250 GeV for the proton beams.

Saturday afternoon we have the first of the sessions that were devoted to low energy nuclear physics. David Dean gave the overview presentation here followed by presentations that were brought in from FRIB and NSCL.

And again, common theme, we had no break. We also cut down the last two talks, in fact Paul Manteca's talk was cut to almost nothing just to make it all fit.

So best laid plans always went awry because of very good questions that came from the subcommittee.

We've seen this already in a slightly smaller view, this is the layout FRIB will have when it's complete. What's being added to the facility already existing at MSU is the LINAC complex tunnel where the LINAC will be stationed and a new target complex where the very high intensity beams coming out of the FRIB LINAC will be separated from the reaction components which will then be sent into the existing area of the NSCL main high bay.

One of the features that this system will have making it somewhat unique from other large high energy facilities is the reaccelerated beams which will be carried out, those experiments will be carried out over in ReA12 area.

So what do we hear from FRIB? Well, again, I'm repeating what other people have already said today. But perhaps giving it a slightly different view, I think if we look back at where we were 20 years ago, trying to understand nuclear structure, we had a very myopic view. We were looking at nuclei that were close to the valley of stability, a bit removed but not too far. And we had models that would work to understand those nuclei.

As we became more proficient in making secondary beams of particles, and seeing what the nuclear structure did as we moved far away from stability, we saw that those models just didn't work. And we had to go back and retool what we were doing.

And that to me is one of the important aspects that we have in nuclear structure, at least, for facilities like FRIB. They'll allow us to get very far away from the nuclei that are the ones that we are dealing with normally, to those that tell us what's really going on with the nuclear interaction as we vary N and Z.

Because of the capabilities that we'll have at FRIB to accelerate beams from very low energy up to very high energy, we also will be able to carry out nuclear reactions over a very broad range of energies which sort of spans all of the different types of reactions that we really want to understand. Many of those will help us out understanding the astrophysics problems. But ultimately, we hope to be able to try to do something that connects structure reactions together in some unified understanding.

The other aspect of FRIB that I would like to touch on, it's not the only other one but the one I would touch on here, is the one for nuclear astrophysics. It will provide a way to make lots of particles on the proton-rich path of the rp- process, and on the neutron-rich path of the r-process.

And so that capability will allow us to really pin down many of the still unknown parameters in those processes. And again, that's one of the things that a very high-power facility can do. You simply can't do at low-power facilities.

Sunday morning we finished up low energy presentations with a talk from ATLAS and also from ARUNA. Then we had a couple of presentations on nuclear astrophysics that sort of took us outside the realm of what you see at a radioactive ion beam facility, what other problems are important as well. We heard from David Kaplan on nuclear theory and from Martin Savage on computational physics.

Let me briefly comment here, you've already seen some things from Tim on ATLAS, he's talked about CARIBU so I won't say anything about that, but I would say on ATLAS is that they are going through a series of rather modest upgrades, both in the equipment for the facility accelerator, with a new front-end RFQ, a new cryo modules for the accelerator complex, and also in the instrumentation to try to position themselves to be a very high intensity, stable beam facility for the future.

ARUNA, I think this transparency without the top banner was shown, gave a presentation, this is the new organization, Association for Research at University Nuclear Accelerators. It took us a while to figure out a name but that's the one we've ended up with.

This includes the laboratories supported by NSF at Florida State and Notre Dame. It includes three centers for excellence from DOE side. The Triangle University labs, Washington CENPA lab and us as Texas A&M. It also includes some facilities where they are operating machines to do both some research and some applications such as at the University of Kentucky and Ohio University. There are several of those that are still operating, doing very good science as well as, as I said, applications oriented research.

These are very important, as we've heard, on the student side. These are not the forefront facilities in the world, but they have a very important piece in the program here. And I should add, looking around the audience, I just reminded myself, that we'll probably very soon add the 88-Iinch to this group as an additional part of the ARUNA effort.

All right, nuclear astrophysics. I said we had a couple of presentations on things outside of what we were doing at FRIB. One of those was talking about the computational issues in nuclear astrophysics, trying to understand ultimately the origin of the elements in the isotopes from the perspective of how stellar evolution occurs. There were a number of topics that were discussed during that presentation. Many of them connect very, very closely to information that we need from nuclear astrophysics. So it's a very tightly coupled scenario between what's going on in computational nuclear astrophysics and experimental nuclear astrophysics in the lab.

We also heard about what was needed besides the radioactive ion beam facilities for make headway in this field. For maybe 50 years now we've been able to do experiments with stable beams and targets, pushing cross-sections down to low values. Very few cases you can point to where we've actually gotten to a point where we're in the regime which these reactions happen in stellar systems.

To do that we've now learned from Grand Sasso you really have to go underground, so this was one of the issues that was discussed at our meeting, the need for an underground accelerator with high luminosity. So you can couple low backgrounds and high luminosity together.

