Official Transcript of Proceedings

NUCLEAR REGULATORY COMMISSION

Title: Advisory Committee on Nuclear Waste 169th Meeting

Docket Number: (not applicable)

Location: Rockville, Maryland

Date: Tuesday, April 18, 2006

Work Order No.: NRC-985 Pages 1-258

NEAL R. GROSS AND CO., INC.
Court Reporters and Transcribers
1323 Rhode Island Avenue, N.W.
Washington, D.C. 20005
(202) 234-4433

UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

+ + + + +

ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

169th MEETING

+ + + + +

TUESDAY,

APRIL 18, 2006

+ + + + +

The Advisory Committee met at 10:00 a.m. at Nuclear Regulatory Commission Headquarters, in room T2B1 of Two White Flint North, Rockville, Maryland, DR. MICHAEL T. RYAN, Chairman, presiding.

MEMBERS PRESENT:

MICHAEL T. RYAN, Chairman

ALLEN G. CROFF, Vice Chairman

JAMES H. CLARKE, Member

WILLIAM J. HINZE, Member

RUTH F. WEINER, Member

ACNW STAFF PRESENT:

JOHN T. LARKINS, Executive Director, ACNW/ACRS Staff

NEIL M. COLEMAN, Designated Federal Official

LATIF HAMDAN, ACNW Staff

I-N-D-E-X

AGENDA ITEM	PAGE
1) OPENING REMARKS BY THE ACNW CHAIRMAN	3
2) OVERVIEW OF ACCELERATOR MASS SPECTROMETRY	6
3) UPDATE ON U.S. DEPARTMENT OF ENERGY (DOE) CHLORINE-36 (CL-36) STUDIES AT YUCCA MOUNTAIN	62
4) BRIEFING FROM NATIONAL ACADEMY OF SCIENCES (NAS) ON ITS 2006 REPORT ON THE TRANSPORTATION OF HIGH-LEVEL NUCLEAR WASTE	96
5) PROPOSED RULEMAKING ON NATURALLY OCCURRING OR ACCELERATOR-PRODUCED RADIOACTIVE MATERIALS	208
ADJOURN	

P-R-O-C-E-E-D-I-N-G-S

(10:03 a.m.)

CHAIRMAN RYAN: Good morning. If I could ask the meeting to come to order, please?

As you can see, we are not in our usual accommodations. We are happy to report the upgrade is proceeding well. And if all goes well, we'll be ready for our meeting next month in the revised room.

We've got new audiovisual equipment and other features that will make presentations even more fabulous than they have been, much thanks to Theron and his team for getting us down the road and making all of these intermediate accommodations the last couple of months. Without Theron's able help, we would be talking to each other without being on the record. And we would have a real mess.

So thank you, Theron. We appreciate your help.

MR. BROWN: Thank you.

CHAIRMAN RYAN: Let me read the opening statement. The meeting will come to order. This is the first day of the 169th meeting of the Advisory Committee on Nuclear Waste. My name is Michael Ryan, Chairman of the ACNW. The other members of the Committee present are Vice Chair Allen Croff, Ruth

Weiner, James Clarke, and William Hinze.

During today's meeting, the Committee will be briefed by a Purdue faculty member on the methodology of accelerated mass spectrometry, will be updated by representatives from the Department of Energy on the status of chlorine-36 validation studies at Yucca Mountain, and be briefed by the representatives of the National Academy of Sciences in their recent report titled "Going the Distance to Safe Transport of Spent Nuclear Fuel and High-Level Waste in the United States."

We will also be briefed by representatives from the Office of Nuclear Material Safety and Safeguards on the staff proposed rulemaking to implement section 651 of the Energy Policy Act of 2005, to include certain naturally occurring or accelerator-produced radioactive materials in NRC's regulations for byproduct material.

Finally, we will discuss proposed letters and reports the Committee has prepared from earlier meetings.

Neil Coleman is the designated federal official for today's session. This meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act. We have received no written comments or requests for time to make oral statements from members of the public regarding today's sessions. Should anyone wish to address the Committee, please make your wishes known to one of the Committee staff.

It is requested that the speakers use the only microphone for this session, which will be in one of the two seats at the end of the table, a bit inconvenient perhaps but that way we'll have you on the record.

So if you want to ask a question, please come up, use one of the chairs, and we'll be able to hear you. And you can identify yourself and hopefully have you speak with sufficient clarity and volume so you can be readily heard. It is also requested if you have cell phones or pagers, you kindly turn them off.

With that introduction, I think we're okay on the record. Okay. Everything is working fine. I've given the recorder a map of who we are. So we don't need to identify ourselves because we don't have our name tags and all of that stuff, as we would in the normal room, but he has the road map. So he knows who we are. And we will go from there.

So without further ado, today's agenda is being led by Professor Hinze. So without further ado, I will turn it over to Professor Hinze. Thank you.

MEMBER HINZE: Thank you, Chairman Ryan.

As you have stated, we have two briefings this morning on the AMS spectrometry studies of chlorine-36, which have played such an important role in understanding the temporal processes at Yucca Mountain.

Our first presenter -- and I would ask David to come up and sit at the table here -- is Professor David Elmore, professor of physics at Purdue University.

David is appropriately the godfather of, at least the father of, AMS and chlorine-36. He was the senior author of the *Nature* paper back in the '70s which kicked this whole type of study off and has continued in those studies since that time.

He came to Purdue in the early '90s to organize and direct the PRIME lab, which hands the cosmogonic isotope measurements for Purdue University as well as external sources, including those from Yucca Mountain to the Department of Energy.

Dr. Elmore will be, as Chairman Ryan has pointed out, David Elmore will be, discussing with us the measurement of chlorine-36 with the AMS technique and be discussing the uncertainties that are involved with those measurements and the many advances that the PRIME lab has underway to improve the measurement of chlorine-36 by AMS.

With that, I will turn it over to you, David. We welcome you here, and we look forward to this. It's been a topic of a lot of ad hoc discussion among the Committee. So we'll be interested to hear what you have to say.

DR. ELMORE: Thank you, Bill.

Let me start by saying that we developed accelerator mass spectrometry, chlorine-36, back in the late '70s. And the technique really hasn't changed very much.

We have been measuring samples. I measured June Fabryka-Martin's Master's thesis and thesis samples back in the early '80s. And what I am going to describe today is really pretty much the same thing. We have improved on some of the things, like the amount of beam we get out of the samples, but otherwise what we are doing is what you are going to hear about today.

Another thing I should mention, Mark Caffee, who measured some of these samples at Livermore, is now the director of our lab. So I work together with Mark.

Okay. So I am going to describe AMS, talk about the problems and challenges, the data analysis that we use for the samples, checks and balances.

Okay. Next. All right. The way accelerator mass spectrometry works, we form a negative ion in the ion source from a small sample, about pinhead size amount, of silver chloride. And so there is, of course, a lot of chemistry that comes before the AMS measurement. I'm not going to say too much about that.

So the negative ion comes out of the ion source. It's bent by a 90-degree injector magnet, which sorts to the mass. The different masses bend different amounts. We're selecting mass 36.

The negative ion is accelerated to the positive terminal, the tandem, which can run up to eight million volts. And then there is a stripper in the middle that removes electrons. So now we have a positive ion, accelerates again to ground, the other end. So we call it a tandem accelerator.

Then following that, there are two more large magnets, a velocity selector, an electrostatic analyzer. And these all, again, select the mass. And our limit of detection is one part in 10^{15}. Okay? I tell our eighth grade tours that if we fill up our football stadium at Purdue with sand all the way up to the top, it will hold about 10^{15} grains of sand.

So we're counting these chlorine-36 atoms, which has nothing to do with the fact that they are radioactive. Okay? We're counting them directly and one at a time in this detector at the end of the beam line. And so all of these magnets get rid of the interferences; in particular, the stable chlorine isotopes.

So what we measure is an isotope ratio. And so we inject both the stable in the radioactive isotopes by changing this magnet here to select the three isotopes. The stable chlorine-35 and 37 are measured right after this magnet here. So we go back and forth. We spend about a minute counting the atoms of the chlorine-36, and then we measure the beam current of the chlorine-35 and 37.

Okay. Next, please. Here is inside the accelerator. It is a 40-foot-long accelerator. The whole beam line is about 200 feet long. And this shows we're in there doing maintenance.

Okay. Next. All right. There are two things I want to convey here. One is the basic technique is really pretty simple. We're counting atoms. We're using magnets to get rid of things, but the apparatus itself is very complex.

This is the ion source. We developed this at Purdue, lots of vacuum pumps and power supplies and racks and beam line components, lenses, and beam profile monitors and the ion sources here. The sample changer moves the sample up through a tube here into a rod and goes down into the vacuum. Each sample goes into the vacuum one at a time.

We built this ion source at Purdue. It's one of the best in the world at doing this. There are only maybe six or eight places in the world that can measure chlorine-36, two in the United States: one at Purdue and one at Lawrence Livermore National Lab.

Okay. Next. This is another part inside the ion source. The primary cesium beam is ionized on a very hot ionizer here. The negative ions then go through the hole into the accelerator.

Okay. Next. All right. So I wanted to say just a few things about sampling. I am a physicist. I do the AMS. I don't do too much of the chemistry and very little collecting of samples.

Some of the problems and challenges. Well, there are many sources of chlorine-36. We have the meteoric produced from spallation of argon in the atmosphere. We have spallation of potassium and calcium on the surface of the Earth. And then we have activation of chlorine-35 subsurface. And then on top of all of that, we have the bomb-produced chlorine-36.

So there are lots of different sources. And when you measure chlorine-36, each atom, of course, you don't know where it came from. Possible problems in collecting samples, you know, one little bead of sweat from your forehead would swamp the chlorine in the rock. So, of course, you have to be very careful with chlorine. You know, if you live along a seacoast, just the chlorine in the air can be a problem.

Okay. If you're looking for one of the lower-level chlorine-36 sources, then you have a problem with contamination from the bomb pulse, which is orders of magnitude higher from the 1950s above-ground tests.

The *in situ* produced chlorine-36, that's produced in the rocks on the surface. And then the weathering of the rocks, that can get in the groundwater. That may be somewhat higher than the meteoric chlorine-36. And, of course, reactor material, since chlorine is a common contaminant in reactors, the chlorine-36 cross-section to make chlorine-36 from neutrons is so high that there is going to be chlorine-36 in any reactor materials and in, for example, groundwater around nuclear facilities.

If the sample is really small, I don't know if this is being done with samples from Yucca Mountain, but we would add carrier, which would mean we would add just the stable chlorine isotopes to give us more sample. And when you do that, you have to be careful about equilibration of that carrier with the natural chloride.

With chlorine, that is not really a problem, though. And something we do is add separated isotope chlorine-35, one of the two stable isotopes. And that tells us the amount of natural chloride that was in the sample when we add the carrier. And that is kind of a trick we use that is working very well.

Okay. Next, please. All right. As far as the measurement goes, I haven't measured. Our biggest interference problem with chlorine-36 is sulfur-36. This is an isobar. It's a rare sulfur isotope. And if the chemistry is good, we can reduce it enough in the sample so we can then distinguish it in our detector at the end of the beam line since as the same mass, it gets around all of those magnets.

And so this is an interference. The

sample preparation is really the key in removing the sulfur. And it has to be done right. Sample loading, which we do at Purdue, we have to be careful we don't introduce dust and things that would have sulfur in them.

And then, finally, once we sputter through a small sample, our cathode, which is our sample holder, will have sulfur in it. And so a small sample, often we have sulfur problems just because we consume the sample quickly and we get all of this sulfur from the holder.

Okay. So that is a challenge to separate the chlorine-36 from the sulfur-36. Another challenge is keeping 30 power supplies to determine the beam going through this complex accelerator.

If any one of those shifts a little bit, then it's going to change the result and we're going to get the wrong number. Okay? But, in particular, the terminal of the accelerator, we need to hold that constant to a part in 1,000. And if it drifts a little bit, again, we get the wrong result.

In order to account for these kinds of problems, we measure standards pretty often. These are samples with a known amount of chlorine-36 in them. And so we can correct for loss in the beam line.

There always will be some chlorine-36 loss. So we need that standard in order to normalize.

MEMBER HINZE: Where do you get the standards? And how are they set up?

DR. ELMORE: The standards come from NIST originally, but they have to be diluted down. And each lab does that independently, but we share standards with each other. So we make sure our standards agree with others. And so that hasn't been a problem with chlorine. And so --

MEMBER CLARKE: If I can follow up on that? The lowest point on the calibration curve is supposed to be close to the protection, whenever possible. That's an awfully low concentration you're detecting.

DR. ELMORE: Yes. And I skipped over the blanks here. Okay. So we also measure samples with no chlorine-36 in them. And then that tells us what our background level is. And that's right. A lot of our samples are down near the background.

So the blank correction becomes more important than the standard correction. And the standard correction is more important to get high precision on the higher-level ones. And so we're doing both of those.

CHAIRMAN RYAN: At this point, you are talking about kind of inside the laboratory divisions?

DR. ELMORE: Right.

CHAIRMAN RYAN: You're not talking about things like field blanks and other kinds of variability that creep in from the taking it out and --

DR. ELMORE: That's right. And I --

CHAIRMAN RYAN: -- all the way up through?

DR. ELMORE: Yes. I have another slide --

CHAIRMAN RYAN: Okay.

DR. ELMORE: -- that is going to address that a little bit. In terms of our beam lines, we know we're losing some chlorine-36. So the standard, we do a correction for that routinely.

Okay. Next. Okay. This is kind of our normal procedure for measuring samples. We measure the iso ratio of chlorine-36 to the stabilized isotopes three or four times. So we're going back and forth between the isotopes. So we're essentially measuring that ratio three or four times.

We measure, as I said earlier, both the chlorine-35 and 37 after the accelerator and make sure that we're getting the natural value for that.

And this is really something that helps a lot for chlorine-36. The radioisotope is midway between the two stable isotopes. So if there are any mass-dependent problems in the accelerator, we see that right away in a change in stable isotope ratio.

Then we put each sample into the ion source two to five times. So we're measuring the sample 20 times total. Okay. So we identify random sources of errors when we do this.

Okay. We measure the standard pretty often, every three to five unknown samples. And we measure the blank every 10 to 20 samples. So we're keeping good watch on all of this.

Now, the blank here again is the laboratory blank. A chemistry blank, which I'm yet to have another slide on, is submitted by the submitter. And that tells us what chlorine-36 contamination we might have in the chemical preparation.

So when I say we measured two to five times, as soon as we get better than about five percent precision, we stop. So we don't measure them all five times. In fact, we might get a three percent measurement after two times.

So uncertainties generally range three to five percent on samples that don't have any problems.

And I'll later show some examples of samples that did have problems.

Okay. Next, please. Okay. Here is the statistical data analysis. And this we do as we collect the data. We're continually updating the statistics. So when we're finished measuring a sample, we really know what the uncertainty is.

Okay. We have internal errors and external errors. The internal errors come from the actual measurement directly. And the main one, the counting statistics, we need 400 counts to get the square root of 400. Twenty is five percent. So to get five percent precision, we need to count 400 atoms. Generally we're counting a lot more than that.

And then if the beam current changes with time, we can put in an internal error for that. And then we combine using standard statistical techniques the errors to get an overall internal error.

Now, the external error comes from measuring the sample many times. And this is computed from the standard deviation. And also we measure the standards in blanks. And we fold in errors from those.

And so at each step in the process, we compute a weighted average of all of the measurements.

And we take the larger of these internal and external errors. That's a conservative approach and actually overestimates the uncertainties.

So after the first few cycles, the three or four cycles, we get the error from the standard deviation. And that then goes in as an internal error at the next level.

So when we measure the sample two to five times, that error is used up here. And again we take the larger of the internal and external error. So this is happening many times and very well accounts for any random errors.

Say one of those power supplies or the terminals drifted during a measurement. Then that measurement would be different from all of the others. Okay? And that would make the standard deviation higher. And then the final result would have a larger error.

And by measuring the standards from the blanks, those correct for any systematic errors there might be.

CHAIRMAN RYAN: Could you give us rough numbers on what those three look like? I mean, is the blank error typically two percent, ten percent? You know, the standard --

DR. ELMORE: Well, it depends. A sample that is a low-level sample, say, 10 times 10^{-15}, --

CHAIRMAN RYAN: Yes.

DR. ELMORE: -- if our blank is running 2 times 10^{-15}, we will subtract the blank. And that will give us 8 times 10^{-15}, but we put a plus or minus 100 percent on the blank. So we'll subtract two plus or minus two. Okay? And so that will put a 20 percent error on our number.

CHAIRMAN RYAN: Where's that?

DR. ELMORE: Right there. And so --

CHAIRMAN RYAN: That doesn't include the others.

DR. ELMORE: Right. And then all of the others will get combined and larger. So the large submitter always dominates from that equation.

CHAIRMAN RYAN: Right.

DR. ELMORE: I know I have gone through this quickly, and it's hard to see what is happening, but I do have some data, which I am going to show you in a minute, that includes some of all of these effects, like the problems with low samples where there is a blank problem with samples that have high sulfur and that kind of thing. We will see some actual data in a minute here.

Okay. Next. All right. Now, these, the checks and balances, are the responsibility of the person submitting samples to us. And in this case, the Yucca Mountain samples are all of the chemistries performed outside of our lab. If we were doing them in our lab, which is a possibility, we're doing the chemistry, we would certainly do all of these things.

Okay. First of all, the chemistry needs to be done well so the sulfur content is low because that is our main interference problem. And we need to have sufficient samples. If samples are much smaller than a milligram, it's best to add carrier to them to bring them up to a milligram because we're not going to get a good measurement for a sample that is much less than a milligram of chloride.

The submitter needs to check his reagents because there might be chlorine-36 in any of the reagents used in the chemistry. And so the user needs to take some dead chloride, which, you know, usually reagent chloride is good enough, and run it through the same chemistry. And that will tell us if there is any contamination in the chemistry.

We need to see multiple samples from each location. Okay? Then if that bead of sweat happens to get in one of them, it probably isn't going to get in all of them. And, of course, there can be sampling problems, I mean, variations in the rock, for example, that might cause trouble when you're leaching out the chlorine.

Multiple collection dates is a good idea. Just things change with time. Possible contamination can change with time. Okay. Then we invite blind repeat splits of the sample.

Two samples, we don't know this, but they are supposed to be the same. They are submitted to us. We would run them both as independently, as unknowns. And we like to see samples submitted in different runs. We run chlorine-36 every 2 months or so. And we can measure the same sample the next time. And it's a good idea to different to different AMS labs as well.

What I was going to say is some of our users have a big rock or, in particular, I am thinking of a guy that sends us meteorite, samples from meteorites. He has one meteorite. Every run he takes a little sample from that same meteorite and reruns it every time. And he knows before we do if there are any problems. He says, "Hey, my meteorite didn't come out right that run." Of course, that doesn't happen much, but it can happen.

MEMBER CLARKE: Within that group of threes, the variation between the different labs, the highest?

DR. ELMORE: Well, unfortunately, it's the one we do least. And we don't actively send splits to other labs. We occasionally do but certainly not every run. Unfortunately, I'm not sure it's done enough, but I don't see the data. You know, I don't really know how much they're doing this. I am not supposed to know.

But the few times that I know about that we participated in group measurements and several other labs do the same set of samples, things have come out pretty well.

MEMBER CLARKE: I guess what I was getting at, is there a laboratory variation so that if you have different rooms in the same lab versus different labs?

DR. ELMORE: Yes. This is a very complex measurement. And there certainly can be problems. For iodine-129, which is a fair bit more complicated isotope to measure, more difficult isotope to measure, than chlorine-36, we had a problem with our standards. And we didn't realize it until we compared measurements with another lab. When the standard was made, the iodine-129 was not in the equilibrium chemically with iodine. It was used for dilution. And light decomposed it.

So our standard was changing with time. And that really stumped us for a while.

CHAIRMAN RYAN: It's a bad thing.

DR. ELMORE: So things like that can happen. With chlorine, the chemistry is much simpler for chlorine-36. And I don't see that happening. But that is one reason we do measure samples.

Now, what we actively do is compare standards with other labs. They measure our standard. We measure theirs. We do that every couple of years or so. For chlorine, this hasn't turned up any problems at all. And so I think we're in good shape, Livermore being the only other lab in the Untied States that measures chlorine-36.

CHAIRMAN RYAN: Is that on a national capacity or is it just for Livermore?

DR. ELMORE: Yes. And there is a laboratory in Australia that we have exchanged samples with. And so yes. Now, what I'm referring to is the standards. We measure each other's standards. I don't know of people sending splits to Australia.

CHAIRMAN RYAN: I understand.

DR. ELMORE: They should be. They should be.

Okay. Next, please. Okay. Now I'm showing some data. This is from Jim Cizdziel. I'm not sure how to pronounce that name -- at UNLV. These are Yucca Mountain samples. And we just measured them in the last month. Okay? And it's three slides showing the data.

I don't know. I'm showing you this not knowing, really, anything about the sampling. Okay. So here we're finding the percent uncertainty versus the amount of chlorine-36. This is what we are measuring, the chlorine-36 to chloride ratio, 10^{-15}, where our blank is running around 1 times 10^{-15}.

Okay. So you see most of the samples are down below about five percent precision. This is our goal. Okay. And some of these, we are going to see why they are higher on the next two slides. Some of these have poorer precision. Okay?

But really what I want to show on this slide is when we get down to below 100 times 10^{-15}, then our precision just because we're not getting as many counts, you know, that internal area that came from the square root of the counts. We're not getting as many.

And so our error creeps up here from about 100 times 10^{-15} down to about 10 times 10^{-15}. Then below that, our blank starts being an issue. And so these ones that are right on zero, some of those, maybe most of those, are chemistry blanks. All right? I don't necessarily know --

CHAIRMAN RYAN: Is that why you have to have a red line that says, "Below detection"?

DR. ELMORE: Yes. Well, but, you see, this comes out automatically from the analysis. So the lowest ones have 100 percent uncertainty. Okay? And you're right --

CHAIRMAN RYAN: No. I mean, there's got to be a line on that graph that says, "Below this, we have no confidence we are reporting real numbers."

DR. ELMORE: Exactly. And what we do --

CHAIRMAN RYAN: And I don't think it's at zero.

DR. ELMORE: You're right. What we do, if a value of the measurement is less than about two times the uncertainty, then we say all we can do is set an upper limit. And that is what we say in our report. That is right.

And that number is around five. It depends on -- some run their backgrounds a little better than others, but our detection limit is around 3 to 5 times 10^{-15}.

CHAIRMAN RYAN: Right.

DR. ELMORE: But that's much --

CHAIRMAN RYAN: That's the red line after that --

MEMBER CLARKE: There's a quantitation limit in addition to the detection limit. Usually it's some multiple of the detection limit, above which you can assign a number and below which you really do --

DR. ELMORE: Right.

MEMBER CLARKE: Do you do it that way? Do you report it in terms of --

DR. ELMORE: Yes. I forget the exact -- I think it's actually three times our uncertainties. These uncertainties, by the way, if I haven't actually said it, are one sigma uncertainties. And so if a number is less than three times three sigma from zero, then we would consider that just an upper limit.

MEMBER CLARKE: Well, that is the detection limit.

DR. ELMORE: That is a detection limit, right. Above that, we report numbers.

MEMBER CLARKE: You report on --

DR. ELMORE: The errors --

MEMBER CLARKE: -- anything above the detection limit?

DR. ELMORE: Right. But, you know, we are reporting what the uncertainty is. So the submitter can take that to mean what they like.

CHAIRMAN RYAN: And this is just your measurement part? This is nothing to do with any uncertainty superimposed from any errors in the delivery to your --

DR. ELMORE: Exactly.

MEMBER HINZE: And it's an internal mixture.

DR. ELMORE: Yes. These are all the larger of the internal and external errors. We have not done any correction for a chemistry blank here. We have done the correction for a laboratory blank.

CHAIRMAN RYAN: This is what I would call the instrument error.

DR. ELMORE: Yes, exactly. This is the instrument error. Now, the submitter needs to submit a chemistry blank. If their chemistry blank is ten, then, you know, probably anything less than 20 or 30 should be considered the limit, but that is their responsibility to set that limit.

CHAIRMAN RYAN: That is a really important bit of information that you're only doing one instrument part. And any errors that are reported on your instrument part don't take into account any of these other errors.

DR. ELMORE: That's correct.

CHAIRMAN RYAN: Okay. Great.

DR. ELMORE: That's correct.

CHAIRMAN RYAN: You're going to tell me why it bounces all over the map?

DR. ELMORE: Yes. We'll do that. What I want to impress on you is you have the instrument error. We have a good handle on it. And these ones that have larger errors, we know what the problem is.

And we're reporting those larger errors. But I think most of the samples that are down as five percent error range, the biggest uncertainty is in the sampling problem.

Well, what is the source of the chlorine-36 atoms in this rock? You know, is it bomb? Is it meteoric? And that, of course, the submitter has to work that out.

CHAIRMAN RYAN: Well, it's the bead of sweat, and it's the --

DR. ELMORE: Yes.

CHAIRMAN RYAN: -- you know, origin of the source and all of that, homogeneity, and homogeneity.

DR. ELMORE: Yes. This has nothing to do with it.

CHAIRMAN RYAN: I mean, it's six dozen things. My guess is, as in most instruments, the instrument is the best thing you have got.

DR. ELMORE: Yes.

CHAIRMAN RYAN: And the rest of it can be anywhere from well-controlled to magic.

DR. ELMORE: But this is one of the more complex of the instruments out there.

CHAIRMAN RYAN: You bet you.

DR. ELMORE: Two hundred foot beam wide with 30 power supplies. So, therefore, I'm working a little harder to show you they're doing their job.

CHAIRMAN RYAN: Oh, no. That's going very well, I might add. Thank you.

DR. ELMORE: Okay. The next one shows the uncertainty versus -- the interference rate is the sulfur-36. And so we can handle quite nicely now samples with under 10,000 or so interference rate. And, for sure, these two samples up here have a high uncertainty because of the high sulfur rate. And so we didn't do very well on measuring those.

CHAIRMAN RYAN: I'm struggling with came out "quite nicely" because down near the lower numbers, you have got lots of uncertainties that are --

DR. ELMORE: Okay.

CHAIRMAN RYAN: -- north of 40 percent.

DR. ELMORE: Right. Now, a lot of these -- see, I really need a three or four-dimensional plot here to show you, but most of these samples here, a lot of these, were the ones that had the lower ratio from the previous slide. Okay? And --

CHAIRMAN RYAN: So you haven't factored out just the sulfur error?

DR. ELMORE: Right, exactly. This is all the data. My three plots that I've got here show all of the data. You know, what you can do is you can look at this one here. It's about 61 percent error. You can find that point on the other plots if you want.

The point is that most of the samples have a manageable amount of sulfur. The few that don't end up with larger errors. We don't have any up here with 30,000 counts per second sulfur that have a 5 percent error. That's not happening. These get a big uncertainty because of that. And mostly it's because there is a contribution towards signal from the sulfur-36. And that varies with time.

CHAIRMAN RYAN: So what's your criteria for sulfur when you're asking to sample? Below what?

DR. ELMORE: Well, it ends up being about a part per million sulfur is where we have trouble. See, unfortunately, sulfur-36 is only 10^{-4} abundant. It's .01 percent of sulfur.

CHAIRMAN RYAN: Right.

DR. ELMORE: And so the part per million translates to part in 10^{10} sulfur-36, but that is still five orders of magnitude above our background. We can handle that five orders of magnitude. We can handle our detector because our detector gives a different signal for sulfur and chlorine. And that lets us handle pretty well up to about 10,000 a second.

And so we actually subtract a background that is linear with the sulfur.

CHAIRMAN RYAN: Just so I'm clear, David, I want to understand these. Are there very large errors that have nothing to do with the sulfur?

DR. ELMORE: Yes, yes.

CHAIRMAN RYAN: Those two would stay in the '70s for other reasons?

DR. ELMORE: Exactly.

MEMBER HINZE: Those would be in this error rate shown?

DR. ELMORE: Yes. And the previous slide and the next slide show the other reasons we have. And the previous slide showed a lot of low chlorine-36. So we didn't very many counts. Those have large error because of the low amount of chlorine-36.

