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NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE

(ACNW)

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162nd MEETING

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WEDNESDAY,

AUGUST 3, 2005

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ROCKVILLE, MARYLAND

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The committee met at the Nuclear Regulatory Commission, Two White Flint North, Room T2B3, 11545 Rockville Pike, at 8:30 a.m., Michael T. Ryan, Chairman, presiding.

COMMITTEE MEMBERS:

MICHAEL T. RYAN, Chairman
ALLEN G. CROFF, Vice Chairman
JAMES H. CLARKE, Member
WILLIAM J. HINZE, Member
RUTH F. WEINER, Member

ACRS/ACNW STAFF:

LATIF S. HAMDAN, ACNW Staff
MICHAEL L. SCOTT, ACNW Staff

NRC STAFF:

THOMAS NICHOLSON, Office of Research, NRC
JACOB PHILIP, Office of Research, NRC

PRESENTERS:

LES DOLE, Oak Ridge National Laboratory
EDWARD GARBOCZI, NIST
DAVID KOCHER, SENES, ACNW Consultant
ANNE SMITH, Charles River Associates International
VERNON ICHIMURA, Chem-Nuclear Systems
CRAIG BENSON, University of Wisconsin
RANDY POSTON, WDP & Associates

I-N-D-E-X

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

(8:35 a.m.)

CHAIRMAN RYAN: This is the second day of the 162nd meeting of the Advisory Committee on Nuclear Waste.

My name is Michael Ryan, Chairman of the ACNW. The other members of the committee present are Allen Croff, Vice Chair, Ruth Weiner, James Clarke, and William Hinze. William Hinze -- Dr. Hinze is out. He'll be here shortly. He had a brief commitment this morning.

During today's meeting, the committee will conduct a working group meeting on waste determinations. Latif Hamdan is the designated federal official for today's session.

The meeting is being conducted in accordance with the provisions of the Federal Advisory Committee Act.

We have received no written comments or requests for time to make oral statements from members of the public regarding today's session. Should anyone wish to address the committee, please make your wishes known to one of the committee staff. It is requested that speakers use one of the microphones, identify themselves, and speak with sufficient clarity and volume so they can be readily heard.

It is also requested that if you have cell phones or pagers kindly turn them off or place them on mute. Thank you very much.

I will now turn the meeting over to Mr. Croff for the remainder of the day.

I'd also ask that for the benefit of our recorder that if you use an acronym or refer to an individual's name, that we make sure we say that clearly and slowly, so he can capture a few of those. He's got a couple of names from yesterday's transcript that we might want to help him identify, and so forth. So just recall that.

And also, folks, if you are speaking or answering a question, be sure to either pull one of the microphones near you, towards you so you can clearly be heard, or make sure your lapel mike is on.

Thanks very much.

Allen?

VICE CHAIRMAN CROFF: Thank you, Mike.

To refresh everyone, we ended yesterday with Les Dole discussing the durability of cements and grout waste forms based on essentially practical experience. While these insights are useful and necessary, it's difficult to project this experience for long times -- as Les described yesterday, maybe on the order of decades.

Our next presentation will discuss progress concerning the ability to predict the properties and performance of cementitious materials from fundamental principles, which offers some hope of extending predictions for much longer times.

The presenter is Dr. Edward Garboczi. He's the leader of the Inorganic Materials Group in the Building and Fire Research Laboratory at the National Institute of Standards and Technology. This group has been at the forefront of material science for concrete for over 20 years, especially in relation to durability and computer modeling.

In conjunction with the industrial members of the virtual cement and concrete testing laboratory consortium, this group at NIST developed the virtual cement and concrete testing laboratory software, and that's been there for more than five years. This is a tool for predicting the performance of concrete from fundamental material science considerations.

Ed, go to it.

DR. GARBOCZI: Okay. I'm ready now.

Okay. I want to talk about prediction, prediction for cementitious materials. As somebody quoted yesterday by Yogi Berra, prediction is kind of hard, especially about the future. Just some pretty pictures about concrete, from the Pantheon 2000 years ago, to Hoover Dam, to a nuclear reactor, which I forget where it is so don't ask me where it was.

And as Allen said, I want to summarize the status of ongoing efforts to predict the durability of cementitious materials for first principles. And I'm doing a general sort of talk. I'm thinking about concrete in basically four ways -- concrete that the concrete industry uses, and they're talking about, you know, 50- to 100-year durability, they're talking about concrete for nuclear powerplants, building nuclear powerplants. We're talking about maybe -- all together maybe 100-year durability.

