I would just wish that they all would do that. It's something that I would like to have seen in the NRC research document. It's that -- what were the assumptions we made about the sensitivities we saw that led us to propose this particular program. Some sections within the dissenters' R & D plan, it was very clear about that. Others, it was less clear, but this idea of establishing criteria by which you judge whether you do work or not, is a really good one and I thought that this assumption about sensitivity was a really good way of beginning at least that process.

Okay, I need to qualify what I say. Igneous activity work looks good, generally. I think that what they're doing for work looks good. That doesn't necessarily mean we agree with their model interpretations. But certainly, they seem to be going after the right issues.

One thing that we are looking at now is some sort of need to consider radionuclide partitioning, tephra particle size distribution, resuspension factors related to the particle size distribution and the radionuclide distribution versus particle size. You know, how much of it really gets ground up and ejected. How far down wind does it go? Is it likely to get into lungs? And are the radionuclides on the particles that will go down wind and into the lungs? It's a lot of question marks out there that will be, I think, pretty critical for establishing a reasonable consequence side of the probability consequence volcanism scenarios.

I'm running through the other parts of their plan on structural deformation and seismicity. It wasn't so clear to me that the Center has established why they think this area is of high importance. For example, in fact, I was just echoing what was just talked about, studying the earthquake probability doesn't seem well justified. It's this idea that well, okay, how high does it have to get and do we really think it would get that high and do you really think that getting the data that you're after is going to help you clarify it to the point where it makes a difference. I don't see that in the plan. Of course, the plan is relatively short and maybe it's justified elsewhere, but I'd like to see that kind of information.

Evolution of the near field environment, the proposed tests look appropriate combining thermal-hydro chemical modeling with the Nopal natural analog work sounds like very good work. Again, I like a lot of the natural analog work that the Center is proposing.

Diffusion from waste form through the EBS would be a value too. If there's anything that they can do to help out poor DOE and their very conservative approach at the moment.

Container life and source term, work on Neptunium solubility and waste form degradation rate, I would argue, is of lesser importance because we don't think diffusion is all that important and when we look at the solubilities for -- at least for the range of Neptunium, it's not all that high and uranium dose is down there farther and so I don't care about it as much. Again, I exaggerate. I know I'm being controversial here, but it's the idea of thinking through how one would prioritize based on what one knows and of course the uncertainties in those models that one needs to think about.

So here's my example for Neptunium 237 sensitivity that's both a solubility and the waste form degradation rate. We looked at combinations for solubility which are below, medium and high and don't ask me exactly what the numbers are. I'll have to look them up for you. And a degradation rate for the waste form between 1,000 and 5,000 years and for the Neptunium range that we thought was reasonable, we get no sensitivity for the full analysis from the total system performance.

So that kind of leads us to believe that gee, maybe for the total performance we don't care as much about getting the Neptunium solubility right, of course, based on all of the assumptions that go into our model.

Repository design in terms of mechanical effects. Certainly, we had agreed that it's important to evaluate DOE's claim that rockfall is a minor issue. If we could get past that, then a lot of these other things would fall out like probabilities of earthquakes, maybe we'd care less about them. Whatever, as long as that's established to be a valid point, that rockfall isn't that big a deal.

Some of the other issues may be of lower priority. I would ask what's unique about Yucca Mountain that requires a special seismic study and I'm thinking of the surface facilities now. There's a lot of seismic work that's been done at all kinds of nuclear facilities and I just want to make sure that what the Center is doing there is unique and it built on what everybody else has done. And maybe I'm misinterpreting what was proposed there.

Thermal effects on flow, again, just

because it's short, the descriptions were so vague that I couldn't really get a handle on whether they were appropriate tests or not.

In their Section 212 on radionuclide transport, the Nopal 1 work seems good, especially if uranium, thorium analyses are included. Yes, I'm being a little inconsistent about asking for those, only because I care more about Thorium actually in the sense of its -- the daughter from Neptunium 237, 229 is pretty important and so getting the Nopal work, they come together, so I would approve it.

The focus on Neptunium and technecium seems appropriate. While technecium for peak dose isn't that important, it could come out early in some scenarios. Where they say certainly we agree with this, that sensitivity analyses and bounding calculations are useful tools to evaluate the DOE approach to radionuclide transport, but the tools must be tempered with independent confirmation of DOE results and more in-depth analyses to avoid undue conservatism or optimism. That certain sounds like the kind of work I'd be wanting them to do.

Sorption seems to be stressed over fracture matrix interaction and I talked about that a bit already.

Here's where we're coming from on this. Where we put in -- this is sorption in the unsaturated zone and saturated zone versus some sort of base case for a wet scenario we call it. Well, we get some effect where we take out sorption, but it's not a huge effect. Again, when we pile it through our model, we don't see a huge increase in peak dose and yeah, there's some earlier arrival, so you could say that yeah, it's sort of important, depending on what you care more about, arrival time or peak dose. Again, be clear about how you prioritize would be something that I would argue as to why you're including something.

Another part of this is we did a sensitivity to fracture matrix interaction and our particular model we talk about block radius. The idea is that you have to get from -- we have sort of a spherical block idea which is the solid matrix and that you've got fractures running around it, so the larger the block, the more dispersed the fractures are and for smaller blocks, the more fracture matrix interaction you're likely to get. So for our base case model where we assume that the block radius is pretty small, we get something out here and for fracture matrix interaction which is much less for a large block radius we can get earlier arrivals. So again, you would say that maybe this is more important, if you're worried about arrival time and maybe you don't care if all you worry about is peak dose.

Knowledge needed for source term and radionuclide transport issues, chemical conditions within the EBS. It was nice to see Budhi's vu-graph up there about attempting to slog through the whole pH question. That's really tough. They're doing a lot with it as is DOE and that certainly should continue as it helps us determine well, gee, does it really affect solubilities or release rates or not?

Role of diffusion within the EBS, I talked about that already. I've talked about really all of these. I'll get back to collating to transport is a lower priority. Again, it gets back to the point that John made earlier. It's like well, even with a lot of sort of maximizing assumptions, it just barely makes it on to the radar scope, so why do I care? Again, I'm trying to be controversial. There may be reasons and I'm oversimplifying, but it's the idea of prioritizing and thinking about these things and writing them down as to why you've decided to fund something is why I'm making these sort of over statements.

Okay, so again for our sensitivity to solubility, where we basically say everything is infinitely soluble. We get a little bit of a factor here, a factor of two or three. And again, because this is due to somewhere down in the dust here, uranium is highly variable, at least in our model in terms of solubility, so when we go to maximum solubility on it, that tends to drive things up. It doesn't really -- it's not Neptunium solubility isn't a big factor here.

And here's the example. For U-238, just looking at one of the uraniums, you can see how much difference there is, so one could say well, gee, you know, I could get a big dose change if I look at just U-238, but because Neptunium is sitting over the top, maybe I don't care so much. But if I got something wrong about Neptunium, maybe I care more about Uranium and again, it gets back to John's point about making sure you've got the other parts right to know where you want to set your priorities.

The recent ACNW letters look good, but I'd sure like to see some more specifics in them. There were some general issues that it would have been nice. There were some examples to clarify what was meant. For example, what is an anticipatory research? I hope we get a chance to talk about it.

MEMBER GARRICK: Be careful here.

MR. KESSLER: I don't really know.

(Laughter.)

CHAIRMAN HORNBERGER: Your presentation has been really good up until now, John.

(Laughter.)

MR. KESSLER: Just shut it off now.

(Laughter.)

Well, put it this way, I'll save air fare the next time because I won't get invited to the meetings next time.

(Laughter.)

I would say that if you're going to say you have a concern about potentially the way research is partitioned between the two, do you have an idea how it might be done better? Maybe you'd want to tell us.

What does ACNW think is the highest priority? I don't really know. Maybe it's not -- maybe you don't feel it's your role, but I didn't get a sense of reading recent letters what you think the highest priority is or maybe I read the wrong ones, I don't know.

Certainly we agree that more realistic

TSPAs are required. I found the evaluation of the TSPAS are a little vague to the point where it sounded probably more damning than you meant it to sound and an example or two of the consequences of not doing this in relation to TSPA-SR probably would have been helpful. And again, calling DOE's TSPA-SR assumption based I think is overstated.

An example or two of what is meant as to how to use the data, didn't they use it, was it really all assumptions based? Okay, give us an example? It would have been helpful there. And I really think it would have been better if you had acknowledged DOE advances since TSPA-SR would have been a little fairer.

Next.

(Slide change.)

That's it.

MEMBER HINZE: Is it appropriate to clap now?

(Laughter.)

CHAIRMAN HORNBERGER: Thanks, John. I'm curious. One question that I have for you is as you've pointed out, what we get to think is important depends upon how we approach a total systems analysis, so that your example is you have fracture matrix being important, so you don't care about the alluvium and NRC doesn't have fracture matrix doing much, so they care about the alluvium.

How are we to know which is right? How do we determine what research is needed in fracture matrix or in alluvium when we can get different answers from --

MR. KESSLER: Well, I'm making a claim based on assumptions that need to be tested. I've discussed with the DOE folks that are managing the saturated zone work that where we're coming from and we've argued rather specifically about going back and doing better Packer tests on these intervals to get an idea of what the flowing intervals really are, maybe doing some more wells. I've said look, I understand why you've picked a certain fracture matrix interaction. You're trying to be conservative because you can't claim that you have any more fracture interaction because of the way you did your Packer tests. Well, maybe it's worth some money to go back and do them if you can get a whole lot more credit for it.

CHAIRMAN HORNBERGER: But how do we decide I do more Packer tests there or put more money into the Nye County wells and the alluvium?

How do I make that decision?

MR. KESSLER: You've have to weigh it against cost and time and when you need to proceed and everything else, as well as what you feel, I think it gets back to John's point again. What decision might you be making that's wrong based on wrong inputs along the way?

So I wouldn't argue you would give up on alluvium research, especially if you right now have a model that doesn't take that much credit for fracture matrix interaction. Of course, you're going to do some sorption in the alluvium, but I would argue that if one of the things you're trying to show to the public is that Yucca Mountain is a good site and it's not just all in the canister, I would think you'd want to spend some more money on showing that some other aspect of the natural system might be of more value.

CHAIRMAN HORNBERGER: Wes?

MR. PATRICK: If I could add something to that, Mr. Chairman. I don't know whether John will agree with this or not. One thing that I found extremely helpful in the risk assessments that have been done so far in the sensitivity studies, both ones we've done and also DOE has done, so much in the early days of those analyses we're doing the one off type of analysis. And that's good, but it only gets you so far because you're always doing a relative comparison and if all my other assumptions are true, what does this contribute.

What I thought was a major contribution was the move toward doing one on analyses because that says what does this contribute and it isn't burdened by what is lost when I take this away, which is burdened by all of those other assumptions. If I do a one on, I start with basically spent fuel in the middle of an open field. What happens when I add sorption? What happens when I add some of these other phenomena. It gives a really different insight, I think.

MEMBER GARRICK: Let me push that a little further and maybe to the maximum. John, in any of your modeling, have you ever just dumped the fuel elements in the drift?

MR. KESSLER: Wait until tomorrow.

(Laughter.)

MEMBER GARRICK: And if you've done that, then what do your solubility curves look like for neptunium?

MR. KESSLER: Can I ask you wait until tomorrow? I'm serious.

MEMBER GARRICK: Yes.

MR. KESSLER: I do show -- in fact, I completely agree with you, Wes. It's how do you want. Tomorrow, I'm going to talk about the criterion that NRC research used that says well, it's an order of magnitude of effect, we'll go after it. Well, how do you measure that order of magnitude effect and where do you get it is what I wanted to go after where I do not only a one off, but a three off, again, trying to be controversial about whether you want to go do some research on cladding or not. And then I do the -- starting with bare spent fuel and then adding barrier after barrier after barrier to try to see its relative contribution is something that I'll talk about tomorrow, again with this idea of where do you see your credit that you're taking and what does that mean about what research you might want to do to support that particular barrier. So I agree with that.

MR. SAGAR: George, I just want to make two brief comments. One is what Wes just said about one off or one in analysis is also true for all sensitivity analyses. You do make assumptions that everything else is correct and this one thing is changing and therefore you should stick with some other understanding before you make decision. You shouldn't make decisions for research based entirely on a sensitivity analysis and one model.

MR. KESSLER: Absolutely.

MR. SAGAR: So the EPRI model may mean one thing and the NRC model something else. That's simply to mean that things are uncertain, and you need to look at which topic to study, probably based on cost and time that is required.

MR. KESSLER: Sure.

MR. SAGAR: The second thing I just wanted to make correct that the CNWR report that you looked at, what we call the operations plans, we are providing technical assistance.

MR. KESSLER: I'm sure it's somewhere else, Budhi. I didn't think you'd be that shallow. It's not like you.

(Laughter.)

MR. SAGAR: But some of the questions may be -- we can answer some of your questions on the side.

MR. KESSLER: Sure.

CHAIRMAN HORNBERGER: Once last quick question.

VICE CHAIRMAN WYMER: Thirty seconds?

CHAIRMAN HORNBERGER: Thirty seconds.

VICE CHAIRMAN WYMER: You'll probably answer this tomorrow too, John, but you should have a dozen or so curves there. To what extent are they dependent upon the alloy 22?

MR. KESSLER: I'll show you tomorrow also.

VICE CHAIRMAN WYMER: Okay.

MR. KESSLER: The short answer is in our model where we try to be more realistic -- and, again, one would have to evaluate whether we're being realistic or optimistic -- but when we try to be more realistic and we completely remove the alloy 22, we're under 15 at all times. We completely remove the alloy 22 and the drip shields and we're under 15 at all times. Finally, when we take out all three -- drip shield, waste package and the cladding -- we get a peak that starts to get over 15.

VICE CHAIRMAN WYMER: Okay. Thanks.