And sufficient energies to do proton and alpha and heavy ion beam studies so you can really learn about the neutron parts of the R process. Where the neutrons come from, in particular.

Okay, finishing up. David Kaplan gave a wonderful talk explaining how nuclear theory pervades all of the areas that we heard during our previous two-plus day session.

He gave us very nice highlights and forward looks for relativistic, heavy ion physics, nuclear astrophysics, lattice QCD, nuclear structure many-body physics and fundamental symmetries. And he also pointed out the importance of the teaching and mentoring role that our colleagues in the theory community play, and we experimentalists like to think we do this too, but I think our theory friends probably sometimes do a better job of it.

So coupling very nicely with what David had told us was the presentation by Martin Savage, which just indicated again how pervasive computing and really very good computing is in our field now. It touches on essentially all of the areas that we deal with, from the origin of the elements, the early universe related issue, all the way into a loop around to applications.

So all of them are important in this area of computational physics.

So that's a very quick run-through of what we spent, as a committee, two and a half days listening to. It was a long time, but I would say it's really unfortunate that not too many people in our community have that kind of chance to hear what's going on. Because it really gave you a very thorough understanding of what we were doing now, and how we wanted to proceed into the future.

So with that, let me finish up this by telling you what we're doing now, and what we will be doing over the next few months. Assuming we don't get different orders from you guys.

At the moment we made four draft assignments for writing, and I believe in some cases writing is actually underway to begin to putting together parts of the report.

We have a chance for community input to the process before -- and I should make this very clear, this is before any decisions are made from the subcommittee-- and that will be at the DNP Fall Meeting. We have town meetings scheduled for the four major areas that I just went through on Thursday evening at the fall meeting, and on Friday evening there will be a joint town meeting, we should hear reports from all those groups, and there will be a chance for community input directly at that time.

This was done thanks in large part to interactions that Bob led as chair of the DNP with the program committee people who had to do some fast juggling to take this opportunity.

We have a resolution meeting that's now scheduled for November 30th to December 2nd. And what I hope are the goals that we can stick with on the report are to have science sections, the introductory sections, and other sections such as international facilities and workforce, things that are very important as well to make this a complete document.

Having all that done before the resolution meeting. Then following the resolution meeting, the plan would be to have a conclusions and recommendations section drafted by roughly the 14th of December, sent out to NSAC committee members, NSAC subcommittee members, excuse me, slip of the tongue, for comments by December 23rd.

We would then hope to have a full report with the comments taken into play by the end of the year. So you can see what I'm going to be doing over Christmas. With the goal of getting a report to NSAC by the goal of January 7, 2013. So that's what I had to report today, be happy to have input from you on things you think we should be doing differently or questions.

**Joshua Klein:** So in the chart letter it mentions two funding scenarios, and actually I think it says at least two funding scenarios. Are there more than the two being examined, and in what way does that -- is that integrated here?

**Robert Tribble:** Where we stand on funding scenarios is that we have not looked in detail at them, although we have seen the broad picture from a presentation that was given to us by Jehanne prior to Friday, we had a Thursday evening session to go through budget-related issues.

The plan is that within a couple of weeks we will have together some information that we will make sure is okay with NSF and DOE to be sent out to our subcommittee to give them some more numbers that they can use themselves to take a look at various budget options. Because as you might guess, different parts of the community have different ideas on how we should proceed.

**Joshua Klein:** Just a follow-up. The two funding scenarios mentioned looked to be quite optimistic. One was flat, one was moderately increasing. But the current funding scenario, if you just extrapolate, is decreasing.

**Robert Tribble:** It was flat based on the 2013 President's request.

The one that had gone down was the one that was to be used as the starting point, and then flat from there as the one scenario to consider.

It might be useful to point out, you know, how bad it could be if things don't change. You know in other words -- if we don't do that we'd be making a big mistake.

**Jamie Nagle:** Maybe this is a question for Don, but in the letter charging NSAC, it leaves it slightly ambiguous that NSAC will submit the answer by January 2013, there's no specific date. And Bob mentioned the fact that the subcommittee sends this to this committee. At which point this committee could change what's in the document, or edit it. What is the schedule for that?

**Donald Geesaman:** So our plan right now is that we would have an NSAC meeting in late January, so that if we receive the report on January 7th there would be time for NSAC to reflect on it, and have some discussions prior to meeting, and have the meeting.

If at our meeting towards the end of January we think there are significant changes that need to be made, obviously we'll not meet the end of January deadline. But that's the risk we take. I mean, if that's the way it is, that's the way it is.

The sense of the recommendations will be known privately to the agencies. And it would be highly unusual for NSAC to totally reverse the recommendations, but it's certainly possible, and I'll certainly allow for that, and if it takes that and takes several iterations, well then, we won't meet the deadline of the charge, but that's -- we have to get it right. That's far more important that we get it right.