We're talking about reactors for canning reactor waste, low level or higher level. There we're talking about hundreds of years probably. And also talking about predicting durability of cementitious waste forms like Les talked about yesterday, and there you could be up to thousands of years.

So I'm not -- I'm looking at the general picture, not just any one of those four topics, but more the general picture, use the concrete in general practice and in the several areas of the nuclear industry.

Okay. Where we're from, we're a federal group, and our main goal really is to provide the scientific and technical foundations for performance-based selection and use of concrete. That pretty much sums up what we do.

The way we do that is by improving the material science base for the standard tests in industry, especially performance-based tests. We started this long ago, 20 or 30 years ago, and quickly it brought up the point that concrete is so complex you have to go to computational material science to make a prediction.

You just can't do experiments alone. You have to do computational things to understand this complex material. So part of our work is developing and validating computational tools for industry and government.

Just a reminder of how complex concrete is, whether it's the cementitious waste forms or grout or full concrete. We're using it at the meter scale, but there's important microstructure are the micrometer scale, at the millimeter scale, and probably the most important microstructure is at the nanometer scale.

Most of our work which I'll tell you about today is in this area. Probably the key to predicting durability, though, is in this nanometer level, the process of the nanometer scale, the structure of the nanometer scale.

We're working on that now, just really starting. As I said, most of our work has been in this area, but you really need all these scales to get a good durability prediction, or a prediction of anything for that matter.

Okay. Just in general, you want to predict durability for first principles, you need to know things like the transport and reaction mechanisms of ions through concrete. That's very general, but you need to know that. That involves thermodynamics as well for long-term durability.

You need to know how the material microstructure and properties interdepend. So if cracks open up, how it changes the properties, how the properties bring in new stuff, how that changes the microstructure, and so on. It's a revolving cycle. You need to understand that, how things change with time.

And also, you need to know the expected service environment. Concrete is sensitive to some things, not sensitive to other things, sensitive to various quantities, not sensitive to lower quantities. You need to know what you're going to be exposed to over the time you're looking at. It makes a big difference.

And also, predictions -- you can't have predictions by themselves. You have to have accelerated test data to go with them.

I'll mention some of -- the status of this now. It's not so good right now. It could be a lot better.

Okay. And as Allen mentioned in his introduction, our main effort in prediction is this tool basically, the virtual cement and concrete testing laboratory where we're trying to use known physics and chemistry material science to fundamentally predict properties of concrete, not just empirical relations but fundamentally predict what the material is going to do, going to be like.

And you have to have experiments go into that, you have to have computations go into that, and I'm going to tell you a little bit about both of them as a sort of example of what things you need for predictions.

Okay. Just so you know, we're not doing

this in a vacuum. We have a consortium where we're working closely with the cement industry, the chemical people who make chemicals that go into concrete, the people who make the aggregates, manufactured aggregates to go in the concrete, and the people who mix it all together and deliver the concrete.

So we're working -- we have a strong consortium with them, so we're working closely with them because they're the people who make the stuff that people use.

And just a quick look -- this is softer. It looks like right now it's a fairly decent gooey form that you basically build of concrete virtually and test it in various ways. There's a degradation part, which is not real great right now, but it could be a lot better. And I'll tell you something about that as well.

Okay. Let me show you now some of the experiments and computations that go into predicting durability or predicting anything. You have to characterize what you have. You don't know what's going to happen to what you have if you don't know what you have in the first place.

So you have to know a lot of things about the cement, the chemistry, the particle size. You have to know about the aggregates, what kind of shape aggregates you have, whether they're round or jagged or -- you have to know what kind of particle size distribution you have for cement and aggregates.

For making concrete, you need to know the rheology, how concrete flows. In heavily reinforced structures like nuclear containment vessels, you often find macro defects, big holes where the concrete didn't get to in this heavily reinforced structure. That's a function of rheology. And so if you knew the rheology better, you'd be able to predict and understand how to formulate the concrete to be able to get into all parts of the rebar structure.

And for higher temperature applications, you need to measure things like temperature and pore pressure inside the concrete. We're interested in concrete in kind of warm applications in the nuclear industry, and some concretes -- and they get real hot. They supposedly spall. You need to understand why that happens to be able to build in prediction models.

The kind of characterization of cement -- talking about briefly is here is cement mixed in epoxy. Just plain cement, just cement as it comes out of the bag, out of the plant. And we do scan electron microscopy, and also do X-ray microprobe to identify the elements. Put them all together, you can identify each particle.