CHAIRMAN HORNBERGER: Thanks very much, John. We're going to move on, because we are on a tight schedule this afternoon. Next presenter is Michael Ryan, CSU, but as you see, it's not Fort Collins, Colorado.

MR. RYAN: I'm from the North Avenue Trade School in Atlanta, Georgia. For those of you that don't know, that's Georgia Tech.

(Laughter.)

I tried to stick hard to the topic that I was offered, which was source turbine modeling issues. Next slide, please. My comments are focused on the four key questions that I extracted from the material: What future information is needed to support NRC decision making, and particularly what knowledge and technical tools are needed, and how can research support the development and what are some of those priorities?

My opinions really come from four points of view. Next slide, please. First, as a scientist interested in a variety of topics surrounding the health physics aspects of waste management and the environmental science aspects as well. Opinions as a licensee. I actually operated a low-level waste site, Barnwell, South Carolina. As an applicant. I was involved in three applications for low-level waste sites, and as a practitioner. And that is -- my definition of practitioner is somebody that has real practical waste management problems to decide on today. I have waste that has to be managed, and we have to move ahead.

So those points of view kind of drive my thinking, and, of course, I'll be slanted much more toward low-level waste thinking than high-level waste, and I find the rest of today's program on high-level waste fascinating, interesting, and I didn't know that there was not that many acronyms I didn't know.

(Laughter.)

One very important starting point to me is the radionuclide inventory. We often take that for granted, and we shouldn't. We have newly generated waste that are generated under 10 CFR 61 rules in a nuclear power plant, whereas 15 years ago they were generated under a whole different set of rules.

The descriptions of the waste and what's in them are different as time goes on, and they're used for different purposes. We have legacy waste at DOE, very often with no information whatsoever. And we're gaining information as we study those materials all the time. Decommissioning wastes are very often characterized for the purpose of decommissioning, not for the purpose of disposing the waste. And we have surface and volumetrically contaminated waste. What do we do with that? Is it total Curies, is it concentration, is it surface concentration, how do we deal with it? Then we have another large category of wastes, NORM, naturally occurring radioactive material; TENORM, technologically enhanced naturally occurring radioactive material; and NARM, natural and accelerated reduced radioactive material.

These two categories are what delivered those to the public today, NORM and TENORM. Those two are not in the same regulatory framework that we're thinking about today. If that's sort of an out-of-the-box thought for ACNW is what are you doing about the wastes that delivered those to the public today versus the wastes that, in essence, don't deliver those. So that's kind of a broad, interesting question to think on. Next slide, please.

Purposes for determination. Here are just some of the purposes that inventories are determined. Disposal site requirements. What's the sin no one wants to commit at a disposal site? Underestimate the radioactivity content of the waste. Overestimating's fine; underestimating is a violation. So if I have something that I can detect at less than some MDA, what do I report on the manifest? The MDA. Now, what happens when I add up that inventory over 30 years of operation? I've wildly overestimated the inventory of the site. Everything that comes from an inventory then -- fraction released, fraction of this, fraction of that -- is in error by that original error I made in the inventory.

Very often transportation requirements drive how you describe materials you ship. Decommissioning requirements. I'm much more interested in decommissioning in what? In what I leave behind, not what I take out. So very often the concentration of information is based on what's behind, not what's removed.

There are lots of different averaging criteria for different things. Irradiated hardware, for example, has lots of averaging rules about taking hot ends and cold ends of LPRMs and whether they're in the same shipment or a different shipment. And very often that can result in differing concentrations of a factor of ten or more and various wastes of the same type.

And all of this means that there can be overestimates or underestimates of the actual radionuclide Curie inventory in various waste descriptions. So rather than thinking about just the source, I'd like you to back up one step to think about inventory and how you get to the inventory, because that's as important as the fraction that goes somewhere. That's a practical person's view. Next slide, please.

The history of this is interesting as

well. The Atomic Energy Act has definitions based on security and safeguards -- source material, byproduct material, and special nuclear material. None of those definitions are rooted in health and safety issues, particularly health and safety issues related to disposed materials so defined. So we live with those definitions today, but it's not convenient for categorizing radioactive material on any basis of health and safety concern to the public once disposed. And while that seems simple, it's not really trivial when you try and work through the system.

They are based on operational worker protection at the old AEC facilities and general operational controls. We have definitions like contact waste, non-contact waste, true came out all of this and so forth. So the definitions that we use to categorize materials are not rooted -- based in health and safety, which is the ultimate goal of all of our assessment and calculations. Nonetheless, we have to somehow resolve all that in our thinking today. I point it out to you because that's a fairly poorly understood issue, particularly in a lot of states. I mean I don't think folks have the depth of history sometimes to know what those definitions were based on.

Here's an example. Technecium 99 and I-129 and low-level waste. First, low-level waste is defined by exclusion. Low-level waste is waste that's radioactive but is not, and then there's a whole list of nots. Earlier the comment was made the concentration-based system doesn't help us very much. I fully agree for the extremes of concentration. For very dilute concentration and very high specific activity concentration, the 61 system is not useful for the purpose of performance assessment. It's workable for the purpose of making disposal decisions, but it doesn't have any relationship to performance assessment. It would be nice if we could somehow unify those two a little bit better. You know, a Strontium 90 eye applicator used in opthamology is greater than Class C waste but has thousands of times less Strontium 90 than one resin shipment. How do you deal with that? And the same thing occurs on the low end.

So a key mistake can be avoided by reporting these two radionuclides at a minimum detectable activity. Now, this was the case when the new licenses started for low-level waste site development in the '80s, after the '85 Amendments Act. And performance assessment folks said, "Oh, woe is us.

We've got way too much Iodine 129 and way too much Tech 99, based on inventories reported on the shipping manifests."

Well, Gene Vance & Associates did some studies at power plants, particularly in the Southeast, to take a look at what was actual content of these low-level wastes for technecium and iodine. Turns about a factor of 1,000 less was the true inventory of these two radionuclides. It varied plant to plant, it varied one nuclide to the other, but roughly a factor of ten to the three. So interestingly enough, paperwork and shipping records produced one number that turned out to be 1,000 times overestimating what was actually there. Inventory to source term and then to assessment, that's very important.

So I think the key point I'd like to leave you with on this is compliance and performance assessment compete. Demonstrating compliance at the disposal site and the needs for performance assessment for information compete. They're not always aligned, and we need to mindful of that.

Next, I did not discover a new element. That should -- I'm fighting with my new version of Word 2002. The spell checker has a mind of its own, so if you would just make that neptunium NP, I'd appreciate it.

(Laughter.)

It liked AC, and I changed it twice but it changed it back. I don't know why.

You know, in source terms, are we sure that what we need to know is no? Very often our classification system I think in low-level waste is based on both operational and long-term performance issues, and sometimes the operational issues drive the bus; sometimes the long-term ones do. But for low-level waste, Carbon 14, Tech 99, I 127, Neptunium 237 always show up in the performance assessments for low-level waste. Why? It's not rocket science. They're long-lived and mobile. That's it.

So, you know, I think do we focus on those in the classification system? Has enough attention been paid to those in the low-level waste environment? Do we know enough about their mobility, their behavior in packaging and so forth? I'm not sure. Do we have all the information that we need with regard to the waste content, engineering features and the natural environment? I don't know. I think we could focus our money a little bit better on some of the key ones that crop up more and more often. Do we really track what we need to know for disposal?

Let's take a look at the next page. Cobalt 60 drives the classification system. Greater than Class C waste is greater than Class C in the low-level waste arena, why? Typically, it's got too much cobalt -- stellite, for example. I contend all the cobalt in any disposal site has no impact. It's too short-lived and too immobile. It should be taken out of the classification system for the purpose of long-term performance assessment.

It is critically the most important radionuclide operationally for worker protection; there's no question about that. Irradiated hardware shipments can read ten, 20, and 30,000 R per hour on contact with the waste package if you want to handle it correctly. But in terms of it being important, in terms of long-term performance assessments, if falls off the bus very early in the game.

So it's a little bit forward-thinking, but should we then go back and look at the system we use now to say some of these radionuclides don't belong in the picture, in terms of performance assessment over the long haul once waste is disposed. How do we separate those operational issues from the performance assessment issues?

Performance assessment period strongly influences this choice. If I'm making an assessment for 100 years, 500 years, 2,000 years or 10,000 years, several things happen. Important parameters change, and the number of decimal points that I can claim should go down. It's fascinating to me to see performance assessments integrated over 10 and 20,000 years that predict doses to three significant digits. We all chuckle, but how many have seen one? I mean that's not science. Yes, the computer does it, that's right. I define a computer as a high-speed idiot generally operated by a low-speed idiot. That would be me.

(Laughter.)

MEMBER GARRICK: I've got to write that down.

(Laughter.)

MR. RYAN: And believe me, I'm behind it steering it too. I do these calculations myself all the time. But I think it's an important point. We need to focus on those things that reduce uncertainty, and I believe what Dr. Garrick said, that we need to prioritize based on how much it improves the answer.

Help me on the high-level waste side. Your dose numbers were ten to the minus two milirem per year? Is that the dose to an individual? See, from a health physicist point of view, I would take all of that and say, "I don't care from there on down." If it's ten to the minus two milirem per year, we live in a 360 milirem per year environment. Wow. When do we stop? That's an interesting question. So if I'm going to assess for long periods of time like that, what's the uncertainty of those numbers? Is it ten to the minus two, plus or minus ten to the two? Or is it ten to the minus two, plus or minus 20 percent? I don't know. Lots of influence on the integration period.

And I think at some point we have to question the natural environment. What do really know in terms of these detailed short-term behaviors that we can predict for the long term? I think Mel said some things about accelerated testing and taking short-term tests to predict long-term behaviors. Those are critical kinds of issues I think as well, not so much in the low-level waste case, but I think, clearly, in the high-level it is.

And how long is long enough? If you wait long enough in any performance assessment, uranium will be the most important radionuclide and does. Well, is it natural analogs that tell us how to think about uranium? Perhaps. So what I've always tried to feel out here, where are the edges? And one key question I'm asked on all applications, when will I be done with the application process? So far in the United States we've had ten or so low-level waste applications, and we're batting zero. California was issued but never put into play.

Some of the issues that we've heard about, I think, in the high-level side clearly exist for the low-level waste forms. Chemical form, physical form have dramatically changed over the last 20 years. In 1982 when I started working at Barnwell, Barnwell received 1.2 million cubic feet year, which was the license limit, and people were showing up, "Do you have any extra space?" Last year, the Barnwell site disposed of less than 50,000 cubic feet. Waste form and waste characteristics, chemical and physical, have changed dramatically.

Drums, for example, come in typically at 1,000 pounds of very densely compacted waste, whereas 20 years ago they weighed 150 pounds, maybe 200. So the question there is, is leaching from radioactivities -- leaching of radioactivity from those wastes changed over that period of time? What do we need to know then to really be doing a good job of estimating? Waste packaging has changed -- cardboard boxes now to steel overpacked with concrete. What are the disposal site characteristics? How have those evolved? What kind of capping has gone into various sites and so forth?

And then, most importantly, in your performance assessment, what credit do you give for these engineering controls? You spend a lot of time on the first four, and then we immediately enter a performance assessment with some degradation model or failure mode, very typically, a very aggressive failure mode. What's the right answer there? Surely, it can't be that all the radioactivity leaches out of the waste in some short period of time like ten or 20 years. Doesn't seem reasonable. So things like that, I think, need a little bit more attention. Next, please.

Performance assessment issues. Release fraction from the waste. If I have two-thirds of my radioactivity tied up in steel in low-level waste and my radionuclides, 65 percent or so, is Cobalt 60, what does that do in terms of impacts on a performance assessment? Two-thirds of the radioactivity disposed as low-level waste is in steel, irradiated hardware -- Nickel 63, Carbon 14, Cobalt 60 and so on. So different wastes, particularly in the low-level waste arena will have very different characteristics in terms of what gets released. Have we got to that level of detail? Not yet, I don't think so.

Without water radioactive material does not move appreciably. So early on in a site's performance while radioactivity is decaying away, you probably have higher integrity than perhaps 100 or 200 years down the line in barriers. How do we deal with that? How do we model that? How do we get some realism into taking credit for what we can take credit for as a function of time? We typically assume things fail fairly quickly or linearly because we don't have a better model. Should we get one?

By the way, let me interject a comment. The research plan that I saw has many of these elements in it. I thought it was very well done and very thoughtful. The one thing I think would be a great addition to it would be try and give some priority, and maybe you've done that in the numerical order that you've offered them, but to really emphasize explicitly what you think the key priorities ought to be. That would be helpful. And one category would be release fracture from waste types of projects I think are real powerful and helpful.

How much radioactive material is entrained from the engineered barriers? If radioactivity gets out of the waste form to the inside of a waste package, does it get out of the package? If it gets out of the package, does it get out of the overpack? If it gets out of the overpack, does it escape the sump and the disposal cell? And so on. There's lots of ways where you can think about some fractionation process where a small amount of radioactivity could eventually leave based on what was disposed.

Something happened here, I don't know exactly what. The slides came over, but that's okay. I think something got repeated perhaps, but that's okay.

Some of these transport issues are performance intervals. You know, it's a whole different story if I'm integrating for 2,000 years versus 10,000. Groundwater modeling. Kd drives the bus. Kd is a direct scaling parameter, it's a dose, and can vary orders of magnitude based on different geologic types, soil types, waste types, et cetera. That is what drives the does of low-level waste. I imagine it has big influence on high-level waste as well. No? Yes?

MR. KESSLER: It certainly influences

arrival time but not necessarily less of dose.

MR. RYAN: Yes. In the low-level waste case, it really drives what's released and therefore drives the dose at the arrival, because the arrival is so short, relatively speaking.