And the next slide, if we can move to that, here we now have a problem with low beam current. Okay? This is essentially the sample size. If the sample is really small, we're not going to get much beam out of it. Okay?

And this last batch, this is a little unusual. This last batch, we had a lot of really small samples. And the beam currents are way low. And these produced a big error down here.

And we can handle them. With pretty low beam currents, we can handle them. But when they get right down under 100 or a few hundred nanoamps, then there is a problem.

So these two over here were the high sulfur and either sulfur or low chlorine-36 content.

CHAIRMAN RYAN: See, when I look, I guess

I'm trying to find the other points in the previous graphs. I don't know how to do that. I mean, I'm looking at those two high points, and where are they in the other graph. There's nothing in the 95 percent. I guess it is on the chlorine-36 one. Is that right?

DR. ELMORE: Yes. I think one of those points was actually off scale on one of the other -- the part with the high sulfur, and it didn't show it.

CHAIRMAN RYAN: I'm just not tracking which one. I mean, if I look on the second sulfur, if you could back up, there's a spot right there under 40 percent on the right side.

Is that data point supposed to show up on another graph somewhere? I don't see it.

DR. ELMORE: Yes. There's one point I know didn't show up on the other graph, and that's because it was off scale. And I didn't want to adjust the scale to get them all on there.

CHAIRMAN RYAN: It's just real hard to follow and suggest. I can't follow one or the other.

DR. ELMORE: Yes. But I'm not really intending you to do that.

CHAIRMAN RYAN: Okay.

DR. ELMORE: I'm just showing that --

CHAIRMAN RYAN: Well, you told me I could.

DR. ELMORE: Yes. Okay. You're right. But the thing is we have the high sulfur. We have the low amounts of chlorine-36. And we have the small sample size. Those are the three things that give us large uncertainties.

CHAIRMAN RYAN: One thing that's not clear -- and it is I think an important part of uncertainty analysis. And let's just for the sake of the argument say that anything below 5,000 is good with regard to sulfur. Anything below -- pick a number --

DR. ELMORE: Okay.

CHAIRMAN RYAN: -- with chlorine, the chlorine ratio is okay.

DR. ELMORE: Okay.

CHAIRMAN RYAN: And anything below -- I don't know. Pick a number here, whatever you like. But what happens at these higher values is things bounce around a lot. And, you know, you would expect if you really get better as this analyte increased, then you would see it smooth out.

DR. ELMORE: Well, but the --

CHAIRMAN RYAN: Now, I see this range of high points down here as a detection limit problem. But what I don't see is that this curve doesn't get smooth. This bounces all over the place from, say, 250 on up to 1,500.

DR. ELMORE: Right.

CHAIRMAN RYAN: Now, I understand there are competing issues.

DR. ELMORE: Right.

CHAIRMAN RYAN: But how do I have confidence in this when I don't know what is causing this to bounce around? So, I mean, for example, if I said, what is the average uncertainty between 500 and 2,000, I would take an average of those numbers.

DR. ELMORE: Yes. Well, as I say, I can't --

CHAIRMAN RYAN: That's what I propagate.

DR. ELMORE: I can't do a five-dimensional display on the screen.

CHAIRMAN RYAN: Right. Maybe I'm being unfair for that reason.

DR. ELMORE: Right.

CHAIRMAN RYAN: I'm just trying to understand it.

DR. ELMORE: Yes. We give the submitter the spreadsheet that has all the details of these measurements if they choose to use it. And they can then see why.

Every one of these is high for a reason. Okay? And looking at the spreadsheet, they can see whether it's low, chlorine-36 low, or high sulfur-36, or low beam kerner from a small sample. Those are our three big problems.

CHAIRMAN RYAN: On your end.

DR. ELMORE: On our end, right. And so the submitter can see where the problem was and --

CHAIRMAN RYAN: But the real secret is when they propagate the error, they've got to propagate it in a way where that error translates to the answer.

DR. ELMORE: Yes. I mean, it's a --

CHAIRMAN RYAN: It's a ratio of chlorine-36 to chlorine stable plus or minus some percentage --

DR. ELMORE: Right.

CHAIRMAN RYAN: -- accounting for all of those things and the wrong sampling error.

MEMBER CLARKE: Well, if you had the 20 measurements, basically 20 measurements, you could come up with the uncertainty on that.

DR. ELMORE: Yes, exactly.

MEMBER CLARKE: If you come up with an uncertainty that's greater than a few percent or ten percent, then you don't use those in the analysis. Is that what you would suggest?

DR. ELMORE: Yes. Well, that's right. And so if the submitter gets back a report that has uncertainties of 40 percent to 100 percent, those samples probably they should weed out and because there were --

CHAIRMAN RYAN: Oh, I would suggest just the opposite, that they be included.

DR. ELMORE: Okay.

CHAIRMAN RYAN: That's an uncertainty of the overall system that must be included. Weeding them out is a horrible thing.

DR. ELMORE: Well, fine. But they need to be weeded according to their uncertainties. If there's a big uncertainty, they shouldn't count it very much.

CHAIRMAN RYAN: No. If it's a big uncertainty, they should include it because it's a measure of system uncertainty.

DR. ELMORE: Okay. Well --

CHAIRMAN RYAN: You know, they're trying to measure an analyte in a sample and just saying, "Well, I followed the detection limit" or maybe that gives you a real, true, effective detection limit for the whole process. Tossing out data is generally a bad idea.

DR. ELMORE: Yes. Well, we never throw away anything.

CHAIRMAN RYAN: Oh, I know.

DR. ELMORE: The submitter gets --

CHAIRMAN RYAN: But, I mean, just ignoring some and accepting some because the analytical work was theoretically better on some, rather than others, that's a risky slump, I think.

MEMBER HINZE: But the uncertainty tells you that something is wrong in the measure.

DR. ELMORE: Right, exactly. Now, if none of these samples --

MEMBER HINZE: In this whole process.

CHAIRMAN RYAN: Well, in the instrument part, which is what we're hearing about so far.

MEMBER HINZE: Yes, but it may be because you have a high sulfur that is not practically taken into account. And that's a part of the sample and sample collection and sample preparation, as I understand it.

DR. ELMORE: Yes. If we know what caused the uncertainty, then they don't have to worry about that coin too much if it's off where it should be.

Where they have to worry is if we report a five percent precision measurement and when they plot it with their other samples, one of them is way off in left field, that is when the bead of sweat got in there, something we don't have any control over. And that sample, yes, should be left there to show that there are other problems.

MEMBER CLARKE: I think we are making a distinction between the instrument limit, which is running the samples without any interferences or any complications, what is usually called a method detection limit, where now you're running actual samples. You've got other things going on.

CHAIRMAN RYAN: And the system error is obviously the combination of both. And it's only when you understand every component of system error that you really understand uncertainty.

MEMBER CLARKE: If I understood what you said to Mike's earlier question, you point to some of the different graphs that are all the same samples.

DR. ELMORE: Yes.

MEMBER CLARKE: So if you numbered those, you would be able to match them up.

DR. ELMORE: Yes, yes. Good point. We could have done that. And if anybody here would like to see the spreadsheet of the raw data, you know I can provide that.

CHAIRMAN RYAN: That would be helpful, actually.

DR. ELMORE: And I think there is only one point that was off scale on this plot. I think it was -- I don't know. I'm not sure right now.

CHAIRMAN RYAN: It doesn't really matter. If we can get the spreadsheet, we can figure it out.

DR. ELMORE: Yes, exactly. Okay.

Then I have a couple of conclusions to make here. Okay. AMS is a complex analysis tool. And, really, it takes the physics department, where we have ten employees and to keep the thing running and two Ph.D. physicists running the accelerator. So it's a very complex tool.

We make a very active use of standards and blanks. We measure them a lot. That helps us identify any problems in the system. A complete error analysis usually identifies samples with problems. Okay?

These are problems in the measurement. And I say "usually." There are other things I didn't talk about here that can be problems, like mixing of samples and stuff. And it happens but pretty infrequency. Maybe once a year we get a sample that got mixed up in the loading or something.

So there are other problems that don't show up here. And, you know, some of those will be showing up with the splits sent to us and other labs or just two samples sent to us. And so it's a responsibility of the user to submit multiple samples in blind split repeats.

I'll be glad to take any other questions. That is what I had to present.

MEMBER HINZE: Ruth?

MEMBER WEINER: Thank you for a very interesting discussion. I just have a couple of questions.

How does this method compare in precisions and resolves and uncertainty with if you just tried to assay chlorine-36 radiologically?

DR. ELMORE: Okay. The half-life of chlorine-36 is about 300,000 years.

MEMBER WEINER: So you get a very weak --

DR. ELMORE: Very weak signal. If you counted for months on one sample, if it was a large sample, you might be able to see. Davis and Schaeffer tried to do this in the '50s, actually. They predicted chlorine-36 would be produced in the atmosphere. And they tried to measure it with decay counting but failed. And so that's really out of the question.

MEMBER WEINER: So this is, really, only the method of choice, then, --

DR. ELMORE: Well, let me --

MEMBER WEINER: -- the only method you can use?

DR. ELMORE: Well, let me say the standards were counted by decay counting. Okay?

MEMBER CLARKE: We could use ICP NMS, but according to what you told us in the beginning, this was at least 1,000 times in lower detection limit. I think that is parts per trillion pretty much.

DR. ELMORE: Yes.

MEMBER WEINER: Yes. That was my next question. What if you used ICP NMS? Could you --

DR. ELMORE: It would have the same problem with the sulfur. And the sulfur is about five orders of magnitude higher than our background signal. It takes the high energy.

I didn't mention that there are two things that you need the high energy, this big accelerator, for. One is to separate the sulfur from the chlorine. We do that from the basis of energy loss in our detector. That only works with many MEV of energy. And the other is destruction of molecules. There can be other molecules of mass-36. And so these would be problems with ICP NMS.

MEMBER WEINER: So you really have narrowed down to a method that isolates the CL-36. And that was why you can't --

DR. ELMORE: That is correct.

MEMBER WEINER: My other question is, who supports this?

DR. ELMORE: The National Science Foundation Solid Earth Sciences. We have block funding from them. Most of the work we do is the *in situ* produced chlorine-36 and other nuclides, bromium-10, aluminum-26, in rocks on the surface of the Earth to measure exposure time of the rock so we can date volcanic eruptions, earthquakes that will expose rocks to cosmic rays, glacial marines, and landslides. That is most of our work.

MEMBER WEINER: When people send you samples, do they pay for the analyses or do you support that?

DR. ELMORE: Yes. If it's research samples with the types of research the NSF funds, then we charge half-price still. And the other half really comes from our block grant.

And for samples such as Yucca Mountain, if that's not considered just basic research, then we charge the full price, which is about $400 a sample.

MEMBER HINZE: Allen?

VICE CHAIRMAN CROFF: In your talking, you alluded to a number of radionuclides that you mentioned. I mean, we focused on chlorine-36 but iodine. You just mentioned some others. How long is the list of things that this will make? I mean, is it a long list or --

DR. ELMORE: Six.

VICE CHAIRMAN CROFF: Okay.

DR. ELMORE: There are six isotopes we measure. And we usually spend a week or two on each one. And so it takes a few months to cycle through all of them.

VICE CHAIRMAN CROFF: Okay. Thanks.

MEMBER HINZE: Dr. Ryan?

CHAIRMAN RYAN: Has anyone reported that you know of a complete analysis of sampling error and system error over a large, integrated number of samples?

DR. ELMORE: No. June Fabryka-Martin was very active in our data analysis, these samples in particular. And she had a good body of data on Yucca Mountain. I don't know that she has published that, but she would be the one to do it.

CHAIRMAN RYAN: Yes. Well, I guess you can tell by my questions I have been struggling with understanding a few basic things. On the instrument side, which I really appreciate your insights, there is a red line. Below the red line, you don't have any confidence in an answer.

DR. ELMORE: Right.

CHAIRMAN RYAN: Wherever you want to draw it, for whatever reasons, that is --

DR. ELMORE: I mean, there are really three red lines --

CHAIRMAN RYAN: Exactly.

DR. ELMORE: -- shown by the three plots.

CHAIRMAN RYAN: So, you know, if I have a known sample, I can tell which red line I am really focused on. If I get a field sample, the weakest of the red lines; that is, the one highest up on the *x*-axis, is the one I have to draw.

So it would be interesting to try and figure out how to take field samples. And you need, you know, obviously more than three or four or six. You know, you need hundreds to really do a good job to say what were the field sampling errors and then what were the instrument errors assuming some detection limits and on the other issues, which you very well discussed, and do a systematic error for all of these samples and then a systematic error for the collection of samples before you can really say or interpret these ratios.

I mean, everybody typically reports an instrument error when they say, "Oh, the error of a gamma spec is X percent of cobalt-60." Well, you know, that's the instrument. That's not the system error that got you to the sample that you want the analyte for and that those are typically the sampling problems dwarf the instrument problems.

DR. ELMORE: Yes.

CHAIRMAN RYAN: In almost every case I know of, that's true. So how do we put all of this together? What would your recommendation be to --

DR. ELMORE: I think it's true that the Yucca Mountain studies measure many samples of the same from the same source.

CHAIRMAN RYAN: Many? "Many" being 50? Twenty?

DR. ELMORE: Well, June did hundreds.

CHAIRMAN RYAN: Hundreds.

DR. ELMORE: We're sure. But, sure, I think -- well, these samples from UNLV, I think here we saw so many that were below the red line because I think they are trying new things. Okay? They're trying small samples in places where there are real low ratios.

CHAIRMAN RYAN: Fair enough.

DR. ELMORE: But I think a lot of the work June did -- there weren't so many below the red line, but, at any rate, I think even that, even this recent data, that there are certainly tens of samples that we got good measurements on. And I suspect that a lot of those are from the same site.

And so then you can pop those in and look at the distribution. And that distribution for sure will have a wider range than five percent.

CHAIRMAN RYAN: And that is the real error?

DR. ELMORE: That is the real error.

CHAIRMAN RYAN: That's what I keep reaching for --

DR. ELMORE: Yes.

CHAIRMAN RYAN: -- to get to hear much about.

DR. ELMORE: But I think AMS is a

complicated enough measurement tool that you might conclude that a lot of that error came from AMS. And I am trying to lay to rest that isn't true.

There certainly are problems, but when those problems occur, we can identify them. We can say, "This sample didn't get measured well because of" such and such a problem. And we do.

For the person who doesn't want to look at that spreadsheet, we tell them why. Whenever any sample is over five percent precision, we them why.

Let me say there have been studies of, for example, glacial marines, where we measure lots of boulders on the marine exposure age. And there have been cases where they all agree to within three to five percent over a field of rocks. And it's unbelievable.

So what I am saying is this -- and this is what we are funded for. And it works really well. And so it's possible to have a low scatter of data from field samples, but the trouble is leaching rocks, which is what we're doing mostly in Yucca Mountain, there are different sources of the chlorine-36 and different rocks or different amounts of leaching will give you different answers. And then that puts a spread in the data.

CHAIRMAN RYAN: And, again, I think we are agreeing that the sampling uncertainties, you know, right from getting the sample to the chemistry on the sample and all of that, is probably a much bigger error than what you are documenting to us.

DR. ELMORE: That's the bottom line from my talk.

CHAIRMAN RYAN: I'm guessing that. So, you know, we're taking that away. But what we really need to understand if the interpretation of this data is valuable is, what is the real uncertainty? What is the system uncertainty?

DR. ELMORE: Yes.

MEMBER CLARKE: So you have helped me reshape my question. Thank you.

DR. ELMORE: Well, if you haven't had Fred Phillips give a presentation here, he might be the -- he's the world expert in chlorine-36 from the geology point of view and hydrology. He's a hydrologist. He's done chlorine-36 with me since day one. And Fred would give a nice presentation from that point of view, I think.

CHAIRMAN RYAN: Thanks. That's a good suggestion.

Jim, sorry. Thank you.

MEMBER CLARKE: I share Mike's concerns. I started out on the chemical side. So I'm using different words than he is, but, you know, this overall what he's calling system error and what I would call method detection limit. And I'm even a little concerned about recording above the detection limit because all the detection limit tells you is it's there. It doesn't tell you how much is there.

Normally there is an area of uncertainty until you get to a point where you -- you know, what you are doing is not unusual. I haven't heard anything about the detection limit.

Allen asked you about other radionuclides. You said there were six?

DR. ELMORE: Yes.

MEMBER CLARKE: You could theoretically tune this to anything? Is that right?

DR. ELMORE: Well, we measure radionuclides because they are what is rare in nature, not because they're radioactive and other isotopes. And we've tried technetium, for example. But the problem is the isobar, the equivalent of the sulfur-36. They swamp us.

We're building a new beam line. So we are starting to work on new isotopes, but each one is a major development.

MEMBER CLARKE: Yes. I was just talking about the technique in general. Obviously you would have sources of interference for different radionuclides. You would have to deal with those, but in principle you could apply the standard to radionuclide. You would just have different --

DR. ELMORE: In principle.

MEMBER CLARKE: You would have different uncertainties.

DR. ELMORE: In practice, we have been doing this for 30 years. And we have tried lots of other ones and failed so far. But there are some that are promising we're working on.

MEMBER CLARKE: And how do you remove the sulfur?

DR. ELMORE: How do you remove the sulfur?

MEMBER CLARKE: Sulfur.

DR. ELMORE: Okay. Well, first of all, in the chemistry, by precipitating silver chloride and redissolving in ammonia, the sulfur doesn't precipitate. So you do that several times and --

MEMBER CLARKE: It's a chemical separation?

DR. ELMORE: So it's a chemical. That's

the first one. Then the other is the detector. The rate of energy loss in the gas in our detector for sulfur and chlorine are different because of the different atomic number. And that gives us a different signal. And this is a standard nuclear physics technique. So we need both of those to remove the sulfur.

MEMBER CLARKE: And that is typically done by the person sending you the sample?

DR. ELMORE: The chemistry.

MEMBER CLARKE: And there is --

DR. ELMORE: The first step, yes.

MEMBER CLARKE: Have you listed the variation?

DR. ELMORE: Well, for sure, some submitters are better able to remove the sulfur than others, but we're now able to handle pretty high sulfur. That 10 to 20 thousand count-per-second limit we have now used to be 2 or 3 thousand. So we have improved our measurement technique by an order of magnitude.

And so most samples are okay. It's not usually a problem. The ones that are a problem are where we don't work in a class 100 clean room and so we get -- one little dust particle can wipe out a sample with the sulfur. So we occasionally get high sulfur ones.

MEMBER CLARKE: Thank you. Very interesting presentation.

MEMBER HINZE: The thing is, let me ask it a little differently regarding this whole system if you will. We have the sample collection. We have the chemical proliferation. And we have the actual AMS measurement.

I know that you are very interested and PRIME is very interested in making certain that the results of these are scientifically interesting and justifiable. But there is this potential for a disconnect between those three elements.

Do you hold courses in helping people to understand what your problems are in measurement so that the sample collection and the chemical preparation really keep in mind what is the end product down here in terms of actual measurement? Is that sort of thing done?

DR. ELMORE: We host visitors pretty often. And we're glad to share our chemical preparation procedures we have written up. We share those openly to anybody.

And our sample submitters learn very

quickly the sulfur problem, the sample size issue, and the detection limit. I mean, they learn those three pretty quickly; if not before their measurements, at least after their first batch of them.

So, you know, I don't think there are enough new users out there to really have a class. We would. In fact, we are planning to start doing that for the geology users for the *in situ* produced nuclides that are most of our business. We're talking about we started having a class on that, kind of a workshop.

I mean, there is a conference, accelerator mass spectrometry conference, every three years. And there are a lot of interactions there on discussing these problems.

MEMBER HINZE: One of the major problems that you have is the sulfur. And I think volcanic peroxide. I think of sulfur. There is a lot of sulfur in volcanic rocks. Do you find the uncertainties higher in measurements that are made in volcanic rocks than you do in others? Do you have any feel for this?

DR. ELMORE: I think there's no real correlation between the amount of sulfur in the original sample and the sulfur in the sample we run.

I think the chemistry is adequate.

The sulfur we see I think is more from blunders and that dust particle there or not doing the chemistry right. I mean, sometimes the first time the submitter sends us samples, there is sulfur in them.

But I think that that is not an issue, really, how much sulfur is in the original samples.

MEMBER HINZE: Do you have better consistency of the results when you actually do the sample preparation itself?

DR. ELMORE: No. Our technician can give us samples that are as high in sulfur as anybody on occasion.

MEMBER HINZE: Okay. We have just a few moments. I will open this up to anyone in the audience who has a question for Professor Elmore.

MR. HAMDAN: David, I have a question.

CHAIRMAN RYAN: Please come and sit at the desk and tell them who you are just because of he microphone problem.

MEMBER HINZE: Use the microphone.

CHAIRMAN RYAN: Tell us who you are, too.

MR. HAMDAN: I'm Latif Hamdan with the ACNW staff.

David, I just wanted to ask you -- I don't know if this is in the scope of your representation or not, but still you mentioned you did something for Yucca Mountain, either present there or if you have an expert opinion on the subject as to whether or not this is all that we know, Yucca Mountain, whether this method, the chlorine-36 method, is a good enough method to get you a good number for the --

DR. ELMORE: Yes. Well, you're asking a physics professor, not a hydrologist or a geologist. I mean, we know where the source is of the chlorine-36. The subsurface sources of chlorine-36 come from decay of uranium and thorium, which produce neutrons that make chlorine-36. And we can predict how much of that there should be. And it's a pretty low number, usually less than 10 times 10^{-15}. Okay?

So anything above 10^{-15}, which is right down near our detection limit, anything higher than that must have come from the surface. Okay? Neither can be combination. That occasionally will happen, but that isn't going to usually be the problem.

It has to come from production in the atmosphere, production on the top meter of the Earth's surface, and production from above-ground bomb tests.

Those are really the three sources. And so if you're finding chlorine-36 down under, it's got to come from one of those. All right?

And the production since it's a 300,000-year half-life, the production in the atmosphere and on the surface of the Earth, that's been happening all along. And so the numbers you see that range up to about 1,000, you know, all that's telling us is the travel time from the surface to the below-ground sampling depth took less than a few half-lives, let's say less than a million years or less than half a million years.

Okay. But anything you see above 1,000 times 10^{-15} must have come from the bomb tests. And I'm not too knowledgeable about all of the work that has been done on that, but it's my opinion that if you consistently see samples above 1,000 times 10^{-15} in one place, that means chlorine-36 from the bomb test, which means the last 50 years, is getting down there. So I don't know of any higher-level sources of chlorine-36.

And for sure, you know, if you leach more chloride out of the rock, then you're getting more of the lower-level chloride that was originally in the rock that only had chlorine-36 from the neutrons. So if you leach more, that's going to lower your numbers because you're diluting it with the old chloride.

So if there are any samples at all that were well above 1,000, it seems to me that has to be from transported by water in the last 50,000 years.

And that's my feeling from what I know about the subject. And I don't claim to be a real expert.

MR. HAMDAN: Yes. I was thinking just in terms of the passage of the measurements, uncertainty in the sampling and the measurement itself, how that is relevant to the passage.

DR. ELMORE: Yes. Well, the vast majority of our samples -- and I think this data I showed you had more with higher uncertainties than we usually have because they were special samples.

Most of our samples have had uncertainties at around five percent. And so, you know, if it's 1,000 plus or minus 50, that's a pretty small range. And so if you are seeing some samples that are a few thousand, for sure that wasn't because of our measurement. That's --

CHAIRMAN RYAN: But again, that's the instrument error, which, you know, I mean, I still say that is not a measure of error of the sample.

DR. ELMORE: Right.

CHAIRMAN RYAN: It's a measure of error in the instrument measurement.

DR. ELMORE: But how could there be errors in the sampling that give you -- if you measured a sample that was, say, 5,000 times 10^{-15}, it would have to be contamination from some source of --

CHAIRMAN RYAN: Sure, right.

DR. ELMORE: -- of chlorine-36.

CHAIRMAN RYAN: But without systematic documentation of that, we're guessing.

DR. ELMORE: Yes.

CHAIRMAN RYAN: We're just making a guess. That's my point. You know, nobody has really taken this on as a real systematic error analysis. You have confirmed that at least.

DR. LARKINS: You mentioned earlier that there was some comparison of blanks for using the same technique between your lab and the Australian lab. What was the variability in the measurement of those blanks, standards?

DR. ELMORE: Well, both. I mean, it is appropriate to look at both standards and blanks.

DR. LARKINS: Yes.

DR. ELMORE: The standards we're now agreeing to better than five percent. In fact, I think it's one to two percent as we're agreeing on the standards among the labs. And so that certainly isn't a big issue.

CHAIRMAN RYAN: But is that a fair test as you have been looking at the same standards now back and forth for years? What would happen if, for example, somebody gave you a split of a known sample and sent it to both of you, one that didn't agree within that two percent? It's a different question.

DR. ELMORE: When we measure the standards, we measure many more times. And there are higher-level samples. So we can do that to one or two percent.

The unknowns, you know, the best we would probably look for would be five percent. And the samples we have compared, I didn't bring any data to show you but have been on the order of five percent. So --

CHAIRMAN RYAN: That's the best you --

DR. ELMORE: So we --

CHAIRMAN RYAN: What is typical?

DR. ELMORE: Yes. Typical comparisons on chlorine-36 -- now, other nuclides, like iodine, it wouldn't be so good, but with chlorine, 5 percent agreement. Okay. But to one sigma error, you're going to have a few that might be ten percent different, but five percent is I think a reasonable uncertainty to be putting on it.

That's one of the reasons we don't measure. We don't try to measure to better than five percent because if we measure a lot of splits with Livermore, probably on average they would agree to about five percent. Neither of us are going to try to do better than that.

MEMBER CLARKE: I think the other point is, what is the value of the standards, the standards made at very low ratios?

DR. ELMORE: We measure standards that range from about a 100 times 10^{-15} up to 40,000. We have set up with, actually, a 40,000 standard, but our typical one is 8,000.

MEMBER CLARKE: And do you get the same precision at the low levels as you get at the high one?

DR. ELMORE: Yes.

MEMBER HINZE: Well, thank you very much, David, for an excellent presentation, very informative. You have been very helpful to us. We really do appreciate it.

With that, we will move directly into an update on the chlorine-36 studies of the Department of

Energy. Drew Coleman will be making the presentation for DOE. And, if I understand correctly, this is concerning the validation report of the DOE that is pending and that we are all looking forward to seeing.

DR. D. COLEMAN: Yes. Thank you for this opportunity to address the ACNW. I appreciate it. I don't like to follow up people like David with a bureaucrat, but I'll see what I can do here.

I'm a geological engineer by training and a geologist by experience. I'm a bureaucrat for the DOE. I'm a task manager, a saturated zone manager, university task manager, and a USGS technical monitor.

CHAIRMAN RYAN: Drew, if I may just -- we're going to hook up some folks in who wanted to listen in. So we will just let that happen.

DR. D. COLEMAN: Okay.

CHAIRMAN RYAN: And we'll pick that right up. Sorry. We should have gotten that done ahead of time.

(Pause.)

CHAIRMAN RYAN: Good morning.

MR. FITZPATRICK: Good morning.

CHAIRMAN RYAN: Just to complete our record, could you tell us who is on the phone and who you are with, please?

MR. FITZPATRICK: This is Charles Fitzpatrick, Nevada.

CHAIRMAN RYAN: Okay.

MEMBER HINZE: Thank you very much, Charles.

CHAIRMAN RYAN: Can you hear us all right?

MR. FITZPATRICK: Yes, I can hear you fine.

CHAIRMAN RYAN: Okay. Fire away, Drew.

DR. D. COLEMAN: Okay. So I'm Drew Coleman. I'm from the Department of Energy. And I'm giving a presentation entitled "Update on Chlorine-36 Studies."

Now, David talked a little bit about some of the parts of the study, but parts I'm going to talk about are what I call the USGS/Los Alamos conflicting reports portion and where that is.

There is also a follow-on, UCCSN cooperative agreement study, that I'll talk about. And when I talk about the USGS part, that was actually a consortium of the USGS-Lawrence Livermore with Mark Caffee, who was mentioned by David, and some ACL people. But the USGS led the study in my view.