**Jamie Nagle:** Maybe a related question is members of NSAC who are not on the subcommittee, what access do they have to this information? So will those members simply see the report on January 7th?

**Donald Geesman:** People who are not members of the subcommittee, or agency representatives who will sit in on much of the discussion, but not necessarily all of the discussion, the reports embargo from people outside of the subcommittee until January 7th, when it's sent to NSAC members, and then it's embargoed within NSAC.

**Raju Venugopalan:** To follow up on this logistical issue. How does the process work? It's my first time on NSAC, so is there a vote taken, or is there going to be internal discussions in NSAC, or --

**Donald Geesaman:** I think there will be considerable discussion before the NSAC meeting. Getting a sense of the committee. NSAC can take several actions. We can send the report back to the subcommittee and ask for certain things to be improved. We can accept the report, and then bury it. We can accept the report and transmit it to the agencies with our complete endorsement.

So theoretically, there's a range of actions that are possible. In practice, the NSAC reports have often been returned to the subcommittee with requests for improvement, and those improvements have been made. And the reports have come out much better as a result of that.

And NSAC transmits the report with a letter of transmittal, and sometimes NSAC in that letter of transmittal expresses a point of view which might not be obvious from someone reading the report itself to place the report in context. So those are historically a range of the options that have been employed.

Yes, there will definitely be a vote on accepting the report.

**Raju Venugopalan:** Maybe you could share your experience from last time.

**Robert Tribble:** Yes, since I'm not new to this, in the last report that we did to try to understand how to implement the 2002 long range plan we did have a recommendation from NSAC to modify one of the graphs that we had in the report. I believe that was the most substantive issue that was brought up by NSAC.

**Susan Gardner:** It strikes me that the fact finding mission in which you engaged in September is not dissimilar from that which must have been executed to realize the National Academy report. And I just wonder how in fact has the fact-finding mission changed with respect to two reports. And given similar inputs, wouldn't you expect similarly ordered scientific recommendations, as opposed to implementation?

**Robert Tribble:** So let me address that, and Stuart, if he wants to chime in, can.

I was on the decadal survey and I can tell you that the process that went through there is very different than what we just did two weeks ago.

The process two weeks ago was really to understand how the major facilities saw their role in the future and what it took to run them. Or to build them, in the case of FRIB.

For the decadal survey we focused on the science solely. Here we're focusing on a combination of the science and the facilities it takes to operate and get that science.

And as a part of that we naturally have to be a lot more worried about budget issues. As Stuart said, that was not something that the decadal survey really worried about, if you will.

Do you want to comment further, Stuart?

**Joshua Klein:** That was along the lines of what I was going to ask, everything you presented you really focused on your sort of fact finding mission with regard to the science, scientific program. But how deeply is the committee delving into operational budgets, and what's a likely operational funding scenario for RHIC over the next N-years, and --

**Robert Tribble:** Okay, so to ask our subcommittee to do that would go way beyond what our expertise is. DOE has every year, I believe, reviews of operations budgets at their major facilities, and they have experts there that understand those -- the details that you need to understand to carry out those reviews.

While we have had some rumblings in the subcommittee about trying to micro-manage, it's just simply not possible, with the time scale we have and the expertise we have.

So we have to take what we're given from the facilities and from DOE as corroborating what they tell us, as what it really takes.

**Joshua Klein:** So slightly along those lines, so when the underground lab fell through, there was this Marx committee that was convened and I think among -- well maybe the most surprising result that came out of it was that some of the science that could be done in a U.S. based lab could be done more cheaply at essentially a new part of SNO lab in Canada being built. In other words, you know, a new facility in Canada was significantly cheaper than a facility in the U.S. And that seemed to me to be a teachable moment, to figure out exactly why it was that science in the U.S. and this goes to operating facilities and things like that, has risen -- the costs have risen so much.

Or our perceptions of the cost have risen so much. Is there any way which that can be looked at as long as you're looking at budgets anyway?

**Robert Tribble:** Well, we have two unique facilities in the U.S., at JLab and RHIC, and we have a new facility that will be unique in the world, it won't be the only radioactive ion beam facility, but when constructed it will certainly be the most powerful radioactive ion beam facility in the world.

And so could we build FRIB somewhere else more cheaply and operate it more cheaply? Maybe, I don't know. But that's not something that we have any way to really gauge.

**Joshua Klein:** Well, I mean the hope wouldn't be to decide that we should put FRIB in China or something, the hope would be to say, you know, we've been going about this in a way that costs more than what other people do to achieve the same thing.

Is there a way that we can save money.

**Robert Tribble:** I'm not privy to the Marx report fully. I've looked at it but I haven't studied it. I would be concerned that you're taking an existing lab and excavating a new part of an existing lab, compared to trying to build a new lab. Which was really what the initial impetus was at DUSEL. So I'm worried it may be apples and oranges.