The arrival profiles. Where does it arrive? Exposing whom? We worry about intruders, we worry about residents, we worry about occupants, we worry about farmers. All of these scenarios are constructed and artificial. None of them are realistic. So when we say dose to an individual, who is that individual? The intruder, by the way, has to be unemployed, because he's exposed 18 hours a day on the excavated soil.

(Laughter.)

But he also has to be a brilliant farmer, because he has to grow vegetables in low-level radioactive waste. So now I'm not -- I make a joke of that, however, it's very important to realize that is a construct from which a regulatory judgment is going to be made. It is not a real dose. Outside of this room it's often interpreted as a real dose, and that's kind of an important thing to keep in mind. All of this science that we're talking about is going to be interpreted in a different light than we created it.

There's lots of conservatism, and the issue has been more smartly articulated today than I can, and that is that I think we really need to focus on what's the right amount of conservatism. Are we overly conservative? Are we conservative to the point of being ludicrous on some of these kinds of calculations? I think we need to address that.

When am I done? When is this technical process of performance assessment complete? Well, what risk are we trying to manage? To me, the context of the risk we're trying to manage should help answer that question. It's dose to members of the public. And if we're predicting doses to members of the public that are down somewhere in this ten to the minus something rem, are we done? By the way, a pismorem is the smallest unit of dose equivalent that you can calculate for which you will receive a paycheck.

(Laughter.)

How does this technical process flow into decision making? That's the key question -- when are we done? Next, please.

Lots has been said about uncertainty analysis. What formal system of uncertainty analysis is most useful? Is it probablistic risk assessment? Is it deterministic statistics? What exactly is it that we want to do? There's lots of models, lots of approaches. Nothing is standardized. It would be nice to have guidance on what the right standard is to look at uncertainty analysis for these kind of long-term performance assessments.

Standards are deterministic, however. All dose standards are valued. How do I take a deterministic standard and measure that against a probablistic result? That guidance needs to be developed. The worker case is a good one. If I came into an agreement state of the NRC and said, "Well, my model says that my worker had a 95 percent chance of receiving under five rem this year," where does that leave me when the dose standard is five rem a year? Unsatisfied would be the answer I would typically get. We want to know if we get more or less than that. So how do we deal with that question in terms of evaluation against the public dose standard? And, again how does this uncertainty analysis fit into the regulatory decision making?

One thing that pleased me, I think it was Item Number 6 in 5.3.1.3, was the use of historical and operational data to guide research. Please dig up old sites and figure out what happened. It will be a whole lot more instructive than all the calculational work in the world. One of my mentors at Oak Ridge, Phil Perdue, said, "If you're not making measurements, you're making conversation."

(Laughter.)

And I think that's quite right. I think it would be very instructive to try and find a couple of the right sites around the country, whether it's a closed low-level waste site or whether it's a site that's going to undergo decommissioning where there's been some waste issues, maybe it's an authorized disposal by burial and a licensee's facility where you could gain some insight as to what has happened. Even if it's a 20- or 30-, 40-year interval, you know, I think it would be great to go out and test some of these models, to actually dig up the waste, see what the inventories really are, see what migrations occurred.

It would also be, I think, very, very helpful to try -- this is a real reach; maybe it's hard to do, but I think it would be a tremendous benefit to folks that are required to do environmental monitoring as part of their license, it would be a magnificent thing to figure out how to add to that some modeling measurements. If I go down a well and take a sample to show compliance and if I measured the water level every month, then I've got something I can model as well as demonstrate compliance.

So if you're cooperative effort could look at how to work with licensees to maybe do modeling and monitoring, to do the compliance demonstration and to look at the science questions, that would be a real asset. And I think if we could somehow figure out how to fit that historical data into the regulatory decision-making process or into the confidence-building process, that would be a win for everybody.

How to do that, I think there's lots of moving parts. One thing would be cooperative agreements, which I appreciate is a very hard thing, but the more of that we can do I think the more we can better improve our knowledge base.

Do we really track what we need to know for disposal? I think there are some things on inventory we need to talk about. How long is long enough for performance assessments? What credit is appropriate for engineering controls? And when is the technical process of performance assessment complete? Another key question.

Couple more. How does the technical process flow into the decision making? How does operational data fit into uncertainty analysis and regulatory decision making? And how does the technical process fit into the overall decision making process? Somewhere along the line the stakeholder interests are going to come into play. Having been through, I don't know, 150 public meetings on low-level waste, I can tell you that's true. It's going to come in, and I think somehow that has to come together.

The areas for research I think that would be interesting to prioritize -- next slide, please -- for me would be can closed facilities and D&D facilities be laboratories? Can we somehow, instead of looking at in addition to some of the basic research, which I think is critical -- I mean you have to do bench research. You can guide yourself well into the field by doing that -- but can we somehow take this 50 years of experience that's gone on in the nuclear industry, whether it's DOE or the private sector or reactor license locations, whatever it might be, research reactors, can we somehow dissect those with the mind that there's very powerful information about environmental behavior of radioactive material, material science questions, all those kinds of things that would help us do that? That would be a win/win, particularly if there could be a research component and somehow the licensee might get some benefit out of it or however that might work. That, to me, would be a real powerful priority that would have a lot of potential benefit in many of the technical areas you've discussed.

Can environmental monitoring for closed sites be mined to enhance modeling certainty? How many records of decision are out there for radioactive waste sites? I don't know. It would be very interesting to look at the pre- and post-record of decision monitoring to see what the decision did. Another interesting area where you could use real data, I think, to enhance knowledge. And how can closure of the performance assessment process be defined?

So what I've tried to, in a real short few minutes, is just outline some technical areas, some experience base, my own experience base of what's important, what drives the bus, and then I've tried to group them into some areas. I'd be happy to answer any questions or sit down and relax till later.

CHAIRMAN HORNBERGER: Thanks very much, Michael. We have time for a quick question. Anyone?

VICE CHAIRMAN WYMER: I'm still gasping.

CHAIRMAN HORNBERGER: That was very

comprehensive. We appreciate it.

MR. RYAN: Thank you.

CHAIRMAN HORNBERGER: That was a good overview. Thanks very much.

MEMBER GARRICK: I have a comment.

MR. RYAN: Yes?

MEMBER GARRICK: How do we correlate what you said your friend said, "If you're not making measurements, you're making conversation," with what my friend, Tom Pickford, used to say, "If you want to learn about something, try to calculate it."

(Laughter.)

MR. RYAN: We'll let them get in a room and fight it out. Yes, obviously, there's a place for both activities. I think what we have focused on is sometimes easier of the two. It's easier to calculate sometimes than it is to go out and measure. The results are quicker, you get information back quicker, and you can make conclusions on that quicker.

MEMBER HINZE: And it's less expensive.

MR. RYAN: And it's less expensive. But the real merit of some of these field experiments, while daunting, I think would be that the return on the investment will be a lot higher.

MEMBER LEVENSON: John, I don't think

those two statements are in conflict at all. If you want to know what something really is, you measure it. If you want to understand it, you need to try to analyze it, and there's a difference between those two.

MEMBER GARRICK: I just meant it to be a half joke.

MEMBER LEVENSON: I know, but --

MEMBER HINZE: It was half.

(Laughter.)

MEMBER LEVENSON: Well, Michael, you'll probably appreciate it in the context of this matter in connection with WIPP when they had a model for predicting hydrogen content of drums, and they went out and sampled 109 drums at random. None of them came within an order of magnitude of what had been predicted, so the conclusion was that the physical world was wrong and the model must be right.

MR. KNAPP: I was in Oak Ridge when the TMI accident occurred, and there's lots of discussion of the iodine release fraction. And I remember one -- I think you might have been in the room, Ray -- when the model discussion was -- well, the model was very conservative because it only predicted the iodine release by a factor of ten to the six. And I think it was Tony Malinauskus that said, "Anything that predicts something off by a factor of ten to the six isn't conservative; it's wrong."

(Laughter.)

With that, I'll leave you.

CHAIRMAN HORNBERGER: Okay. On that note, thank you, Michael.

What we're going to do, next I'm going to rearrange the schedule a little bit. We are going to hear from Jane Long. Jane has done a --

MR. SAVIO: Can we take a break first? It will give us an opportunity to get Jane Long's laptop hooked up to our system.

CHAIRMAN HORNBERGER: Oh, okay. That's fine. We'll take a 15-minute -- we're going to reconvene at quarter past three because we're on a tight schedule. It turns out that we have a presentation scheduled for later in the afternoon, 4:30. I might be able to push it a few minutes but not too much longer, and so we're going to have to terminate this right about 4:30. So let's reconvene at quarter past three.

(Whereupon, the foregoing matter went off the record at 3:04 p.m. and went back on the record at 3:16 p.m.)

CHAIRMAN HORNBERGER: Much as I hate to ruin all the fun you've been having just chatting with one another --

MS. LONG: Just wait till I --

CHAIRMAN HORNBERGER: Okay. So we have a presentation. Jane has a commitment tomorrow, and she was good enough to come into town early to share her thoughts with us.

MS. LONG: Thanks, George. Well, I may be a little bit off base on this presentation. I'm not sure I was completely clear about what you were up to, but my sense was that you wanted to know about what geologic research should be done at the Nuclear Regulatory Commission. So I'm going to give you my personal point of view on that.

The first thing that I think is really important to me to understand is that a lot of the research on the programs surrounding nuclear waste were generated out of the nuclear power community, and, consequently, I think especially in the early years, there was a lot of difficulty in understanding the basic difference between engineering above-ground systems and trying to understand what's going on below ground. And the fundamental concept, a very simple, simple-minded concept that you can't see what's happening underneath the ground very easily drives a lot of problems that aren't as big problems, generally, above ground.

I think if you look at -- the most studied underground situation in the world is probably petroleum reservoirs where you have much shorter time scales. Things are happening -- you can monitor things in real time. We have to figure out what's going to happen in 10,000 years; they need to know in a year or even a few days. And they have lots of monitoring, lots of geophysics, and they still have a really hard time getting it right. So we have a harder time.

And I'd like to show this. This is like if you were in the -- trying to look through the ground and you get a kind of peep show through the ground, through bore holes, and you pick up part of the picture. And what you're trying to figure out is what that really is. And you can see that there are some black zones and some places with stripes, but it's pretty hard to figure out that the picture might be something like that. That's essentially the equivalent sense of the problem that we have.

So it comes down to -- and our last speaker really pointed to it -- whether it's wrong or right or whether it's precise. And the issue for me, an important thing that I think NRC should be looking at, is whether the models are accurate. It really -- we know how to handle precision. We do lots and lots of parameter studies. In this case, for our zebra, you could model how many stripes you manage to find and what their frequency was, how big they were. But it's really important that you know it's a zebra and not a tiger. And that's the essence of what I'm going to say today.

So to be accurate, you have to have the right concept or the right conceptual model. And if in all of your studies you fix the concept that you're working with and just crank the parameters, then you're fundamentally not dealing with conceptual model uncertainty, and that's what I think the major issue is in repository research.

The conceptual model problem I think is embodied very well in the safety case type approach to repositories, and I'm going to -- my version of a definition of safety case -- I think there's a very long NEA definition, but I'll just give a simple one, which is just that it's the set of arguments that we make to say why you think the site is safe. What are the concepts that you're relying on? What are the types of structures you think you have, and what do you think the type of behavior is? And the safety case is inherently about accuracy, not about precision.

So just to give you a stronger feeling for that, if in the solution space of behavior you are doing model results and those model results give you some kind of bullseye of what the behavior is but if instead reality is way over here, you're not being accurate. And so what I want to know is that I've got this bullseye and not that bullseye. And I think the precision parts can then be handled.

So I'd like to -- just to hammer this in, I'd like to contrast what I think the TSPA is doing versus what a safety case does. I think the TSPA deals with conceptual modeling only indirectly. It's based on conceptual models, but it isn't actually necessarily comparing the validity or the sense of -- it isn't comparing data with the conceptual models in an explicit way. It's really an extensive parametric study, and therefore it's basically about precision.

The safety case is different way of focusing on this. It deals explicitly with accuracy. It has very little quantification, and it's concerned with what the important parameters are and not necessarily their values. But is permeability important or is porosity important, for example, trying to understand what those are.

I took a quick look at the new regulations that came out for Yucca Mountain, and a quick review. I don't see anything in the regulations that gives me assurance that the performance is going to be accurate. If you look through Part 61, which lists the contents of the license application, it doesn't say anything about a safety case. And I that's, I think, the concern.

It's my belief that the safety case should be the fundamental building block of a program. I think it's descriptive. It can be understood by people. The performance assessment is very obscure to most people. And furthermore, the important thing about it is that if you take that conceptual model that's in the safety case and you use it to underlie a TSPA, then I think you have a better feeling of confidence in the millions and millions of numbers that come out of a TSPA, which are very confusing.

I don't think there's an explicit safety case built for Yucca Mountain now. I haven't seen one. I think that TSPA is clearly the main vehicle for licensing, and NRC, as I said, I don't think is requiring one, and I don't think DOE is producing one.

It's my recommendation that it should be explicitly articulated, that the safety case should be formally written up, and it should be based on conceptual models, which are based on data, getting back to if you're not measuring things, you're just having conversation. It should be based on interpretation of real data.

This formal report should lead to further investigation in a staged approach, and the data that's collected in that new stage should be compared to the existing concepts, and you should regularly and formally revise the report, as indicated by the data.

I think one of the most important things to say about this process is that it's not necessarily confirmatory research. Confirmatory research is a very bad term, in my view, because it may go the other way. Each stage of this project you look back and see whether your safety case has to be radically revised or your concepts are no longer any good. And if you were lurching from one safety case to another, then your project isn't converging, and you're not confirming the behavior; you're going all over the map.