So on a particle base, you can understand the chemistry and the structure of the cement particles, and you need to know that to be able to understand what happens when you make a concrete, and also what happens when you degrade the concrete, because a lot of the structure -- these particles don't always -- often hydrate.

As Les mentioned yesterday, for hundreds of years the cement could keep on reacting, and so some of this structure stays behind for a long time, and that's part of the degradation process. It can be part of the degradation process.

If you know all that, you can build up 3-D models of realistic shapes, of realistic chemistry of particles, and so you have to have that characterization in order to be able to do that. If you can't do that, you're not going to be able to predict much about what the cement is doing as it forms a concrete.

Using X-ray microtomography, X-ray tomography, you can characterize cement particle shape. You can also characterize aggregate shape, so you can know 3-D what kind of shape aggregates you have and how that affects the rheology in particular. And that's a big deal for heavily reinforced structures.

You can measure rheology. You measure at different levels -- at cement paste level, at the mortar level, cement paste with sand, at the full concrete level, at the concrete truck level. You can measure the rheology, and that's a characterization for what the concrete is going to do as you make a structure.

And how you make a structure, what the concrete looks like after it has hardened, is going to affect how it degrades over time. So you need to know that as well.

You're interested in hot applications. This is a fire application, so probably hot -- hotter temperatures than you'll see in nuclear applications. But you can drill holes, put in sensors, and expose to heat and measure things like pore pressure during heating. That could tell you if the concrete is going to crack or going to blow up, etcetera.

This is more short-term high temperature. The nuclear application is more interested in lower temperature long term. The same kind of experiments would apply.

That's the kind of experiments that go into predicting capability. Let me give a few examples of computations that go into making up a predictive capability. We've got cement hydration, and that applies to degradation as well, monitoring rheology, and predicting some properties.

This shows hydration. We take the cement particles and break them up into small volume elements, and then do various models to actually characterize cement chemistry as the particles dissolve and hydrate and grow phases. That's the same kind of chemistry -- qualitative chemistry as concrete degrades. As sulfate ions come in or alkalides interact with cement, that we can also model degradation reactions. Same kind of modeling would apply to both.

We're on our third generation hydration model now. The rest of the world is probably mostly on the first generation now. We're going to -- we've been very successful so far, but not with real kinetics. You need kinetics for durability and time things.

So this third generation we're working on now will have real kinetics in it, and some real thermodynamics in it, has the capability of handling these kind of degradation problems much more realistically than our older models did. That's being worked on right now.

And that's all stuff you don't need to know.

Okay. Monitoring rheology, the rheology of concrete as it flows into heavily reinforced structures really depends on what's there and what the aggregates look like, how big are they, how small are they, how wide a particle size distribution they have, what shape they are, so we can model that and put real shapes in our models, move the particles around, and look at real rheology and real applications.

Okay. I think -- how am I doing on time? I'll skip a couple slides here.

Okay. Now, we're looking at transport in concrete at the micro and nanoscales. The one model we have is called 4sight. That's been developed by Ken Snyder at our lab, working with Jake Philip of NRC for 10, 15 years I think.

This is a model of transport and reaction in the concrete pore solution, as ions come in how it affects -- how it reacts and affects the pore structure. And the original emphasis was looking at low level -- looking at barriers for low-level reactive waste.

These are the fundamental physical chemical treatment of concentrated ion solution. I say that because somebody mentioned yesterday about seven molar sodium and things -- you know, concrete and the application we're interested in has very high ionic concentrations. It's far beyond the normal kind of solution chemistry you learn in a textbook, and so it's very important to build that in.

This model can predict the onset of severe degradation, but the key point here is it's the result of a full research program. It took a 10-year research program to develop this model. It's not a question do we have the tools. It's just sort of slap them down and you've got the thing. This is a heavy-duty research program because of the concentrated ionic solution, and looking at the ions in concrete is a nasty problem. So they were able to develop this, but it took a while.

This model should be extended to -- or can be -- should be extended to radionuclides, interacting concrete pore structures, surface interactions of ions with the surfaces in air, and probably the dirty water chemistry that Les mentioned yesterday. We've been looking mostly at clean water cement chemistry. The same kind of approach I think could be used for the dirty water chemistry as well, though that's a hard problem.

And just lately we have also been looking at microdynamic modeling of the nanoscale, the pore structure of the nanoscale and the calcium silicate hydrate that Les mentioned yesterday. This is the main glue that holds concrete together. So if you're really talking about what's happening to concrete over time, you're talking about what's happening to the CSH over time.