And the Los Alamos part, it would be June Fabryka-Martin's early work and then some follow-on work by a guy named Bob Roback that worked on that. And the UCCSN work is Jim Cizdziel as a chemist; Fred Phillips as the, I guess, chlorine-36 guru on the effort; and Jean Cline, who has done some sampling and some activities prior on the project and was sort of their sampling lead for their UCCSN effort.

So you folks have had a presentation on this subject. I think it's been a few years ago. And there is not really a lot to say, but I was going to go over the history just briefly and if you've got questions I guess try to answer them during the questions part.

In 1996, the TBM was mine in the exploratory studies facility. And systematic samples and feature-based samples, which focused on, say, faults or fractures or other kinds of features, followed the TBM in some cases right behind it, in some cases followed on maybe after a few months had gone by. But those were the systemic and feature-based samples that June did some chlorine-36 measurements on and reported the early chlorine-36 results.

The chlorine-36 testing, there had been an assertion that a layer of non-welded tuffs, the Paintbrush non-welded units or the Paintbrush Tuffs, the non-welded units, provided a tin roof or some sort of a barrier to infiltration and that it was likely that no infiltration made it through those.

And so the bomb pulse chlorine-36 reading below those units in the repository horizon was an interesting result. Now, the word "localized" that appears in that third bullet is a key word.

No one asserts that there's any more than just a few localized areas where there are fast pathways, as indicated by the data that we have collected. But there were a few at the Sundance Fault, at the Drill Hole Wash Fault locations, which is where studies have sort of focused.

Now, the fracture mineral, we continued with the excavation through the ECRB. And people have looked at chlorine-36 in there. I think there have been a few hits.

So, with these bomb pulse hits, these few fast pathways, the DOE chose to fund a validation study and have another organization other than Los Alamos take a look at the results and see if they could replicate it. And that was a decision I didn't participate in, but it was a decision that was made.

Now, there is a typo on this last bullet. It says, "Lawrence Livermore National Laboratory was funded to provide a measure of oversight." That should read, "Los Alamos" because we wanted to keep the Los Alamos, who had made the original bomb pulse report, in the study.

So next slide. The USGS developed this sampling methodology, and they were worried about contamination. This was long after the TBM had gone through and there had been some wall washing and things.

So they decided to drill core holes. And they drilled 50 core holes. They drilled 40 of them 4 meters deep into the Sundance Fault area, where one of the hits had been reported, and another 10 in the Drill Hole Wash area, which was in the north ramp and wasn't so much of a repository horizon, but they drilled some there because there had been hits there also. And they focused on that.

Two meters, it was the furthest back in the wall to sort of minimize assertions of contamination. And they worked with Mark Caffee. And he leached those samples. And he reported very low levels, lower than any that have been reported by Los Alamos. And they started to look at his technique.

And he was leaching the samples. USGS got the samples and then actually just sent the samples in bulk to Livermore and let them do all the work from there on. And he leached the samples by crushing them and rotating them in a drum for eight hours.

And when people got to looking at that, they thought that was somewhat too aggressive of a leach. He defended his original work as chlorine is hard to get out of rock. But in the end, they decided to perform some leaching studies.

And the USGS and Los Alamos did a lot of work to look at how leaching affected the samples and what kind of results you got and settled on a mutually agreeable path that short passive leach, for example, an hour was desirable as sort of passively genuous, short passive leach, where you just put the sample in the water and let it leach for an hour and then take the sample out and send the results in to be looked at.

CHAIRMAN RYAN: Just a quick question. Leaching is a surface area question. Are you talking about taking a chunk of rock and just sticking it in a leach solution?

DR. D. COLEMAN: Right, crushing it and sizing it maybe to die size, but I think essentially in some of Jean's work, she put whole blocks in a pot and leached them, crushed them sometimes and leached them. And these I think were crushed and sized and leached but yes, pretty much just putting it in and letting it --

CHAIRMAN RYAN: They were crushed, then sized and leached, though?

DR. D. COLEMAN: Sized and leached, yes. And they looked at the size effects of things. And that is all described, but I will get to where that is described.

So on the samples that they performed the leaching studies, the USGS leached some samples using the mutually agreed-upon technique and sent aliquots -- so that's the water, the leachate -- to Los Alamos and Livermore.

Now, Los Alamos preferred to spike their own samples and precipitate their own targets and get them all ready for David to just put in his AMS, however that works, but it could be described in the process.

Livermore, the USGS sent them to Livermore. And they did that same process, the spiking, the targets, and putting them in the AMS. And those two agreed on a one-to-one line on a graph when they were plotted. And that is kind of a key result.

It is that result that led us to be confident that the wet chemistry or the AMS part was really not the problem. They could both get the same answer from a water sample taken and sent and the wet chemistry done each according to their own AMS done that they could replicate each other's results.

Now, admittedly, it was a low result, but I guess for me, the -- what I am wanting to call the wet chemistry and the AMS, the part that David was talking about today. I don't believe that is where the area of disagreement is. I believe it's elsewhere.

But the tricky part is there was some core that June had originally done that showed a bomb pulse signature, and it was for Niche 1, which is near the Sundance Fault area, where other hits had been reported.

So it had some nice advantages, whether it be block samples, like June had used; it was core, like the USGS had used; it had shown a bomb pulse hit before in the Los Alamos work, and there remained a requisite two kilograms or so necessary for each site to have a split.

So there was a section of core remaining, maybe with some gaps in it and sort of short core lengths in a box. And they didn't just divide it in half. They took a piece for you and a piece for me, a piece for you, a piece for me until each side had half of the sample and it was roughly a kilogram, which is about the minimum you need to get a leach or you start getting those low-end values that everybody worries about. And they leached those.

Now, the difference between this one up here is the USGS leached both samples and then had the wet chemistry done at Los Alamos and Livermore. But down here it was LLNL leached their own and the USGS leached their own. And then the USGS sent the leachate to Livermore. And Los Alamos precipitated and then sent to Livermore.

And Los Alamos replicated their early result of bomb pulse. And the USGS replicated their early results of no bomb pulse. So at that point we still had a -- conflicting results is what I would term them. And we were out of sample that was suitable for testing for bomb pulse, and we were kind of at a decision point.

There were three years worth of work that had gone into this. The leaching protocols had been agreed upon. The GS had sort of done all they could to validate. They had used up all their validation core. I had to go get new samples, and it wasn't clear to me that maybe just the taking of the samples was a problem.

I mean, when you're at a point like this, you've got to really think about what your next move is going to be. And what we chose to do was have them write the report up and write a joint report that represented both viewpoints.

CHAIRMAN RYAN: There could be one viewpoint that consolidates all of the data. And that is, if you went out and replicated these samples a number of times, you would end up with the same result. The range would be if I am reading this right 244 to 8,580. Reality is somewhere in between that range.

DR. D. COLEMAN: Yes. The --

CHAIRMAN RYAN: There is a possibility that nobody did anything wrong and this is just the natural variation of what you're going to --

DR. D. COLEMAN: Right. There is no clear admission by me that either one is wrong or that it's necessary that either one be wrong.

CHAIRMAN RYAN: Right, exactly.

DR. D. COLEMAN: The difficulty is, you know, what samples do you go get and test. And, you know, the USGS never did show an bomb pulse. Los Alamos showed it rarely but at least occasionally.

And, you know, another -- I mean, we could have done more work with them. Both groups were confident that they could drive it to a resolution. They didn't want to give up on it.

They to this day are not ready to give up on it. They would love to take over the work. But the department took a look at it and thought the best idea was to write up the results in a report that discussed the areas of agreement, the areas of conflict, discussed every facet of the work, and that both scientists would stand behind as representing their points of view, even though they were divergent a little bit.

I mean, at some point you're looking like you're not going to write anything up until you get an answer that you like. And that is a perception problem that you might have to deal with.

I mean, from this point looking back and getting ready for this presentation, I kind of relooked at my decision. And I don't really second-guess it. Now, I wasn't the only one that made the decision. There were lots of people higher up into the department than me that participated, but I don't really second-guess the decision.

So they wrote it up. The report is entitled "Chlorine-36 Validation Study at Yucca Mountain, Nevada." It's completed all of its reviews. The last comments were being resolved. When I talked to them just before I came here, they had gotten a few more comments from the QA guys, and they were a little bit upset about that. But they're working to resolve those.

And the report should be available pretty shortly. Of course, I will work with Neil or somebody to get a copy of that through the public release review and to interested parties.

This is one summary figure that is in the report. It shows all of the work from all of the various phases in one figure. There are figures on tritium. There are figures on the overall tunnel data. There are a lot of figures. But the significant one is that little box at the very top and the one below it are probably the last results where Los Alamos validated their previous findings of bomb pulse in this vicinity.

And all the USGS squares are kind of below 1,000, some getting up close but sort of below 1,000. And so those never showed bomb pulse from this repository horizon area. And then the yellow boxes are some earlier LLNL work in the vicinity of the Sundance Fault here.

So the second part of my talk was the UCCSN follow-on work. Now, I've got to correct this slide. I said, "Proposals for a follow-on study were requested," And I was talking to a guy who did that work. And he had asked me to get together a proposal from the university system.

And the sense I got from him at the time is that he was getting several proposals together to look at. But it turns out he was just interviewing various people to see who the real good experts were. So I would change "Proposals for a follow-on study" to sort of "Interviews of chlorine-36 community were conducted. And possibilities were passed forward, evaluated, or something like that. I just wanted to note that because I was asked by one of my researchers about proposals.

And the proposal I got together from the university, I just asked them to pull one together. But they had, again, Fred Phillips, who was talked about by David as one of the early experts.

I looked at his list of publications, and there were 90-something publications from 1970 something forward. And I'm wanting to say 95 percent of them had "Chlorine-36" in the title. I don't know that he was doing any of the lab work or the sampling. He was more oversight.

And Jean Cline, a university professor who had worked on fluid inclusions earlier, her areas of expertise were Carlin coal deposits and high temperature food inclusions. And she did some low-temperature fluid inclusion work for us and dined out on that for a while.

And then her other area of interest is sampling biases, what effects they can have on a study. And she believed that the answer was in the fractures and the sampling and the way you looked at it. And she was kind of similar to your earlier statement. I believe maybe both of them were right if you just understood what you were measuring.

She wanted to look at the plains, the fracture plains, making them soft, just the plain itself, where fluid would have dropped, would have traveled, and then leach that, as opposed to leaching a lot of rock that may not have seen the fluid traveling fast.

But, anyway, that was the study. And we funded that. Their study was entitled "Bomb Pulse

Chlorine-36 at the Proposed Yucca Mountain Repository Horizon: An Investigation of Previously Conflicts and Collection of New Data."

And in their proposal, they were going to attempt to determine the cause of the conflicting results and obtain additional data and, at least informally, they told me that they were going to try and figure out what had happened to lead to the earlier conflicting results.

So they were gung ho to go. And they developed their scientific investigation plans over a couple of months. And right around Christmas of 2003, we're just about to go get their samples. My safety and health arm wanted to upgrade some mine power centers and some different things in there.

The bottom line is we put them on hiatus for a year while we upgraded those mine power centers. When you upgraded them, workers in the underground couldn't be there because maybe you had the ventilation off or you had safety issues.

And so they looked at all the work that was going on and judged it as to how critical it was. And I tried to have this be critical, but it's difficult to argue against safety. And in the end, I acquiesced. And we put that study on hold.

Of course, you lose a little focus with your team when you are on hold for a year. But early in 2005, we started back in to try to get some samples and finished our sampling in about July of 2005. And they leached their first set of samples for chlorine-36 in August of 2005 and sent me a spreadsheet of the results. And they had some data that were just really high.

If you were to look at this figure back on page 7 and see the range there is 100 to 10,000 and on the spreadsheet that they gave me out of those first samples, they had some numbers that were 300,000 and some of those were the samples that were measured during the early leaching experiments -- we still had some of that material left. And they were using that. They were trying to replicate, of course, earlier results also. And so they had some really high values.

I was on a telecon where Fred was talking it over with them about what --

MEMBER HINZE: Excuse me. Who did their sample corporation? And who did their measurements?

DR. D. COLEMAN: I think they did their own targets and leaching. They didn't send it to you guys to --

DR. ELMORE: I don't know.

DR. D. COLEMAN: You don't know for sure? I think they did their own. I think they prepared them sort of similar to Los Alamos, did their own leaching, did their own spiking, and did their own target preparation. But they're in the process of writing up their results. I'll be able to give you an answer. I'll take that question and try to get you an answer as to who did their spike.

Is that what you're asking, spiking?

MEMBER HINZE: Who did the analysis?

MEMBER WEINER: I guess Livermore did.

DR. D. COLEMAN: Analysis, AMS were sent to PRIME.

MEMBER CLARKE: They were sent to PRIME?

DR. D. COLEMAN: Oh, yes. Yes. He mentioned Jim Cizdziel earlier as somebody -- in fact, I think the results he showed were some of their later studies.

Now, when these high values came, we had a telecon with Fred on it. I mean, you know, I have to be careful what I say about somebody's lab -- it's the way they make their livings -- and, you know, having a rookie make a lot of statements that are wrong about what went on in someone's lab, but the bottom line was the only thing around I think that had that kind of levels was their new standard. So something had gotten away, you know.

I was going to actually ask David. When you buy a standard, how hot is a standard?

DR. ELMORE: It can be very hot, but --

DR. D. COLEMAN: A standard could be 300,000 parts per 10 --

MEMBER HINZE: Excuse me. David, would you --

MR. D. COLEMAN: Move to the table.

DR. ELMORE: Typically users preparing samples do not prepare a standard. Okay? The standard only comes from the --

DR. D. COLEMAN: But you buy a NIST, a bottle of --

DR. ELMORE: Yes. I don't know what they sell, but there is a lot of chlorine-36 around. And a problem can be if a laboratory dealt with reactor materials for any purpose; for example, neutron activation analysis.

Chlorine can be a volatile forms and can be around for many years before in a lab. So it's very important to do a swipe test, to prepare a blank from dust in the room and that kind of thing.

DR. D. COLEMAN: I think they have blanks and stuff, you know, but --

DR. ELMORE: But yes. Certainly there are high sources of chlorine-36 that can get into samples --

DR. D. COLEMAN: Right.

DR. ELMORE: -- without you knowing it.

DR. D. COLEMAN: Right. So the bottom line was they had these high values. And Fred's recommendation was that they can destroy all their glassware and move to a new lab. And they took a shot at cleaning up their lab and cleaning up their glassware, but they still didn't like the results. They've now moved to a new lab.

And, you know, when that happens to you, then you spend a couple of rounds of AMS time trying to convince yourself that you've got numbers that you can believe in. So you're mostly blanks then, and you're sending them in to see if your lab is good. And that is kind of a lot of what they have been doing here recently.

I think the AMS runs chlorine-36 something like once a quarter. Is that accurate? So the time goes slow on sampling, but they got -- their most recent results came back on March 31. And it may have even been some of those that you showed, those Cizdziels or he had some other ones in not too long ago. And that is kind of where they are at.

And contractually when you are working with coop tasks, you get -- if we can go back? I forget exactly where I'm at now, but maybe page 10.

MEMBER HINZE: I think 9 is where you --

DR. D. COLEMAN: Yes. Nine maybe I left off. So if we're on 10, they took measures to reduce their background and prepared and tested additional blanks to verify their techniques. They're reasonably confident they got these issues resolved.

And they have tested some rocket soil samples. Again, the samples were analyzed by PRIME on 3/31 or maybe not on 3/31, but he got the results back on 3/31. So that may be. You may want to correct that little statement.

And they're being reviewed. Now, he didn't want to discuss his results with me, and I didn't really want to put them up on the board here until his team had reviewed them. You know, I'm sympathetic to the researchers that --

CHAIRMAN RYAN: That's fair enough.

DR. D. COLEMAN: -- about holding onto their data until they're ready to --

CHAIRMAN RYAN: Fair enough.

DR. D. COLEMAN: -- put it out. So their study has actually concluded because contractually their study, they thought they could do this study in 18 months. And, of course, we put them on that one-year hiatus. And we gave them a one-year no-cost extension, but that's all you get under the way that contracts work or even grants, as these are our cooperative agreement with the university.

So my discussions with them indicate they are interested in pursuing further the study, but they're writing up their results to date. And they have some 60 days from the end of the contract at March 31 to write their results up and get them QAed and get all their data into the system. And they might be able to get a few extensions for that.

But that is pretty much where we are there. My management or at least my immediate management is interested in pursuing the chlorine-36 work with the university. I'm interested in pursuing it. I think they're interested in pursuing it.

But the actual work of either getting it in the annual plan or putting together a change through DOE's processes is kind of in the works right now.

And they have quite a few samples that remain unleached. They got a lot of samples so that they could slice off portions and test it. And they're confident that they haven't contaminated those existing samples, although when I was talking to them just the other day, they probably would want to come and get some more samples.

But I think I will end my talk there and try to field questions as best I can.

MEMBER HINZE: Well, we thank you very much, Drew, understanding the limitations of not having the report from the validation study or report from the university cooperative work.

DR. D. COLEMAN: Yes. Maybe one further point. I agree with you and David that the instrument is the least likely source of the big errors and the errors are elsewhere or the conflicting results.

I guess, although maybe characterizing clearly the errors, like the point you made, is a good one, I think our difficulties are involved in maybe the contamination or the sampling or the preparation of the samples.

My sense is once you get it into water and get it to them, it's really a pretty routine measurement. A lot of people use it and a lot of people have confidence in my investigators taking a water sample from depth and sending it. They would have no worries. It's somewhere in the leaching and sampling and contamination world that people worry about the technique.

CHAIRMAN RYAN: The real secret is when we turn your sense of that into numbers.

DR. D. COLEMAN: Yes.

CHAIRMAN RYAN: That is when we will know what is right and wrong.

DR. D. COLEMAN: Right.

CHAIRMAN RYAN: I mean, I just find that your talk was interesting. Your folks are obviously qualified and have done a good job. You know, when they start this process, they end up having all sorts of headaches and problems.

This is not, as I think was pointed out earlier, an easy measurement to make and certainly systematic. To me, it screams out for a systematic assessment of uncertainty.

DR. D. COLEMAN: Yes.

MEMBER HINZE: Let's make certain we have all the questions asked. Dr. Clarke?

MEMBER CLARKE: Thank you, Drew.

Can we go to slide 5? I'd like to go

through this slide, maybe the next one for two reasons: one, to make sure that I understand it; and, two, I think that will give us an opportunity as we go through the slide to point out the possible sources of error.

So, as I understand it, one core is reached. There were two cores. One core was reached, and that leachate was sweat.

DR. D. COLEMAN: Well, I'm wanting to say cores as sort of plural here. Core would be any one of the sets of core from the 40 bore holes.

MEMBER CLARKE: No, no. What I am saying is that the U.S. leached samples from the validation core and they split the leachate.

DR. D. COLEMAN: Okay.

MEMBER CLARKE: Well, you know, if there were two cores, then that's a source.

DR. D. COLEMAN: All right. You have the same core.

MEMBER CLARKE: If there was one core and they split the leachate, I would think splitting the leachate would be -- I don't think I would be too upset about that.

DR. D. COLEMAN: Yes.

MEMBER CLARKE: But now they go to

different groups. And those groups process the sample, I guess, to remove the interferences that David mentioned.

DR. D. COLEMAN: The spiking, the precipitation of the target. And it's just the process that these kinds of AMS guys use. Maybe you can weigh in one it. I don't know.

MEMBER CLARKE: No. Here I think we're getting into some real possibilities for variation.

DR. D. COLEMAN: Okay.

MEMBER CLARKE: So I would put a circle around that second bullet.

Then the results of the two generally agree. Now, is that the range of all of the data or is that the range of the disagreement?

DR. D. COLEMAN: They had a graph. And I'm not as good at Powerpoint presentations as I would like to be, but this would be USGS and Los Alamos. And they had a one-to-one line. And those samples just lay right on it from that using, one, their technique; and, the other, his technique. And they were all low. They were in the range of 250 to 500, but they had a really nice one-to-one line fit there.

MEMBER CLARKE: Then the other point I guess I would make is that these samples went to the same lab. Is that right?

DR. D. COLEMAN: The same AMS lab?

MEMBER CLARKE: Yes.

DR. D. COLEMAN: Yes. I think they both went to --

MEMBER CLARKE: Livermore.

DR. D. COLEMAN: -- Livermore at that time.

MEMBER CLARKE: So you've got --

DR. D. COLEMAN: Los Alamos didn't use them a lot. And the USGS purposely used Livermore. But then toward the end, I mean, Caffee had been at Livermore. And then he went to PRIME, and that sort of confused the whole --

MEMBER CLARKE: Let's just stick with this slide.

DR. D. COLEMAN: All right.

MEMBER CLARKE: Okay? Because we have identified that there was one core. So we're not into variation between core to core. We do know that they processed the samples perhaps differently at different locations. That could be a major source of disagreement.

DR. D. COLEMAN: Well, they got the one-to-one fit, though, on those.

MEMBER CLARKE: Well, but, you know, again, we're looking at variation of the data.

DR. D. COLEMAN: Yes.

MEMBER CLARKE: And then if they went to the same lab and there is no inter-lab variation, there is just intra-lab variation, in other words, these samples would have been at variations times. There could be variation from room to room.

But I think if you go through this in the end and nail down where it went and where did it apply, I think you can identify the sources of disagreement. You can do the same thing for the next one.

So I think I like Mike's suggestion. Again, this may all be one distribution.

DR. D. COLEMAN: You know, one of the nice parts about having them wire the report up is to have it all laid out in black and white so anybody can read it. Maybe somebody can spot the point problem, "Oh, there's your problem right there."

CHAIRMAN RYAN: Hopefully the raw data will be in the report.

MEMBER CLARKE: The only thing that this suggests is that there really is merit to describing variation from method to method. And there is merit to somehow quantifying uncertainty from lab to lab if these are going to continue. And then I think you can start to get a handle on it.

So a systematic approach through what was done, who did it, where it was done, you know, could I think be very helpful. You haven't done, I guess, enough analyses. You have no inter-lab variation -- maybe you do; I don't know -- or inter-method variation between these different sample processes.

DR. D. COLEMAN: Yes. We have probably got data available to take a look at different --

MEMBER CLARKE: If you have got the data, then you could do a fix on that to some extent.

Thank you. That was my only -- really, it wasn't a question. It was one --

MEMBER HINZE: Dr. Ryan?

CHAIRMAN RYAN: Thank you, Professor Hinze. Just one last comment.

I'm reading the last slide. The technique that is testing rock samples from deep, unsaturated zone for bomb pulse chlorine-36 needs additional confirmation to build confidence in the measurement interpretation of data.

I guess I still agree with that. That is what I said -- I don't know -- two years ago at a meeting in Vegas. I'm concluding that the ball isn't much further down the field at this point.

DR. D. COLEMAN: Yes. I would say that is a pretty good conclusion.

CHAIRMAN RYAN: Okay.

DR. D. COLEMAN: You know, there are some suggestive things in there.

CHAIRMAN RYAN: Right. You've got to --

DR. D. COLEMAN: We can look at them, but --

CHAIRMAN RYAN: We haven't gotten to first down.

DR. D. COLEMAN: Again, I hope this doesn't imply that it's just using this technique for fast pathways to the deep unsaturated zone, leaching that water out I'm talking about here, not the AMS or water samples or --

CHAIRMAN RYAN: No. Again, I appreciate the fact, one, this is a very difficult measurement; two is it takes real expertise and precision to do it. But in spite of the best efforts, it seems like it's hard to really nail down, you know, what is an actual sample value and what the distributions might be. And we're still struggling with that in the application of taking a sample all the way through to the end and reporting a measurement.

DR. D. COLEMAN: And this is sort of a DOE conclusion. If you were to ask some of my researchers, particularly Los Alamos ones, they might have a different look at this. They have validated their results. And so that is probably something I should mention here also, you know.

MEMBER HINZE: They'll have a chance to express that in the report, right?

DR. D. COLEMAN: Yes. You can read that for yourself.

MEMBER HINZE: Okay.

CHAIRMAN RYAN: Allen?

VICE CHAIRMAN CROFF: I guess, as a practical matter, do you have any alternatives to this whole approach?

DR. D. COLEMAN: Well, our analysis is not inconsistent with a few fast pathways. And I think that is a reasonable modeling approach. So yes, I don't -- I mean, this data could be helpful. And it's interesting data to pursue.

And knowing that on the speed of travel of water to the repository horizon is a good thing to know. And I don't want to minimize the value that it could have, but I don't see this as snagged up or anything waiting on this measure.

I mean, this is a measure that we undertook. And we're doing some more work to try to make sure we understand it. But it's not on the critical path, I don't believe, to -- you know, maybe moving forward with the license for the repository. That would be my -- so we're going to continue to pursue it and hopefully resolve it. And hopefully it will give some understanding of the --

VICE CHAIRMAN CROFF: Roughly how long is it that it is believed that water takes to go from the surface on one of these fast paths down to the repository horizon? Is it a 50-year or 500 or --

DR. D. COLEMAN: Yes, 50 or so years. If it's got a bomb pulse signature that you can confidently conclude, then that took place. Somebody could give you the hour, minute, and second that it was a bikini atoll test or something like that.

VICE CHAIRMAN CROFF: It was in the mid 1950s. Somebody initiated the experiment that put the tracer in at the surface. I mean, granted, it may take a few years, but --

DR. D. COLEMAN: I don't think that's what they were thinking about, but people like David here are figuring out ways to utilize them. And that worked to increase everybody's knowledge, I guess, interesting work.

VICE CHAIRMAN CROFF: Okay. Thanks.

CHAIRMAN RYAN: Ruth, take it away.

MEMBER WEINER: Is there anywhere in the world where you can measure the variation of chlorine-36 without the interference of a bomb pulse or where you could with some confidence subtract the interference of the bomb pulse? What I'm getting at is, what is the variation that you get in chlorine-36? What is the range of variation without that? Do you know or is there any way to figure that out?

DR. ELMORE: There's been a lot of work done with ice cores from Greenland and Antarctic. And most of that ice is deeper than -- the H's are known pretty well for the ice. And that's where the bomb pulse is measured in an ice core.

And so the deeper ice, which goes back as far as 300,000 years -- and there have been profiles back there. And then there is nothing that sticks out anything close to the bomb pulse. It's all below 1,000 times 10^{-15}.

MEMBER WEINER: That answers my question.

DR. D. COLEMAN: I've seen graphs where it varied between 500 and 1,000 or something. And this one has holocene. If you go to slide 7, you've got sort of holocene and maximum pleistocene, but it's varied through time. And I'm not sure exactly the mechanism for that.

It would be like magnetic field changes or something like that. That is the range sort of that it's varied between, I think.

DR. ELMORE: Yes, 500 to 1,000. I mean, there is another source, from mountaintops, where the cosmic ray flux is higher, production in the top meter of the surface. And that can go to a few thousand probably. So it can go higher than this but nothing like the bomb pulse.

MEMBER WEINER: There is nothing that would be comparable to the very high levels that you see in these reports?

DR. ELMORE: That is correct. And even if it's, I mean, somehow contamination from a nuclear facility or something, which could be more recent. But we know none of that goes past 1945. So prior to 1945, there are no manmade sources either.

MEMBER HINZE: A last question. When can we anticipate a report from the university and community college system?

DR. D. COLEMAN: Well, 60 days from March

31 is their deadline to have their data in and through QA and some sort of a report. Now, it may not be this report that the USGS is working on in Los Alamos is two inches. And I'm thinking 60 days, you're going to be looking more like, you know, 10 pages with 4 figures or something, which in some ways is a better report than your two-inch report anyway. So 60 days, I guess, is -- and they may be able to apply for some extensions but should be available maybe in the summer here.

MEMBER HINZE: Well, we'll appreciate a heads up through Neil Coleman on that. With that, I thank both of you gentlemen for excellent presentations. It's been very helpful, very informative. And with that, I'll turn it back to the Chairman.

CHAIRMAN RYAN: And, with that, Professor Hinze, it's time to adjourn for lunch and reconvene at 2:00 o'clock.

(Whereupon, a luncheon recess was taken at 12:01 p.m. until 2:01 p.m.)

CHAIRMAN RYAN: This is Mike Ryan, Chairman of the ACNW. I would like to call the afternoon session to order and if I could ask the folks on the conference call phone to tell us who you are.