And I think Yucca Mountain provides a very difficult problem in this case, because since the site has been chosen -- if you look at other repository programs, such as, say, the Swedish program, where they fixed the concept, the safety concepts that they wanted to use, and then they looked for a site. In the Yucca Mountain case, we're doing more or less the opposite. We have picked the site at some point, and now we're trying to understand why it's safe. So that the concepts that we're using to say that it's safe are lurching a little bit from place to place. All of a sudden we have to have titanium drip shields. We went there because it was dry. If it was dry, we weren't going to have corrosion, didn't need canisters. Now, maybe it's not so dry, so now we have to have these canisters, but they're going to be aerobic conditions. So maybe they're more -- corrosion is more of a problem. So our understanding of why this site is safe is moving back and forth. It needs to start converging if we're going to develop confidence.

I don't think DOE is doing this kind of research as much as they should. I think there are three things that have gotten in the way. QA, which asks you whether what you're doing is right -- whether you're doing what your doing, not if you're doing the right thing. And so QA has gotten in the way.

Politics has gotten in the way because there's so many entities involved in the program. I was actually involved in the program a number of years ago when I pushing them to write a safety case, and in the end they wrote a safety case that basically said, "We think this site is safe because it meets the regulations." And that wasn't a safety case, but the reason they had to do that was because there were too many competing entities working on the project, and they couldn't decide whose part of the project was causing the project to be safe for political reasons. So there's been politics in the way.

And I think regulation, based on what I'm seeing in this latest regulation, will also get in the way. If you're asking it to -- you're asking the licensing to be formulated totally on TSPA, you're not asking for a safety case.

So I think you should do this kind of research. I think you should define the conceptual models that underlie the performance assessment and challenge them. And NRC has been pretty good about doing some of this, and I think you should be doing more.

There are three examples that I'll bring up now. There's a lot more. I'm only going to talk about one today, which is the vadose zone flow in fractured rock, but, certainly, the other ones that are well known and have been mentioned today is the effects of heat on the repository, the coupled behavior problems and the long-term behavior of alloys that haven't been around very long. How can we understand what concepts will govern their behavior over time?

So I'm going to talk a little bit, just to give -- and the purpose of doing this is just to give an example. I'm not recommending this as the conceptual model that should be adopted. I'm just going to give you another conceptual model and show you what difference it makes so that you can understand why I think this kind of research is important.

A traditional view that goes back since I started in -- since Yucca Mountain project first started is that a fractured rock in a vadose zone will actually be a barrier to flow, and that flow can only occur where there's a tight neck. And the reason for this is capillary forces. People felt that capillary forces would dominate the flow behavior, so all the moisture would be sucked into the rock like a sponge, and the fractures would be these big voids that would preferentially attract the air. And, therefore, fractures were barriers to flow in the fractured vadose zone.

Well, recently, you mentioned before this Chlorine 36 data. This is some of June Fabryka-Martin's data, and she went down along the drifts and took samples and measured the Chlorine 36. And what you see here is elevated Chlorine 36 primarily in feature-based or fracture-zone-based areas of the drift. So in other words, she's walking along the tunnel where she sees water coming out of a fault zone, a major feature. It tends to be higher in Chlorine 36 and in fact higher than the present background and consistent with the bomb pulse of 50 years ago. So she's seeing fast transport of Chlorine 36.

Now, you asked -- I guess it was Milt -- you asked a little bit about whether this is real, true data or not. My understanding is now that they're remeasuring this and that the data is controversial. One reason I find this data very appealing is that the elevated Chlorine 36 only occurs where there's faults, and that is consistent with a hydrologic interpretation of what's going on. My understanding is that they're now remeasuring all of this.

But what isn't happening and what should be happening is that it shouldn't just be Chlorine 36 that is measured at these places. You should be measuring strontium and tritium and other things all at once. And if it's not showing up now, does that mean it was measured wrong in the first place or does it mean the pulse went on through and we don't see it anymore. So there's an awful lot of questions about this data, but I think it is should be making us think, if it's true, what could explain it?

So based on this data, what would the highest velocities -- what high velocities could we expect? And that, of course, is an issue for transport. After water has accumulated in a repository and it moves on through down to the groundwater, we want to know how fast that happens.

But there's another problem, which is how much flux gets in, how much quantity of water gets into the repository, and that's a problem for the waste package. So we know -- if we believe this Chlorine 36 data, we know we have high velocity, but that doesn't necessarily mean we have high flux, or does it? We don't really know.

So here's a new model that was developed by Maria Draglia. She was a Ph.D. student with us a few years ago, and she looked at this. And she said, "Well, I'm going to look at analyzing film flow down a fracture." And in this case, it doesn't matter how big the fracture is as long as the water doesn't touch the other side. You can have a little fracture or a big fracture and you'd get a film flowing down. It's a free surface, and the flow is governed by gravity. It's gravity-driven dynamics.

Well, she looked in the literature and Mary interestingly found some work by a guy in Russia by the name of Kapitza. A couple of years ago, I was on a delegation to Moscow to look at nuclear waste, and I met one of Kapitza's students there. And he told me that Kapitza was imprisoned by Stalin in Estasha and spent the time looking at rain going down his window, and that's how he came up with this model. It's an interesting little vignette, but it also does point to the fact that a lot of the kind of work that has to be done is observational.

So in his work, he looked at the idea that the flow that would be going down a surface like that is unstable. And you've seen this if you've watched rain go down your windshield, that you get soliton waves that move very quickly down the surface, and those waves ride on a substrate which is water that clings to the surface and then essentially forms a very slippery surface for the soliton to move down. And she analyzed that.

And this little cartoon shows what she analyzed. The trucks are the solitons moving down the surface, and they represent about half the mass. Because they're moving faster, they represent 94 percent of the flow. So she then looked at a little model where she said, "Okay, things will go from matrix flow into an open fracture and back into matrix flow," and she had a little seepage face in between each one. And she looked at the percentage -- as a function of the percentage of matrix flow, she compared that to a tough model, which would be all matrix flow. And you can see that the film flow model gets down a lot faster than the tough model.

Another way to look at that is if she looked at various totally matrix flow to totally fracture flow, and these red bars represent the estimates based on the Chlorine 36 data. And then her model can explain the fast transport of the Chlorine 36 data. That's not to say it's right, but it's worth pursuing.

Now, the Chlorine 36 data may or may not be right, but there's data all over now that indicates that some of these radionuclides are being transported very quickly. The INEEL site has I believe it's a plutonium transport that's been observed at the water table in a fracture with salt. There's data from fracture systems in the Negev Desert, and there's also other data from the Nevada Test Site on tritium and strontium. Now, the problem is that none of these things have been studied all in the same place, and I think that points to the fact that you have to have a coherent focused program on doing research on these concepts and whether they're correct or not.

So we don't know enough about this problem, and until we do, I don't think we're going really understand what kind of barrier this geologic system is going to provide. We need to understand the basic physics that are controlling this behavior. If it isn't capillary physics, then what is it?

If you believe the Chlorine 36 data, then you know you've got fast transport, but that still doesn't answer the question about flux. If Maria's model is right, that means the flux is high too. There are some other people that are saying, "Well, we've got some fast transport, but most of the flux is low." So I don't know what the answer is. There haven't been any field tests. And, of course, as the water flow rate goes up and you get closer and closer to saturation -- in Maria's model, for example, as you get more and more water, at some point it's going to hit the other side of the fracture, and all of a sudden you're going to get a capillary force, which is actually going to slow it down. So at what point do you continue to have increase in flow, and does that continue monotonically or not?

So in summary, I think NRC should conduct research which determines the accuracy of the models underlying the TSPA. I think these are mostly field and lab tests. They're not a lot of modeling, except for using the models to try to understand your data. But doing parameter studies is not part of this. This is really observational research. I think three of the key issues again are the vadose zone, the thermal effects and the alloy behavior, but I'm sure there are others.

Finally, I'd just like to make one comment in closing. One question that came up during Ken Rogers' committee was who should do the research? And I guess NRC has had a policy of trying to keep out conflict of interest. If you're doing research on

DOE's work, then you can't do research on NRC's work. I would recommend very strongly that you do competitive research, and with a small amount of money the most important thing is to do excellent research, and peer-reviewed research, not to keep conflict of interest -- I think conflict of interest is a secondary factor. So thanks.

CHAIRMAN HORNBERGER: Thanks very much, Jane. I suspect that there may be a question or comment. John, do you want to start?

MEMBER GARRICK: Yes. I have just one question.

CHAIRMAN HORNBERGER: Just John first.

MS. LONG: Only one.

MEMBER GARRICK: Jane, I enjoyed your comments about the safety case versus the TSPA. Let me ask you, if you performed a perfect TSPA, by which we meant you're calculating the correct measures of safety, that you've exposed and ranked the important contributors to that measure, and that you've done a good job of dealing with the issue of uncertainty, would it be possible, if you had that perfect model, to extract a safety case -- to abstract a safety case from such a model?

MS. LONG: Well, it implies a safety case.

MEMBER GARRICK: Yes.

MS. LONG: And then the question is, is it accurate?

MEMBER GARRICK: Yes.

MS. LONG: Yes. I think you can look at the TSPA that DOE is using. You can extract the safety case that must underlie it and must be implied it, and then you can ask yourself if it's accurate.

MEMBER GARRICK: I guess the question is, isn't the safety case a subset of a perfect TSPA?

MS. LONG: Well, I guess it's just your point of view. I think that the TSPA should drive -- the safety case should drive the TSPA, but that's not important. The important thing is that you've written a safety case and that you ask the question, is it accurate?

MEMBER GARRICK: Thank you.

CHAIRMAN HORNBERGER: John Kessler, did you have a comment that you want to make? You have to use the microphone.

MR. KESSLER: Which came first, the safety case or the TSPA, it's an interesting dilemma. I guess the point is, is at the beginning you may not know what you're safety case is, right? I mean even the --

MS. LONG: Yes.

MR. KESSLER: -- Swedes had to come up with something based on some sort of understanding of a model plus observation, and sort of it was in iterative process by which one begat the other or whatever.

MS. LONG: Right.

MR. KESSLER: So I mean I guess I tend to fall more in John Garrick's side, which is that you may have a disparate models, assuming that they're all the correct conceptual models, and out of which, after you do a bunch of analyses using all kinds of different techniques, you may realize, oh, okay, this is what's really important. So I guess I come at it from that way simply because you don't know what you're safety case is at the beginning, necessarily. I worry that if you have to start with the safety case, that you may -- it's based on preconceptions about --

MS. LONG: Well, I'm terribly hung up on whether you do it first or not. I mean if you want to do a chicken and egg, and the egg is the safety case and the chicken is the TSPA, just don't let the egg roll down the road and forget it, okay?

MR. KESSLER: Right. I guess I would

argue that the project has been working on something they called a repository safety strategy --

MS. LONG: Right.

MR. KESSLER: -- which has something in it that looks like what you define as a safety case.

MS. LONG: I haven't seen it.

MR. KESSLER: Yes. They're on version -- rev 4 now or something like that. So they're working on it.

MS. LONG: Well, that's good.

MR. KESSLER: It's still pretty long, though

CHAIRMAN HORNBERGER: Budhi.

MR. SAGAR: Jane, you said -- I think this was the last slide where you recommended that the NRC research ought to focus lab and field, observational, not modeling. First of all, the parametric studies are not entirely precision. I think different sets of parameters do define different conceptual models in some way. It may not be totally fundamentally different like your film flow kind of model, but it will define different scenarios differently. So it's not entirely procedural.

MS. LONG: You can't avoid some of it.

MR. SAGAR: You cannot avoid some of it.

So it is important to do the parametric studies, I think, even for a regulator. And, also, I think you do need to do modeling even before you do an experiment just to understand what you're doing, and then follow it up after you do the observations to see if indeed what you studied makes sense. So I think to disjoint the observation from the calculation phase would be a mistake, in my point of view. You may disagree but --

MS. LONG: Well, I'm trying to be a little bit pointed here, but I agree with you, Budhi. I mean the bottom line is that I guess I could conceive of valid research being done on conceptual models that didn't include any modeling, but I can't conceive of valid research being done without lab and field measurements.

CHAIRMAN HORNBERGER: Wes, did you -- are you waiting to get in here?

MR. PATRICK: Actually, I think a clarification with your analogy. Instead of getting hung up on whether it's a chicken or an egg, I think the real question is, is it a chicken or is it chicken dinner? Is the safety case -- take John's argument. He's saying --

MS. LONG: I skipped lunch, so you know --

(Laughter.)

MR. PATRICK: I guess I've always seen the safety case -- from an engineering point of view, not as a performance assessment person, which I'm not, I've always seen the safety case as having a component that is PA, but there are other arguments that are marshaled there as well. And I'm just curious what your point of view and whether anyone else here shares that? I mean I don't see it as one leading to the other, that's the chicken or the egg analogy, but I think the salient thing is that if DOE came forward with only the perfect PA, could NRC make a licensing decision or not? I've always imagined the answer to that question as no.

MS. LONG: No.

MR. PATRICK: The safety case is more than just the risk calculation, that it would be some source of supplemental modeling, it may be some detailed calculations and measurements and analogs and all of that other stuff that kills confidence that safety will be there with reasonable aspect.

MEMBER GARRICK: That's why I qualified my definition of perfect TSPA with it containing the proper risk measures, safety measures.

MS. LONG: I guess the thing that I'm

looking for is a coherent process which says, "Here are the concepts we think underlie our TSPA. And we're not so sure about this one, so we're going to collect this data," and then you formally collect that data and formally compare it to the concept. And I don't see that happening in a formal way, in a formal iterative, staged manner. I just don't -- you know, they may be producing this report and they may be on all these stages, but I don't see a formal process that says, "Here's my conception model report document, here's the data I collected, subsequently, and here's the analysis of the conceptual model as a result and the change in it." Now, it may be happening, but it's not explicit. And it's that process, to me, that gives me confidence that the TSPA is perfect enough. It doesn't have to be perfect, but perfect enough.