And so we think the modeling approaches there will pay off big, and that's coupled with experimental approaches as well -- neutron scattering, etcetera. So this is probably the key to the future to be able to handle this problem.

Okay. Go back to durability and then just some more general remarks about predicting durability. He needed to know how the concrete got there, so the concrete formation -- we should go do that now with HydratiCA, and that can also work some for the kinetics of concrete degradation as well.

But the biggest thing for concrete degradation you need is to get the kinetics from accelerated experiments. You have to accelerate the correct mechanism. I think Les mentioned yesterday, you know, you accelerate the wrong mechanism you get rotten eggs rather than chickens.

And we think that the use of small samples in a controlled environment and high throughput techniques will help a lot, and I'll talk about them soon. But the accelerated experiments right now in concrete is not a real great situation.

There's lots of standard tests out there for "durability of concrete." When you look at an ASTM handbook, there's all sorts of tests for measuring durability of concrete under sulfate attack, under alkalide silicate reaction, etcetera.

Most of all them purport to the accelerated tests. You don't have to wait 20 years or 30 or 40 years. You can do it in six months or something. But the acceleration is totally empirical. Actually, it's totally empirical. I can't emphasize that too much.

The typical test is you make a bar -- about a foot long bar of mortar or something, you take a bucket of concentrated bad stuff, whatever the bad stuff is, you drop it in the bucket, usually that degradation makes the concrete grow a little bit. You can measure length change every so often, every couple days, every week.

After six months, you're not sure of the answer, so you do another six months, and you repeat endlessly. It's a little bit exaggerated but not a whole lot. These tests -- empirical acceleration and the large samples, uncontrolled conditions, they don't really tell you much about concrete durability.

Industry has done as best they can out of it, to extract what they can out of it. It works sometimes. But if you're looking at controlled durability prediction for tens, hundreds, thousands of years, these aren't going to do it.

There's a hint of helpful change. I think I see the way to -- to getting these accelerated tests on board, so we can have predictive capability. We've been working on an accelerated sulfate attack test. Sulfate ions will attack the concrete microstructure. We've been working with Portland Cement Association, PCA.

We've been able to get small samples, controlled environment, still empirical acceleration -- more active than the old method and much faster than the old method. So a little more fundamental, much faster, and that's kind of contrary to industry. Industry thinks the more basic you do the slower it takes. I think we can show that you get better results, faster results, if you do more fundamental science.

And the reason we're able to do this is you have sulfate coming into a concrete microstructure. We're able to do scanning electronic microscope probes and found that the actual damaged part of the microstructure is only less than a quarter-millimeter. So there's no sense to make a 12-inch long, one-inch wide bar, when all the degradation is just the first quarter-millimeter.

It's a stupid waste of time, so to be able to go to two centimeter long samples, get rid of this big bar, that itself accelerates the results three to five times.

Also, controlled environment -- you don't control the environment here. We found that by controlling the environment we get much cleaner results. That's kind of simple, but it hasn't been done so far in the standard tests.

Okay. The impact of the results is if you control the environment, if you -- smaller samples, if you have high throughput, you do many samples at once, you can have a big payoff. The concrete industry really cares about standard durability.

In a 1,500-year concrete durability, they put a bridge in, they want to be able to guarantee it's going to last 50 years. Long-term durability predictions like NRC wants, you need that as well, to go beyond 1,500 years. Even to go 1,500 years you need correct science. To go beyond that, you absolutely need correct science.

The biggest research need for this area beyond the technology of doing small samples, and that sort of thing, is fundamental work on properly accelerating the correct degradation mechanisms. So I emphasize properly and correct. You've got to find the correct degradation mechanisms you're worried about, and then find ways to properly accelerate those, and then your research results are valid and useful.

So you need mechanistic research. And if you want to extend this to complex chemistry of the WIR applications, like the dirty water, you're going to need that as well because that's even harder. But the same approach will work, I believe, it's just -- it's a more complicated system even than the normal concrete.

Okay. If you have all that -- but I think the models will be in place to predict durability.

Right now we work -- these are sort of the main durability mechanisms we work with -- chloride-induced corrosion rebar, sulfate ion attack, the pore solution interacts with the aggregate and you get cracking. NIH has a lot of these in their parking garages. Leaching is probably the main thing for cementitious waste forms.

We have model-based predictions of some of these, but we don't have good kinetics right now. We think we will have that with this new model, and to get the accelerated test program going that will be there to combine with the models to get predictive capability.