**Joshua Klein:** Just a comment, it was under the scenario that LBNE was going ahead, and the underground lab was already being excavated and you were just adding space to do double beta decay. Adding space to SNO lab to do double beta decay, which was also an existing lab at that point, was like $100 million cheaper. That was the cost of the whole experiment. So anyway.

**Richard Kouzes:** I actually have three questions. The first is when will the report be made public. The second one is there are various community meetings that have occurred or will occur, and how is that input being incorporated in the report.

And the third one is will you go as far as to prioritize facilities and research in terms of making funding decisions?

**Robert Tribble:** Okay, those are all very easy. The first one I punt to NSAC, because they're the ones that dictate the time frame on when the report becomes public.

**Donald Geesaman:** So the report will be made public essentially as soon as it's accepted by NSAC. There will be a presentation at the NSAC meeting on key elements of the report, but to the extent that they could change as a result of NSAC action, the report itself is not -- will only be available after it's accepted by NSAC. And transmitted to the agencies.

**Robert Tribble:** So for the second question, you know, we are trying to take community input as we can. If there are relevant pieces of that input that we're missing now, they should be sent to me, which then gets it out to everybody on the subcommittee.

The third piece of that is I don't know.

Any other questions?

**Jamie Nagle:** Is there any possibility that in the course of the November time frame that the charge to the committee changes, or the time frame changes, since that seemed to have been set based on certain issues of the election. Or is the assumption that this is independent, and this is the time frame, and that's it?

**Timothy Hallman:** So since I know something about how challenging it was to get the charge out, no, I think the answer to that is no, Jamie. That as far as the charge goes and the time frame goes, your course is set.

**Donald Geesaman**: I'd like to add to that. I think even if the light would dawn and a miracle would happen, I still believe that a report on this time scale can be very effective and important for nuclear physics making exactly the point that Stuart made. So even if the budget pressures should miraculously lessen, or if the budget pressures should increase because of sequestration or things like that, I still believe a report at this time is very important. Congress, both houses of Congress have asked for it, and it would be a real mistake for us to ignore those requests.

And so I don't see that changing the time scale. It might, however, change how enthusiastic we are about it.

**Jorge Piekarewicz:** I don't know if you mind going through your very first transparency, which is that very sobering picture of the budget. So I'm sure that there might have been some discussions already among the subcommittee about essentially what looks to me like an impossible task of trying to maintain a vibrant program under those conditions.

So I mean, can you share with us some of the comments by your subcommittee in that regard?

**Robert Tribble:** It's very sobering; everyone on the subcommittee recognizes that it's very sobering if we have to live with these kind of budgets.

And speculating on what that means now, I'd think would be a little bit premature on our part, but I think everyone realizes that it would be really bad.

And there are some people around the table that can tell you if I characterize that correctly or not.

**Donald Geesaman:** All right. If there's no further questions, thank you very much, Bob. Obviously Bob, any of the subcommittee members, welcome any input from NSAC members, and in fact the entire community. We need our collective wisdom to move forward. We need the best physics arguments, we need the best arguments about the place of nuclear physics in the economy of

America, and in society, so we really need and want your help.

**Donald Geesaman:** Okay, we've been listening to some very important science arguments about the important discoveries that our field is making. I think it's also very important that we understand some of the applications that the program at the NSF and DOE enable. We have two major programs there, of course. The nuclear data program, the isotope development and production program, and then we have grant application for applications in nuclear physics.

So over the course of NSAC meetings as time allows I wanted to make sure that the community is really aware of the value these things bring to the nuclear physics enterprise.

**Michal Herman:** Thank you. I want to present the nuclear data programs in front of this committee. I would like to mention that nuclear data program is over 60 years old, and that's actually the longest running, continuously running program at the Brookhaven National Laboratory.

Just to start, you will have to reset your scale now. It's much, much lower energy, much lower funding, and much fewer people involved in the experiments and programs you were talking before during today. The nuclear data community is essential. If the community would stand between nuclear science community and application community, if we just bridge the gap between the two, we'd communicate with both of them in both directions. And that which we do will ensure that results of the nuclear science community, results of all experiments and theoretical calculations and theory development can flow to the applications.

And why we need this? There are different reasons why this community and this [other community] do not talk to each other directly. First of all, they have different backgrounds. One are nuclear engineers; the other are usually nuclear physicists. They have different goals and different motivations. The physicists are interested in nature and how things are done. These people are interested in making these things here run. They don't go into details on the physics which is behind it.

There is also the language barrier. A typical example is the capture cross-section which here means an (n,gamma) reaction, here it will mean every reaction which makes a neutron disappear. So it's really (n, alpha), (n,p) , (n, gamma), whatever. So they don't have the common language, even.

That's where we stand here, is to provide this communication, so that our assumption is to compile the data, first of all, then evaluate these data, that compilation is a step in the evaluation, first of all we have to have all the data which were measured, available at hand.

Then we disseminate, we send them to the application community, we collect their feedback, and eventually we transmit that feedback back to this community and archive them.