MR. PATRICK: Next think you know, she'll be asking for people to pose hypotheses and test them.

(Laughter.)

CHAIRMAN HORNBERGER: Jane, you started out reminding us, of course, that in the geological environment it's quite difficult to make necessarily all the observations you might want. My own suspicion, perhaps yours as well, is that there may be alternative conceptual models --

MS. LONG: Right.

CHAIRMAN HORNBERGER: -- alternative safety cases, and you may not be able to collect data to decide on one versus the other. What do you think the NRC should be doing in terms of research along these lines? How would you advise them to decide one way or the other?

MS. LONG: Well, I think it's -- I guess depending on how -- I mean you don't have a huge amount of money.

CHAIRMAN HORNBERGER: Right.

MS. LONG: So you have to pick the major competitors. I mean if you keep -- and then I guess I would go back to what Budhi recommended: If you have to choose between them and your modeling is telling you that one doesn't matter, then you could probably find rationale for picking more important ones.

CHAIRMAN HORNBERGER: Do you think there's a way to --

MS. LONG: I tried to pick one that I thought was pretty important.

CHAIRMAN HORNBERGER: Right. And to a certain extent, I think that one was obvious in the sense that I was just going to say Mal Knapp suggested that we might focus research by going after pursuit of flaws, and I might characterize the film flaw in fractures as one look at that. I mean do you think that's a good strategy?

MS. LONG: Yes. I thought there were three things that are really obvious about the program: the alloy behavior, the heat -- the coupled behavior due to heat and the way in which water moves through the mountain. Those are really critical areas where a good conceptual model, whatever the physics and chemistry that dominate that performance. I think you can -- there's a term that comes out of -- you know, if you've been in the Army and you've been taught how to respond in a fire fight, they tell you to point, don't aim. And there is an ability to look at this whole thing and point to issues that are clearly big, and I think those are three big ones. You know, I'm not saying they're the only ones, but I think you can do that. I think you can prioritize.

CHAIRMAN HORNBERGER: Any last question? Thanks very much, Jane.

MS. LONG: You're welcome.

CHAIRMAN HORNBERGER: We have asked several panelists to offer their comments, as they like, on the variety of things that we've heard, and I think I'll just go down this list in order, and William Hinze is first.

MEMBER HINZE: Well, thank you very much. I believe that the four presentations we've heard this afternoon are as good as they are different. They certainly have brought up a number of interesting points. Before I go to the RTE document, just perhaps some general comments, because I thought you said that we could do that.

First of all --

CHAIRMAN HORNBERGER: And if I didn't, you would anyway.

(Laughter.)

MEMBER HINZE: Well, one of the things that we saw in the expert panel report and also that John Kessler brought up this morning was the linear no-threshold hypothesis. It seems to me that this is one of the very significant weaknesses in our knowledge of radioactivity and its effect upon human beings. It seems to me that the American public is going to demand that the government come up with some better evidence regarding this. Now, the NRC may be doing something in relationship to this on an international cooperative basis, but you see very little action. And I think that any long-term research has to address this problem, and I really hope it will, because that is a topic dear to my heart.

One of the things that is sitting around the ACNW table for a number of years essentially all of the topics that came before the ACNW cried out for more information, more data, more research, if you will. And we've mentioned a number of these. One of them that, as some of you may recall, is quite dear to my heart is time of compliance. We've seen that on the screen in a number of different forms today, both low-level and high-level waste.

The decision should not be based upon political realities, I guess you'd call them, but it shouldn't be done on the very conservative aspect, for example, as we saw in a National Academy report suggesting that the time of compliance be made a million years, which was, to my view, taking a very extremely conservative approach to the problem. I think, again, that the time of compliance and this question of where do you terminate the performance assessment is one that's going to have to be addressed.

The only person that I've really listened attentively to today is John Kessler, and that's because John has included natural analogues, and perhaps Budhi mentioned it too. But it seems to me that when we talk about realistic TSPA, whatever that is, and that's in the beholder's mind, I guess --

MEMBER GARRICK: Oh, God, you haven't changed a bit.

(Laughter.)

MEMBER HINZE: What's your name again?

(Laughter.)

It seems to me that the way to get a handle on realistic TSPA is through natural analogs. I'm not saying that this should completely displace performance assessment, but it should be made to control performance assessment, and I really like the suggestion of Mike that we have closed repositories, so it's kind of the ultimate natural analog and tear those apart and see what goes on. I think that's really great thinking.

Talking about the radionuclide transport and environment draft research program plan, that's got a lot of kudos today, and so I will take the opposite view and suggest that there are some -- in particular, there's one area in which I think it fails to have a sufficient amount of vision. And this relates to hydrologic parameter estimation, and I think this is what Jane was getting to in her discussion.

The hydrologic parameter estimation, of course, is part of this uncertainty -- the model uncertainty and the parameter uncertainty -- and this Committee is well noted for its concern about limiting uncertainties or at least controlling them so that they are in some kind of consistent fashion.

The model uncertainty certainly is a problem, but I think that's something that we're starting to get our hands on. What is not there is this parameter uncertainty, and that is in the various nodes in the model that one has some kind of realism in the parameters. The problem here is largely in the heterogeneity of the subsurface. I'm afraid that my engineering friends -- and, incidentally, it is apparent I come from the geoside of things; I hope it's apparent -- my engineer friends think about steel, and I think about steel as being a pretty homogeneous thing -- I realize it isn't.

But when we go to the subsurface, we have a great deal of inhomogeneity, and I picked up a book out of my shelf that was published under the aegis of a committee chaired by George M. Hornberger, so this has to be -- and this was published last year, George -- so this has to be right.

(Laughter.)

I'll just pick out a few sentences. "Spacial heterogeneities" -- and this is a National Academy scene into the Earth -- "Spacial heterogeneities and the physical properties of rock units prevent complete characterization of subsurface rock formations from observations made in outcrops or cores, and parenthetically, one usually has anywhere from one part in a million to one part in a billion of volume that is investigated compared to that which you are trying to model."

"In environmental and engineering studies, properties of interest, such as porosity, hydrologic conductivity and chemical or mineralogical composition, including sorptivity, might vary over short distances within a single geological unit. No reliable mathematical model for interpolating between observation exists. The importance of minor geological details in geotechnical engineering is well known in another phrase. The key to effectively describing the subsurface heterogeneity is most likely integration of different types of information at different types of scale." And this gets to Budhi's third overhead, which I thoroughly agree with. I thought that was great.

Well, what this really is getting at is that what we need to do is we need to -- if we're going to decrease our uncertainties, we have to have some kind of better sampling procedures. And this is sampling by direct sampling, by cores, by outcrops, so that we have direct observation. And that has to be tuned to the geological history of the area. It has to be tuned to the types of rocks that are present and the origin of those rocks and their subsequent tectonic history.

And as a geologist, you probably could say, "Well, that's impossible," but, no, I don't think it is. This borders on use of such things as chaos theory, fractals and so forth. There's a good deal that could be done here, and this is pertinent not only to high-level waste, which certainly is important too, but is associated with all of the other types of waste activities that deals with the subsurface that the NRC is involved with.

Yet when we go to Page 26, we see hydrologic parameter estimation, develop test, and the focus here, in brief, is on decommissioning and estimating parameters from historical data sets. Now, Mike made a point that it's great to go to historical data sets, and I agree with you wholeheartedly, but you have to be able to extrapolate that into the realm in which you are using the data. And in addition to that, we have to have a lot more data. Now, in many cases, what we need to do is we need to obtain this data, not by direct sampling, but we need to obtain it so that we do not or at least we minimize the loss of integrity of the site or we maintain the integrity of the site. And that means someone today had remote sensing -- and I think that was you again, Budhi -- the remote sensing to seek out these characteristics.

Now, when we use remote sensing, what we're doing is we're determining primary properties, seismic velocity, P wave, S wave, whatever kind of velocity you want or electrical properties. But we would like to be able to translate those at the really meaningful properties from a waste standpoint; that is, Kds and porosities and so forth. And there is a general lack of information on how to do that. There is certainly no theoretical way of doing that, and the empirical ways of doing that are extremely limited.

Thus, I think that I would very much like to see that this radionuclides and transport in the environment broaden out their view about the elimination of -- or the constraining of uncertainties to do a broader job of hydrologic parameter estimation. And that's about my 15 minutes.

CHAIRMAN HORNBERGER: Tim McCartin.

MR. WILSON: I have one quick question.

CHAIRMAN HORNBERGER: We have time for one quick question. John, you'll have to come to the microphone, though, because we're on the record, and you'll have to announce your name and affiliation.

MR. WILSON: Yes. I'm is John Wilson from New Mexico Tech in Socorro. One person's model uncertainty is somebody else's parameter uncertainty. It varies all over the place. But my personal opinion is, for someone who's worked on heterogeneity and parameter uncertainty for decades, is model uncertainty is far the worst actor. Basically, I can be uncertain about the spacial distribution of some property, it can have an influence, but if I do not account properly for the physics and biology and the chemistry, that influence is not properly handled.

For example, heterogeneity, coupled together with things like gravity and viscus instabilities and other things like that, can lead to fingering. The fingering will occur because of the heterogeneity or because of the instability that's present. If I leave out the physics, I don't necessarily get that kind of instability. I've got to take it into account for a wide variety of processes the kind of modeling uncertainty that comes about because I don't know what processes are acting, I don't know how they interact, I don't know how they couple, I'm not certain what's going on, I don't know how any one of them works. All of this has, to me, a much more profound influence on what goes on than the heterogeneity.

The heterogeneity is something you've got to deal with; there's no question about it. But in the absence of that heterogeneity, the misunderstanding we have about processes just really causes us far more problems. I've done a lot of applied modeling, and it's the conceptual model problems that drive me nuts as a consultant.

One last thing, though: I think as time goes on and we do more and more process-oriented research, we will come to grips with how these processes work more. As we know, like Darcy's law, for example, already. As we come to grips with how they work in principle, we do not necessarily come to grips with how they work on a particular site, and that's more like your parameter uncertainty.

Eventually, we can reduce some of the process level uncertainty, that part of conceptual model, down to essentially zero.

But I don't think we're ever going to reduce the heterogeneity and the other aspects of model uncertainty that will remain simply because we can't characterize these systems well enough, and never will, unless we have some kind of geophysical or other remote sensing instrument that gives us far more resolution than anything we see on the horizon today.

CHAIRMAN HORNBERGER: Thanks, John. Tim?

MR. MCCARTIN: Yes. I'd like to start off at least to give NRC's perspective on performance assessment. And I know people define it a lot of ways. I know the NWTRB tends to define it fairly narrow. We at the NRC, and maybe we failed, have tried to define it very broadly. And the performance assessment includes not only the calculational results, all the supporting information, all the testing of hypotheses, testing of alternative conceptual models, comparison with laboratory field data, comparison with detailed process models, comparison with natural analogs, and we consider all that under the umbrella of performance assessment. We do not try to separate the calculational results from all the information that got you to the point how you built that model and why you believe those results. And I want to give at least that perspective. We tried to put that in the Part 63. We may have failed, but at least we have attempted at every chance we could to define performance assessment in a very broad context.

And in that regard, I think in looking at the different talks, and there were a lot of excellent ideas put forward, but I would like to put three particular bins for where I think research fits. And one would be conceptual models, performance assessment results and best available data. And I look on those three bins that I'll describe earlier it really is an iterative process. And maybe some of them should be put first rather than third, but it is iterative and you're going through the cycle as many times as it takes before you have a certain comfort level.

And, first, in terms of conceptual models, although we have a relatively complex performance assessment tool at the NRC in our TSPA code, we did start with a simple model, and I think research could be very beneficial even at this stage, still looking at simple concepts. And by that, I would look at why do things work? Why might things fail? And I think you could look at -- have research basic concepts.

And I can look at some of the things that were said. We have matrix diffusion, and Kd and the matrix is a very significant, we believe, reason for the system to work. It will provide a significant amount of retardation, even with fracture flow. Let's understand that better. Why does it work that way? Are we representing it correctly? Conversely, colloids is just the inverse of that. It may totally defeat matrix diffusion and subsequent retardation in the matrix. So you've got these two things. Why does it work? Why might it fail? I think research is probing and looking at different concepts.

Along the same lines, we have limited water contacting the waste. Is one of the reasons the site performs well? The converse of that, I'll say, is what John Kessler alluded to, is a very large diffusional release. That may be a very conservative assumption on the DOE's part, but if you have a very large diffusional release, whether you have limited water contact or not, may not matter. And so that's sort of why does it fail, why does it work? And we at least -- whether we agree or not how conservative that is, it's something to look at.

I think there are a lot of concepts out

there that having research look at why does the site look good but what are things that could defeat it? And I think those are areas of research -- and we heard a lot of different ideas in terms of those kinds of concepts. You're developing a simple model in your mind in that sense of why it works, not necessarily computational, but it is a simple model.

At NRC, we're probably at our fourth or fifth generation of developing the TPA code and revising it. It gets a little more sophisticated, a little more complicated as a result of that. But the reason you have that code is, yes, I have these simple concepts of what works, what doesn't work at the site, but now stringing it all together, what does it really mean? And I think that's my second bin is PA results. And now you do the calculation of the results, and you try to pull out -- tease out as much information as you can from those results. And I know Dr. Garrick has pushed us a lot of times on different ways to probe the analyses. Budhi has looked at some of the barrier analyses. John Kessler's done other types of one-off analyses.

There's also the question of uncertainty. How do you factor in -- certainly, conceptual model uncertainty, I would agree, is clearly the most dominant thing. Parameter uncertainty is small typically in comparison to conceptual models. But are there -- research could be done to look at different ways we can explore conceptual models uncertainty, different ways to tease out the results, like the one-off analyses, one-on analyses.