MR. FITZPATRICK: Charlie Fitzpatrick from the State of Nevada.

CHAIRMAN RYAN: Okay. Thank you. Anybody else?

MR. JENKINS-SMITH: Yes. Hank Jenkins-Smith, Texas A&M University.

CHAIRMAN RYAN: Hello Hank. Okay. Thank you for introducing yourselves. We have reconvened the Committee and our afternoon speakers. Our session is going to be broken into two parts this afternoon. The first part Dr. Weiner will lead us in a discussion of a recent National Academy Transportation Study and then we'll go onto the NARM, Naturally Occurring or Accelerator-Produced Radioactive Materials rulemaking.

But before we do that, I'd like to introduce again to members and staff and particularly the staff beyond the ACNW staff a recent addition to the ACNW staff. Dr. Antonio Diaz joined the ACNW staff on April 10th. He will be working as the Team Leader for ACNW Technical Support branch. Dr. Diaz has a Bachelors degree in Electrical Engineering and a Masters degree in Nuclear Engineering from Brazil and he is a Ph.D. in Nuclear Engineering from the Massachusetts Institute of Technology.

He joined the NRC in November 2001 as a Technical Reviewer in the Spent Fuel Project Office where he was involved in the review of several transportation and storage applications in the technical areas of Thermal Criticality and Containment. He also participated in inspections of waste storage not only reviewing operations but also their associated procedures.

He acted as Section Chief for two months supervising the Technical Review group, TRA. Prior to joining the NRC, Dr. Diaz worked for several years as a consultant providing services to many U.S. utilities as well as the Electric Power Research Institute.

His main area of expertise was assimilation of multi-dimensional time-dependent neutronic and thermal hydraulic postulated events for light water reactor. Dr. Diaz's early MS work addressed the behavior of light water reactor fuel elements during normal and transient conditions in order to understand possible fuel failure causes.

Dr. Diaz, welcome to the staff and welcome to the ACNW.

DR. DIAZ: Thank you very much.

CHAIRMAN RYAN: And with that, Ruth, I'll turn over the next segment of the session to you.

Thank you.

MEMBER WEINER: Thank you, Mr. Chairman. The next section of our meeting we'll be discussing the recently issued National Research Council report on Transportation of Spent Nuclear Fuel and High Level Waste entitled "Going the Distance." And our panelists are, our panel, in fact, will be led by Kevin Crowley who is the Study Director for the Nuclear and Radiations Studies Board and he is assisted here by Joseph Morris who is the Senior Program Officer who will help him.

We also have Dr. Mel Kanninen who will talk on long duration fires and on anything else that you would like to add to. On the telephone, we will have hopefully Dr. Claude Young from the U.K. Although it is 7:00 p.m. in the U.K. now, he has graciously agreed to be present by phone and Dr. Hank Jenkins-Smith from Texas A&M University to talk about social and institutional challenges.

Please if there are any people in the audience who would like to make a statement or ask a question our normal procedure is to go first, have the presentations and have members of the Committee and staff ask questions and then there is enough time allowed, I believe, for anyone who wishes to make a comment or ask a question to speak up. If you haven't signed in, I will recognize you at the time. With that said, Dr. Crowley, go ahead.

DR. CROWLEY: Thank you very much for the invitation to be here today. I'm sorry that more of our committee members couldn't join us, but they are pretty busy folks. What I'd like to do if it's all right with you is to take ten to fifteen minutes and just give you a high level overview of what's in the report and then we can dive into the issues that you've identified. Does that sound all right?

MEMBER WEINER: Certainly.

DR. CROWLEY: And it will be up to you whether or not you want to stop me along the way or whether you just want me to get through this. It's your pleasure.

MEMBER WEINER: I've noticed that the Committee is not shy about asking questions when they arise.

DR. CROWLEY: All right. So I have somebody to change the slides for me presumably. All right. Let's go directly to the next slide. I'm going to hit some of these very quickly and then spend a little time on some of the others.

This slide is just to remind you that

there are really two parts of this study, a part that was self-initiated looking at the risks of transport and key technical and societal concerns particularly over the next two decades. When we were almost completely finished with this study, we added a congressionally-mandated task looking at the matter in which DOE selects routes for shipment of research reactor fuel. So we had an additional meeting and we basically had to completely reorganize the report in order to include that extra task. Next slide.

Just a list of the study sponsors of which the Nuclear Regulatory Commission is one. Next slide.

This is the list of the study committee. You'll recognize some of these people but not all of these people. It was chaired by Dr. Neal Lane of Rice University who was formerly the Director of National Science Foundation and the President's Science Advisor.

And if you go to the next slide, you can see the collective committee expertise that is represented by the members. When we put this committee together, we tried to make sure that we had certainly the right mix of disciplinary expertise, but we also tried to have a balance between members who have worked in the nuclear spent fuel and high level waste transportation area and members who have relevant technical expertise but who haven't worked in this area. Next slide.

So let me just go to the bottom line messages for the study. This is the first one and I think probably the most important one. The committee could identify no fundamental technical barriers to the safe transport of spent fuel and high level waste in the United States, but there are a number of societal and institutional issues, institutional challenges, to the successful initial implementation of large quantity shipping programs. The committee defines large quantity shipping programs as programs that ship on the order of hundreds to thousands of metric tons of spent fuel or high level waste and it specifically identified the Yucca Mountain Program and the Private Fuel Storage Program as examples of those types of large quantity programs.

This message, the committee spent a lot of time talking about this message. It's very carefully and narrowly constructed. It focuses on the technical aspects of transportation program. It's based on an assessment of past and present research programs and would apply to future programs only to the extent that they continue to exercise appropriate care and adhere to applicable regulations. Next slide.

This is the committee's message with respect to security. When the Statement of Task was initially constructed which was before September 11th, the focus was not on security. It was on safety. After September 11th, we began to have discussions with the agencies, particularly the Nuclear Regulatory Commission, about trying to expand the task. We actually had a small group of committee members including Mel, Dr. Kanninen, who received a classified briefing from the Nuclear Regulatory Commission on the work that they were doing, although we didn't get any details on the results.

It was really just a scoping briefing and the committee concluded that there was enough information to perform, to do, a security review as part of this report, but we essentially ran out of time to do it and also there were questions about what information the unclear members of the committee could get and what we could put into an unclassified final report. So the committee was unable to perform an examination but recommended that such an independent examination be done and noted that in order to undertake such an independent examination, it would require the cooperation of several federal agencies.

Next slide.

Chapter 2 of the report addresses in detail package performance and this is something that Mel will be talking about a little later, but these are the committee's bottom line messages on that that the committee felt that current international standards and U.S. regulations are adequate to ensure package containment effectiveness over a wide range of conditions. But there might be a small number of extreme accident conditions involving very long duration fires and the committee recommended that the Nuclear Regulatory Commission undertake additional analyses of very long duration fire scenarios.

The Commission was in the process of doing work while the committee study was underway. Some results came out just as the committee was publishing its report. The committee noted those results but didn't have an opportunity really to examine and assess them. I think what the committee is really looking for is the Commission to demonstrate that it has an bounding level understanding of real world conditions that might lead to very long duration fires and that the Commission should put into place any appropriate operational controls and restrictions to reduce the likelihood that such fires would be encountered or to reduce their consequences if they were encountered. Next slide.

Package testing, something else that Mel will be prepared to talk a little bit more about. The committee strongly endorses full scale testing and recommends that full scale testing should continue to be used as part of an integrated testing program. The committee also recommended the full scale testing of packages to deliberately cause their destruction should not be required. And again, this is a recommendation that the committee spent a lot of time on the wording and I want to emphasize it says "Full scale testing should continue to be used as part of integrated testing programs." Basically, what that means is keep doing what you're doing. Next slide.

Transportation risk, Chapter 3 of the report has a fairly lengthy discussion of transportation risk and the conclusions, the findings, from that are shown here. The committee found that the radiological health and safety risks associated with transport are generally low, again, with a possible exception of long duration fires, but the Committee also noted that the likelihood of such fires appears to be small and that their incurrence and consequences can be further reduced through relatively simple operational controls and restrictions.

This finding that the radiological health and safety risks are low are based on a number of issues that were examined in the chapter, looking at historical shipments, looking at historical accidents and incidents, looking at the large number of analytical and computer modeling studies that have been done and looking at the full scale testing studies that have been done. Next slide please.

Social risks, this will be something that Hank, I hope, will address in more detail. The committee found that the social risks for transportation pose important challenges and that transportation planners can take early and proactive steps to establish formal mechanisms for gathering advice about these risks and the committee recommends that DOE take two steps to try to deal with the issue of transportation risk by creating a new advisory group and augmenting our current advisory group. I want to point out that the committee did not -- One of the things I've heard in the press is that the committee has called for more research on social risk. Actually, the committee's recommendations are for very pragmatic, problem solving steps that should be taken not just go back and do more research. Next slide.

There are several measures of comparative risk that are provided in Chapter 3, two of them of which I'm going to show you figures for on the next two slides, but one other quantitative measure just comparing the number of estimated latent cancer fatalities for a Yucca Mountain transportation program based on the final EIS that DOE published versus the number of cancer fatalities that you would expect just in the general population and the comparison is one to three latent cancer fatalities for normal transport for a Yucca Mountain program versus the four to six million fatalities that you might just expect from other causes. Next slides.

The committee presented what it calls a "Risk Ladder" for normal transportation risks. Let me step back for a minute and talk a little bit about the committee's philosophy in developing risk comparisons. Again, this was an issue that took a lot of time in the committee's closed meeting for discussion, but the committee decided very early on that it did not want the report to appear to be advocating for any particular level of risk and truly wanted to present information that someone who didn't know a lot about this topic could look at that information and then could decide for themselves what the transportation risks were.

So what the committee attempted to do in comparing risks for high level waste and spent fuel transportation was to bracket them both above and below with risks for other kinds of societal activities. Early on in the committee's deliberations, there was discussion about what sorts of risk should you consider. Should you consider, for example, spent fuel and high level waste transportation risks and compare those to smoking or driving in a car and things of that sort and the committee said, "No, that's not where we want to go. We want to try to compare like risks."

So in the comparisons, they were really based on for normal transport conditions, exposures, other kinds of exposures to radiation and those exposures are shown here. There's a whole list of them, things like background radiation, radiation that you would get from airline flights, radiation that you would get from medical procedures. There's a lengthy discussion in the report about the pros and cons of presenting that kind of information, but this is basically where the committee came out.

In this figure, I know you can't read all of it, but the black bars show the various estimated exposure types of a Yucca Mountain transportation program. The bar at the top is worker exposure. As it turns out, workers according to the DOE EIS are going to have fairly high exposures. In fact, workers are going to be burned out according to the EIS. As you get down into the public exposures, they are considerably lower and the lowest exposure, somebody who lives along a rail route that is used to transport spent fuel and high level waste, the committee could not find anything that was lower, a lower bracket below that, and that's the lower black bar around the figure.

MEMBER WEINER: Let me interrupt you just a moment.

DR. CROWLEY: Sure.

MEMBER WEINER: I assume that since this is from the FEIS that the worker dose was based on workers having to transfer bare fuel from the transport containers to the waste packages.

DR. CROWLEY: No, these are the workers that are going to receive doses during the transport program. So these include the people who will be driving the trucks, people who will be in the escort cars, people who will be doing the inspections of the cars.

CHAIRMAN RYAN: Could you define "burned out"?

DR. CROWLEY: They would receive the maximum allowable dose in a given year.

CHAIRMAN RYAN: You show 20 millisieverts and elements 50.

DR. CROWLEY: The DOE administrative limit is two.

CHAIRMAN RYAN: Administrative.

DR. CROWLEY: Right.

CHAIRMAN RYAN: "Burned out" is a relevant term. I just want to make sure that's clear.

DR. CROWLEY: Well, the report does not --

CHAIRMAN RYAN: You're not talking about anybody exceeding a regulatory limit.

DR. CROWLEY: No, and in fact the report does not use the term "burned out." That was a term that I used.

CHAIRMAN RYAN: I just want it to be clear because we're on the record. Thank you.

DR. CROWLEY: Okay. All right. The next slide shows a calculation that the committee did for accident conditions of transport and we used the complimentary cumulative distribution functions here comparing spent fuel and high level waste to other kinds of hazmat transport and again the committee made an effort to find an upper and lower bracket, but was unable to do so. The top three curves show the CCDFs for three different kinds of hazmat, chlorine, propane and methanol and you can see the spent fuel CCDF is several orders of magnitude below that.

MEMBER WEINER: Again, what was your source of data for releases for the spent fuel?

DR. CROWLEY: That was from the Sprung and others, the 2000 Reexamination Report.

MEMBER WEINER: And did you consider in the accidents the sort of accident where the truck just sits there for hours and hours until somebody comes along and moves it when you have a fender bender? It doesn't affect the cargo, but the truck just sits.

DR. CROWLEY: No, this would be an accident that involved the actual release of radioactive material.

MEMBER WEINER: Thank you.

MEMBER HINZE: Does that include a fire?

DR. CROWLEY: Actually, I think the maximum releases are in a fire. You get the maximum releases in a fire, not from the mechanical impacts.

Okay. Let's go on. Again, I'm going to

just get through this quickly so that we can then come back and discuss some of this. Chapter 4 deals with research reactor spent nuclear fuel routing and this was the add-on task from Congress looking at DOE's program for selecting routes for research reactor spent fuel. There are two major findings here and Joe will be able to speak in more detail to this because he helped the committee prepare this chapter.

But the committee found that DOE's procedures for selecting routes within the U.S. for shipments of foreign research reactor fuel appear on the whole to be adequate and reasonable and the DOT routing regulations are a satisfactory means of insuring safe transportation provided that shippers actively and systematically consult with states and tribes along potential routes and states follow route designation procedures. Next slide.

That's all I'm going to say about Chapter 4. Now let me finish up with just going through some of the findings and recommendations in Chapter 5 which is "Improving Spent Fuel and High Level Waste Transportation in the United States." Many of these findings and recommendations focus on Yucca Mountain, but the committee states in the chapter they would also apply to other large quantity shipping programs and the committee notes that private fuel storage is an example of such a program. I also want to point out the committee did not attempt to undertake a detailed programmatic review of the DOE transportation program, although during the study the committee received several briefings from DOE and kept itself informed of the latest changes in DOE's program. Next slide.

The committee strongly endorsed DOE's decision to use mostly rail and to ship by dedicated train and recommended that DOE fully implement those decisions before commencing the large quantity shipment to the repository and also examine the feasibility of further reducing the need for cross-country truck shipments. The real concern here is, I think, if the Yucca Mountain repository were to open before the rail spur were finished, DOE might spend a lot of time and a lot of money standing up a truck program and might actually not have the time and the money to finish the rail program and could be stuck with a long term truck transport program. Next slide.

The committee recommended that DOE make public its suite of preferred highway and rail routes as soon as possible. Again, this would be for Yucca Mountain to support state, tribal and local planning.

And the committee recommended that DOE follow the practices of its research reactor spent fuel transport program which we discussed in Chapter 4 of involving states and tribes in the routing selections even for rail routing for which the states now do not have a formal role in selecting routes like they do for highways. Next slide.

The committee had something to say about the acceptance order for transport of spent fuel to a Yucca Mountain repository. Right now, the standard contract requires DOE to accept whatever fuel an owner wants to give it when the owner's spot in the acceptance view comes up and the committee recommends that DOE should negotiate with the spent fuel owners to ship older fuel first, not the oldest fuel necessarily, but older fuel first and that should these negotiations prove infective, Congress should consider legislative remedies and then finally, the committee recommended that DOE initiate transport to the repository with a pilot program involving movements of older fuel from closed reactors.

There were several things that drove the committee's thinking on this one, but one of the concerns again was the worker exposures. If you remember a few slides ago, the workers are getting fairly high exposures. Also another concern is if there were to be an accident or a terrorist attack and you did have a release from the spent fuel package, obviously the colder and radiologically cooler that fuel is the better it is. Next slide.

The committee had something to say about emergency responder preparedness and recommended that DOE should immediately begin to execute its responsibilities and also federal agencies should promptly complete the job of developing, applying, disclosing criteria for protecting sensitive information. Protect what needs to be protected. Make the material that doesn't need to be protected open and accessible and we can talk a little bit more about the emergency response if you want in the follow-up.

MEMBER WEINER: I may reserve this question for Dr. Morris, but it's my understanding and has been my experience on the Whip project that DOE has been preparing emergency responders for some years now, that there is an on-going program. Are you going to comment on that?

DR. MORRIS: I wasn't planning on commenting on emergency response.

DR. CROWLEY: I can respond to this.

There is an active emergency response preparedness program for WHIP. That's correct.

MEMBER WEINER: I think there's also been an on-going emergency response preparedness under the applicable sections of the Nuclear Waste Policy Act, isn't there?

DR. CROWLEY: At this point, my understanding is that DOE has a Transportation External Coordination Working Group and they've been discussing emergency response, but at this point, DOE has not yet begun to execute its 180©) responsibilities.

And then finally, the next slide, yes, No. 6, the committee makes a recommendation about the structure for DOE's programs for transporting spent fuel and high level waste to a federal repository and recommends that DOE's Secretary and U.S. Congress examine several options for changing that structure and the three possibilities that are discussed in the report are listed there, a quasi-independent DOE office, a quasi-government corporation or a fully-private organization and it would have been beyond the committee's task to recommend any one of those in the report. But the report does go through a fairly extensive discussion of the pros and the cons of each of those organizational structures. And then finally, last slide.

We released the prepublication version of the report in February and it's available on that website. We're now working on the final version of the report which we'll have editorial and copy editing corrections and that should be issued in June. That in a nutshell is what's in the report.

MEMBER WEINER: Thank you. Are there any further questions from any?

CHAIRMAN RYAN: Just a quick follow-up. It's on the worker exposure question. Was that based on an analysis of calculational approaches to estimating worker dose or actual worker dose for folks that have moved that kind of material already?

DR. CROWLEY: In the DOE EIS for Yucca Mountain, it was based on assumptions, fairly conservative assumptions I should say, about the radiological age of the fuel that would be moved.

CHAIRMAN RYAN: So I think it's really not fair to say that workers will receive a dose or they will be burned out or anything of the sort because it really is a calculation and an estimate which by your own reckoning is conservative. There is a body of data on people that have made those kind of shipments around the country whether it's military materials or spent fuel or Navy fuel or other things. So it would be interesting to see if your prediction meets actual experience and that experience is out there.

DR. CROWLEY: Yes, it's not our prediction. It's DOE's estimate which --

CHAIRMAN RYAN: DOE's estimate, but one you've embraced.

DR. CROWLEY: That's correct.

CHAIRMAN RYAN: But I think the actual experience is where the rubber meets the road and I'm going to guess it's not anywhere near 2 rem per year.

DR. CROWLEY: We did look into the possibility of getting data on actual exposures. Those exposures are not reported to the Nuclear Regulatory Commission. They are probably held by the individual operators, but they weren't accessible to the committee.

CHAIRMAN RYAN: And in fact, that can be tough, but there is real data if one wanted to move away from an estimate and into the real world.

DR. CROWLEY: Certainly.

MEMBER WEINER: I'd like to follow up on that comment and that is did your committee have any sense of how conservative the estimates at the ISR.

In some cases, I know that there was an assumption that there would be not shielding. In other words, there were assumptions made which are contrary to ordinary occupational practice. Did you comment at all on that? Did you give that any credence?

DR. CROWLEY: When we went through all of the analyses, not only the DOE FEIS but also the Reexamination Study which also formed the basis for the DOE FEIS, as the committee went through and analyzed the various assumptions, there were comments made in the report about the relative conservatism or nonconservatism. What you find in the, for example, the Reexamination Study and the DOE FEIS, there is a mixture of fairly significant conservatisms with realisms and it's not clear when you mix all of those things together. It's certainly conservative, but it's not clear how conservative it might be.

MEMBER WEINER: Bill.

MEMBER HINZE: Kevin, would you expand just a bit about your bottom line messages to an independent examination of transportation, security, etc? What do you mean by independent? Free from the governmental agencies?

DR. CROWLEY: Yes, in other words, this should not be an examination that the governmental agency does itself. It should be an examination done by some organization independent of the government that has control over who is appointed to do the examination and also individuals who are free from conflicts of interest. It doesn't mean that -- I mean they could be experts, but they shouldn't be people whose careers or financial outlooks will rise or fall with the results of the study.

MEMBER HINZE: Was this recommendation prompted by any concerns or was this a matter of making the public perception very transparent, making the situation very transparent?

DR. CROWLEY: The committee saw nothing during the study that would have led it to believe that there was a transportation security problem in part because the committee just didn't get much information. There's a little bit of information in the open literature but not very much and some of it has been pulled back since September 11th. So there's a limited open source database from which the committee could have made any analysis. However, during the course of the committee's information gathering meetings, the committee heard again and again that this was a major public concern and one of the parts of the committee's Statement of Task was to identify major technical and societal concerns and address them. So the committee tried to address them, wasn't able to and felt this is an important concern and somebody should address it.

MEMBER HINZE: Thank you.

MEMBER WEINER: Jim.

MEMBER CLARKE: Just a quick question, Kevin. You gave us the committee definition for large quantities. Your slides have two terms, long duration fires and very long duration fires. Did you have similar definitions for those?

DR. CROWLEY: They both refer to fires that exceed the regulatory 30 minute fires. The terms that are used in the report to characterize both of those are hours to days. Based on historical record, there are fires from accidents, train accidents mainly, that have burned for days. So that would be the committee's definition of a very long duration fire. Yes, the Howard Street Tunnel fire which is one of the accidents that is being analyzed by the Nuclear Regulatory Commission would probably fall under rubric of a long duration fire. It burned for hours.

MEMBER CLARKE: Thank you.

MEMBER WEINER: Allen.

VICE CHAIR CROFF: Can you elaborate just a little bit on what caused the committee to recommend new organizational structures for the program?

DR. CROWLEY: You know how our committees are, Allen. We had a lot of discussion over a lot of meetings about that, but I think the bottom line was, and remember this report was completed before the recent schedule, the new schedule for Yucca Mountain was announced which has put things back by many years, the committee was operating under the assumption that the Department of Energy was driving for a license application first by the end of 2004 and then as soon as possible thereafter and opening a repository in a 2011 and 2012 time frame looked at what the transportation program had been able to accomplish or not accomplish not because the staff were not up to the task. In fact, quite the contrary, the committee thought that a lot of the staff in the program with which it dealt with were pretty top-notched, but they just weren't being given the resources and the management attention that they needed to get the job done.

The committee felt that there might be a conflict here of mission because the transportation program was answering to management for the repository development program. They were competing for the same pot of money and the same management attention and the committee felt that really the DOE Secretary ought to look at alterative structures that would take the transportation program out from under the repository development program and give at least equal billing within DOE.

The other concern that weighed in the committee's analysis was the fact that not only are we talking about a transportation program for a repository, but now we appear to be talking about a transportation program for interim restorage and possibly for even an integrated spent fuel recycling facility. So the committee sensed that the Government's need for a transportation capacity would be growing in the future and that having a transportation program that was again sitting under the repository development program was not the kind of transportation capacity that could service these other potential future needs.

VICE CHAIR CROFF: Thanks.

MEMBER WEINER: I had just a couple of additional questions. One is to follow up on Dr. Clarke's question. Your definition of large quantity, you would doubtless consider 100 shipments a large quantity. Is that more or less correct? Or let's say

1,000 shipments. That's a big quantity.

DR. CROWLEY: The definition that the committee used was based on mass shipped not on number of shipments.

MEMBER WEINER: You can translate it to number of shipments by saying so much mass per shipment.

DR. CROWLEY: It depends on the mode that you use.

MEMBER WEINER: All right. Let's say trucks for assemblies per cask.

DR. CROWLEY: You're going to ship about between 0.5 metric ton and 2 metric tons per shipment depending on -

MEMBER WEINER: Right. Per shipment. So what would you consider a large quantity shipment?

DR. CROWLEY: A large quantity shipping program would be a program that ships on the order of hundreds to thousands of metric tons. So you would be looking if you said each truck carried on the order of 2 metric tons, then a program that involved 50 truck shipments would be considered in the committee's view to be a large quantity shipping program.

MEMBER WEINER: Is that independent of how long the shipping campaign is? In other words, you're not considering that the shipping campaign could last ten years, twenty years.

DR. CROWLEY: There is nothing in the report that has the time scale, although most shipping campaigns are for a more definitive period of time unless they're a very large quantity shipping program where you're shipping all of the time.

MEMBER WEINER: This gets exactly to my point. We have had shipping campaigns that have taken place inside of a year that have involved roughly, let's say, ten shipments just using ten spent fuel shipments and the truck estimates by DOE would be five or six shipments a year. Did that play any part in your designation of concern or your concern about large quantity shipments? Clearly, ten shipments, ten truck loads of spent fuel, ten highway truck loads of spent fuel, is not a large quantity shipment.

DR. CROWLEY: Not in a particular year, but if you had ten shipments year after year after year, that would be a large quantity shipping program at some point.

MEMBER WEINER: What would make the difference? Why would that be a large quantity? What assumptions are you making to make that large quantity shipment program? That's what I'm kind of driving at.

DR. CROWLEY: I don't think I understand the question.

MEMBER WEINER: All right. Are you assuming --

DR. CROWLEY: Try again.

MEMBER WEINER: I'll try again.

DR. CROWLEY: Okay.

MEMBER WEINER: Are you assuming that there is a cumulative effect of shipments year after year that would make a shipment of a relatively small quantity continuing year after year for say 20 years because the Yucca Mountain shipments are, there is a number at the end of there that would make that a large quantity shipment to be of concern? Is it the fact that you are inherently assuming some sort of cumulative effect?

DR. CROWLEY: You are assuming a cumulative effect because you're looking at an ultimate quantity and in that is entangled the number of shipments per year, the number of years that you're shipping. I should say that this demarcation, it's not a sharp demarcation and that's why the committee used words like "on the order of" and what it was really trying to get at was the occasional shipments that one makes, for example, from one reactor site to another to even out-loads and spent fuel pools versus a program where you're having a focused, continuous, long term shipment of spent fuel from a lot of different sites, for example, to Yucca Mountain or a lot of different sites to an interim storage facility. A lot of those sites would be made -- Sorry. A lot of those shipments might be made along the same routes year after year. You're putting a lot of fuel on the road.

MEMBER WEINER: So there is an implication that if a lot of shipments are made along the same routes year after year that those shipments have a cumulative effect.

DR. CROWLEY: I think that's correct.

CHAIRMAN RYAN: I'm sorry. I'm lost. What is cumulative effect? Effect on what? By whom?

MEMBER WEINER: Effect on that people who live by the side of the road.

CHAIRMAN RYAN: So the point is the doses go up or if we're talking about collective dose, we're going up the wrong tree.

DR. CROWLEY: This has nothing to do with collective dose.

CHAIRMAN RYAN: Okay. I'm stuck with cumulative effect and I'm not sure what you mean, Ruth.

MEMBER WEINER: Yes. Well, I meant -- Thank you for the clarification. I was trying to work around calling it collective dose, but clearly, the concern is risks posed by these shipments, is it not?

CHAIRMAN RYAN: The risk to an individual of a shipment going by is the same for each shipment theoretically.

MEMBER WEINER: Exactly.

CHAIRMAN RYAN: But it doesn't add up because it happens at different times.

MEMBER WEINER: That was exactly what I was getting at.

CHAIRMAN RYAN: It's not cumulative.

DR. CROWLEY: I don't think this -- It's not directly related to risk. It's the committee has called for a number of actions to be taken by organizations that are involved in the shipment of large quantities of spent fuel. The committee has actually made a fairly subjective judgment about steps that should be taken by these large shippers versus steps that would not be taken by the small shippers and the committee was particularly concerned that certain steps be taken by organizations like DOE and Private Fuel Storage for example in the training of emergency responders for these shipments that involve large numbers of shipments, large quantities of fuel, that will go on for long periods of time.

MEMBER WEINER: Okay. Thank you. Did you have something to add?

DR. KANNINEN: I was interpreting your --

MEMBER WEINER: Could you lean into the microphone and tell us who you are?

DR. KANNINEN: Oh, who I am?

MEMBER WEINER: Yes, for the report.