Archive means that, for instance, you've got neutron data at your fingertips. Whatever was measured from the beginning of human kind essentially you can get in matter of few seconds, with fewer and fewer key strokes, from our system.

Who needs nuclear data, and what for? These applications, not all of them, but first of all it's basic science for testing theoretical models, for designing experiments, for analyzing experimental data.

Astrophysics for the origin of elements, which was mentioned already here. Then nuclear power, that was really the community which caused the nuclear data effort to start. They really need data to be able to simulate whatever happens in the reactor or in any device which they were constructing.

And until now, reactor research and design are the major application of nuclear data in nuclear power.

Fuel cycle. They have to know when they run the reactor, they have so-called fuel containment, they move fuel elements from one place to another. For this you have to do some calculations to know which fuel should go where and when.

Radiation shielding, operational safety, of course. Waste disposal and transmutation. Then you have nuclear medicine, here are radioisotope production, those calculations, and two major applications are radiotherapy and diagnostics.

Finally national and homeland security, again we need data to simulate research and development of devices, stockpile stewardship, criticality safety, which are juxtaposed to stewardship, nuclear forensics, and detecting illicit trafficking of nuclear materials. So all the applications for nuclear data play an essential role.

And there is a number of industrial applications, which I will not go into detail because that would take too long.

What nuclear data are, so these are essentially numerical values of nuclear physics quantities. Whatever the quantity, whatever the name -- the number we assign to this quantity, that becomes nuclear data.

We distinguish three types of nuclear data. Bibliographical data is an index of publications so that we can easily find out where a certain experimental, certain quantity was measured, and published.

Now, we compile these publications and this database contains already the numbers. So that's exactly where you can easily get all nuclear measurements from the beginning, from the discovery of the neutron to material from, say, months ago.

And then evaluated data. That's a culmination of the process where the values are recommended, as the best we can estimate them at the moment.

So this is done, assessment of available experimental data, changing them sometimes, we normalize them if we believe they were measured with respect to the standard we have changed already in the meantime.

We may drop some data if the evaluator doesn't trust the experiment, thinks there was something wrong in it. Combined with nuclear theory modeling, supported by experience and, if possible, validation against integral experiment.

These integral experiments are just opposite to the differential experiments that you are familiar with. It's some big slab of material, and they shove neutrons through them and measure them on the other side, or they build a small cap of the reactor and measure K effective.

These experiments are very difficult to interpret. You cannot measure cross-sections with them, because it's integral over different processes and different ranges. But they are extremely precise, and they can be used to validate the libraries which were created through differential cross sections. So that's the objective of the USNDP, is to provide in a timely manner the highest quality nuclear data responding to the user's needs.

Our two major products are evaluated nuclear structure data files and evaluated nuclear data files. So this one is the structure and decay data. These ones are just reactions.

Traditionally the nuclear data community is divided between structure guys and reaction guys. And that is a very strict division. Even though there should not be. Because after all, reactions are using structure data, and structure people are using reactions to get their numbers, but it's a completely different way of doing things, -- of running business, so therefore they are still separated.

Only the bibliographical database is common. It used to be divided, now it's common. They are compiled to databases respectively for both, and then evaluated databases.

There are about -- actually there are more than 3,000 isotopes in the structure database, so we know more than 3,000 nuclei. There is still something like 3,000 to 5,000 which we don't know.

And ENDF B7.1 is the latest release of the reaction database. 14 sub-libraries. Can't really compare the numbers here, it's really more complicated.

Other products which are pretty popular are Nuclear Data Sheets, that's a publication which is issued monthly, with the December issue dedicated to nuclear reactions, and all the rest to nuclear structure. There's a chart of nuclei interactive web application which gives you all the decay schemes, masses and then so on.

The first user oriented library of covariances, that's a hot topic very recently. It's not enough to know the value, it's important to know also what is the uncertainty associated with this value.

This one was developed for the advanced fuel cycle initiative and in close cooperation with application people. This is a nuclear reaction model code which at NNDC we use for producing evaluations.

This is a very well-known publication, Nuclear Wallet Cards with properties of the ground states of the nuclei, and monumental Atlas of Neutron Resonances, this contains all neutron resonances for all nuclei which are known, as it were.

This is our retrievals that somehow were going, it is not surprising the web retrievals usually go up, I'm not sure about this year because it's not still complete. We -- NNDC gets something like 2.7 million retrievals per year, the whole USNDP adds another half a million, so it's 3.25 million last year retrievals per year.

Now, how big the program is. Compared to all what you were talking about before, it's not very big. It's about 20 people [FTEs] altogether, it's a bit more than 50 heads, because an FTE is not equal to the head count, and the budget is $6.5 million in 2012.

These are the labs which contribute to USNDP, they're coordinated by the National Nuclear Data Center. We have the annual nuclear data week every November, and we are going to organize a big nuclear data conference in New York City, 2013 in March. A big conference which is held every third year.