There are a lot of things that could be done, I think, in that regard to better understand what the results are telling us. But those results are important. They are a way -- sometimes you -- some of the results are counterintuitive. "Gee, I would have thought this was important. It didn't turn out important. Why?"

There's a lot of understanding. Running the performance assessment code is a trivial exercise. I think at NRC we can run the PA code overnight. Getting those results and pouring through them and analyzing them gives you all the understanding. Why did it come out the way I thought it did? Certainly, it's a feedback to the conceptual model development. "Gee, I thought this should have been important, and it wasn't or just turned out more important." But that thinking process, are there, like I said, different tools that could be developed to help us better present the results, interpret the results and just tease out more information. I think that's an area for potential research.

And then, thirdly, what I'll call the best available data. And I'd have to say in my 20 years in waste management, one of the more impressive speakers I've ever heard that I always felt, "Gee, he gave me confidence that what he was explaining was correct." It was Dr. Neuman at the University of Arizona. And there was one reason for that. I think he is extremely unique in that he can give you a process or a theory or a concept and then rattle off five or six field examples around the world of where that was proven true. And I think that is extremely important.

And along the lines of what Mike Ryan was saying, we now have between superfund sites, NRC, former low-level waste sites, decommissioning sites, there is a tremendous wealth of information out there of environmental transport and possibly some source term issues. In addition to the natural analogs, I would argue that it may be worth it for some of these concepts for research to look, "Gee, is there a story out there being told that we just haven't heard, because we haven't pieced together, gee, these 30 sites where, whatever, americium was moving, and these are the things that were observed?" Maybe one of those sites doesn't say much, but 30 of those sites there's a story there.

And I think -- I remember -- Bill may remember this, I don't know -- maybe 15, 18 years ago, research did some studies looking at the old low-level waste sites. Well, maybe there's some value in going in -- research going in and looking at, like I said, all these other sites. Are there common themes that say something about transport or source term that might be pulled forward?

I would argue in terms of public confidence, something that was talked to a little bit in the morning, I think that's where you get much more public confidence if you can show to people, "Gee, at these sites, we've observed these kinds of processes, and we believe it's appropriate. These models are representative of what's going on in the subsurface." I think that carries a lot of weight, similar to the way I think Dr. Neuman will rattle off many, many examples in the field to support his conclusion. And with that, I'll keep quiet.

CHAIRMAN HORNBERGER: Bill?

MR. OTT: It's been a very interesting day. It may, in the long run, be more helpful to us than it is to you, because I appreciate the fact that you've brought all these people here to get ideas on research, which is basically what we're trying to get on the plan. I wrote down a whole list of things to try and talk about, so I'll bring out my list and at least think about it while I'm making these observations.

I think I wanted to make a few remarks about the conceptual model problem, and it's received a lot of attention. I just wanted to give you the perspective that we've been working on and basically where we came from.

I think part of it comes from both our natural analog work and our work in the international community back when we were still in the high-level waste program, when we were involved with Intraval and Hydracoin and the Natural Analog Working Group. And there was a report that came out of one of those that -- I think it was the Natural Analog Working Group -- that talked about the conclusions of a certain phase of their work. And they said if you look at the data available on a site and say you take all the hydrologic data and then try and take that data and develop a hydrologic model or a model for the system, you may come up with two or three different interpretations of that data. Maybe you can come up with ten interpretations of the data that are hydrologic models that you could use to represent that site.

And then somebody else comes in and they say, "Well, let me look at the geochemical data and see if it's consistent with that." And they'll look at just the geochemical data, and they'll develop their own concept of the site, and they'll come up with ten or 15 models. And low and behold they aren't the same. There may be a subset, an intersection of these two sets of models, which are consistent.

Then if you go in and you look at the geophysical data, you can get a further limitation and maybe further expansion, another set of models that doesn't agree with either the hydrology or the geochemistry. What it boils down to is you're trying to interpret a natural system with limited data. The more data you have the more you can limit it, but I don't think we can ever have enough data to say that we truly know what that conceptual model is.

Then the question goes, well, all right, if there are multiple interpretations of the data, how do we know which one is right, and how do we calculate the uncertainty which results from one particular selection of those particular conceptual models?

That's the problem which we've thrown at Shlomo Neuman to try and address for us. He hasn't got a result yet, but he's working on a methodology to come there, to get to that point.

I challenged him at one point when he came in to discuss the progress on his work and I said, "Can you ever quantify this?" And he said, "At this point, I don't think so." So he was saying that at that point he wasn't certain that he could ever quantify the uncertainty associated with conceptual model uncertainties, i.e. that you might have the wrong model, but a different model might be right. He's since backed off from that, and he says, "We may be able to do limited quantification." He hasn't said he can put a number on it, but he says, "I might be able to do better than no."

He's not done yet. We're still working on the problem, but conceptual model uncertainty is not, in our mind, simple a matter of parameter variability or heterogeneity in the natural system. It's due to the fact that we have extremely limited information and there are multiple interpretations of that information that may lead to the actual way the system evolves.

This list I've got is very disjointed

list, so I'm going to go through it and try and make these observations. Prioritization system. There were some people today who said they didn't -- it looked like it had evolved as an apology to the other system. Well, it wasn't quite that bad. Basically, the Office developed a prioritization system. We investigated the systems used by a whole host of other -- well, not a whole host -- a limited set of other federal agencies. We looked at what the National Institutes of Health do, we looked at the National Institute of Standards and Technology, we looked at USGS, we looked at the Federal Aviation Administration.

We went down and we talked to their research arms, and almost every one of them had a very, very large component of expert judgment in their prioritization of research, as did we. Unfortunately, that was one of the things that we were getting an awful lot of trouble from, of all people, our Advisory Committee on Nuclear Waste.

So we looked very hard for some way of coming up with a systematic and quantifiable approach. The only one of those five agencies or six agencies that we talked to that had a quantifiable approach was the National Institute of Standards and Technology.

And they had gone to their Economic Division, and their Economic Division had actually worked with the Building and Fire Research Division to develop a prioritization scheme based on the analytical hierarchy approach. Just one of many systems that's evolved over the years that tries to give you a way of comparing apples and oranges. It is subjective. It's designed to be subjective, because it's designed to respond to the priorities of the decisionmakers.

The prioritization that was -- system that was developed was forced to be responsive to the goals of the Agency. The Agency has a strategic plan, and we have strategic goals, and the structure of the prioritization is built along those strategic goals. Unfortunately, all of the offices in the Agency have the same set of strategic goals, just the words underneath them are described differently. And the words that were generally put together when that system was discussed really focused on reactor research.

When we wanted to deviate from this, we thought it was politically wise to try and move from the prioritization system adopted by the Office, slowly away, to try and at least be consistent with the approach that was being used but try to tailor it for waste. I think we've received some acceptance of the policy that we should be treating waste differently, and I think a wider departure would probably be acceptable now.

You'll hear tomorrow from Jack Rosenthal who's looking at the prioritization system within the Office and what we can do about both materials and the waste arenas, because they are so substantially different from the reactor arena. And he'll talk with you in more specifics, so I'm not going to go into that anymore.

That's sort of the way we evolved. We deliberately tried to go from what the Office had adjusted rather than try and defend a drastically new approach that might challenge the validity of the other approach. It was a case of choosing for evolution as opposed to choosing for something radically different, and I think we're making progress.

There's been a lot of discussion on realism versus conservatism and risk significance. I think the plan says in there that our objective is more realistic analyses. We've been very carefully told that the Agency doesn't make decisions which are unrealistic; they make decisions based on the best information available, and those are good decisions. The decisions themselves may be more conservative or less conservative based on the information that's provided on them. Risk significance should certainly be an input to that. I think we would agree that you need to consider risk significance in trying to approach realism.

Anticipatory versus confirmatory research, this is an argument that's gone on for a long time. Quite often anticipatory research becomes confirmatory research once the user office decides they need it. Quite often it's anticipatory because you can't get direct strong support from the user office, perhaps because it does have a longer time frame and they don't need it now. There's very little other way to talk about it.

If you go back historically, and I don't want to go in this place very long, back when the Office of Research stopped doing high-level waste research, we had a fairly large program at the Center. The problem was that funding got reduced substantially, and the Agency had to maintain the integrity of their analytical capability. They sacrificed anticipatory research to do that. So there was a significant reduction in research, and there was no longer a real reason for the Office of Research to be involved; in fact, it was complication that wasn't helpful. So everything was transferred over there, and as a result, operations plans are only one year long, as Budhi said at one of the breaks. They don't look beyond that year in terms of what's the long-term for the work that they're doing at the present time. Much of the work that we had that was terminated at that time was looking at the longer range.

And some stuff that -- in hindsight, if I looked at it now, I would wish that we had continued to do some of the work at the Apache Leap Research Site, which is probably the best analog outside of Yucca Mountain for fractured flow in an unsaturated tough. Unfortunately, that was one of the victims of the congressional reduction in funding. We managed to finish out the actual primary goals of the project. Some of the secondary goals of the project weren't realized, but it was just a matter of funds availability. There was nothing you could do about it, because to do anything else would have compromised the technical integrity of the support structure at the Center, and that wasn't an acceptable option in anybody's mind.

A couple of other areas. We had a

volcanism program in process that we had scoped out at about $5 million for the entire scope of the program. It involved three projects to be started up sequentially, one starting not before the other had ended, the last one being one that would look at structural controls and volcanism. That project never got started. That was our primary mechanism for trying to deal with probabilities of volcanic -- of igneous events at Yucca Mountain.

In the absence of doing that part of the research, I think what you see is an argument that is based primarily on statistical analyses of existing volcanic features. It probably would have been much better in the long run if you had the other. It may be adequate science to do what we have. I can't make that conclusion, because I'm not a volcanologist. But what I'm saying there is that, again, this was anticipatory research that because of political decisions on the Hill essentially had to be cut back, that we considered anticipatory because it was long-range. The short-term work continued and has probably provided adequate support for that.

Monitoring instrumentation, performance indicators, engineered barriers. We're seeing, in many instances -- not seeing -- we're hearing from places like Savannah River that caps installed on disposal units are failing -- about five percent at the current time. But that may be the first edge of a wave of failures over time. Everybody has said that we do not have long-term performance data for barriers. We're going to need to monitor them. What do we monitor? Do we monitor them after we fail or do we monitor the characteristics that are going to lead to failure. So I think that monitoring is something that we think is an important thing to continue to follow.

Instrumentation. We haven't been strong on instrumentation. We've been doing a little bit of instrumentation work. The Apache Leap program had a very strong component of instrumentation, because we were trying to figure out how to measure things in the unsaturated, fractured environment. And from that perspective, that program probably made a major contribution to being able to adequately characterize Yucca Mountain.

Instrumentation may be an issue for things like looking at engineered barriers over a long period of time. Most monitoring systems haven't been designed to work in place for 30 years, 40 years or 50 years. And you're not going to have a continuous record if you have to take it out and replace it and put in new instrumentation or change the instrumentation.

And then the question is what do you measure? That's the performance indicator thing. What do we measure that's -- what can we measure that's an indicator of performance of the site? Not necessarily radionuclides. If you look in the saturated zone for radionuclides, you detect failure. If you look in the unsaturated zone for evidence of water or if you look at the barrier for evidence of fractures, you detect failure before it occurs, and perhaps you can remediate. So to a certain extent, we're looking for preventive monitoring techniques as opposed to those kind of techniques that allow you to detect that you've failed.

We talked about prioritization. We've talked a lot about models today. I just gave a long discourse on my perspective on conceptual models. I think the biggest problem that we have is people that actually start to believe in the models, okay? And this could be that simply because the conceptual model of we don't know that it's even the right model. So that's about all I'm going to say about that right now. Belief is not necessarily a good thing.

Good versus adequate science. It's hard for us to tell when we have good or adequate science. This was a point raised by John Garrick earlier. As a regulatory agency, we aren't looking to set the world on fire with science. On the other hand, it's difficult to get really high quality scientific investigators to commit to doing adequate science. They want to resolve the problems, and they want to resolve them well. I think we try to design programs that have good science at their heart, and then we look for a place to end it before we hit diminishing returns and start polishing the results to meet scientific curiosity.

Collaborative R&D. George stopped me earlier, and I'm going to at least say a few remarks about collaborative R&D.

CHAIRMAN HORNBERGER: I'm going to stop you in a couple minutes again too.

MR. OTT: Okay. Well, I've only got -- this is my last one. We have started a working group on conceptual model and parameter uncertainty under this MOU on multimedia environmental research that George stopped me from talking about before. This particular working group involves six federal agencies, involves the U.S. Geological Survey out of

George Leavesly in Colorado, involves EPA, involves the Department of Energy, the Agricultural Research Service and the Corps of Engineers. And they are developing a plan, and hopefully when we get the final phase two proposal in, which will be a much more significant document than the old one, than the phase one proposal, we'll bring that to you and let you see what we plan to do over the next four years in cooperation with these other federal agencies, both on parameter uncertainty and on conceptual model uncertainty.

The same MOU has another working group that's working on these large platform models that are supposed to deal with complex sites. The phase two proposal from that working group is also due in the next month or so.

We're planning additional working groups. The MOU itself is a framework, and it's gotten the agencies to commit to cooperating together. It won't do any good unless we get scientists to come up with ideas to work together, and that's what we're working on right now. We're pushing to get working groups started that will have these scientists working together. The working groups themselves are limited to federal employees, but support work for those working groups is not limited to federal employees. We can involve contractors in that as well. So there are going to be opportunities for places like the Center for Nuclear Waste and Regulatory Analyses for their experts to work with and contribute to these working groups, and I think we will try to do that as much as we can.