DR. KANNINEN: Mel Kanninen just speaking out of turn. I thought perhaps in an attempt to clarify your question I was thinking you might have meant cumulative effect on the containers and/or other hardware of which there could be a concern if you are reusing these over long periods of time and that is something the committee did think about but did not think it was a major issue.

MEMBER WEINER: Thank you. Do any of the staff have a question at this point? Hearing --

MR. HAMDEN: (Inaudible.)

CHAIRMAN RYAN: The answer is no.

MEMBER WEINER: The answer is no. Thank you. Dr. Kanninen, you're on next I believe.

DR. KANNINEN: I don't really have a

presentation because basically Kevin took all the good stuff for himself. So I'd be left with nothing but detail. What I thought I would do is preserving the maximum amount of time for questions that I was hoping that you might have to just give you my own perspective on how the committee approached the question of package performance and standards and the concerns that we had and underlying the recommendations and conclusions that Kevin already gave you.

From my own perspective, I was not a member of the committee at its outset. I got a call from Kevin Crowley after the first meeting saying that the committee had decided that it needed an expert in materials and structural behavior and I guess they couldn't find one. So they asked me. It was supposed to be a funny line.

DR. CROWLEY: That was a joke.

DR. KANNINEN: Anyway, they did find me and I had not previously been associated with shipping radioactive containers and shipping. I was of course aware of it because a number of my colleagues had been doing that kind of work, for example, at Pacific Northwest and I interacted with them informally as to what was going on way back as far as, I don't know, in the 1970s when all this started. So I guess that could be considered a good thing in a way because I certainly had no biases to anything except a bias towards good engineering.

I joined the committee for the first time at its meeting in Las Vegas and we heard quite a lot from the State of Nevada and their concerns in particular with regard a perceived need that they had for full scale package testing and other aspects and of course we heard from others as time went on. I did have some concerns initially in that if we're not doing full scale testing here, how could we possibly justify that these things are perfectly safe. As time went on, we began to get a broader horizon.

I think the greatest aid to my understanding was the week that I spent at a conference. It was the biannual conference of the PATRAM which is Package and --

MR. EASTON: And Transportation of Radioactive Materials.

DR. KANNINEN: That's it. Earl was there. So he would know the acronym.

MEMBER WEINER: Would you repeat it for the recorder?

DR. KANNINEN: PATRAM is Package and

Transportation of Radioactive Materials. Thank you, Earl. At that meeting, my eyes were opened, were opened even wider than they had been in the sense that we had people from all over the world involved in this, Europeans, Japanese, Asian countries, the United States of course, Canada and others and they were doing what I consider to be good engineering in the sense that you are doing experiments, small scale experiments; you're doing computer simulation, very sophisticated computer simulation.

And you're not doing these in isolation from one another. You're doing them together in a unified way, an integrated way and introducing full scale testing, a very expensive thing. So you have to be very cautious about not using them, but you're using them in the proof of principle way and that is the right way to do that. So I think as a result of my week at that conference I came away with a very strong opinion that the regulations are well founded, the IAEA regulations I mean, the drop test, the puncture test and not so much about the immersion test and I'll leave the fire out of it for just a minute because you appear to want to separate that.

But I had the impression and actually having the opportunity to witness a drop test of a several ton article 30 meters and it's very impressive when you stand about 100 feet away and you hear the ground shake when it hits and hardly any damage visible. So as a result, the accumulation of all this kind of experience and of course, there are others on the committee too, I don't claim all of the structural mechanics and materials as well, there are others as well, Clyde for example, but I think we all are pretty much of the same mind that the regulations are well-founded and well carried out and respected and they've been in place for quite a long time and as the people at that meeting were fond of saying "We haven't had any accidents of any major sort or even minor sort with these regulations in place." The conclusion that we came to and Kevin reported to you is we're pleased with those and we think they will do the job.

The fire, I suppose I should get into that now, is a different story. The contrast there is you look at the mechanical testing, drop test, puncture test, you're looking at a combination as I said of good engineering which is computer simulation, modeling, large scale tests. You don't see that in the fire arena. You see a lot of computer modeling. You don't see the testing and the other thing. You also see a lot of people doing calculations or simulations I should say for the same event and coming up with quite different conclusions.

And the best example of that, of course, is the Baltimore Howard Street Tunnel. The people at the University of Nevada, not just to pick on anybody particularly, is coming out with analyses of that incident that said these things would probably fail. Then the Nuclear Regulatory Commission looking at the same set of circumstances comes out with a different conclusion and these simulations were going on at that time that we had to wrap up our report. So this is the basis of us saying we think, I don't think we used the words that there's a problem there, but we really think that this, in my own words now, we don't think that the fire part of the four part regulations is in nearly as good a shape as the others and work ought to continue there which is not to say that anybody isn't doing a good job or they're not bringing their best resources to it. I think they need to continue on and I would add that they ought to look at the way that they have developed the other regulations and try to spread themselves out from merely looking at, this is my own opinion again, in fact, I don't think that this is in the report, that they ought to be looking at that same kind of a triage of things looking at model experiments, full scale experiments to the extent that that's feasible, together with the computational simulations that they have.

I hope that gives you a little bit of background as to where I'm coming from and any questions you have I'd be glad to take. Kevin might want to add something.

DR. CROWLEY: Can I add something? Again, with respect to the long duration fires, there's a real contrast between the testing that has been done, this is real world testing, basically demonstration testing, and certification testing as well, the certification testing is where you're actually doing package drops and you're measuring the forces on the packages and you're looking at the deformation, and then the demonstration testing, for example, the Sandia testing done in the `70s where they crashed locomotives into casks and they ran trucks into walls and that kind of thing and then the Central Electricity Generating Board tests in the U.K. in the `80s. It's pretty clear from all of those tests that were done that the hypothetical accident conditions in 10 CFR 71 are more severe a test of package performance than crashing a locomotive into a package at 100 miles per hour and that's very clear from the testing that's been done. We have enough testing now to know that.

That's not the case for thermal testing. The thermal test is a half hour optically dense, fully engulfing fire. It's not clear that that's a more severe test of package performance than you might encounter out in the real world and one of the reasons we don't know that is because we just haven't done as much work in that area. Certainly, full-scale demonstration testing has not been done to the extent that it has for mechanical testing and the simulation testing that is being done now. That's one of the things the Nuclear Regulatory Commission has been working on quite diligently and is still working on. But again, so far you just haven't seen the level of work there that you've seen in the mechanical test area.

CHAIRMAN RYAN: Kevin, could you or your colleagues maybe sharpen the point on that a little bit? You gave the criteria for the NRC criteria and you haven't really said how that translates to what happens or could happen in a fire. I mean is the temperature too low. Is the engulfing aspect, the time, too short? Do you have any sense of what other criteria might look or sound like and again, I'm not trying to hold you to your rules as you see them now but just to explore that a little bit. It's not good enough or there hasn't been enough is okay, but why?

DR. KANNINEN: I think if we thought it wasn't good enough we would have raised some very big red flags. So obviously, we didn't come to that conclusion.

CHAIRMAN RYAN: Okay.

DR. KANNINEN: And we're uncertain.

CHAIRMAN RYAN: I thought I heard Kevin say there was concern.

DR. KANNINEN: Well, there is a concern that that may be the case.

CHAIRMAN RYAN: Okay. Fair enough.

DR. KANNINEN: But we did not reach that conclusion by any stretch of the imagination.

CHAIRMAN RYAN: Okay.

DR. KANNINEN: The temperatures in the regulatory are what? Eight hundred Centigrade they are.

DR. CROWLEY: Fourteen seventy-two.

DR. KANNINEN: I'm sorry. Fourteen seventy-two, less than was calculated to where these various accidents, at least the Baltimore Tunnel case and the time duration is certainly much longer than the regulatory limit. So one would be a little --

CHAIRMAN RYAN: Then the next question is if a fire went on at some temperature for a longer period of time, would it really be an impacting thing or not. I guess I'm trying to help design a test while I was sitting here to see what the range might be to go from what's in the regulation to what might be more encompassing of the real potential experience. Do we put our magnitude away or a factor of two away?

DR. KANNINEN: Well, that's difficult to say. I mean you could obviously destroy a cask by giving it enough temperature for a long enough time.

CHAIRMAN RYAN: Sure.

DR. KANNINEN: So you must make a judgment based on what's really possible out there and you must always be guided by that.

CHAIRMAN RYAN: And those are the parameters I'm looking to understand a little bit better on what is really possible. Is it 2,000 degrees C for five hours? Did you explore that at all?

DR. KANNINEN: No, we did not and I don't see how we could have either.

CHAIRMAN RYAN: No, I'm not saying you should have. I'm just saying did you.

DR. KANNINEN: No.

CHAIRMAN RYAN: Okay.

DR. CROWLEY: Let me add to that. I think again we've broken this into two classes. We'll call them the mechanical class and the thermal class. On the mechanical side, it's a little easier to bound things than it is on the thermal side and there have been a number of studies done and particularly the Central Electricity Generating Board Study where they very carefully thought through what are the kinds of mechanical accident scenarios that we might encounter and they really subjected the package to a severe test, probably more severe than they would actually really expect to encounter and they still showed that the mechanical forces put on that package were less than the 30 foot free drop test.

On the thermal side, again it's much harder to know what the upper bound of a fire is and I don't know that so much that it's the temperature because a hydrocarbon fire burns at the temperature a hydrocarbon fire burns at. But in my mind, it's really the duration and you have to think about the situations in which a package might be subjected to a long duration fire, very long duration fire, well in excess of the 30 minute regulatory test under a circumstance where it would be very difficult to get to that fire and put it out.

I mean an obvious place where that could happen would be a tunnel, but there could be other places where that could happen as well. For example, you could have, this wasn't in the report, an interaction between a train carrying a spent fuel package and a train carrying hazardous materials in a remote location where it would be very difficult to mount an effective firefighting response. That package, that fire, could burn for hours before you could get out there and put it out.

So I think what the committee was calling for in the recommendation was really for the Commission to think through this process of what is a credible upper bound for the kind of a long duration fire that we might encounter and then to run the simulation and see how would these various packages behave in such a fire. And if there is an issue with behavior, are there simple operational controls that you could put into effect to avoid those kinds of fire scenarios?

MEMBER WEINER: We have -- Could you identify yourself for the reporter?

MR. RULAND: Yes. I'm Bill Ruland. I'm

the Deputy Director in the Spent Fuel Project Office. If it would please the Committee after the National Academy has finished their presentation, we're prepared to talk a little bit about these issues. Actually, I'm not prepared to talk about them. Earl is prepared to talk about them, but we came anticipating these issues would come up. So again, if it would please the Committee after this presentation, we'd be happy to talk about some of these.

MEMBER WEINER: Certainly. Would you like to be recognized immediately as part of the fire discussion or do you want to wait for that?

MR. RULAND: It's strictly up to you.

CHAIRMAN RYAN: Ruth if I could, let me make a suggestion. If you have a comment as we go along, it's probably more appropriate that you make that comment at the time because we are on a schedule this afternoon. We don't have an unlimited amount of time.

MR. RULAND: Okay.

CHAIRMAN RYAN: So putting in another series of formal presentations or even informal ones, really we ought to stick to our agenda and if you have comments now or want to participate, raise your hand and we'll be happy to have your comment as we go along. That might actually help the audience as well.

MEMBER WEINER: I'd like to --

PARTICIPANT: (Inaudible.)

MR. RULAND: Yes.

MEMBER WEINER: Would you like to come up to the table, Earl, and make a comment now? While you're coming up, I have a question, two actually. When you were at the demonstrations in Germany, Dr. Kannenin, did you have a chance to observe the propane tank explosion sitting next to a spent fuel cask or did that take place at some other time? I have a film of it. That's why I'm asking.

DR. KANNINEN: No. The --

(Discussion off microphone.)

DR. KANNINEN: You're talking about a real demonstration.

MEMBER WEINER: Yes, it was a real demonstration.

DR. KANNINEN: No, the Berlin thing was done specifically for the PATRAM conference people from all over the world, but they only did that one test.

MEMBER WEINER: I see. No, I was asking really if you were familiar with the propane tank test, would you consider that in excess of the regulatory fire or couldn't you make just from that one test a judgment?

DR. CROWLEY: That's a very different animal. In that test, the fire was very brief and intense and it basically blew the cask away from the source of the fire. So it wasn't a long duration, fully engulfing fire.

MR. EASTON: It lasted seven seconds that fire --

MEMBER WEINER: Thank you. Identify yourself.

MR. EASTON: I'm Earl Easton. I'm with the Spent Fuel Project Office staff and first let me agree with Mel. I don't think the fire part of this, the thermal, has been as visible over the years as the structural part. But I do know there's been a lot of work, there's been a lot of testing, done on this. It just hasn't received the notoriety because I don't think it was determined to be the sexy issue at that time.

But let me just put this whole thing in some sort of perspective. In the past 30 years if you look at the FRA data, there's been around 21 billion train miles and if you look at that time just the hazmat reports where there's been reported release of hazmat, that's about 1300 incidents and that's a drop to several tank loads. And if you go through and you look over those 1300 reports which I have, you will find that there may be five or six examples of where you really get a long duration, fully engulfing fire because it's not only the temperature. It's the location because you know most of the heat transfer is through radiation and if you're some distance, the view factor falls off and if you're down in the fire, the view factor falls off.

Again, the fire test is one-half hour, fully engulfing, 1475. But the important part people don't state is at the end of that test virtually nothing happens. So when we run out the analysis, we are able to run out the analysis six or seven or eight hours fully engulfing fires and you don't get any what we think are in the danger zone.

CHAIRMAN RYAN: Let me just translate that if I may, Earl. You do a 30 minute test and you extrapolate that by calculation for eight hours.

MR. EASTON: Yes, we do a fully engulfing extrapolation or computer simulation out to eight hours. The seals fail, but the seals are not the important things in accidents.

CHAIRMAN RYAN: I just want to understand because you're using a lot of jargon and I'm just trying to translate it so I understand it.

MR. EASTON: Okay. Sorry.

CHAIRMAN RYAN: We have a lot of data on the half hour tests.

MR. EASTON: Right.

CHAIRMAN RYAN: You've somehow created a computer model that will allow you to take that reference first half hour and then model it out to seven or eight hours.

MR. EASTON: Right. We use the same assumption for the 30 minute test and we continue running that computer simulation out of seven or eight hours.

CHAIRMAN RYAN: So you're just making the assumption that everything stays the same for eight hours.

MR. EASTON: Right. And then --

CHAIRMAN RYAN: Let me just ask a couple questions if I might. Does that continue to heat up the inside of the contents?

MR. EASTON: Yes, it does.

CHAIRMAN RYAN: All that stuff. So all that real physics is included.

MR. EASTON: Yes, it does.

CHAIRMAN RYAN: Okay. I just want to understand that.

MR. EASTON: And then again when you look at it, there are several barriers to release. One is the seal, but that's primarily for normal conditions. I can explain that later if you want.

CHAIRMAN RYAN: Well, let me -- I don't want to get too far into the details because frankly this is for them to give their report. But it sounds like you're implying, tell me if I'm incorrect in assuming this, that the NRC test is okay as it is.

MR. EASTON: I think it's so and especially if you look at it in terms of risk informed. There are maybe five accidents that you might put in this category out of 21 billion rail miles.

CHAIRMAN RYAN: I hear you that the risks seem to be low or are low based on the statistics you've quoted, but what does that have to do with the test?

MR. EASTON: Well, if you look at each one of those five accidents and where the cask would have to be placed to get a fully engulfing fire, you find it's almost impossible. So really the fully engulfing test simulates how much heat goes in per time. You very seldom get long duration and fully engulfing.

CHAIRMAN RYAN: But there's two questions there. If we had a fully engulfing and let's leave the probability of that alone for a minute, that's one question. The probability of that ever happening is a separate question. You're giving the information about the probability of it happening which is below the radar screen is what you're advising us to observe.

MR. EASTON: Right.

CHAIRMAN RYAN: Given that it is something people want to consider then we're back to what does that profile look like.

MR. EASTON: If it is something you want to consider, the test after a half hour of a fully engulfing fire --

CHAIRMAN RYAN: I'm not saying I do. I'm saying if it's something somebody wants to consider irrespective of the probability.

MR. EASTON: The test after a half hour, fully engulfing fire, the acceptance criteria is basically nothing happens to that cask.

CHAIRMAN RYAN: I know. I agreed to that.

MR. EASTON: A release. So if you carry out the analysis, you will find you really have six or seven hours before you get the fuel cladding heated up until you start worrying about the fuel cladding. You never really lose the lid to body contact with --

CHAIRMAN RYAN: Yes, you've reported that to us before. We don't need to cover all that.

MR. EASTON: Sorry.

CHAIRMAN RYAN: You talked about the seals going away and the metal -- and all that. We're up on that.

MR. EASTON: Okay. So we think that the test is pretty good. It's well understood and it really bounds all the accidents that we have really seen. And one further comment, if you go to dedicated trains and you go to a no pass rule in tunnel, this would probably have eliminated every historical accident in the last 30 years that you could have gotten a fully engulfing, long duration fire.

CHAIRMAN RYAN: Are you going to somehow memorialize that analysis in a report?

MR. EASTON: Yes, as soon as we put all the data --

CHAIRMAN RYAN: I know it's a lot of work, but it would be real helpful if that analysis was formalized and shared with everybody that had interest because that is in fact the real data.

MR. EASTON: As part of taking these guys very serious like we do --

CHAIRMAN RYAN: Thanks for your patience, Ruth. I'm done.

MR. EASTON: That's one of the ways we're going to respond to their call for --

DR. CROWLEY: Let me say. I think we've had a lot of interactions with Earl and Bill Brock during the study and I think they got the sense early on that this was an issue that the committee was concerned about and to their credit, they've done a lot of additional work to try to put this issue to rest. So I think they should be patted on the back for that.

CHAIRMAN RYAN: I agree. That's great.

MR. RULAND: Yes, I just wanted to say we appreciate the committee's comments in this particular area. We're already working as Earl has already said. We're not waiting. We're already looking at the research and the data. We're already taking action to try to incorporate operational controls which was a big part of one of the committee's recommendations. So again, we wanted to say thank to the committee and thank you for this Committee for listening to this important issue.

MEMBER WEINER: Thank you.

MR. EASTON: In fact, just Kevin put a bug in my ear, but when you go back and look at all the historical rail accidents and especially these five, they all involve the derailment of a single train. If you use dedicated trains, that largely goes away. FRA, we're in discussions with them. They're under a mandate whether to require dedicated trains based on risk.

The other thing is the other accident that doesn't go away is the tunnel fire which we're studying and we have already approached the Association of American Railroads about the possibility of changing circular OT-55 which would prohibit the practice of trains passing in tunnels carrying flammable liquids and that which we feel would have virtually eliminated the Baltimore Tunnel fire; although a dedicated train rule would eliminated the Baltimore Tunnel. So we're moving out very rapidly to try to get a better understanding of this whole thermal issue and when we come back in the summer and talk about the Baltimore Tunnel fire, maybe we'll have some of this memorialized for you.

CHAIRMAN RYAN: Before talking.

MR. EASTON: Okay.

MEMBER WEINER: Thank you.

CHAIRMAN RYAN: Thank you, Earl.

MEMBER WEINER: Jim, do you have any further questions? Bill?

MEMBER HINZE: I'm a little bit confused by Mel's comments and I'm referring to the committee strongly endorses full scale testing for -- performed under both regulatory and credible extra regulatory conditions. I heard you say that this type of testing was only really needed for the thermal area. Is that correct?

DR. KANNINEN: No, no. I said it is well practiced in the mechanical testing area, in other words, using that as a mirror for the people who are too worried about the fire to consider.

MEMBER HINZE: So the mechanical has been taken care of.

DR. KANNINEN: Yes, very much so.

MEMBER HINZE: But it doesn't say that in this recommendation that the emphasis should be on the thermal area.

DR. KANNINEN: You are referring to the statement.

MEMBER HINZE: I'm referring to slide nine.

DR. CROWLEY: I think you're really looking at two different issues there.

MEMBER HINZE: So help me then.

DR. CROWLEY: The committee, one of the issues that came up during the study and again this was raised by several people at the committee's open meeting was the whole issue of testing and whether testing should be required for every package and whether in fact you might want to production test certain packages and whether or not you might want to test packages to destruction. I think what the committee was trying to say there was current practice is really good. It's an integrated process right now. This is how it should continue to be done.

MEMBER HINZE: Okay.

CHAIRMAN RYAN: Is this what it actually says in the text or is there explanatory material other than this bullet?

DR. CROWLEY: Of course, there is explanatory.

CHAIRMAN RYAN: I think maybe we're picking on a bullet and maybe the text explains what you just said. Is that right?

DR. KANNINEN: But if I could just add to what Kevin said, there was at the very beginning people who were not enamored of the whole idea of transporting radioactive waste who would insist on full scale testing as Kevin already said and what they meant by that was testing to destruction whereas full scale testing in the sense of the regulatory thing is what we endorse and we do not endorse testing to destruction. So this bullet is sort of aimed at that particular point of view.

CHAIRMAN RYAN: Thank you very much.

MEMBER WEINER: That's a very good clarification. Thank you. Allen.

VICE CHAIR CROFF: I'm not sure I understand this and it's the full scale part. Why did the committee recommend full scale as opposed to current practice which is mostly fractional scale, calculations and this kind of thing?

DR. KANNINEN: No, the current procedure is to use full scale but to use it sparingly in a company with computation simulation and scale modeling. So you're working them all together. So you're using it in that way. You certainly are doing full scale testing.

CHAIRMAN RYAN: I'd have to go back and read our letter, Allen, but I think the committee is in agreement with what you just said, limited full scale testing for certain performance questions.

VICE CHAIR CROFF: Yes.

CHAIRMAN RYAN: Calculational approaches which we agree that we saw some pretty sophisticated modeling tools in our Commission gathering and then the testing to destruction for the sake of destroying something, we said didn't make any sense.

DR. KANNINEN: That's correct and that's exactly what that bullet is aiming at.

MEMBER HINZE: A lot is lost in the brevity.

CHAIRMAN RYAN: And fair enough because you can't put it all in three lines of bullet.

VICE CHAIR CROFF: You mean you haven't already reported that.

(Discussion off microphone.)

MEMBER WEINER: Just to interject, it is the full recommendation which is considerably longer than what you summarized on your slide that is quite clear that it endorses the use of full scale testing to determine how packages will perform under both regulatory and credible extra regulatory conditions and I think that explains that you wish to continue to determine how packages continue to perform under these conditions that you have identified.

DR. KANNINEN: Correct.

MEMBER CLARKE: Excuse me, Ruth. The first part of Bill's questions addressed on Slide 8 with a recommendation is made that the NRC to undertake additional analyses for long duration fire scenarios. It did have a column of -- Are there any specifics associated with that recommendation or is the recommendation to go back and look at different scenarios and go from there?

DR. KANNINEN: Well, our report was wrapped up prior to much of what the NRC is doing as you just heard Earl talk about. So they have really done that themselves and that of course is the best possible outcome for us to suggest and for them to act. So we're very pleased by this.

DR. CROWLEY: The committee did make a couple of comments and suggested that perhaps the historical record would be a good place to start if you were looking for credible, long term duration fires and two examples were mentioned. There was a Livingston, Louisiana fire, I think it was 1972, but I could be mistaken about the date, that burned for three days. Now they let that burn. Presumably they could have put it out if they wanted to.

MEMBER CLARKE: Was it a train derailment?

DR. CROWLEY: It was a train derailment. It was carrying plastics or plastic products I think. And then there was the Summit Tunnel fire in the U.K. which burned I think for about four days. In fact, based on the Summit Tunnel fire, the U.K. Department of Transport put a rule or a regulation in place that prohibited trains carrying flammable materials and trains carrying spent fuel from being in the same tunnel together.

MEMBER WEINER: Bill, any further questions? Allen?

VICE CHAIR CROFF: I wanted to be explicit on one point. We've been talking about the long duration fires, but something that I think was said was you're not recommending consideration of higher temperature fires, higher than the 1475.

DR. KANNINEN: I don't think -- We didn't make any recommendation with regard to specific targets either in temperature or time. We think that would have exceeded our capabilities.

VICE CHAIR CROFF: I'm asking sort of -- You've said in general take a look at long duration, but what about higher temperatures?

DR. CROWLEY: There's nothing.

DR. KANNINEN: We did not exclude going to higher temperatures.

VICE CHAIR CROFF: So the report is silent on that.

DR. CROWLEY: Yes.

DR. KANNINEN: Well, no, not entirely because as Kevin just said a moment ago, we recommended that we look at the accidents that have already happened and be guided by that. So if these accident suggest higher temperatures, then certainly you want to use higher temperatures.

VICE CHAIR CROFF: Okay.

CHAIRMAN RYAN: Or at least think about why or why not.

MEMBER WEINER: I'm going to ask. Do we have Clyde Young on the telephone.

PARTICIPANT: We weren't able to get him.

MEMBER WEINER: Oh well. Thank you. That is too bad. Mike, did you have further questions?

CHAIRMAN RYAN: No. Thank you, Ruth.

MEMBER WEINER: Okay. Moving right along, Dr. Morris, you're going to talk about emergency response, emergency preparedness.

DR. MORRIS: Actually, I was not.

MEMBER WEINER: Well, it's what it said on my little cheat sheet here.

DR. MORRIS: I'm sorry. No, I did not come prepared to talk on that.

MEMBER WEINER: That's all right.

DR. MORRIS: In fact, Kevin has covered all the parts of the report that I might have commented on and so I think I'll just leave it there and respond to any questions within my knowledge that you might have as we go on. We do have one other committee member on the phone and he know he's prepared to comment on some things that Kevin perhaps didn't touch on.

MEMBER WEINER: Yes.

DR. CROWLEY: Joe was really involved in preparation of the route selection chapter, Chapter 4. So if you have questions about that, you should direct those to Joe.

MEMBER WEINER: Is there any -- Well, let's leave that for the end as long as you're here. Hank Jenkins-Smith who is on the telephone. Hank, are you still there? Hello?

MR. JENKINS-SMITH: I'm sorry, Ruth. I lost you for a second.

MEMBER WEINER: I'm sorry. Well, you've sent some slides I understand.

MR. JENKINS-SMITH: I did.

MEMBER WEINER: And you're on, Hank, to talk about social risks, emergency preparedness and any other topic that you would like to address.

MR. JENKINS-SMITH: Sure. I should start with the social risk of talking to an audience you can't see.

CHAIRMAN RYAN: Hank, just to make life easier, if you just tell us "next slide please" we have TV sets around the meeting room here where everybody can see your slides and everybody at the table has a copy of your slides. So next slide please will keep us up-to-date with you.

MR. JENKINS-SMITH: Very good.

CHAIRMAN RYAN: Welcome. Thank you for being with us.

MEMBER WEINER: And welcome and this is not an all-together unknown thing for us. So we're aware of the risks.

MR. JENKINS-SMITH: Let me apologize for not being able to be there in person. Teaching schedules and so forth precluded that, but I do welcome the opportunity to talk to this group. I should note by way of beginning that someone gave me a remarkably optimistic title here outlined as "Social and Institutional Challenges and Solutions" and, boy, I wish I had them. But I would rather title it "Social Risks, Challenges and Recommended Solutions."

And what I will do is briefly make some remarks on variations in risk perspectives that underlie some of the problems we have in discussions about technological risks. I will then address some of the implications of those varying perspectives and try to put all of that into the context where the rubber meets the road where agencies are attempting to carry out policies that have the characteristics that lead to the sorts of social risks we're addressing. And then I'll very briefly make some connections to the report recommendations because I think you need to see the pathway about what we did. Next slide please.

Dealing with social perspectives on risks is interesting and somewhat difficult. Now we're used to typical formulation which is usually short-handed as probability times consequence. When we're thinking about a potential hazard, we look across some suite of scenarios, identify the probability of outcomes and their consequences and we can essentially identify risk that way.

The problem with that is that many folks don't understand risk that way. A near cousin is prospect theory and in prospect theory, the value that's put on a loss or a gain is hinged on the status quo. So if you think that a technological risk is going to take away something that you have you'll value it quite differently than you would the gain of an equivalent quantity of good things.