In spite that we are not that big, we have a very well developed international connection. This is our institute, we are collaborating directly. There are two international organizations, this is Nuclear Energy Agency of OECD in Paris, and IAEA, the nuclear data section of IAEA in Vienna, and there are two international structure networks, and a reactor network, which we are part of.

So now I would like to show you a few applications of nuclear data which somehow illustrate the impact that we may have on everyday life, or sort of.

So that's a nearly-missed reactor accident in India, it was 2004, that the regulating system failed, and as a result, the power of the reactor went up to 100 percent. Nothing happened because security systems were in place, they worked properly, they shut down the reactor, so there was not much notice, even, hardly anybody knew it. However, the design manual of the power plant would predict that this should not happen. In such circumstances the power should go down, not go up. And that's scary, of course. So the regulatory board shut down the power plant until they understand it and explain what happened.

There was a new version of the library released a few months before, and calculations done with this library showed, explained, why the power went up. Just simply some data were not accurate enough at the time the plant was designed.

So that explained the thing and brought the power plant back into operation. And of course then these people were really very grateful to the guys who really did the calculations, because each day of the power plant being shut down, it's really millions of dollars.

That's something which is quite popular now, we want to be sure that no nuclear material crosses the border unnoticed. And one possibility of doing it is just looking to high-energy gamma-rays. If we have illicit material, there are fission products and neutrons. Capture of the neutrons on the fission products will produce a certain number of gamma rays, some of them high energy, 6 to 12, which are difficult to shield. And they are an exact fingerprint for capturing isotopes. So if we can detect these gammas and we have a good library, we can identify what material is there, then it is very useful for cargo screening. So that library is being developed now at Berkeley.

High energy physics; the other region of energies. GEANT and FLUKA are two codes which are used for designing detectors, both of them intranuclear cascade models at higher energies, but once the energy goes below let's say 20 MeV they switch to our data, so they're more accurate than the cascade model is able to predict. That's for example an ATLAS detector system simulated in GEANT4.

Neutrinos again. These are antineutrinos. There was this famous Daya Bay experiment recently which confirmed the oscillations of neutrinos, and the participants of this experiment from Brookhaven came to us and asked if we could calculate the antineutrino spectrum from the reactor. And actually that would take the decay library that we have in ENDF and the fission product yield which we have in ENDF plus a bit more calculations, and that's what we have calculated.

This is relative. It's relative because they don't know exactly the efficiency of their detector. But generally the agreement, even for this preliminary result, is very good. We will have [a] better calculation, they will have much more precise data, because they're collecting more and more.

But the agreement is quite striking. That's definitely the best a priori calculations of the antineutrino spectrum. There exists one done at Caltech based on ENDF 4, and it's in much bigger energy steps, so it's not that detailed. Now, a part of the basic science, of course, because now having well calculated the spectra, they can analyze better their experiment and be more confident in the conclusions.

This might also be -- the usage might be -- reserved, because if we can predict the spectrum well, and different uranium- plutoniums [mixtures] they have different spectrum, they differ. In fact this structure here is from uranium-238, this shoulder here. If we have the spectrum and can calculate them with a certain precision, we can use it to monitor the nuclear power plant from a distance. And there is no way this experiment can be obstructed. The neutrinos do not interact, so you cannot change it.

It can also be used in geophysics sort of tomography for the earth because of how it ties in (indiscernible). So it's data which we're planning to include in the version of 7.2 ENDF.

Isotope production, this is a major isotope that -- doctors milk the molybdenum 99 to get Technicium-99, which is by far the most used radionuclide in diagnostics.

The problem is in the U.S. there is no reactor producing it anymore. So here is a patent which shows how you can do this without the reactor, and just an accelerator, by Bob Schenter, and when they were doing this patent, they used our nuclear reaction code to perform calculations and to optimize the results.

And that brings us to the role of nuclear reactions, nuclear data evaluation. The problem is that nothing can replace experiments, but no experiment will cover the whole energy range and cover all channels. So we have to calculate them. And in this sense the nuclear data community is a major user, and a major developer also of nuclear data modeling -- nuclear reaction modeling.

These are all the models which are, if we go from light nuclei, which are really few-body in nature, then we move to heavier nuclei which are more statistical, but if we go [to] low energy we have to deal with resonances. There's no way we can model them, they have to be measured.

But there's a no-man's land of [the] unresolved resonance region, which is even more difficult to treat because it's not yet statistical, but it cannot be even measured.

So nuclear reaction codes are used to fill gaps in experimental data, provide a full set of observables, to choose among discrepant measurements, and to ensure consistency of the evaluation. That's a very important part of it. Because before evaluations were done, people just drew a line through [the gaps]. But of course once you do that you never have a consistent evaluation. They would never sum up properly as they should.

Now, a few words about opportunities. One thing which we were looking at recently, and that was, by the way, funded by this ARRA project, was a simulation. So if you look to the way that the users community, the engineers, are using evaluations, is that they usually have to adjust it to reproduce some integral experiments which are close to their application.