We've already mentioned the NEA sorption project. There are, I think, 13 nations working on that project. We, ourselves, have four working groups, three working groups that are going to be working on modeling test cases of sorption parameters. And encouraging Jim Davis, the USGS PI for our work on that to develop another working group proposal under the MOU on geochemical issues. I'd like very much for the agencies to start combining and coordinating their understanding of chemical processes and natural systems. EPA, certainly, with regard to contaminated chemical systems, the USGS has been helping them. The Corps of Engineers has a tremendous amount of information from contaminated military sites. I think there are tremendous potentials here for collaboration that's in the spirit of the recommendations or discussions that were here earlier today. That's it. You don't even have to stop me.

CHAIRMAN HORNBERGER: Good. Thank you, Bill. John?

MR. KESSLER: Just a quick question, Bill. The Apache Leap work, did you ask DOE to help fund that work?

MR. OTT: We gave them the opportunity to work at the Site. I mean we can't just ask them to fund something. And they did fund some work at the Apache Leap Site, and they got some benefit out of it. There is a whole lot of core that was collected for that Site, and the University of Arizona was recently asking what they do with it. And we asked DOE whether they wanted it, and they said no.

MR. KESSLER: Okay.

MR. OTT: So to a certain extent, the opportunities at Apache Leap are closing out. We've still got some work going on there, but it's at the near surface. That's one of those things that you don't like to see happen but it did.

CHAIRMAN HORNBERGER: Any other comments, questions for anyone? Jane?

MS. LONG: I just wanted to --

CHAIRMAN HORNBERGER: Have to use a microphone, sorry.

MS. LONG: Oh, okay.

CHAIRMAN HORNBERGER: It's not for us to hear you, it's the recorder.

MS. LONG: I know you don't want to hear me. That's okay, George.

(Laughter.)

I just wanted to comment on conceptual modeling uncertainty. There are at least a couple ideas that are -- we working on when I was at OBL that I think are interesting. One of the ways to get after conceptual modeling uncertainty might be to look at inverse methods where you look at the data and then try to use an inverse process to determine what the model is that explains all the data simultaneously. Traditionally, when that's done, the principle of parsimony comes in, and that's a principles that says use the simplest possible model. And, therefore, what you do is you actually impose a conceptual model, and based on that conceptual model you do the inversion.

Another way to do the inversion is to allow the model that you are inverting the base model to be as conceptual model-free as possible. In other words, instead of putting in permeability and porosity, the parameter that you put at each node is how fast can something go from A to B? And instead of saying, "I think this is the basic structure, and I'm inverting for the parameter that applies to that structure," you allow the structure to become part of what you're inverting for.

So to the extent that you perform these inversions, they're going to be non-unique, and you're going to get a lot of them. And that distribution of models that you get gives you some idea of what the conceptual model uncertainty is. It's not a very popular form of research, and when we were doing it, we got a lot of criticism because it wasn't parsimonious, but I do think that hits right at the problem.

CHAIRMAN HORNBERGER: Budhi?

MR. SAGAR: I'm intrigued at Michael's idea about going back to the low-level waste sites, dig them up and make some observations. Is that even a practical idea in the sense that you would have to worry about the workers getting dose which is a necessity and that you might be -- this is human intrusion, in a sense, that -- I mean it's a very good theoretically, but in practical sense, does it really -- can it pan out?

MR. RYAN: Yes. You know, I think you can do it.

CHAIRMAN HORNBERGER: Excuse me, Michael.

You have to use a microphone.

MR. RYAN: Oh, I'm sorry. I'm guess I'm not sure that I'd just pick a low-level waste site per se. I'd want to look at low-level waste sites. Parts of those might be appropriate, parts might not where there was maybe irradiated hardware. I'd want to look at D&D sites, other opportunities like sites where there's been a record of decision for radioactive material, and there's a monitoring plan in place and other things.

I don't know that I'd start with a low-level waste site, but there's certainly an opportunity there if there is monitoring and modeling that exists and there's the opportunity to see something moving. I mean that's certainly something. Whether it means you intrude right to the waste as an initial enterprise, I don't know. But at some point, you know, we're digging up sites, we're decommissioning research and test reactors. Our recent test reactor project found that the concrete substructure was in fact leaking into the aquifer, things like that. And maybe are those missed opportunities, where if there was an environmental component to the study, we could have gained something.

So I don't know that I would start with

the notion let's dig up a low-level waste site until I said here's the list of all the kinds of sites. Where can I maximize my opportunity to learn something important and useful for the money I'm spending? Maybe it is a low-level waste site, maybe it isn't, but I'd sure want to -- I think it's well worth the exercise of trying to make that list of where the opportunities are.

CHAIRMAN HORNBERGER: Of course we could point out that there at least one or two DOE sites where some radionuclides have migrated and --

(Laughter.)

MR. RYAN: Exactly. Sure. And the monitoring's already going on.

CHAIRMAN HORNBERGER: And the monitoring's already going on. And the other thing, of course, we could point out that those of us who know something about something post-audits that have been done on groundwater models know that the thing you're going to learn is probably not going to make you very happy.

Let's see, Mal and then Milt.

MR. KNAPP: Just, while I applaud Mike's idea, I think it would be great if it could be done, one of the other practical matters is simply the responsibility and whether or not the present owner or previous owners of the site might, and frankly very legitimately, fear the legal consequences if anything were to happen. And I suspect more likely a site that is currently being decommissioned is going to be more fertile in terms of just getting the willingness of the property owners to cooperate.

CHAIRMAN HORNBERGER: Exactly.

VICE CHAIRMAN WYMER: Yes. It's certainly hard, in any event, to get the participation, but if somehow you can create an opportunity for everybody to gain something out of it, that's the way to try and approach it.

CHAIRMAN HORNBERGER: Milt?

MEMBER LEVENSON: The question I have, which got triggered by what you guys have just said, is that our primary concern in validating the model doesn't arise because there's a radioactive atom there, but much more important is the type of soil and terrain. And could we learn even more by selecting a location even if it what we're looking at is a municipal dump, which you could model and check and avoid the problems of radioactivity and find strata that are more relevant to what we would like to talk about?

CHAIRMAN HORNBERGER: Just perhaps a point of interest for those of who aren't hydrogeology oriented. The U.S. Geological Survey has, for many years, had what they called their toxic hydrology program, and one of their current projects is at the Norman, Oklahoma Landfill where they're doing all sorts of things, just as you mentioned. The other thing that you might recall is that there's something called the Nevada Test Site, there's the Beaty Low-Level Waste Site, there's all sorts of monitoring that has been done, even at sites where vadose zones are important.

So I'm not sure that it's that we don't have any other information out there. It's a question of whether or not --

VICE CHAIRMAN WYMER: It's being used.

CHAIRMAN HORNBERGER: -- well, or how pertinent it may be to a specific -- there are lots of smart people around who know that these things exist.

MR. RYAN: Yes. Part of it's the modeling question. I think the relevancy of the environment is critical there. But the other part of it is the inventory and the source term and what gets out, and that's a little bit less dependent on the site. It's certainly dependent on the hydrology, but it tends to be a little bit more site independent, and I think those kind of sites are where the decommissioning sites and maybe some of the others could be of benefit.

CHAIRMAN HORNBERGER: Did somebody up here have a -- Bill?

MR. OTT: I was just going to observe that we do have political difficulties occasionally getting access to private sites, but we are also able to get around those. The slag research that we've done, we visited a number of sites that we had to go through meetings with lawyers and technical staff and all that kind of stuff to get access to the sites, and sometimes we got stopped at the ninth hour, at the 11th hour. We were getting ready to go out there, and the head office comes back and says, "No, we don't NRC doing research on our site."

On the other side, we've got the NADA reader program going where USGS has been out in Colorado on a DOE remediation site for three years drilling holes all over the place and taking a tremendous amount of data. So we've had mixed success at going to contaminated sites. But in addition, the observation about USGS in particular being involved with a lot of contaminated sites is particularly relevant.

The surface complexation modeling that we've been doing with regard to uranium is work that parallels work that the same USGS investigator did on chromium contamination at a New Jersey waste site. So there is a commonality of interest, and this is one of the reasons why that MOU is so interesting to us right now and has so much potential.

CHAIRMAN HORNBERGER: Okay. We're going to have to close this off pretty shortly. Any other comments or questions from anybody who has patiently sat here all day? No takers? Jane? No.

Actually, we do have another item on our agenda, so what I'm going to do is call this workshop closed for the day. We are going to have time to have further discussion even on some of the topics that we've raised today when we restart tomorrow.

We will have a five-minute recess, and then we will reconvene to have a presentation on performance confirmation.

(Whereupon, the foregoing matter went off the record at 4:39 p.m. and went back on the record at 4:53 p.m.)

CHAIRMAN HORNBERGER: We need to reconvene, please. I have to find out where I am on my other schedule so I don't make a mistake. I see that Jeff is undoubtedly going to do some kind of presentation, but I want to make sure I know what it is before he starts. Okay. So this is a presentation on the performance confirmation, and the staff has been, I guess, having some discussions and has something to share with us. So, Jeff, why don't you take off?

MR. POHLE: Okay. Thank you. Jeff Pohle, the NRC staff. We'll talk a little bit about performance confirmation today. First, we'll say a few things on the NRC perspective on performance confirmation, then there will be a section where I've taken excerpts from Part 63 that deal with performance confirmation and kind of close up with planned activities in the future.

I'll move somewhat quickly through this first part. Given I have a lot of excerpts from Part 63, we probably don't need to worry too much about specific words or anything when I discuss it, because we'll actually get into the definition that's in the rule later on.

What is performance confirmation? Broad-based technical program of tests, experiments and analyses to be conducted by DOE to confirm repository design and performance over the time frame from site characterization through permanent closure. And that time frame is specified in Subpart F, Section 63.131 Paragraph B.

What is the NRC's role? Well, first, we require that DOE do a performance confirmation by rule and that is integrated into our concept of a multi-step licensing process. The NRC will review the program and oversee its implementation by DOE ultimately to verify the subsurface conditions and the long-term repository performance.

Our main objective is to ensure safety is maintained by the Department of Energy, and it's part of a process to build public confidence. And what do we need to do that? Well, an important aspect is to maintain our capability throughout the long-term process. We want to identify what the risk uncertainties are and then to probe certain areas to identify any inadequacies or gaps in DOE's program. And, obviously, a lot of our support will be coming from the Center.

How will we do that? Well, there will be a series -- a number of activities through reviews, our own evaluations. Inspections are going to be a part of this program down the road. It was such a long-term program that we'll actually have to get in the business in the not too distant future of writing what the inspection procedures are, what it is we want to look and get people ready to do that.

We will continue, I'm sure, to do our own performance-related assessment through TSPA. And then we'll do some independent experimental investigations, whether confirmatory or exploratory, however one wants to define that. Part of the process actually is to identify what the topical areas should be.

Now I'll kind of walk through the rule. I've pretty much pulled out every area in Part 63 where performance confirmation is mentioned or even conceptually discussed without actually using the words in one case. The definition is straight from the rule. It's the program of tests, experiments and analyses as conducted to evaluate the adequacy of the information used to demonstrate compliance with the performance objective that's in Subpart E.

I think this definition fits Tim's concept that he discussed earlier, what is performance assessment, where we had a very broad review. It's just not a series of computer runs, it's all the underlying information and the works and experiments and the methodologies that went into gathering the data that's used to support that. So, basically, when we talk about evaluating the adequacy of information, that's very broad. It's flexible, but there's really no limitations on us.

Part 63.21 lays out the required contents of a license application, and this would be the application that would come in to support the decision for construction authorization. And it would be our task to review that.

Now, subsequent to construction authorization, during construction and prior to issuing a license to receive and possess, 63.24 makes clear -- I highlighted in Paragraph B.3 -- that in the updates of the application, the results of the program carried out to confirm the adequacy of designs, conceptual models, parameter values and estimates, performance of the geological repository should be incorporated into that. This is a little broader than just to confirm the adequacy of the information. It starts bringing in concepts like conceptual models, trying to establish a philosophy that we're not necessarily limited to engineering parameters -- this number is within this range.

The following page I've added two sections which relate in a way to the performance confirmation program. There seems to be some interest on DOE's part to do research and development on their own apart from a performance confirmation and apart from the oversight of the NRC. Part 63 allows DOE to do that. They can carry out tests, experiments at the site that are not discussed in the SAR, but it lays out roughly eight criteria under which they can do these tests. And that's all laid out and specified in the rule, the various items that have to be addressed in order for them to do so.

And, finally, it answers some recordkeeping requirements. So it's something that could be inspected against in the future. While we're not directly overseeing, let's say, some of their work for whatever reasons they choose to do that, we can see that as long as inspected in the context of have these criteria been met, like it doesn't affect the performance of the repository, et cetera.

And 63.74 basically is the paragraphs which gives the Commission the authority to require the DOE to carry out tests at the site, as well as allows the Commission to carry out its own tests at the site. Now, this Section's broken into two parts. One part's basically a paragraph that references that performance confirmation program is one of the tests under 63.74.

Then there's a few paragraphs that talk about other tests, which are outside performance confirmation program and whether DOE -- we would require DOE to test on these items or the Commission to do its own. And, basically, it deals with we could perform tests of radioactive waste, radiation detection and monitoring instruments and other equipment and devices used in connection with receipt, handling or storage of radioactive waste. So this is an area of tests that really doesn't fall directly under Subpart F, performance confirmation program, but there could be other tests that the Commission requires DOE to do.

Now, we haven't thought out in a formal way what our needs are in that area, but I could conceive that, at least through some long-term thinking as to what we're looking at in that area, under performance confirmation as an addendum to a plan we're trying to develop, there's some initial thoughts. At least who should look at this and how we're going to deal with it. I'm sure DOE will want some guidance in that area too.

And then, finally, for the license amendment for permanent closure, the update assessment must include any performance confirmation data collected in the program acquired by Subpart F and pertinent to compliance with 63.13, which are the post-closure performance requirements. And I believe the last paragraph in 51 probably refers to the environmental impact statement. They have to be updated a final time considering any new information pertinent.