What we are typically dealing with is a prospect theory-like setting in which losses loom larger than potential gains. So thinking about it in probability times consequence terms is a little tough and you know we have a huge debate now over the precautionary principle and the way that we should be addressing risk and it spans continents and governments and policy issue areas and what we're dealing with falls in a chunk of these formulations of risk.

The problem for us when we try to address risk in a social setting is that the nature of risk itself varies. There are quite different dimensions on which people understand the phenomenon of risk. You'll probably all have seen on occasion discussion about the psychometric dimensions of risk, the notions of dread, uncertainty, whether the risk is voluntary and from the perspective of the receiver of a risk, it's often the case that these characteristics lead to massive differentiation in the way the risk is understood and acceptable or opposed.

More generally, when you look across the landscape of potential hazards, people end up having to identify which kinds of events are the most fearful, and we trade them off against one another, I mean, the hazards of guns or gun control. And these kinds of problems permeate the societies and the kinds of dialogues they have about risk, and we're in the middle of that kind of a dialogue with respect to nuclear risks with shifting tides over decades about disks of things nuclear are understood.

Even more tricky is the way that risks get considered when we're making collective decisions in regulations, and legislation, and as it spills over into elections. And just as an example of some of the things that happened here, risks are often ill-suited in the sense that they don't match easily the kinds of choices that we make in the elections or in policy decisions, and they become, essentially fugitive problems. They are problems that can't really be cast as the choices that we normally make, or in terms of the institutions that we use, so problems that are seen more generally are recast in terms of risks and threats.

A specific example is what I've termed

ideological displacement here; and that is, that when broader political values are at stake, and these can be quite broad-ranging, they are often recast in terms of risk, so this social perspective problem leads to quite a cacophony when we start talking about risk. And the difficulty is particularly acute when you are part of a community of researchers or in an agency where there are very specific tight definitions about what risk means, and then you come into contact with somebody who's talking about it very differently, or understanding it very differently, and it leads to substantial confusion, often the perception that somebody else is being misleading, and it breaks down prospects for communication. Next slide, please.

The implications of these different kinds of perceptions of risk are large, and I will focus on what are generally termed perception-based impacts, and just go down a quick list here. The most immediate kind of implication that you can see from perceived risk at the individual level is increased stress and anxiety and kind of health complications that can result from that, or generally there's a loss of a sense of well-being of a sense that one is secure, and that these types of things can substantially reduce the overall quality of life of an individual.

Broadly, in a social context, the imposition of disks, even if they are technologically well understood and the traditional measures of risks are quite small, a sense that these risks are being imposed can lead to a loss of trust, or even a breakdown of the usual social patterns within institutions in the extreme. These kinds of impacts can have huge implications for society.

On a more familiar ground here, we've talked a lot about in the committee about stigma and economic impacts that results from stigmatization of a place because of its contact with a known hazard like radioactive materials, place based losses includes such things as reductions in tourism, loss of agricultural value, shifting of economic activity like conventions. Much of this type of research has been focused on the potential implications for Adda and Clark County, in particular.

In addition, however, place based stigma losses can result in reduced property values, some of which has been measurable in cases of radioactive waste transport programs. These kinds of research, however, really haven't been able to nail down precisely what these processes are. There's a great deal of uncertainty, there's a big difference between what one would expect in normal operations versus accident situations, and we don't have a great deal of experience with severe accidents involving radioactive material handling in the United States, which is a good thing, in general, but it means that the kinds of empirical evidence we have are limited.

Probably the most severe problem you get is when you compound historical patterns of exposure or reduced ability to handle these kinds of risks with ostensive social injustice. These lead to what risk communications folks refer to as outrage, which is a complete breakdown of ability to communicate, and a sense of zero sum, or negative sum gains in which one side views what the other is doing as incredibly harmful to their well-being. Next slide, please.

In a bit of context, when you think about the social perspectives on risk, these may just be conditions that one has to live with, but when I think of these things, and as much as the discussion in the committee went, you have to place them in the context of the people who are actually having to carry out programs or an agency perspective, and it might be DOE or NRC, but somebody has been charged with programmatic responsibilities.

The difficulty for those who are charged with radioactive materials management is that they have multiple principals, or I guess a shorthand might be bosses to whom they have to respond. They have to respond to various committees, a White House, various constituencies. Amongst those constituencies would be the locally affected communities who tend to be the place that the social risks loom largest. But an agency doesn't have the capacity to take one perspective and run with it. They're in a web of perspectives there. They also operate within budgets and deadlines, and the pulling and hauling of an overtime political process. And I certainly don't need to explain that to you, you live there.

It's also the case that in that agency context, the people that they contend with, especially those who have alternative perspectives on the programs, maybe even directly opposing what the program is doing, they're working with people with very different capacities to operate. And it often appears, though it may not be the case, that the other players have greater degrees of freedom than the agency does. They certainly do with respect to the kinds of things they can say, and the forms of expression, and action that can take place in a policy process. But what that does, is it creates a context in which social risk is discussed where the key players, the agencies charged with carrying out these policies, perceive themselves to be caught between the proverbial rock and a hard place. And this results far too often in a kind of a bunker mentality for those who are charged with carrying out these policies, a perception of the public as hostile or stupid, particularly interest groups that are engaged there. And perhaps the most damaging effect is that this kind of a context where highly controversial, complicated issues expressed in competing languages, where agencies have limited room to maneuver undermines the capacity for policy learning, and that is a substantial concern to those of us who are on the committee thinking about these problems. Next slide, please.

I want to emphasize that the leading concerns we had in trying to devise recommendations were to address this inability, or to address the capacity for learning that really is the life's blood of making forward progress on policies like this. And the main pattern for doing this is to have a greater flow of ideas, even learning language between those charged with carrying out the programs and the individuals who they're working with, around them, those who they are charged to protect and so forth. And if one can increase the breadth of the perspective and capability in the advisory groups, one can go a long ways toward doing this. It decreases the probability of a costly social misstep, the sort of problem that we see too often in programs that take decades to overcome. It increases avenues for informal representation. The greater the breadth of the types of people that are included in these advisory groups, the greater the capacity for informal processes of communication for breaking down of mistrust, essentially creating pathways by which people believe they've been heard, and their concerns accounted for. It opens up avenues for two-way learning between agencies and the social risk practitioners, and some of the broader sort of ripple communities that those risk practitioners operate within.

I don't want to argue that this is a panacea. I guess I'd call it more akin to a necessary than a sufficient condition for success, but it's one that I think is urgent, particularly given the propensity for perceptions to rigidify and create these types of defensive postures that so crowd the aim of risk management. Next slide, please.

More specifically, we had two recommendations along these lines in the report. The first was to expand the technical external coordination working group. Now this group which I've had the good fortune to speak to on a number of different occasions currently is made up of technically trained individuals, people who are involved in various kinds of official capacities, and what we're recommending is that it be expanded to include people who more specifically and broadly deal with risk communication, understanding risk from various perspectives in order to open up the potential for greater two-way communication.

I should say that a great deal of this happened already. What we want to do is reinforce it and stabilize it to make sure that it's a continuing feature of the program. The second point was to retain and modify the nuclear waste technical review board. It's already an existing institution. It's functioned as an independent and generally respected voice in looking at technical issues. We'd like to see it expanded to handle the social implications of the nuclear waste transportation problem.

Now this would be a group that would

chiefly be speaking to those who are implementing or regulating the process, essentially translating social science and practitioner knowledge as we have it for those who need to use it. And in some sense, this group needs to be one that could have access to relevant classified materials so that they would be able to speak to those issues, so in some sense, think of these two recommends, one as more porous and open, and broadly two-way in the kind of communication that it's bringing from external communities through the TEC into agencies, and scanning for potential missteps, and creating greater trust, and reducing the barriers to learning.

The second one is more to have the sorts of people who can take the body of knowledge that's developed in research and practice and provide it to those who are having to make these decisions, but doing so in a setting where they are able to attend to the full suite of problems, including those having to do with security.

That's the gist of what I have to say. I, again, apologize for not being there to do it in person. I'm happy to take any questions you might have.

MEMBER WEINER: Start with the committee.

Jim, Bill, Mike? Allen, do you have --

VICE CHAIRMAN CROFF: No, it's all your's.

MEMBER WEINER: All right. Then I guess it is.

I have some, Hank, as you might expect. First of all, to what extent have you, or has anyone that you work with actually studied committees that combine or that have a breadth of membership? And in order that this not appear to be a leading question, I'll tell you what the background of the question is.

I've served on a number of such committees, as I believe we all have. And I do not observe that it increases either the appropriate, what we could consider an appropriate perception of estimated risk or an appropriate perception of perceived risk. I don't think the communication, to put it bluntly, it's been my observation, which is in no way statistical, that all too often, the wanted communication does not happen. And I'd like to know if you have studied such committees, if you have any examples of where it does happen?

MR. JENKINS-SMITH: Yes. There's been quite a bit of work on trying to understand small group development of perspective. There's a particularly good scholar at the University of

Washington named John Gastil, who's written quite a few books on the way that these kinds of conversations can lead to more general understanding.

Now I would also say that if you structure the incentives in ways that sort of misalign the interest of the group, you get serious problems. And it's not easy, it's certainly not guaranteed that you're going to break these things down. But I guess I would use the experience of this NRC committee, the one that wrote the report on transportation of spent fuel, and when we began this communication, there were a lot of tensions. We brought people together who hadn't worked together before, and who were in very disparate kinds of communities. And as the communication progressed, there had to be a lot of trust-building, the belief that people were being heard. It takes work, and I would suggest that in the creation of these committees, one has to attend to the potential problems here, that there needs to be committee chairs or managers of this process that are watching for those kinds of problems and addressing them.

It also means there's got to be some selection on the process of who you put in. I mean, if you put in people whose job or whose perspective is to represent one point of view, and not to listen, and to essentially be gadfly or a naysayer with respect to a policy, then it's very difficult to work with them, so there's both a selection process and a management process associated with it. And no, the committee doesn't imagine that it would be a panacea, as I said. But I think it's a necessary condition, and it's one that's worth working at to make it go.

MEMBER WEINER: I guess my other question is a more philosophical one. In the natural sciences, you can do experiments, and you can repeat them. And if I burn the same, just to take a fire example, if I burn the same quantity of octane under the same conditions, I'm going to get the same temperature, and the fire is going to last the same length of time. Experiments are repeatable, and it's on that basis that the natural sciences do projections. They say well, this has happened every way, this way every single time we've done it, and we've done it independently and so on. That does not happen in the social sciences.

It's very difficult to predict on the basis of behavior, not only look at what happened with the waste isolation follow-up project. Everyone would have thought that the transportation of waste to the WIP was going to end civilization as we know it, until the WIP actually opened, and now no one pays very much attention to those trucks at all. In fact, you've met with them in some very heavy traffic routes, and you get more members of the media than you do protestors. You get one or two protestors and a dozen TV stations. How do you reconcile that? How do you use perceived risk, social risk, if you will, to make predictions that you can have any kind of confidence in?

MR. JENKINS-SMITH: You've said quite a bundle of things there. One was the distinction between the social sciences and what you're calling the natural sciences. I suppose humans are unnatural, but --

MEMBER WEINER: Well, no, works in biology.

MR. JENKINS-SMITH: In general, any science has to struggle with the magnitude of the events and concepts that it intends to explain. And the wonderful thing about much of the natural science is the events, and the particles, and the interactions are nicely bounded, and easily defined. In the social sciences, the closest we get to that, typically, is in economics, where the event is a transaction or a trade, and under those circumstances, there's a very well verified empirical body of literature that is able to make good sense of human behavior.

Psychology comes reasonably close, and psychologists do use experiments in which they are able to control many of the relevant variables, but the limitation that we chiefly come across in addition to the complex kinds of phenomenon we're attempting to deal with is we can't do natural experiments with much of this. We have to use statistical controls rather than experimental controls, and often we can't control for all of the relevant variables, so it does leave us with greater uncertainty.

Within risk perception, however, there are some aspects that are pretty well understood, that have been measured over quite a range of different circumstances. And in general terms, the kinds of phenomenon extend well beyond the nuclear risk to other kinds of hazards, and one can draw on a body of knowledge that's developed with respect to those. I guess I would challenge you a little bit on your perception of what is known and can be known, and understanding human behavior generally, and risk more specifically.

With respect to your other point about the

WIP case, now you have to disentangle propositions that are made for political purposes from those that are made for social science purposes. And I started out talking about the way that risk works in context, and if you understand risk in terms of what is the expected loss, versus what is the expected gain, or in terms of prospected area, I think the case of WIP makes good sense.

Before the shipment actually began, people were facing a prospective loss, however small they might have feared it to be, or however much uncertainty they took into the calculation. Once the trucks were actually moving, the status quo had changed, and the way the people understood the risk changed.

It's funny, this kind of a proposition has been around for a long time. It's often been called the bow wave effect, that with the onset of a new technology, there's an initial hurdle that has to do with prospective loss, that you're adjusting the status quo. Once you're passed it, it looks, kind of looking back, it looks ridiculous that people were so concerned. This is something that we've seen with fluoridation, with compressed steam, and with all kinds of technologies over time. And so I'm not so sure, as you are, that it's a counter-instance to explanatory power.

It is the case, however, that when you listen to all of the propositions made before an event, you're going to hear all kinds of strange stuff. I think it's incumbent on us to ask quite carefully what is the empirical and theoretical basis for the claim before we treat it as if it was a scientific proposition. But I think that sort of gets at my general answer to your question. We could go on at length on this, especially if you gave me a beer.

MEMBER WEINER: A final question - what advice beyond broadening the makeup of these committees did you have? What advice would you have for a regulatory agency that is dealing with social risks?

MR. JENKINS-SMITH: I would encourage regulatory agencies that are dealing with these kinds of problems to actually have staff positions; in other words, people who are part of the agency who are trained in those areas, who actually have studied these kinds of social risks, and engaged in that kind of research.

The difficulty that agencies have typically when they bring in people who chiefly have engineering and other technical backgrounds is that it's hard to make judgments about what counts as theory and what's just a claim. I think one of the bad raps that social science gets is it's so entangled in politics that people who haven't formally studied it, don't really have the ability to differentiate the kinds of problems that are before them.

I also think that having people on the inside of agencies who understand that language and the concerns that are associated with it would permit much more productive conversation between that agency and affected communities, and also with elected leaders.

MEMBER WEINER: Thanks. Does anybody want to add anything? Kevin is looking as if he does.

MR. CROWLEY: This is Kevin Crowley. I would like to add to Hank's excellent comments. As he was sitting here talking about these issues, I was smiling because I was thinking about all of the discussions that went on within the committee. We had 15-16 individuals of whom three were social scientists, hard core social scientists, and the others were pretty hard core technical people, and then some people who kind of straddled the area. Neal Lane was a good example of a straddler, even though he's a physicist by training, through his government and policy work, he's been exposed to the social side, and I think is very intuitive on a lot of these issues.

But there was an initial time during the committee's meetings - we spent a lot of time just breaking down the barriers between the technical people and the social science people - and it wasn't just learning the language, but that was a part of it, because social scientists are very precise in their use of terms, and the technical people tend to be a little sloppy in their use of a lot of social science terms. I think there's a sense that gee, we're all people, so we all understand the stuff.

CHAIRMAN RYAN: Full scale/half scale.

MR. CROWLEY: The point I wanted to make was this. I think that when we really began to make progress was not only when we learned to speak each other's language, but particularly on the side of the technical, what I'm calling the technical experts, although social -- we never found good terminology to differentiate these two groups, values, technical and social science.

There was an acceptance on the part, I think, of the majority of the technical people that this was important, and it was legitimate. There's a tendency, and I see it all of the time, without referring to any particular committee, but within some of our committees that have both technical and social science, and also out in the world when you get into a meeting and you have a social scientist standing up and speaking to a group of technical people, there's a tendency to dismiss a lot of that stuff. And when we really began to make progress in the committee was when there was a recognition and an acceptance that, you know, this is important. We may not understand it very well, but this is a hurdle that we have to get over.

And I think where the committee came out on a lot of its recommendations, particularly in this area, is that this is not a problem you can solve. It's a problem that you try to learn more about, and it's a problem that you work with the affected people to try and manage. And I think the term "manage" here is key. And that's why you're seeing in the report the recommendation for expanding these advisory committees, or augmenting existing committees so that you bring people on who can help in this understanding and management.

MEMBER WEINER: Thank you.

MEMBER CLARKE: Excuse me, Ruth.

MEMBER WEINER: Yes.

MEMBER CLARKE: Could I make a comment?

MEMBER WEINER: Please.

MEMBER CLARKE: As I was listening to Hank's answer, I was also reminded of one of those committees that Allen and I were on, the management of legacy waste sites, the first stewardship committee. And we had social scientists, and one in particular, and there are barriers, and I think many of those barriers were broken down through those conversations that you have outlined, so I think that is a good suggestion.

MEMBER WEINER: We have not discussed route selection. Oh, we have another comment.

MR. RULAND: Yes. Bill Ruland, again, the Spent Fuel Project Office. We've read the report and, of course, we scratched our heads a little bit when we saw that social risk discussion. As you can see, the two recommendations don't directly effect the NRC. But what we can do, and we have been doing is really we have an extensive outreach program in the Spent Fuel Project Office to talk to groups that are affected by transportation. You've all met Earl before, I know, and we devote a significant portion of his time to go out to these groups to listen to their viewpoints, and to try to bring back some of what we hear. And it's something we've done up to this point, and we continue to take a serious look about what we do in this area.

And, in particular, we're right now considering a contract where we kind of try to help ask SANDIA, not necessarily a group of social scientists, to help us come up with some demonstration aides so we can kind of demonstrate and try to educate folks. At this point, that's what we see our role in this area is. Yes, we continue to kind of think about it, but as the committee has learned, it is important. It's part of really the fabric of our office, and I believe it's the fabric of the NRC at-large, to really listen to the number of publics we have out there. Thank you.

MEMBER WEINER: Joe, would you like to comment, moving to another topic, unless somebody else wants to make a comment about social risk. Hearing none, would you like to give us a brief rundown on what you did with routing?

MR. MORRIS: Actually, Kevin's presentation did cover the key conclusions and recommendations on routing, and I'll just remind you what Kevin said. The committee had two distinct tasks; one was the broad task of looking at the technical and social concerns connected with potential future large-scale shipments, mainly of commercial spent fuel. And the second task was a specific Congressionally mandated task to look at how the Department of Energy made routing decisions for shipments of research reactor spent fuel in the United States. And this has to do with most of the shipments that have taken place of research -- the largest quantity in the last decade, at least, has been foreign research reactor spent fuel that's repatriated to the U.S., and comes in mostly Charleston, South Carolina, and goes to DOE facilities in Nevada or in South Carolina. But there are other flows of research reactor fuel, as well, including fuel from university reactors, which is not the direct responsibility of DOE, because the universities here are NRC licensees, and also shipments from DOE reactors.

The committee's understanding was that this study charge came out of a specific -- primarily was the result of a specific controversy over a specific one of these shipments of some foreign research reactor fuel from the Savannah River facility in South Carolina by truck to Idaho in 2001, that the

State of Missouri objected to, and attempted to stop, so that was the background of it.

The committee's conclusions have to do, first of all, with DOE procedures, which the committee thought were reasonable and sufficient for planning the routes for these shipments. And also, with the DOT regulations. DOE is responsible for the management of certain of these shipments, DOT writes regulations that govern highway shipments of high level radioactive waste and spent fuel. And the committee concluded in that regard that DOT's highway routing regulations, which largely govern the way the Department of Energy routed that 2001 shipment that went through Missouri were also reasonable regulations if they were followed and practiced in the way that their framers intended them to be.

Beyond that the committee, I believe, saw that experience as very useful historical experience of an actual ongoing program involving the ongoing shipment on a routine basis of spent nuclear fuel on an orders of magnitude smaller scale than what would be involved in disposing of all the commercial reactor fuel in the country; but, nonetheless, a good example of how DOE confronted and overcame some of these problems having to do with routing and other aspects of management of the program. And the committee highlights the routing practices, in particular, the formal procedures for consultation with states, and with tribal organizations along the routes as a worthwhile example that is applicable, keeping in mind the differences in scale that greatly complicate the problem, but is applicable as an example for procedures that DOE can follow in larger scale programs.

Beyond that, I think if there are questions about the --

CHAIRMAN RYAN: Just a little piece of anecdotal experience that picks up on Professor Jenkins-Smith's comments. As a resident of Charleston, South Carolina for many years, I can attest to the pattern of lots of press, lots of interest about the first shipment of reactor fuel, research reactor fuel, then it went away. The first MOX fuel shipment that went to Duke that came in through Charleston. Charleston is an interesting entry point for lots of things. It's I think the third-largest seaport on the east coast for material coming into the United States, but it was very interesting, I think, to me, and it's interesting to hear that there really is a social science pattern to that, that the, let me call it uproar, for lack of a better word, or attention that it got, was very intense, very short-lived, and then non-existent. So shipments come and go, and I'm sure there have been dozens of them over time, or hundreds maybe.

MR. MORRIS: Hundreds. On the order of hundreds, I believe.

CHAIRMAN RYAN: Hundreds. And it was only the first one or two that kind of got the attention, and everything has been running like clockwork, as far as I can tell, ever since. It's interesting.

MR. MORRIS: That was an instance where the specific circumstances, the specific political circumstances are unique to every case, but that was an instance where, in fact, the states and DOE, and others involved; for example, the railroads, did realize that they were compelled to sit down and work out some understandings, which were then put into writing in transportation plans that govern that activity, in a sense.

CHAIRMAN RYAN: And I guess that leads me to the question, I wonder if it would be helpful to collect as many of these kind of case studies, as possible, and see if there's some other pattern that might better inform trying to get the process going earlier rather than later, thinking of a new activity along these lines. Because, I mean, there are lots of them, there's not just the Savannah River, Charleston. There are lots and lots of shipments that have occurred. We've heard about lots of hazardous waste or hazardous material, or radioactive material kinds of shipments. It would be interesting to see if we can tease out some patterns that might be informative. And again, I'm thinking more broadly, just the social aspect as one, and also the government affairs aspect. I mean, those tend to be pretty complicated puzzles to sort out. And Lord knows, South Carolina's politics are relatively unique in many ways.

MR. MORRIS: I believe that, and maybe Hank Jenkins-Smith can comment on this, but I believe that some of the social science research on these phenomena that have to do with public reactions to radioactive waste shipments were situated on the experience of these foreign research reactor shipments.

MR. JENKINS-SMITH: Yes. A great deal of the initial work was based on that and, of course, on WIP. But the interesting finding that is still out now on the South Carolina case was that in part, if you recall, when that started up, the state sued the federal government to stop the shipments, and the boats bringing the spent fuel over to the United States had to circle around in the ocean for a while as the court sorted out what it was going to do. And when the shipments were about to arrive, the *Charleston Courier* had a front page story with a line in red showing where the route was going to go alongside the governor's rather extraordinary statements about the likely affect on the health of the citizens when it did. And that event is a classic instance of both the expectations of loss in the initial case, and the mobilization of interest in trying to define the risk. And we've learned an awful lot from that, and it's still being studied, in fact.

CHAIRMAN RYAN: Thanks.

MEMBER WEINER: We have another comment. Judith, would you introduce yourself.

MS. HOLM: With your approval, I'm Judith Holm. I'm from the Civilian Waste Program in the Department of Energy, and we are a co-sponsor of the report, and I thank the committee again, and Kevin for all the work they did. Our feeling is that yes, we agree, there is a good system out there. We have been working within that system, and I wanted just for this committee's information, we welcome NRC's support, and

Earl and I have worked together a long time. The groups that he has been providing information to about the regulatory structure and NRC's role are the four state regional groups that we sponsor, the Transportation External Coordination Working Group is a group that our office sponsors and has managed for some 10 to 15 years now.

We agree that it's important to have the dialogue between technical and social science, and policy people. Don't forget the policy people, they're critical to this discussion, so I wanted to welcome any of you who are interested to attend any of these regional group meetings, of the tech group that meets twice a year. Come and see what we're doing. We do have a number of activities in committee underway addressing routing, and establishment of criteria, as you've suggested, working out the emergency preparedness funding system. We're hopeful that we'll be able to publish policy on that fairly soon, so we take your suggestions seriously, and are working through some of those suggestions right now in DOE. Thank you.

MEMBER WEINER: Thank you. Does any member of the committee, do you all have any further items you want to comment on?

MR. CROWLEY: You've mentioned emergency response service times, and I was prepared to just make a few comments about that, if you'd like.

MEMBER WEINER: Please.

MR. CROWLEY: Okay. Allen, did you have a --

VICE CHAIRMAN CROFF: No, go ahead.

MR. CROWLEY: Okay. All right. Well, you may remember that the recommendation, again, was DOE should begin immediately to execute its emergency responder preparedness responsibilities. And the committee recommended four particular steps that DOE might want it to take. And before I mention those, let me say that I think there was a lot -- again, this was one of those issues that got a lot of internal discussion during the committee meetings, and we had a committee member, Lacy Suiter, who was an Emergency Management Professional. He ran the Tennessee FEMA for 12 years, and then was in a high level position in the U.S. FEMA before he retired, and he still does a lot of consulting work, so he understands this area very well. And I think it was his sense that DOE is really missing an opportunity here, because emergency responders tend to be -- they're highly thought of within communities, they tend to be ambassadors to communities, and when a public official or a member of the community says - they hear about the spent fuel and high level waste shipments coming through the community and they say, is this safe? Can we handle this? Having emergency responders who are trained to understand what's going out and can respond to the political leader or to the citizen, yes, this is okay, we can handle this. There's a real advantage to that. So in that spirit, the committee made four explicit recommendations for steps that DOE could take.

The first is to establish a cadre of trained emergency responders and to do this early, and to focus on the long-term professionals. Over 75 percent of fire departments are either volunteer or part-paid fire departments, and there's a lot of turnover. So what you want to do is you want to focus on the people within those fire departments who have been around for a while and are likely to be around, and in the very early stages, focus on train the trainer activities, and try to get some input to your planning process. And I know some of that is already going on within the TEC.

The second recommendation was to work with DHS to provide consolidated all-hazards training. Under Section 180©) of the Nuclear Waste Policy Act, DOE is responsible for providing financial support for emergency responder training. The committee felt that that support could go a whole lot farther, and be a lot more effective if emergency responders received training for spent fuel and high level waste emergency response as part of the other types of hazard training that they get, so that was the second one.

The third recommendation was to include trained emergency responders on escort teams. The committee noted that this would be a little easier to do if you were transporting material by rail, and particularly by dedicated train. But the thought there was, again, part of it is making your limited resources go farther, but the other part of it was, these people can establish liaisons with the emergency response organizations in the communities through which these shipments pass, so that you are, again, developing the good ties that you want to have to make the program success. And also, if there is an accident or an incident, that the emergency responder on the shipment is there as a resource to the local incident commander.

And then finally, the fourth recommendation was to use emergency response preparedness as an outreach to communicate more broadly with communities about transportation plans and programs. And the idea, a couple of suggestions there the committee made were, for example, establishing a website where communities who would see these materials coming through could find out about DOE's emergency response programs, and about preparedness in their communities. And also, perhaps, even getting school children involved in, for example, making environmental measurements along sites. So the committee felt that there was a lot that DOE could be doing and wasn't doing to take advantage of the outreach opportunities in the emergency response area.

CHAIRMAN RYAN: One follow-up question, Kevin. That all sounds great, and I know a lot of that goes on, but delivery in an emergency is a whole different matter. And in that transportation accident, under the authority of the governor in the state in which it occurs, the governor has authority over what happens in the state. And if he wants somebody else's help, he has to ask, he has to ask the Feds and whoever it might be.

MR. CROWLEY: Well, initially the --

CHAIRMAN RYAN: Let me finish my question. So what I'm thinking about is how do you develop a plan - this is a general question - it's not specifically about the governor's authority. How do you develop a plan of implementation to go along with your plan of training, because without both, I think you're wasting your money on either side by itself.