So instead of continuous cross-section[s], they will make multi-group, let's say 15 or 33 groups, cross-sections covering from zero to 20 MeV, run the reactor code, or the transfer code to calculate, for instance, $K_{effective}$, they want to converge on the integral data, and if they don't get the right answer then they will modify these cross-sections to get $K_{effective}$ equal 1.

Doing this, they act more or less in darkness, they just change something to make it work fine. And such a library will work, but will only work for this type of application for different reasons.

Now, the way we do evaluation now is that we have multiple parameters, we have the reaction code, we use propagated parameters through the reaction code, produce cross section[s] and evaluations, compare the differential data. And if we do not see the differential data, then we change our parameters and try to reproduce everything with the model. That's how the modern evaluation is done.

Doing this we also create the covariance matrix, of course. Now, we could collect the things together, and then instead of doing this assimilated adjustments on this level here, it can go straight back to the model parameters.

This way we've got an evaluation which is consistent with differential data, integral data, and we have also the covariance matrix, and eventually it should be even more precise because the uncertainty on the evaluation will be lower, because it will be constrained also by integral data.

Now you can go even a step farther and say well, we did it for one isotope, one material, we usually have to deal with a number of materials, let's say about 20 which are important for fast reactors, for instance. There are about 400 in the whole library that we have, so we could do it simultaneously for all of them. That of course would mean an enormous covariance matrix, and a lot of comp[u]ting power needed to do it. But in the long term I believe it is possible. We need time to get there.

So that's what we are planning is to further improve the fission channel, advance covariance methodology, use more microscopic parameters in the reaction calculations, and we might explore even the possibility of using the SciDAC results here. We were talking to George Bertsch about the possibility of distributing codes, results of SciDAC, from our servers. That would be no problem with the codes. The problem would be with the results, because all the wave-functions are terabytes per year or so, so it's somehow a challenge how to store and distribute these things that's effective.

But I think it's worth it, because otherwise they will be lost if we don't do it.

New facilities were mentioned here. We'll eventually produce large amount of data, and even if it's not a large amount of data, it will be data which could be a very high quality. Might be much superior to that what we were dealing with so far. So a lot of evaluations will have to be redone, and if we extend to this 3,000 nuclei which are not yet discovered, then there will be much more work to do all the evaluations due to this new input data which will be flowing.

Experimental activities. These are on a very limited scale at the moment. We have two proposals accepted for doing it. The point here is, first of all, to preserve scales, make program more attractive to young evaluators. And finally, the evaluator has to keep contact with experiment with the real science. Otherwise they become a clerk, and they have to decide which experiment is right. Eventually. So you can't do it if you don't get your hands dirty with experiments.

So that's why we want to have some experimental activity at NNDC, and generally, through the data program.

Then IT technology of course is expanding very, very fast, we have to keep fast with them, so we have a resource collaboration system for developing evaluations, so people really can get to the system without sending emails, all is done automatically, we have subversion, versioning systems, so we can trace all the changes.

We are planning to automate verification so each file submitted will automatically run through all the validation codes so we know it immediately if something is done.

We are looking also to applications on mobile devices, and physics calculations on demand. So instead of downloading the code, installing in the real machines, learning how to do it and then running calculations, if somebody is doing it occasionally, he might be able to go to our servers, upload the input or choose some options of the code from the website, and run the codes on our server and get the results.

That's fine as long as security people will not complain too much about it.

And finally there is new format, which should move us into the 21st century. We are still using -- I wouldn't say punched cards, but the form we use really goes back to the punched card era. It's basic characters, all the libraries are 80 characters, there is no -- you really have to know the format to read it.

So it's not a way we should do business in the 21st century, but it's very expensive to change it. Because all the codes are set up for the old programs. For the old format. I think the time is right to make this change.

So finally the conclusion is that we provide essential support for basic science and applications. They could not communicate without us. We preserve knowledge by archiving experimental and evaluative data. And we develop state of the art modeling of reactions. That's the most development in the reaction modeling comes from the data community at the moment.

Future opportunities, advance[d] evaluation methodology, unification of structure and reaction data, which I've mentioned is now separated. Enhancing the experimental program. Modernization of formats. And finally, that's what I did not mention because it's more organizational, that the reaction file is not worldwide, it's regional. Europe has one, China has one, Russia has one, U.S. has one.

We are now considering maybe we should join the effort and have the one worldwide file instead of several separated ones.

Thank you.

**Donald Geesaman:** Thank you very much, Michal. One of the things you didn't address is how you get input from your customers in order to prioritize where your efforts go. Could you explain that a little bit to us?

**Michal Herman:** That's a question that comes quite often. For the structure, we evaluate everything. We evaluate everything because it's feasible. There are only 3,000 nuclei, there are 100 mass chains, we have a number of evaluators so that within eight years they can reevaluate each mass chain.