And 63.102 is just a general discussion in the concept section. This is not really a requirement per se. It's not, "DOE or the Commission shall do this," it was just more of a philosophical discussion. Basically, the idea is that it's more of a focus on post-closure performance.

In 63.111, this is really in the subpart that deals with what we call pre-closure, but performance confirmation comes into play, that when you design the GROA you need to consider in advance that you're going to have to do a performance confirmation program, and the design must allow you to implement that plan. Just a look ahead.

And, finally, the requirements for the performance confirmation program are outlined in Subpart F. Basically, four different sections: general requirements, more specific requirements on confirmation or geotechnical and design parameters, design testing and monitoring and testing the waste packages. I will only just discuss the general requirements, again, try and look at it from the broad perspective, what we're after.

Two paragraphs: Must provide data that indicate, where practicable, the conditions encountered and changes are within the limits assumed. And the natural and engineered systems and components required for repository operation and that are designed are assumed to operate as barriers after permanent closure are functioning as intended and anticipated.

Our thinking there was, again, to key it in the post-closure performance and what's important, and certainly what DOE defines in its safety case is the barriers that are important to meeting the standard. I mean that's where the focus should be, presumably.

Planned activities, generally, this coming fiscal year. We've been tasked with putting together a performance confirmation action plan. The idea was just a general -- try and take some long-term vision, look down the road what it is we need to do. And I'm working with English Pearsy at the Center to come up with this plan, and, generally, we've been going back and forth on what the scope and objectives should be. And things can grow very rapidly without much difficulty.

A good part of this will be the technical in that we want to get to a point, much as what the ACNW's doing now. What are the topical areas, the technical areas that should be our focus or DOE's focus, and we haven't gotten to the point to polling the technical staff to start working through that, iterating through that input yet. But whatever results this Committee comes up with could be a factor that we would incorporate into that plan.

We also need to familiarize staff with performance confirmation. Generally, it's not something that the staff has dealt with on any routine basis, to date. And we also need to interact with DOE. Now, the review plan for Yucca Mountain should be coming up, let's say, second quarter, and I would think that after that would be an opportune time to get with DOE. At least we would have the benefit of comments on the review plan. Now I know in advance there's a ways to go. As we go further and further into more detail to evaluation of what we want, the review plan itself would likely become more detailed.

And it's my understanding that DOE is

currently working on a revision of the draft performance confirmation plan, and I think the tentative date I heard was August of 2002, but that's certainly subject to change, I'm sure.

CHAIRMAN HORNBERGER: Just a quick comment in passing, Jeff. Given the common understanding of the initials PC, you might to avoid using them in your slides.

(Laughter.)

MR. POHLE: Oh, okay. Right. Point taken.

MEMBER HINZE: I thought it meant post-closure.

MR. POHLE: And just some points on the plan. Use the insights from our own calculations, DOE's calculations, the various levels, the models, the laboratory studies, the experiments everyone's done, field investigations to try and identify and prioritize an initial set of elements for NRC oversight. Regardless, in a sense, I think the first task would be to identify what should be done and then worry about who. Clearly, we'll be focused on the importance of safety, waste isolation, risk, uncertainty.

And here comes the part where I'm still

struggling with. I'm sure Management will be interested in, well, what are the administrative outcomes of the technical work? How do we want to manage this program in the future? I'm sure that's something we'll have to deal with.

And, certainly, in that sense, some of the things we'll probably be discussing and working on will be beyond the technical, like that example on tests. Well, someone needs to look at it, and it will probably end up being under this umbrella, at least internally.

And a plan is a plan. It could be -- you know, things change, just like you would expect DOE's program has and would change in the future as new information becomes available. You'd be revising what it is you want to focus on through time as new information is available. And that's it.

CHAIRMAN HORNBERGER: Thanks very much. Questions? Milt?

MEMBER LEVENSON: Yes. I've got one, Jeff, on your Slide 16, which is 63.111. What's the time frame or the definition of "through permanent closure?" Does this imply that you're going to be doing testing, measurements, et cetera, after permanent closure? I'm just wondering what the words "through permanent closure" means?

MR. POHLE: No, no, 63.111 was always intended that these requirements are permanently Part 20, the operational requirements, that end at closure.

MEMBER LEVENSON: So it doesn't go beyond when it's permanently closed.

MR. POHLE: Correct.

CHAIRMAN HORNBERGER: Ray?

VICE CHAIRMAN WYMER: Let's see if I understand what you just said. There will not be performance confirmation after permanent closure?

MR. POHLE: Well, let's say the regulation requires a performance confirmation program that begins in site characterization and continues to permanent closure. Beyond that I can't say right now.

VICE CHAIRMAN WYMER: Seems to me one of the most significant parts of the performance of the site is whether or not it holds the stuff in there for a few thousand years.

MR. POHLE: Yes. There is a -- well, that's probably another item to be added to my scope. In the section of the rule that deals with the license amendment for permanent closure, the DOE will be required to come in with plans for post-closure monitoring of the geologic repository. Now, I'm sure nothing exists right now that goes beyond that as to what our needs are. So that's another item we'll probably have to take a look at.

VICE CHAIRMAN WYMER: That's the most significant part of the performance is whether or not it holds the radioactivity.

MR. POHLE: True.

CHAIRMAN HORNBERGER: But the concept of permanent disposal does not include permanent monitoring.

VICE CHAIRMAN WYMER: No, but there are other cases in the DOE --

MEMBER GARRICK: Are you thinking of stewardship?

VICE CHAIRMAN WYMER: Well, approach that go well beyond that. Let's hear what Tim has to say.

MR. MCCARTIN: Yes. Tim McCartin. Well, I mean there's two separate purposes here, though. I mean the performance confirmation program is getting information up to the time of closure --

VICE CHAIRMAN WYMER: By definition.

MR. MCCARTIN: Yes -- to get confidence in the decision that the Commission has made. That's over and done. Now, there is a post-closure program for monitoring of the repository that goes on.

VICE CHAIRMAN WYMER: That sort of goes with performance.

MR. MCCARTIN: You've made the decision to close, so you're clearly -- you can't use future information to help you with the decision. It's a separate decision point. So performance confirmation is at the time of permanent closure to assist that final decision.

MR. POHLE: Originally, a performance confirmation was always just inextricably combined with the decision to retrieve.

VICE CHAIRMAN WYMER: Okay.

MR. POHLE: And once you reach that decision point, "We will not need to retrieve, we will close the facility," then you've kind of went beyond that administrative concept.

VICE CHAIRMAN WYMER: I'd stick a couple more adjectives in front of performance confirmation, like -- or behind performance confirmation, "until closure" or something like that.

MR. MCCARTIN: But your concern that would there be monitoring after closure, the answer is yes, the regulations do require a program for post-permanent closure monitoring, but it's not to assist the decision to close, because you've already made it.

MR. POHLE: And Part 63 does not really prescribe what that monitoring is to be.

MEMBER LEVENSON: In other words, the title to this really is performance confirmation to authorize closure.

VICE CHAIRMAN WYMER: Yes.

MEMBER GARRICK: Or performance confirmation during operations.

VICE CHAIRMAN WYMER: Yes.

MEMBER GARRICK: Is this confirmation plan modeled after any other activity that NRC has ever done before?

MR. POHLE: The action plan?

MEMBER GARRICK: The performance confirmation plan.

MR. POHLE: Not to my knowledge. I've never seen an example that I could use to work with or anything like that. I think it's pretty unique. Certainly, at this time scale is involved.

MEMBER GARRICK: Does it kind of substitute for the oversight function that's performed on nuclear reactors, inspection kind of activity?

MR. POHLE: Well, I guess I'm not really familiar with the reactor side of the business, so I probably couldn't deal with that metaphor.

MEMBER GARRICK: There seems to be a number of different ways to do it. EPA, for example, on WIPP recertifies WIPP every five years. That would be another way to do this. I was just looking for what was the origin of this particular process and whether or not it was inspired by previous activities of the Agency or what have you.

MR. POHLE: This is reflecting from memory of the ancient documents from the early '80s. I think it was recognized that this was unique in terms of other licensed activities we've done at the time scales involved. And the whole difficulty in dealing with what constitutes proof and how you develop confidence in your decisions and reasonable assurance that this was developed to be part of the process which would allow decisions to be made as you went through time and allowed you, ultimately, a way to back off from the final decision, I mean retrievability. So you can't remove the requirement for doing this type of program from the decision on whether do you need to retrieve the waste or not.

MEMBER GARRICK: Okay.

CHAIRMAN HORNBERGER: So, Jeff, is where you are now -- I'm trying to figure out where you are now and where you're going. Did I understand your last slide correctly that you're headed toward an action plan, but you're not there yet?

MR. POHLE: Correct. Yes. That will be in the operations plan for the Center. That's deliverable for, I think, September -- for the end of this fiscal year.

MEMBER LEVENSON: Is this the only proposed inspection and surveillance program of the operations or will there be another one?

MR. POHLE: No, no, no.

MR. REAMER: Bill Reamer, staff. There will be other aspects, components of the inspection program to assure compliance with the Commission's regulations. This is one element of the Commission's regulations, the performance confirmation plan, that will be part of the inspection program.

MEMBER LEVENSON: Will this be potentially duplicating other things or will these be coordinated programs?

MR. REAMER: I can assure you it will be coordinated.

CHAIRMAN HORNBERGER: I would not have expected a lawyer to say anything else.

(Laughter.)

MEMBER LEVENSON: But he used the word

"assurance" and not "insurance."

(Laughter.)

CHAIRMAN HORNBERGER: Other questions? Comments?

MR. LARKINS: Yes, a quick question.

CHAIRMAN HORNBERGER: John.

MR. LARKINS: On the last page, you say you're going to use TPA calculations to identify an initial set of elements for the NRC's performance confirmation oversight. Now, DOE's already put together a preliminary plan, and they've listed a number of experiments and instrumentation for measurements and things like that. How do you get agreement on what's reasonable, what's acceptable? Are you going to have to set up some kind of acceptance criteria? What are your metrics to --

CHAIRMAN HORNBERGER: Jeff?

MR. POHLE: You're looking this way. Well, that is going to be a mix. I mean there are certain things you can put in a review plan early, starting from the regulation itself -- you know, it was A, B, C provided. In terms of has DOE specified what it is they need to do and why, I mean, clearly, with the Center's help, we'll have our own opinions on that. Right. Now, ultimately, you would take it down to reviewing the methodology used in the experiments when you're at that point. Clearly, we're at the point where we need to be interacting and reaching agreements with DOE at the highest level and work our way down.

When we -- I had Jim Manners, and I asked DOE to find a date are they revising the draft plan they have and do that, and would it mirror our detailed comments today? And the opinion they gave back is, well, at a higher level, the process is something would be their priority to deal with, but for us to, let's say, comment on the detailed experiments themselves and the draft plan is probably not worth our time now, given that that's in revision. So that's kind of -- I can see this as something we would deal with -- begin to deal with it in a pre-licensing time frame as quickly as we can, maybe down to a very detailed level.

MR. LARKINS: So in a sense, you're going to negotiate interactives to what really should be included in DOE's performance confirmation program.

MR. POHLE: Right. And, of course, we'll be dependent to a large degree on DOE's safety case, you know, what are the barriers and how they choose to define the system that functions to isolate the waste.

MR. MCCARTIN: Jeff, if I could just add one point.

MR. POHLE: Sure.

MR. MCCARTIN: As with all of 63, we have tried to provide DOE a lot of flexibility. Performance confirmation program would have the same flexibility, and as Jeff's indicated, it really is the risk significance of the barriers that would drive our review of the DOE plan. Barriers that are very important, we obviously look at with more rigor in terms of the performance confirmation plan. Things of lesser importance -- and so it would, as with the rule itself, somewhat risk informed. But the flexibility is with DOE, both in identifying the barriers they're relying on and then just determining what the right appropriate level of confirmation is.

MR. LARKINS: Yes. It just seems like at some point you're going to have to develop some set of acceptance criteria. These things -- this is an acceptable set of things that you need to measure and if things come out in this range, that's acceptable. But you're probably at an early stage.

MR. POHLE: Well, I don't -- when the review plan comes out there are criteria in there that lead one down the path in making decisions to focusing on what's important. Now, I don't necessarily see the day where I would describe a delta of such and such for parameter X in the review plan necessarily. That might be a step too far. That doesn't mean one doesn't discuss it with the Department of Energy and reach an agreement on things, any differences. That's another -- at that level of detail.

An interesting comment was made at EPRI's workshop -- I think it was Budhi made it -- just philosophically, while we're focusing on risk significant issues. And he brought up climate as an example. It may be a no never mind now, but should the DOE program at least be keeping an eye on the literature over the next long period of time should something change that would change one's conclusions about a given THEP, which is today unimportant and can be excluded.

We've so focused on the risk significant things that how does items like that fall into the picture? What weight do they have as something to do? What level of commitment do you expect DOE to make on items like that? Something to think about.

CHAIRMAN HORNBERGER: Other questions? Comments? Okay.

Thanks very much, Jeff.

I think we've reached the end of the part of the meeting that needs to be recorded, so we won't need the recorder any further.

(Whereupon, at 5:25 p.m., the ACNW Workshop was concluded.)

CERTIFICATE

This is to certify that the attached proceedings before the United States Nuclear Regulatory Commission in the matter of:

Name of Proceeding: Advisory Committee on Nuclear Waste 130th Meeting

Docket Number: (Not Applicable)

Location: Rockville, Maryland

were held as herein appears, and that this is the original transcript thereof for the file of the United States Nuclear Regulatory Commission taken by me and, thereafter reduced to typewriting by me or under the direction of the court reporting company, and that the transcript is a true and accurate record of the foregoing proceedings.

Pippa Antonio

Pippa Antonio
Official Reporter
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