MR. CROWLEY: When you say "a plan of implementation" what are you --

CHAIRMAN RYAN: How are you going to roll it out? I mean, we're going to train a bunch of firemen, pick a town, in Washington, Bethesda, and we're going to get the first responders, who are firefighters. We are going to get emergency medical people, and others that might be potentially involved in a response, police, the whole works. So we train them all, and now there's a rail accident, and the railroad emergency response team, who owns the track, says we're in charge. And, of course, firemen -- how do you deal with the fact that when you have a response to a significant event, that you really have to understand the hierarchy and have some integrated plan on how you're going to make all those pieces fit in reality, and exercise that in some way. Having been involved in a few transportation emergency exercises over the years, that's the number one issue. It's not how do we get the equipment there and break out the radiation detection gear and all the rest.

It's who's going to give us authority to go measure something.

MR. CROWLEY: Well, if Lacy were here, I'm sure he could give you a very detailed response to that.

CHAIRMAN RYAN: But does the report go into that, I guess is my question.

MR. CROWLEY: The report recognizes that there is a system in place for response to accidents. If it were, for example, a rail accident, the rail operator does have a certain responsibility, but there would be an incident commander, and the incident commander would be appointed. It would be the county official or the city official, depending on what --

CHAIRMAN RYAN: Or the state. It could be a state official.

MR. CROWLEY: Well, if it were a significant emergency, the state might come in.

CHAIRMAN RYAN: Well, in some counties they don't have resources. I can name several --

MR. CROWLEY: But, again, there's a different system within each state.

CHAIRMAN RYAN: That's my point.

MR. CROWLEY: And usually, there's a memorandum of understanding among the localities in the state that guide that.

CHAIRMAN RYAN: Good training without an implementation plan doesn't go very far. That's my point.

MR. CROWLEY: I don't think the committee would disagree with that.

CHAIRMAN RYAN: But I think that's something that we ought to take to heart, because how you train and who you train doesn't really come from an external view of who should be trained. It comes from how does that system work politically and socially, and who are the decision makers and the responders. Those are the people you train.

MR. MORRIS: One point that is in the report that's a little bit related to that concern is that the way to approach preparing the emergency responders for these kinds of incidents is to integrate that training and preparation in with broader -

MR. CROWLEY: All-hazards training.

MR. MORRIS: -- all-hazards emergency response training, rather than to see it as a separate activity.

CHAIRMAN RYAN: The other thing that maybe it's a component you thought about, or maybe it's one should be thought about, is typically, the owners of transport units, casks, have emergency response teams that can be on a plane and get anywhere in the United States in relatively short order, typically under four or five hours, because their asset is at risk, so I think integrating real experts on the equipment that might be involved, whether it's a railcar, a rail cask, or a truck, or whatever it might be, is a dimension that people ought to think about, too.

MR. CROWLEY: There has been a lot of that thinking done. For example, I know that there's a memorandum of understanding among nuclear power plant operators, if you have an accident close to a plant, that operator can be called on for technical assistance. I think a lot of those are in place. Now whether or not they function as they're intended to function is another question. But DOE doesn't have responsibility for the implementation side, they have a responsibility for the training side. They do exercises in order to try to exercise that to make sure that everybody's on the same page.

CHAIRMAN RYAN: But there's a national RAP program, Radiological Assistant Program, and they do respond, and there's Broken Arrow responses.

MR. CROWLEY: That's right, there are

several levels of response.

CHAIRMAN RYAN: So DOE does respond to that RAP program.

MEMBER WEINER: Allen, you had a question?

VICE CHAIRMAN CROFF: Yes. I had a question on the routing business. Did the committee form any insights as to the adequacy of the databases that drive these routing analyses; in particular, population densities along the various routes, and whether they're adequate?

MR. CROWLEY: Joe, you should take that, because that came up explicitly in the research reactor routing study, and I'm thinking there the broad risk assessment that was done.

MR. MORRIS: Yes. The committee did not review the models in detail, but certainly their understanding of it was that the models - well, DOE used the highway and interline routing model in the RAD TRAN Risk Estimates in the EISs. The EIS's conclusion was, essentially, that this trans -- EIS is for the research reactor spent fuel shipments. The EIS's conclusions were basically, this is a very safe activity. They didn't quite put -- I forget exactly what the words were. It's quoted in there, but basically that the risk en route don't rise to the level of where this is a major concern, is basically where it ended up. EIS didn't attempt to use those models for comparing routes, and this was kind of part of the problem with down the road when the State of Missouri and the State of California started objecting, how do you know that our root is really safer than the route through the State of Nebraska?

When DOE started working with the states in these working group to pick the routes, the first thing they did was apply the DOT regulations, which are very cut and dry. And DOT regulations say basically, you go by an interstate, you go by the shortest route, you go by the shortest route from your point of origin to the point on the interstate that will take you where you're going, and the states can write exceptions to that, the states can make specific exceptions to that. So if you apply all those rules - and these regulations go back to the 70s, I believe, and DOT when they enacted the regulations did not require any detailed route-by-route comparisons - so when you apply all the rules, you come up with a route basically without going through detailed risk assessments of alternative routes. And so the committee's recommendation or conclusion was that that's a reasonable process, even though it does not involve detailed route-by-route comparisons.

Now the report goes one step farther than that, and it says that in some of this more microscopic examination of the routes, it seems like they were making decisions that could have been informed by data had it been available, but which were not, because the data were not available. And just one example was that it seemed like the states had a preference for routes where emergency responders were already trained, because that would mean they wouldn't have to train new emergency responders on some other route. But the implication of that is to tend to bias routes, or might be to tend to bias routes toward high population areas because that's where emergency responders are most likely to have training.

Well, there's no model that I'm aware of, or that seems to have been applied in these situations for analyzing those kinds of choices. Now maybe you could have, or simply other tradeoffs had to -- routes were avoided because they went through mountains - train, for example. Again, was any specific analysis done in that kind of a microscopic setting, and I don't know of any, so that's a long-winded answer. But the models that exist were not used for route-by-route comparisons of risk, because that's not what the regulations call for. The regulations shortcut that. The report says there are worse situations where there should have been more analytical power brought to bear on the problem and it wasn't, and probably tools ought to be developed for that and they don't exist.

MEMBER WEINER: Just to expand on that, that was partly a result of the tools available at the time. There has been considerable expansion of the applicability of these tools, and the incorporation of the sort of data that you're talking about. One big lack, which has always existed, is that none of the tools used incorporate things like mountainous terrain. That's always a decision that you sort of make on the ground, so to speak. But if we were to redo that foreign fuel research analysis, you would see a much finer division, and it could be used for route-by-route comparison. In fact, DOE does a great deal of that right now for all kinds of things, not just spent fuel. It's something that is very easy to do.

I was going to ask about emergency response. Since most of the transportation of spent fuel will not take place for more than five years, probably ten, and since most of the emergency responders, which is, as Joe has noted, are not in the urban areas. Most of the routes and most of the emergency responders are volunteer fire departments, and the personnel tends to change over on the more than annual basis, are you suggesting a continuous program of training, or is there some optimum time at which the training should begin? I mean, looking at the fact that there are not infinite resources available for this, did you discuss that in the report?

MR. CROWLEY: Well, there was long discussion about that within the committee, and there is discussion about that in the report. The committee heard from several individuals during the study that it's too soon to start training, and I think Lacy, in particular, but also some of our other committee members just didn't buy that argument. They said there are some things you can do very early, and one of the things that they highlighted that you could do very early would be to identify the permanent members of those departments. While there is a lot of turnover, there are also a cadre of people who are there for a long time, and you can focus your initial efforts on those people.

The committee was not advocating a large-scale training program of people who weren't going to be there when you were ready to ship the fuel, but I would also point out that the committee also said one of the other things you should do is you should work with DHS to provide training as part of all-hazards training. This is training that everybody gets, and so DOE doesn't have to establish these individual training programs or provide aide to states to establish individual training programs. You make it as part of training the firefighters and other emergency responders get just as a matter of course.

MEMBER WEINER: Are there any further comments from anyone in the room? Hearing none, I'm going to turn - oh, sorry. Excuse me. How could I neglect you, John?

MR. KESSLER: John Kessler, Electric Power Research Institute. EPRI also co-funded the study and I want to thank the NES panel for a nice discussion of the issues. It certainly was a tough task. There's a couple of gems in there I really like. One, for example, is the description of the drop test onto an unyielding surface, the combination of the two.

I think that what confused me in the end all boils down to this issue of risk, and definitely have to put it in quotes, at least for a technical guy like me. Lest we technical people get a little too smug, Ruth, I guess I would argue that we certainly deal with a lot of uncertainties on the technical side, as well. And in the end, what's always missing from these arguments, and I think is also missing from this one, is there's only so much money we can spend on this. And in the end, am I supposed to, as a policy maker, spend my money refining the release fraction from a certain kind of fire event, or am I supposed to spend my money on maybe instituting some sort of new external citizens advisory board or something like that?

I guess my uncertainty, pardon me for using that word, was I'm having a hard time understanding the commonalities in the use of "risk", the use of "risk-informed", the use of "bounding", the use of "credible accident scenarios", and how I mix social risk versus what, for lack of a better word, technical risk all together. I guess specifically on the fire one, there were words sprinkled through the report about using risk-informed approaches or risk-based approaches. I've probably got the words slightly wrong there, and yet we come to the fire, it seems as if we're talking about credible accident scenarios, where you're talking about sort of a bounding situation. And just on the technical side, that seems somewhat inconsistent, and I was looking for a clarification, which maybe I just missed when I read the report.

And I guess my last feeling about the risk is that well, I keep thinking we talk about social risks, and are we really talking about risks, or maybe it's on the technical side that we don't know what risk means. And I keep thinking that we bandy these terms around just to make the other side happy, and in the end, I'm still left with this idea of okay, in the end we, as a society, have to make a decision, are we going to do this, or aren't we going to do it? And if we're going to do it, how are we going to do it? And while I appreciate the need for social risks to be mixed up with the technical risks, the end of the day, I still wasn't quite sure what the NES was recommending regarding how one uses social "risks", and mixing it up with the technical risks.

MEMBER WEINER: Thank you. Hank, if you're still on. Hank? I guess he's not.

MR. CROWLEY: Can I take a crack at that?

MEMBER WEINER: You certainly may, Kevin.

MR. CROWLEY: Well, I think there are a couple of main takeaway messages from the report. The first is that from a technical basis, there really are no barriers to the safe transport of waste. I hope that was the message that came through pretty clearly in the report. And I think the other message that I hope came through pretty clearly is that the radiological risks are generally well understood, and they're low. Those are really the two big messages.

There were a variety of other messages. One of the messages, I think, and again, this was from the social science side, is that there are these social issues that have to be attended to. And that if you ignore them, you ignore them at your peril. And again, we're not talking about solving them, we're talking about understanding them, and working with the affected communities to manage them. And the committee tried to recommend pragmatic ways to do that.

With respect to the very long duration fires, actually, I think what the committee wanted to see happen is what you heard Earl say he was doing, which is to demonstrate a bounding understanding. And one can do that probably without spending a lot of money. In fact, one of the points that the committee made was that the number of additional analyses that might need to be made are pretty small, so I think those are the messages that you want to take away.

MEMBER WEINER: Before calling on John, I have to ask one question. You said, "You ignore the social risks, and ignore them at your peril." Am I quoting you correctly?

MR. CROWLEY: That's what I said, and that's not what is in the report. That is my interpretation of what's in the report.

MEMBER WEINER: Okay. My question to you, for your interpretation, is what peril? I mean, suppose there is a massive objection to something, and to some shipment, as there is, and you do it anyway because you know that the risks are small, if even at all. And it happens, and then it's done, and that's that. And generally speaking, as we have heard, the social risk goes away, as well, so what peril?

MR. CROWLEY: I would hazard to say that you'd probably get disagreement with Hank on that. If you manage the social risks well, and you run the program well, they're manageable. I think that's what he would say.

The other point I would make about that is that the committee sensed, and you'll see this in the report, that sustained implementation of large quantity shipping programs was likely to be a challenge from a social point of view. And it's not the shipment that you make here and there, it's these large programs that are going to be shipping for long periods of time, where you really have to attend to these issues.

MEMBER WEINER: Yes. I recognize the committee made a big point about consistently seeing to it that regulations were abided by. John, did you have a further -- hearing no further comment, I'll turn it back to the chair.

CHAIRMAN RYAN: Thanks. And that's an excellent discussion. I'm glad we had lots of participation and comments from your sponsors, as well as comments from you about your report. And thanks, Ruth, for leading us through a good discussion.

We're at a break in the agenda a few minutes early, so in order to conserve the later hours of the evening for other activities for us and you, why don't we just take a -- come back a few minutes early, at say 25 minutes to 5, and we'll start a little bit early. Instead of 4:45, we'll make it 4:35, and see if we can get a little bit ahead of the game on our next presentation on NORM radioactive material.

(Whereupon, the proceedings went off the record at 4:22 p.m., and went back on the record at

4:37 p.m.)

CHAIRMAN RYAN: All right. If we could go back on the record please? Without further ado, Lydia Chang is here to talk to us about natural and accelerator-produced radioactive material rulemaking and how the formerly unincorporated radioactive material will now be incorporated into 10 CFR.

So, Lydia, welcome. And we are pleased to have you with us today. Thank you.

MS. CHANG: Thank you. Again, my name is Lydia Chang. I'm with the Rulemaking Guidance Branch within NMSS.

And today I'm just going to kind of give you an overview of the normal rulemaking efforts. First I will briefly touch upon the Energy Policy Act of 2005, the waiver that we issued, and the approach that we took and the strategy that we tried to implement. And give you a very high-level summary of the proposed rule and talk a little bit on the implementation consideration, the current schedules, and the next steps.

As you know, the Energy Policy Act of 2005 was signed into law on August 8th within Section 651(e) of the Energy Policy Act amended the definition of byproduct material in the AEA Section 81(e). And also within that section, it amended the definition within an agreement state portion. It also amended AEA Section 81 indicating that this newly-defined byproduct, it is not low-level waste and therefore provides additional disposal options for disposal of such material.

It does require NRC to issue the final regulation within 18 months which is an extremely aggressive schedule. It also allows NRC to grant a time limit waiver so that we can have a smooth transition from NRC authority over to the agreement state.

Specifically, Section 651 amends the definition to include discrete sources of radium-226, material made radioactive by use of product accelerator and also any other discrete source of naturally occurring radioactive material that poses a similar threat in radium.

The material is also limited only to radioactive material that is produced, extracted, or converted after extraction, before, on, or after August 8, 2005. And it is used for commercial and medical research activities.

Since the NRC policy act does allow the Commission to grant a waiver, NRC evaluated the situation, and published such waiver on August 31st in the Federal Register notice. That waiver allows any individuals engaged in activities involving NARM material to continue with their activities and also allows the states to continue to regulate the NARM material.

The waiver is effective through August 7, 2006 for import and export NARM and it is effective through August 7, 2009 for other NARM materials within the Energy Policy Act and also within the waiver, NRC did indicate that we may terminate such waivers sooner. And we are planning to do that.

Our rulemaking approach is to try to cooperate with states. And the way that we tried to cooperate with states is forming working groups that actually had state participation. Not only the agreement state representatives but also non-agreement representatives.

We have four members from the states along with the NRC headquarters and regions working together in developing such proposed rule. We also have a steering committee that has made decisions throughout the way. As you know, there are several issues that we need management decisions on that so the steering committee was involved in that.

We also had two state representatives sitting on the steering committee making the decision along with NRC management.

Another --

CHAIRMAN RYAN: Lydia, just a quick question --

MS. CHANG: Sure.

CHAIRMAN RYAN: -- if I may on states. Do you feel like so far there has been good flow of information from those representatives out to say CRCPD and OAS in all the states?

MS. CHANG: Since we have so many state representatives I think within the working group task force and steering committee, the communication is very well. They also have done some ad hoc communication with them through the working group and also through the task force to collect information from the agreement state on the type of regulations they have, the type of issues that they now have.

They also have communicated with them on compatibility issues and, of course, back in early January when we sent out the draft Federal Register notice, it was distributed to all states. Not just the agreement states but also the non-agreement states. And we did, you know, receive a fair amount of comments from them.

So from that perspective, I think we have pretty good communication with them.

CHAIRMAN RYAN: Great.

MS. CHANG: Whether they are happy or not, that's another story.

CHAIRMAN RYAN: Okay.

MS. CHANG: As I said, you know, we also have an Energy Policy Act Task Force who has helped me in developing some of the integral bases. And they are also the one who is working on positioning issues to make sure, you know, the final rule will be transitioned to the agreement states in an orderly fashion.

We also consulted with stakeholders by having a public meeting back in November of last year. It was a roundtable public meeting with a lot of the communities, especially from the medical field, that participated. We also had other federal agencies such as EPA, DOE, FDA.

We also had small meetings with select federal agencies such as FDA. We had a couple of meetings with them to better understand their process in evaluating power facilities. We also had meetings with DoD, DOT, and EPA in developing our definition of discrete source and also in consulting with them on whether there are any radioisotopes with naturally occurring radioactive material should be considered to be including the byproduct material.

We tried to inform the public as much as we could so we have developed a website within the rule forum to include all the background information within that. So we have the public meeting summaries, transcripts, some background information on the Energy Policy Act. And most recently we also have placed the draft proposed rule on the website.

Our normal rulemaking strategy is very simple. As I have indicated before we started the presentation, radioactive material is radioactive material. So in our minds, the naturally-occurring radioactive material or accelerator-produced radioactive material, they have the similar properties as radioisotopes produced in reactors.

So where I tried to fit into our current existing regulatory framework and, of course, by using the suggested regulation as the model standard as required by the Energy Policy Act, we did look through the SSRs and tried to incorporate anything that is related to NARM into our regulation.

We are proposing to regulate all

radioactive material from production --

CHAIRMAN RYAN: If I may just interrupt you?

MS. CHANG: Sure.

CHAIRMAN RYAN: And this is kind of a clarifying question --

MS. CHANG: Sure.

CHAIRMAN RYAN: -- for some members who may not be familiar that the SSR is the suggested state regulation are what the CRCPD offers to its members but, you know, so they can adopt them.

MS. CHANG: Right.

CHAIRMAN RYAN: And if they do adopt them into their own formatting structure, because usually it is a matter of formatting and numbering the changes, they are, in essence, compatible with NRC requirements. Is that right? Is it far to say the SSRs in and of themselves are pretty much compatible with --

MS. CHANG: Right. Compatible with existing regulations.

CHAIRMAN RYAN: So that is a big, important step that the SSRs, if they are current and states use them, they have really taking a giant hurdle to be in compliance with their agreement state obligation.

MS. CHANG: That is correct.

CHAIRMAN RYAN: Now there may be specific details that have to be addressed or if the state wants to be a little more conservative on some points, they might have a reason to do that. You sometimes get into evaluations of those details.

MS. CHANG: Right. I think lots --

CHAIRMAN RYAN: It is a big hurdle.

MS. CHANG: -- right, I think lots of states do have their own format.

CHAIRMAN RYAN: Right. They do try to include additional stuff to suit their state-specific needs.

CHAIRMAN RYAN: Right.

MS. CHANG: But in general, I would agree with you that the SSR is compatible with the NRC regulations. And I think in the past, of course the relevant section of SSR --

CHAIRMAN RYAN: Sure.

MS. CHANG: -- because the SSR includes a lot more than the AEA materials.

CHAIRMAN RYAN: Fair enough.

MS. CHANG: And I do believe that our state program has reviewed those draft documents prior to them revising it and putting in final SSRs.

CHAIRMAN RYAN: Yes, great. Thank you.

MS. CHANG: We are proposing to regulate all radioactive materials from production accelerators. During the rulemaking process, we actually tried to categorize the accelerators from one that produces material and one that doesn't since the Energy Policy Act does indicate that we are only to regulate the material that is produced for medical, commercial, and research activities. We tried to make that kind of distinction.

And so in here if it produces material, then we will want to regulate both the intentionally-produced material as well as the incidental material.

CHAIRMAN RYAN: Just let me just throw out another quick question. One interesting question about accelerators is, particularly in the medical arena, is that they are getting energetic enough where there is neutron activation in material of construction, you know concrete walls and things like that. Are those materials going to be covered under --

MS. CHANG: If it is PET cyclotron that they are producing radioisotopes then it will be covered from the operational safety perspective.

CHAIRMAN RYAN: I'm thinking of a medical therapy unit where photo neutrons are produced.

MS. CHANG: Yes, I think about linac, from what I understand, we have been talking to several agreement states and their opinion is that the energy level is still not high enough to pose a concern. Usually it is about six MPAs.

And they believe that a lot of the activated materials are short lived. And even the ones that are higher lived, it is still within the system and they do not feel that anything special that needs to be treated.

CHAIRMAN RYAN: But thinking ahead for decommissioning, if you have to tear that building apart --

MS. CHANG: Yes.

CHAIRMAN RYAN: -- there is some level, low level though it might be --

MS. CHANG: Yes.

CHAIRMAN RYAN: -- and I guess what my real question is at this point, that is under your authority under the Energy -- that would be?

MS. CHANG: Once the waste is generated, yes, it will be.

CHAIRMAN RYAN: Okay. So --

MS. CHANG: As probably commercial activity.

CHAIRMAN RYAN: Yes. But I mean there is not a license for radioactive material by activation per se.

MS. CHANG: No.

CHAIRMAN RYAN: But when they go to decommission, they would still have to satisfy --

MS. CHANG: They would still have to dispose of them appropriately --

CHAIRMAN RYAN: Okay. And that may or not be --

MS. CHANG: -- as a radioactive waste.

CHAIRMAN RYAN: Right. And that could very well be a, you know, unimportant quantity or some other kind of determination which --

MS. CHANG: Right. Right. We just don't want you regulating for 20 years when we don't see any significant hazard associated with their operation.

CHAIRMAN RYAN: Fair enough.

MS. CHANG: And my last bullet, of course, is the other side -- if they do not produce any radioactive material then we do not want to regulate the activity on a component.

The draft proposal summary, I just want to give you a very, very high level of the stuff that we have included in the proposed rule. Of course we have amend the existing definition of byproduct material along with some other definitions such as low-level waste.

We also have added a couple definitions. The discrete source definition is required by the act to be included. We also have added some other definitions such as accelerator, accelerator-produced radioactive material, things of that sort.

We also included a radium 226 and accelerator-produced nuclides to all our 10 CFRs such as in Part 30s, Part 33 regulations. We also have added a section on generalized issues specific for radium 226, part that is for items containing radium 226.

We provided some grandfather provisions to recognize certain FDA and state programs and also certain individuals. We also allow for noncommercial distribution among medical use licensees to kind of reduce any impact that might have on the rulemaking.

CHAIRMAN RYAN: Just a couple of comments --

MS. CHANG: Sure.

CHAIRMAN RYAN: -- on those definitions.

And tell me if this is helpful or not. But in my first read of the draft when it first came out, recognizing you were going to give us a presentation, I sort of saved this question.

I think the definitions are probably the critical thing, or one of them. If we get those right, we won't have many problems. And if we get them wrong, we're going to have all sorts of questions and comments and special cases and all that.

MS. CHANG: Right.

CHAIRMAN RYAN: And the real question I ask is how are they risk informed. I struggle with discrete sources in particular and let me tell you why. If it is concentration-based, that is not a measure of risk. Not by itself.

Concentration without quantity doesn't mean anything much.

MS. CHANG: Or without a pathway.

CHAIRMAN RYAN: Well, but really just thinking about the radioactive material itself. If I have a very concentrated source but it is a small quantity, I might be able to put it in my shirt pocket and walk out the door. A static illuminator is one example. The source itself is highly concentrated. But the absolute quantity is small.

MS. CHANG: Yes.

CHAIRMAN RYAN: On the other hand, if I get something that is a little more dilute but it is 100 curies of something, you know, cesium, pick an isotope. You look at something that is pretty substantial in terms of potential for risk for somebody, you know, inadvertently handling it without knowledge or those kinds of things.

So how are we going to with discrete source being something we have got to define, go to the very concentrated but small quantity up to the maybe not so concentrated but specific up to the maybe not so concentrated but significant quantity from a direct exposure or other kind of risk perspective.

MS. CHANG: I don't know whether you have read the description of discreet source we came up with. We really did not go into the specific concentration or quantities. We actually tried to take a look at the intent of the material. And also how it is extracted.

CHAIRMAN RYAN: I guess what I am suggesting is maybe it would be a take away, homework problem for us to maybe look at that specific action and see if we have any additional thoughts or comments that might be helpful. But again, I'm thinking not necessarily to criticize your definition but to think about it from a practical standpoint --

MS. CHANG: Right.

CHAIRMAN RYAN: -- did we see any cases or opportunities for pitfalls where that definition might not work?

MS. CHANG: Yes,

CHAIRMAN RYAN: Or could we change in a small way and make it work for everything? You know those kinds of things.

MS. CHANG: Right. I mean we struggle with the definition a lot, you know.

CHAIRMAN RYAN: Sure, come on down.

Well, thanks for that clarification. I didn't mean to get too far off your presentations.

MS. CHANG: Right. I was just going to read the definition for you.

CHAIRMAN RYAN: Oh, please, yes.

MEMBER HINZE: I just want to ask a follow-up question to where Mike was going. The driver for this is the Energy Policy Act? You are revising your regulations to be consistent?

Do you have flexibility to explore the definition?

MS. CHANG: Yes.

MR. MOORE: I can answer that.

CHAIRMAN RYAN: Just help us again for the record.

MR. MOORE: Sure, I'm Scott Moore. I'm Chief of the Rulemaking Governance Branch in NMSS. And that's the branch that Lydia is in.

To answer your question about do we have flexibility to explore the definition, the statute left it to NRC to actually define discrete. So the answer is yes. We do have flexibility to define discreet.

And discreet applies to radium 226 and it applies to NORM as well. It does not apply to the NARM provisions in the act. But it applies to radium and to NORM. And we have provided the staff with the rule, which is up with the Commission now.

CHAIRMAN RYAN: Okay.

MR. MOORE: So you have the rule. It is with the Commission now. And you can see in the rule what the definition is that it is proposing to the Commission and the rulemaking.

CHAIRMAN RYAN: Okay. So if we maybe took a careful look and, you know, formulated our comments, maybe had a short session to discuss anything we might have identified or the fact that we did or didn't identify anything maybe at our next meeting, would that be possible?

Because what I'm trying to think is how we can give you timely comments recognizing your schedule.

MR. MOORE: The Commission's -- I think timewise, if you are looking at timing, we had originally planned for the Commission to get an SRM back to us, I think, this month. But it is also possible that the Commission may hold off on voting until after a May 15th meeting that they are holding with the staff and with stakeholders from industry.

The OAS CRCPD, the Council on Radionuclides and Radiopharmaceuticals, and also another advisory committee, the Advisory Committee on Medical Use of Isotopes is meeting with the Commission on May 15th. So it is very possible that individual Commissioners may not vote until after May 15th.

So if the ACNW chose to write anything or say anything to the Commission, then you know anything before that May 15th meeting may be timely. But right now the rulemaking package is actually with the Commission and they have it for a vote.

CHAIRMAN RYAN: Okay. Can we get copies of that around tomorrow? Great thank you very much.

Appreciate it, Scott.

MS. CHANG: Yes and the definition of discreet source is actually on page 111 of the proposed rule Federal Register notice. And the discussion, of course, in the supplementary information if you want to see the background on how we developed --

CHAIRMAN RYAN: How is it -- what your definition now -- is it relatively short?

MS. CHANG: It's only three lines.

CHAIRMAN RYAN: Okay.

MS. CHANG: It basically says the source with physical boundaries which is separate and distinct from the radiation present in nature and in which the radionuclides concentration by been increased by human process with the intent that the concentrated material will be used for its radiological property.

CHAIRMAN RYAN: That is interesting. It sort of allows you to take that and then create a range of sources. Or look at a range of risk settings for material that meets that definition. On first blush, that is interesting. It sounds pretty good.

MEMBER HINZE: According to that definition, how well do we monitor those sorts and how do we know where they all are? And so --

MR. MOORE: May I? It is important to remember that the definition for discreet source that is in this rule only applies to radium 226 and also to the NORM provisions. And I don't know, because I came in a moment or two late and I apologize for that, with respect to the NORM sources, I don't know if you have said this already, the staff is not proposing in this rulemaking package that any NORM sources be included in the areas that we pick up under jurisdiction.