So far, there are no prioritization for the structure part. It's completely different on the reaction side. We cannot evaluate everything, and it's clearly application oriented in this case.

So we know which materials are the most important, I mean [the] obvious are the big three, uranium-235, uranium- 238, and plutonium 239, these are absolutely the most important ones.

Then there are structural materials like iron, chromium, sodium-23 as the coolant in the reactor, oxygen and hydrogen because of water, so these are the most important materials.

Now, we have all the sensitivity studies and we know which reactions in which region on which materials are the most important for certain applications. It depends on the type of the reactor or application in general. They will be different.

We also know where our libraries do not perform well from the validation. And this -- even when we release a new library, we know where it has the weak points. But it is not easy to fix, we cannot fix it within a week or month or year. Some of them will take years to reevaluate.

But generally speaking, we know what we should do.

**Donald Geesaman:** Is there a review committee that interacts with you on these priorities, or do you have customer meetings and conferences that influence priorities? I know for the reactor community sensitivity studies are very important, and they help you pin down where to go. In the broader scale, the astrophysics community, for example, or in general, if somebody thinks more nuclear data is needed in a certain area, how do they communicate that with you?

**Michal Herman:** Well, we have an annual meeting and an annual evaluation work group meeting that is held each November. And that's where essentially the reactor people and all evaluators meet together.

On top of it, we have also some priorities which come from NNSA, from criticality safety for instance, they have their own list. There are also available request lists for the most important measurements or evaluations.

So essentially we are in the permanent contact with the users, and they let us know -- for instance, we know that iron is wrong, they always complain about iron.

**Donald Geesaman:** Okay, questions?

**Zheng-Tian Lu:** So I'm surprised that nuclear data prior to 2004 were not good enough for the safe operation of nuclear reactors, regarding that Indian near-miss incident.

If that were indeed the case we'd better review the nuclear reactors all over the world, because I bet most of them were designed using nuclear data prior to 2004. Do you know the specifics of this incident? What reaction cross sections or what decay rates were to blame?

**Michal Herman:** No, I don't know exactly which reaction was in issue here. I was not told what was the problem. Yes you're right, in principle each time we release from the library all the reactors should be recalculated. But you would have to convince the reactor community and regulatory commission to do this. It's a big task to do the reactor calculation.

**Zheng-Tian Lu:** It seems to me it was a serious claim to say that the prior nuclear data were to blame for this.

**Michal Herman:** Yes.

**Zheng-Tian Lu:** There is a possibility that somebody simply did a wrong calculation the first time.

**Michal Herman:** Yes. I mean, this library was pretty old. And 6.8 of the library I think was released in [19]96, if I'm correct, about 10 years before the release of 7.0. And many of these evaluations are taken even from much older.

And that was probably something which was thought not to be very important. In fact, it was not -- it wouldn't affect the normal operation of the reactor. Here, though, there was an issue of the regulation mechanism, and therefore, the reactor went into the regime it was not supposed to be in principle, and that's where you see it.

**Susan Gardner:** The procedure you described in regards to continued adjustment and simulation in regard to tuning nuclear theory or reaction theory to integral experiments, would seem to mute possible deficiencies in the theory itself.

So in that context, do you have any explicit concrete example of how improved reaction theory modeling, as offered by SciDAC, has practical outcome[s]? Can you give an example?

**Michal Herman:** We haven't used SciDAC yet. We can use the microscopic input, but not from SciDAC yet.

The point is our experience shows that there is no way you can do without differential experiments, and there's no way you can do without integral experiments.

You may have a wrong cross-section, you adjust it and it will get the perfect $K_{effective}$ equals to 1. But you will violate the agreement with the differential experiment.

On the other hand side, to get the $K_{effective}$ exactly to the precision that the reactor community wants you would have to have such a precision in the differential experiment which is completely impossible, of the order of below 0.1 percent. But we've learned that minimal changes in the cross-sections are enough to get $K_{effective}$ right. It's hard to see on the plot, even, but that's exactly what is needed.

So there's no way that one can do the right evaluations just based on differential experiments and then say you'll get $K_{effective}$ within 10 -- 100 PCM or something like that. You need both of them.

**Donald Geesaman:** Any other questions?

Okay, thank you very much, Michal, we very much appreciate it.

All right, I got no statements that people would like to make any public comment, but we do still have a little bit of time. Is there somebody who would like to make a public comment?

Okay. If there's no objection, I'll declare the meeting adjourned. Thank you all very much for coming.

(Meeting concluded.)

The floor was opened for additional public comment. There being none, the meeting was adjourned at 4:30 p.m.

The minutes of the Nuclear Science Advisory Committee meeting held at the Hilton Hotel Washington North/Gaithersburg, Maryland, September 21, 2012, are certified to be an accurate representation of what occurred.

Donald Geesaman,
Chair, Nuclear Science Advisory Committee