There is a provision under the act that allows the NRC to pick up jurisdiction over discreet sources of NORM that have the equivalent risk of discreet sources of radium 226. And while we are including that in the definition, at this time, we don't envision any discreet sources of NORM that have any risks equivalent to discreet sources of radium 226. Essentially there ass a placeholder.

MEMBER HINZE: Is that taking into account the cores from ore bodies and the exploration?

MR. MOORE: It takes into account everything that we know of at this point.

CHAIRMAN RYAN: That we know of? That's fine.

MR. MOORE: You know we don't, we don't,

or aren't aware of, and I think the proposed rule would be an opportunity for the public to comment on it and suggest any, if they believe that there are any. But at this point with respect to what we are aware of as a regulatory agency, we don't believe that with respect to discreet sources of radium 226, there aren't discreet sources of NORM that are of the same risk.

CHAIRMAN RYAN: The interesting part is I think the words were separate

MS. CHANG: Yes.

MR. MOORE: Then the definition.

CHAIRMAN RYAN: Yes. And then the definition. You know when I think about NORM ora NORM that is somehow enhanced, I think about the sewage treatment plant, and exchange resins and filters and, you know, stuff where materials like that accumulates. How would that fill in. Are those discreet sources?

MR. MOORE: Actually the water treatment facilities, we talked about that.

CHAIRMAN RYAN: Yes, we have.

MR. MOORE: And it would not come under this. There is a couple key provisions that they thought of under this, the staff thought of under this when they came up with the definition that is used in definition. The distinct from radiation present in nature, separate and distinct from the radiation present in nature where the radionuclide concentration has bee increased has been increased by human processes with the intent that the concentrated material would be used for its radiological properties.

In the case of the water treatment facilities, you are removing it from the water. But there is no intent that you use it for its radiological properties. It is probably just waste. And so in that case, it wouldn't be a discreet source.

And it is arguable whether it is separate and distinct in some cases. So we believe -- the staff is of the opinion at this point that water filters resin beds and those kinds of things, would not be a discreet source.

MS. CHANG: Oh, and that was --

CHAIRMAN RYAN: But it still would be regulated under the NORM -- the new authority under the -- that you would now have over NORM materials because it is NORM or not?

MR. MOORE: No. Under the Energy Policy Act, we only have authority over discreet sources of NORM that are equivalent in risk to discreet sources of radium 226. And at this time, the staff does not envision any --

CHAIRMAN RYAN: And that's -- and I think we discussed the fact that is a pretty big apple to bite into --

MS. CHANG: Right.

CHAIRMAN RYAN: -- all at once. So I think we understand the strategy is to get this part of the apple digested and then think about other parts maybe later on.

MS. CHANG: Right. And I guess another thing is that the Energy Policy Act did indicate radioactive material used for medical, commercial, and research activities. So if it is not used for those purposes, we feel that it is not under our jurisdiction.

So when we developed the definition with our federal agencies, we intentionally put in for radiological properties you exclude T NORM material. However T NORM would still be regulated by states.

CHAIRMAN RYAN: There are lots of T NORM out there. You look at the oil industry and the phosphate industry, I mean there are very large industry components with T NORM. So --

MS. CHANG: Right.

CHAIRMAN RYAN: -- like I said, that's a big apple to bite into.

MS. CHANG: Right.

CHAIRMAN RYAN: You know it is interesting, too, and I think -- I'm sorry, Derek, but most of the agreement states -- well, most of the states have the authority and integrate management of those materials in part or in whole with their radiological programs and people that are agreement state people. So at least from the agreement states, there is a lot of overlap.

MR. MOORE: That is a very good point. And so there ought not be any lapse in regulation between most of the states and the NRC on most of these.

CHAIRMAN RYAN: And I think it is important for our record to reflect the fact these are not orphan materials that have no care and feeding at the moment. They are materials that are kind of shifting gears from being regulated in a state setting to now being integrated into the NRC setting and then passed back through the agreement state programs.

MS. CHANG: Right.

MR. MOORE: Right. Did I answer your question?

CHAIRMAN RYAN: Yes, Scott.

CHAIRMAN RYAN: Derek?

MR. WIDMAYER: Yes, Derek Widmayer with ACNW staff. Scott and Lydia, this is an area that you are specifically asking the public in the proposed rulemaking --

MS. CHANG: That's correct.

MR. WIDMAYER: -- right -- for feedback as to whether there are any sources.

CHAIRMAN RYAN: We'll take it as an assignment to pay attention to that part in particular and think hard about it.

MS. CHANG: Right. That leads to the other proposals we solicit public comments and input on. There are areas that we do that.

CHAIRMAN RYAN: Okay. Sorry. I didn't mean to get so far off track.

MS. CHANG: Oh, that's fine. No, no, no. Let me just continue. I think we already touched on that. We provide grandfather provisions to recognize the state programs in FDA and also individuals. We also allow for noncommercial distribution.

One thing that to us is very important is the implementation strategy for the proposal. Usually we don't include effective date but here we actually include a section discussing our intention. And we are proposing to have an effective date 60 days from the day of the final rule.

We also --

CHAIRMAN RYAN: Did you get comments on that?

MS. CHANG: Probably. But, you know, as soon as they can provide us the basis why it should be longer --

CHAIRMAN RYAN: Sure, no, I understand.

MS. CHANG: -- I mean we definitely would consider it. Another thing that we have included within the rule is to authorize or allow continued use of the NORM if they comply with our requirements such as reporting requirements, RAS safety requirements. And also we also submit the appropriate documents that we request them to do.

We are allowing the individuals to submit license amendment within six months from the effective date. And also allow them for one year to submit the new license application.

Our transition plan --

CHAIRMAN RYAN: Just a second just so I understand. If somebody has NARM under the definitions, they can submit within six months from the effective date for a waiver termination a license amendment.

MS. CHANG: Yes.

CHAIRMAN RYAN: So they are amending their license to keep the material or not keep the material?

MS. CHANG: To keep the material. They actually already have the material in hand.

CHAIRMAN RYAN: Now why do they have to submit within one year from the effective date of the application and waiver termination?

MS. CHANG: We've got to allow them extra time since if they have, for instance, if they have a PET cyclotron, it might take longer for them to prepare license application.

CHAIRMAN RYAN: Oh, that's for a situation where they have no license now?

MS. CHANG: That is correct.

CHAIRMAN RYAN: Oh, okay.

MS. CHANG: That's correct.

CHAIRMAN RYAN: So there are really two cases. If you have a license and you want to amend it, you get six months. If you don't have a license, you get one year.

MS. CHANG: That's correct.

CHAIRMAN RYAN: Thank you.

MS. CHANG: And we are planning to terminate a waiver sooner rather than later. And we are trying to do it in batchwise so we can have an orderly transition.

The NRC is required to prepare and publish a transition plan to facilitate orderly transition of the regulatory authority for NORM. We are treating the non-agreement state a little bit differently than agreement state within the Energy Policy Act. The governor from the agreement state can actually submit a certification that their program is adequate. Then we can automatically fold the byproduct material into their agreement. And then they can, of course, continue to regulate.

For non-agreement states, it will be a little bit interesting depending on their intention whether they want to become an agreement or not. We might want to make some judgment calls on how to batch different non-agreement states on when we want to terminate the waiver.

CHAIRMAN RYAN: Oh, that's interesting.

MS. CHANG: Yes.

CHAIRMAN RYAN: Is there a wide variety of materials that will come under this state by state?

MS. CHANG: Not the materials but you have a different level of regulatory authority within the non-agreement states. Some agreement states have very good programs that almost look like the NRC regulations.

And then you also have some mediocre type that use registration process but they really don't touch a lot on the specifics. And then we also have a few states that have no program whatsoever.

MR. MOORE: Well, there is some variety in the types of materials. For instance, for the radium, there is -- I mean just radium -- just discreet sources of radium 226, there's radium needles, there are radium dials. There is --

MS. CHANG: Antiquities.

MR. MOORE: -- antiquities that are out there that people still have.

CHAIRMAN RYAN: That's a big category.

MS. CHANG: Big category, very low concentrations.

CHAIRMAN RYAN: Yes.

MR. MOORE: Other radium sources.

MS. CHANG: Lining rods. You have a variety of stuff with different concentrations.

CHAIRMAN RYAN: Vasoline glass.

MS. CHANG: What?

CHAIRMAN RYAN: Vasoline glass -- the green glass.

MS. CHANG: Oh, right, right.

MR. MOORE: For PET materials, there are all different kinds of positron emission tomography. And then there is other type of NARM-produced materials that are intentionally produced. And then there is the activated products and the accelerators themselves.

So there's a fair amount of different types of materials. But from a licensing health physics standpoint, for the NARM materials a lot of them have short half-lives. So, you know, we would only be concerned about the longer half-life ones.

CHAIRMAN RYAN: Well, some of those PET scanners are interesting, particularly the ones that produce radionuclides, some of the short-lived ones.

MR. MOORE: Right.

CHAIRMAN RYAN: You know we don't detect it so it's not a problem. That doesn't answer the question about dosimetry.

MR. MOORE: Right, right.

CHAIRMAN RYAN: So it is interesting to -- I mean there will be some challenges, I think, as people think more and more about those.

MR. MOORE: There will be some huge health physics challenges. The doses from some of them are very, very high. Yes.

CHAIRMAN RYAN: Not just to the patient but to the --

MS. CHANG: But to the workers, especially for the extremities when they do extraction.

CHAIRMAN RYAN: Sure.

MR. MOORE: An interesting issue under the rule itself is that the rule only gives us authority over the materials produced in the accelerators not over the accelerators themselves. So it is a fine point in the statute but it doesn't give us authority, say like we have in nuclear reactors over the whole reactor. It only gives us authority over the accelerator-produced material.

So -- and we have discussed this at length with the agreement states. We have a public meeting in November. But we won't be licensing the operation of accelerators. The states will continue to do that.

We will only be licensing the --

MS. CHANG: Material.

MR. MOORE: -- the material and the use of the material.

MS. CHANG: The use of the material.

MR. MOORE: Right.

CHAIRMAN RYAN: Boy, that's a coordination challenge.

MR. MOORE: Yes, it is. It is. And so a lot of Lydia and the team's effort has been on interacting with the states. A huge amount.

MS. CHANG: Yes. I guess with an agreement state, it is really going to be seamless because they are already regulating it. And for non-agreement states, hopefully it is not a huge issue since they don't have all our programs.

So -- but it is going to be a coordination challenge.

Within the transition plan, the NRC does plan to include a waiver of termination process and the criteria we are planning to use in determining how to terminate the waivers.

The current status and schedule, as I indicated earlier, early January we did send a draft proposed rule to the states and also to the ACMUI for review and comment. Last month, EDO signed a SECY paper and forwarded it to the Commission for a decision.

We also have posted the draft proposed rule on the website early this month. And, of course, the biggest challenge is trying to meet the statute deadline of issuing the final rule on February 7th, 2007.

The next step, we are still waiting for Commission decision. Once we have the SRM, we will revise the proposed rule accordingly. And then publish in the Federal Register for a 45-day comment period. And we are planning to have a public meeting during that public comment period.

That's all I have. Any more questions?

CHAIRMAN RYAN: Oh, that was great, Lydia. We appreciate the exchange as we go along. It helped.

MS. CHANG: Thank you.

CHAIRMAN RYAN: Let's go ahead and go through. Ruth, do you have any questions?

MEMBER WEINER: It's very, very good.

VICE CHAIRMAN CROFF: One curiosity. In your slide, I guess it is eight, provide grandfather provisions. As I read this, it doesn't start before -- but only includes material procured after the policy act itself. So what has to be grandfathered?

MS. CHANG: No actually the regulatory authority goes before that. It is before, on, and after August 8th. And what we are trying to say is that a lot of the agreement states already have problems in existence. For instance, they might have a specific license to allow manufacturers to distribute general license materials or exam distribution products. And we want to recognize that.

And we also want to grandfather individuals such as authorized users who has been working in the medical field for accelerated-produced material, learn to recognize them, so that they con continue to operate as authorized user for those materials.

VICE CHAIRMAN CROFF: Okay. Thanks.

MR. MOORE: The statute actually covers material produced on, before, or after the date of the act.

VICE CHAIRMAN CROFF: Okay.

MS. CHANG: Sort of like retroactive.

CHAIRMAN RYAN: I mean one example I can just -- there used to be in Barnwell at a company that made optical glass that had thorium dioxide in it as an additive for strength. And optical properties.

So they distributed, you know, all this optical glass hither and yon under a general license. And, you know, when it left the manufacturer, it was in essence glass. And that was the end of it. It is those kinds of things I think you are talking about in that category.

MS. CHANG: Right.

CHAIRMAN RYAN: Yes. Well, so it is a real simple story. And all the challenges are easy.

(Laughter.)

MS. CHANG: We wish.

MEMBER HINZE: Does this also include my activated golf balls that go much further? I mean --

CHAIRMAN RYAN: They are irradiated, they are probably not activated. Were they activated?

MR. MOORE: Were they activated in a reactor or in a cultivator?

(Laughter.)

MEMBER HINZE: The committee visited White Shell one time and we were all provided with --

CHAIRMAN RYAN: Jim?

MEMBER CLARKE: Just to clarify one question. One example came up, the water treatment byproducts, resins, or whatever. The answer is they are being used for radiological purposes so they are not covered under this rulemaking. That is kind of an implicit, you know, answer.

Are you contemplating any exemptions or any clarifying guidance that would bring that to the attention of people?

MS. CHANG: The supplementary information actually describes that, you know, what is includes and what is not. So I don't think we need any more clarifications

MR. MOORE: And then separately with respect to the water treatment facilities in the areas of drinking water, the Commission has a paper that they are considering now. And one think that we would consider, depending on where the Commission goes on the NARM drinking water issue is communication directly with the water treatment facilities about whatever direction the Commission decides to take on that.

CHAIRMAN RYAN: And that was the subject of Scott's last presentation.

MR. MOORE: Yes.

MS. CHANG: That is really specific to source material. I mean NRC always has authority over source material.

MEMBER CLARKE: Could I just have one other quick one here?

CHAIRMAN RYAN: Yes?

MEMBER CLARKE: Final rulemaking is -- the deadline is February 2007. The rule becomes effective -- was that the --

MS. CHANG: Sixty days after the February.

MEMBER CLARKE: -- 2009 that the plant blew up? Or when --

CHAIRMAN RYAN: April -- this was tax day.

(Laughter.)

MEMBER CLARKE: Is there a period before which the rule goes into effect after the final rule is issued?

MR. MOORE: It is a complicated role actually. It is effective 60 days after the --

MS. CHANG: The publication.

MR. MOORE: -- the publication. And right now, there are waivers out for everybody -- actually for everybody until August 2009. What we would do is -- what the staff has proposed to the Commission is rescind the w=respect to federal facilities and Indian tribes immediately upon the effective date of the act.

And the reason for that is those facilities, federal facilities in Indiana tribes are self-regulating. They don't have any other body, for instance, a state regulator to oversee them. So they would then have that six month and one-year period to kick in. They would be allowed to continue using the material but have six months to apply for an amendment if they already have an NRC license or a year to apply for a new license.

And then for everybody else in a non-agreement state -- excuse me -- the facilities in agreement states on the date of publication of the transition plan, the agreement states by insurance of the statute, there is a provision in the statute that the governors of those states in the agreement states, can certify that they have an adequate program. And then NRC can review and accept their certification of adequacy.

So if the governor certify on the date of publication of the transition plan, that they have an adequate program. Then the agreement states then become regulating for their states. So the waivers rescind for the agreement states leaving only the non-agreement states left to cover.

And what we would plan to do is then in the intervening period between the effective date of the rule and the termination date of the waivers in phases phase out the waivers, starting with --

CHAIRMAN RYAN: Do I understand when the DOE and Indian tribes part, that they would get a license from --

MR. MOORE: Not DOE. Federal facilities and Indian tribes. Federal facilities being primarily

DoD, VA, you know, EPA. Not DOE. DOE is separated under the Atomic Energy Act.

CHAIRMAN RYAN: Okay. And Indian tribes would be like DoD and FDA and those?

MR. MOORE: Yes.

CHAIRMAN RYAN: Okay, great. Thank you. I just wanted to clarify that.

MEMBER CLARKE: Thank you.

MR. MOORE: Sure.

CHAIRMAN RYAN: Anybody else?

MR. WIDMAYER: I have one question.

CHAIRMAN RYAN: Yes, sir.

MR. WIDMAYER: Could you give us a little background on the compatibility issue controversy with the agreement states and what your feelings are about what kind of impact that might have on your final rule?

MR. MOORE: Yes but first I'd like to point out that you are guilty by association since Derek used to be part of the working group on which this rule was written.

CHAIRMAN RYAN: Well, the good news is now he is going to help us document useful solutions.

(Laughter.)

MR. WIDMAYER: The other thing is I have

three pages of questions and I'm only going to ask you one. So I am off the hook.

MR. MOORE: Okay. Lydia, why don't you --

MS. CHANG: Oh. Why don't you go ahead?

MR. MOORE: Yes, I'll just briefly summarize. With respect to the compatibility issue, with agreement states, the -- and you can see this in the rulemaking package that was provided to the Commission, the agreement states believe that the rule should be compatibility level D. And compatibility level D -- I want to be careful how I quote this -- compatibility level D would allow the states to implement their own programs and we would not review those programs under MPAP and the agreement states would not necessarily be required to make any changes to their programs.

They would just decide if they have an adequate -- or, excuse me, if they have compatible programs themselves and if they need to make any changes, it would be up to the states to decide whether they wanted to make any changes.

CHAIRMAN RYAN: That's the staff's view or their view?

MR. MOORE: That is the agreement states' view. The reason they believe this is attached to the paper they put forward argument and they really should speak for themselves but I guess I would paraphrase it as they believe that NRC's rule should be compatible with their rules because the statute said that the rule that we put forward should comply with the suggested state regs to the extent possible.

They believe that they have far more experience in this area than the NRC does because they have been doing it for years and years. And any other definitions of compatibility may require them to change their statutes which is a difficult thing for the agreement states.

NRC went through its compatibility categorization process, as defined in Management Directive I think 3.9 --

MS. CHANG: 8.9.

MR. MOORE: Pardon me?

MS. CHANG: 8.9.

MR. MOORE: 8.9 -- and came up with a compatibility categorization -- actually it is not compatibility categorization -- a categorization of H&S. If you go through all the compatibility categories and you get down to DE, you have to ask yourself another question. Is it needed for Health and Safety?

And if the answer to that is yes, then you come up with another identification level, an H&S identification level. And so it becomes identified as an H&S. And if you have an H&S, then the states would be required to review their programs to see if they need to make changes to their programs for adequacy purposes. And it would require an action on the state's part to review their programs.

So the states object to any designation other than a category D. And the NRC staff believes that and H&S is the appropriate designation. And I think I will leave it there. And then, you know, you all can read the Commission paper.

We were very careful how we worded it in the Commission paper. I don't want to speak for the agreement states. We let their words speak for themselves in the Commission paper. And I think they would be offended if I spoke for them. So I'm being careful.

CHAIRMAN RYAN: And that's good. I mean I'm glad you are sensitive to their words and we appreciate hearing you quote them in essence. So that's good to hear.

MR. MOORE: I will say they are invited to the May 15th meeting with the Commission. And I'm sure they are going to speak for themselves at that meeting.

CHAIRMAN RYAN: That's great.

You know when I think about it, I try and think of what is the risk-informed approach. You know on the one hand, if it is D, it sounds like they don't have to do anything and nothing changes. So why are we going through this exercise if that is the case. That's just my two-second summary of what a D really is. I'm thinking out loud. Maybe I'm wrong but I don't know.

But if there is going to be a true integration of these materials that you have been asked to regulate under the Energy Act, then maybe it is an evolution over some period of time. It is an awful short schedule to get it done. And I recognize that is not your choice but something you are working toward because it is the requirement of the law.

It leads me to the question have you left in the regulation -- are there enough points of flexibility or placeholders or other things that can evolve over time simply and easily?

MR. MOORE: We think so. We think that there is a framework built into the regulations. But right now we don't even have implementing guidance that is there for it. And so right now it is really just a framework.

And so I believe that it is very flexible at this point. If anything, it could probably be challenged on is there enough proscription out there to know how --

CHAIRMAN RYAN: Well, I mean it is a broad spectrum.--

MR. MOORE: Right.

CHAIRMAN RYAN: -- of new things. So flexibility and having the ability to interpret and evolve over time through guidance is not a bad plan.

MR. MOORE: Right.

CHAIRMAN RYAN: And if it is that flexible, and it has that built in, I mean I think that's reasonable. I mean, you know, we've -- and I think, you know, I've certainly made the point that -- on the low-level waste regulation that, you know, license conditions, permit conditions, and regulatory guidance can cover an awful lot of the landscape by being flexible and adapting to individual states or individual waste streams or material streams or whatever it might be.

MR. MOORE: Right.

CHAIRMAN RYAN: You can certainly do a lot in that area. And if that is built in, then maybe some of that anxiety will diminish with time.

MR. MOORE: I guess I'll let you all know what we said in the Commission paper. The staff's position on H&S is that a designation of H&S for the definition of byproduct material requires the staff to continue to assure that the essential objectives for 11(e)(3) and 11(e)(4) byproduct material -- and that is the NARM material and also the NORM -- the radium -- discreet sources --

CHAIRMAN RYAN: Discreet radium.

MR. MOORE: -- discreet sources of NORM, if there ever will be any, are met. The essential objectives are met. And that assurances obtained by review of the complete set of regs that a state requests in an agreement and the review of newly adopted or amended agreement state regs and the review of the status of an agreement state regs is part of the IMPAT program.

And the staff notes that under a designation of D for such assurance would not be obtained since the program elements designated as D are not a required part of the agreement program. They could be dropped from or not included in the agreement state program. And the program could still be found to be adequate and compatible. And, therefore, not reviewed by NRC staff.

So the staff's position is that it needs to be an H&S. But that is only the staff's position. And the agreement states don't agree with that at all.

CHAIRMAN RYAN: Do you go into further detail what the basis in public health and safety is? Or worker health and safety? And why that is the case?

MR. MOORE: We do in an attachment to the paper.

CHAIRMAN RYAN: I think, you know, to me that's where the rubber meets the road is that if you have made the health and safety case of why you think the review is important and needs to be done -- now you may end up with yes we're adequate or no, we need to tweak. Or we're actually overkill. We could be, you know, anywhere along that range.

But you are saying that it is H&S in order to force the review or have the review be part of the program rather than to change anything specifically that somebody is doing, you are just saying that you need to review it --

MR. MOORE: Right.

CHAIRMAN RYAN: -- with this in mind --

MR. MOORE: Right.

MS. CHANG: Right.

CHAIRMAN RYAN: -- to get there. And now that is H&S. C, of course, and B and A are, you know, go up the line.

MR. MOORE: Are at a much higher level.

CHAIRMAN RYAN: A much higher level of thou shalt.

MR. MOORE: Right.

CHAIRMAN RYAN: As opposed to you may think about --

MR. MOORE: Right.

CHAIRMAN RYAN: -- and, you know, things of that sort. So -- interesting. So that is in an appendix that discussion?

MR. MOORE: Yes.

CHAIRMAN RYAN: Yes, okay. Great.

MR. MOORE: An attachment to the Commission paper.

CHAIRMAN RYAN: Well, we've got -- Latif, hi.

MR. HAMDAN: Can I ask a question?

CHAIRMAN RYAN: Please. Step on up. Have a seat.

MR. HAMDAN: Latif Hamdan, NRC staff.

Just clarifying questions. The regulations for 11(e)(2) and (3) are in Appendix E. So this rulemaking you are doing is separated from Appendix E?

MR. MOORE: Apart from it, yes.

MR. HAMDAN: So the second question is in the discussions here achieved for the rulemaking branch, what are you going to do about Appendix E? If everything has changed and so on, then eventually you will have to change Appendix E, right?

MR. MOORE: Actually, no, we don't believe so. And Lydia or Derek may be able to answer this better than I. But we believe that the waste impacts from this Energy Policy Act rule are very, very minor. So no. We had the division of waste management.

MR. HAMDAN: That is surprising because especially since they are making a new rule on their ISL, which was agreed on their Appendix A and now there is this definition of 11(e)(2) has changed, and, you know, Appendix A is tacked on to Part 40 so you would think one thing you want to do is --

MR. MOORE: 11(e)(2) isn't changing in Part 40. What we are doing is adding on on 11(e)(3) and 11(e)(4). So we are not changing 11(e)(2). We're adding on at 11(e)(3) and 11(e)(4).

CHAIRMAN RYAN: Latif, it sounds to me --

I appreciate your question -- but it sounds to me like that some care has been taken to try and not mess up the many --

MR. WIDMAYER: Yes, that's correct. In fact, the Commission had a large hand in developing the language that went into the Energy Policy Act to make sure that it was adding to the definition and not changing the existing definitions.

CHAIRMAN RYAN: Because we all know there are lots and lots of fingers out from 11(e)(2) to the rest of the Rosetta Stone we have created.

MS. CHANG: That's right. Part 40 is not changed.

MR. HAMDAN: But if nothing else, if the definition of 11(e)(2) has changed --

MR. MOORE: It didn't.

MR. HAMDAN: At least there it needs --

CHAIRMAN RYAN: W@ell, they are saying it hasn't - 11(e)(2) has not changed.

MR. HAMDAN: Yes.

MR. MOORE: No, it is adding 11(e)(3) and 11(e)(4).

MR. HAMDAN: Okay. Thank you.

CHAIRMAN RYAN: Those are different.

MR. HAMDAN: Thank you.

CHAIRMAN RYAN: He doesn't believe us but that is okay. That is interesting.

MR. MOORE: We changed the definition of byproduct material to include this new 11(e)(3) and 11(e)(4).

MS. CHANG: Part 20 and Part 30 but nothing in Part 40. We did not change the definition in Part 40.

CHAIRMAN RYAN: Let's see. So now we have two definitions of byproduct material?

MR. MOORE: We do. And we talked about that for weeks.

MS. CHANG: Actually there are three definitions of byproduct material.

MR. HAMDAN: That is exactly my point.

CHAIRMAN RYAN: Well, you know, you didn't say that. Now what we understand what you were trying to say.

MS. CHANG: We have three definitions of byproduct material. And Part 20 is the one that is all inclusive. It includes 11(e)(1), (2), (3), and (4).

CHAIRMAN RYAN: Right, because that is --

MS. CHANG: In Part 30, it --

CHAIRMAN RYAN: -- the health and safety.

MS. CHANG: -- right, in Part 30 it is only related byproduct material. Therefore we only include the definition of 11(e)(1), (3), and (4).

CHAIRMAN RYAN: Right.

MS. CHANG: And Part 40, since that is related source material, the byproduct material definition only include 11(e)(2) so we actually have three different definitions.

MR. WIDMAYER: The staff was challenged with a notion as to whether they had to fix everything that was broken in all of 10 CFR in order to accommodate this change. And a decision was made early because of the schedule they can't do it. So it adds to the definition.

CHAIRMAN RYAN: Is all this clearly laid out in the package -- the Commission package? Because I'm thinking ahead.

MR. MOORE: I think so.

CHAIRMAN RYAN: You know we are sitting around here and we're, you know, somewhat smart folks and we're trying to struggle through all this. Of course you are experts on it. You have developed it. But it will be interesting to deal with these questions as it rolls out. And I'm sure that will be part of your key challenges, you know, going forth once it is a rule. And helping agreement states implement it. And training and education and details will be really important. Interesting.

Where are we? How are we doing on time?

MR. WIDMAYER: We are way late.

CHAIRMAN RYAN: Any other questions?

Well again folks, thank you for a very informative presentation. We promise we will do our homework and come back with some version of substance for you, either yay, nay, or in the middle. We will talk to you next month. How does that sound? Maybe, Derek, we could take a placeholder of a half hour or so where if we do have feedback, we can work with you on trying to present that to you in a timely way.

MR. WIDMAYER: Okay. Thank you.

CHAIRMAN RYAN: Thank you both very much. We really appreciate it. Good job.

Okay. I think that is the end of our formal presentations today. We're going to consider letter writing. So at this point, Neal, we don't need the recorder any more I don't think. So we'll end the transcript at this point and just take up our discussion of letter writing.

(Whereupon, the above-entitled meeting was concluded at 5:36 p.m.)