Okay. So like I said, the prediction tools are there. They need to be upgraded and improved. Accelerated tests, experimental accelerated tests need a lot of work, but I think the way is clear where to go, how to go, and -- but combining them you should be able to have accurate predictions.

This is not a two-year effort, three-year effort. This is a long-term effort. But I'm saying the road is clear to how to get there. It's going to take a while to get there, but I think the road is clear toward getting there.

And NRC needs long-term durability

prediction. The concrete industry needs predictable durability. They'll say, "We guarantee this bridge for 75 years." That's the kind of things they need. Here we need, you know, how is it going to act after 1,000 years? So it's a different -- this is qualitative, different kind -- quantitative, different kind of prediction, but it's the same kind of science that goes into it.

Historically, we've worked more with the industrial thing, done work with NRC over the years with 4sight, as I mentioned. But the same kind of science goes into both of them.

I think -- yep, I'm done.

VICE CHAIRMAN CROFF: Okay. Thank you.

I think at this point, before moving on into sort of a different subject area, I think the best thing to do is to take some questions on the whole cement waste form predictability business to a major extent to add, but also any followup to Les from yesterday, if anything occurred overnight or Ed's presentation raises anything that Les might be able to address.

So with that, Professor Hinze.

MEMBER HINZE: Does your durability include cracking?

DR. GARBOCZI: Yes. 4sight has cracking in it. That changes the transfer properties.

MEMBER HINZE: And it includes the thermal cracking, for example? Does your modeling take that into account?

DR. GARBOCZI: Is Ken here? Is there thermal cracking in -- Ken Snyder is here. I'm going to let him answer that. He's the expert on 4sight.

MR. SNYDER: This is Ken Snyder from NIST. You're asking about the cracking. The model doesn't go so much into how the cracks get there. It really is kind of, "These are the cracks I have. How can I incorporate this into primarily the transport?"

And then, it treats it as kind of a composite material based on the spacing between the cracks, the width of the crack, and, more importantly, how deep the cracks penetrate into the concrete, and then it builds up kind of a composite picture of the total predictability.

MEMBER HINZE: Let me ask another question. Where are your major uncertainties in your model? You know, you know this model better than anyone else. You know, if -- if you were going to critique it, where are the uncertainties, and how massive are they?

DR. GARBOCZI: For durability prediction, which I haven't done much of yet, it's the serves environment. That's probably the main -- the main -- you know, what's the environment like? How does it change over time? That's very uncertain. the --

MEMBER HINZE: So the input of the environment.

DR. GARBOCZI: Yes. That's probably the major uncertainty. At the material level, the uncertainties are pretty controlled. You have well-characterized materials going into it, and the models are -- tend to be fundamentally based. You're talking about fairly small -- 10 percent type uncertainties, 10 to 20 percent uncertainties.

We've made predictions and compared to experiments, so, you know, lab, material science experiments versus the predictions, and they are 10 percent type agreement. And uncertainties are, you know, around 10 percent. Durability we don't know.

MEMBER HINZE: What about the aggregates? You talked about the form, the morphology of them. What about their physical characteristics? What have you assumed and how important are those in the predictive models?

DR. GARBOCZI: Okay. The shape is very

important for things like rheology, and those we have pretty well characterized. The chemistry of the aggregates plays a role in the alkalide silicate reaction where the pore solution reacts with certain kinds of amorphous silicate in the aggregates.

We don't have the chemistry of the aggregates in there right now, but we could include that. You could characterize the aggregates chemically just the same way you characterize the cement chemically. Right now we have -- do not have that built into the models, but you can -- you can do that.

MEMBER HINZE: Well, isn't that quite important, though, in terms of the levity of your modeling?

DR. GARBOCZI: For certain areas it's -- for amorphous silicate reaction, yes, it's extremely important. If you want to measure rheology, in the typical concrete applications the building concrete -- the aggregates are fairly inert chemically. And so you don't really know -- need to know much chemically about that.

The only thing we know that chemical -- chemistry matters is the alkalide silicate reaction, and there you'd have to know the chemistry. But in most applications the chemistry of the aggregates don't matter very much.

MEMBER HINZE: Thank you.

VICE CHAIRMAN CROFF: Okay. Mike?

CHAIRMAN RYAN: No, that's okay.

VICE CHAIRMAN CROFF: Okay. Ruth?

MEMBER WEINER: Since you have 50- and 75-year old concrete structures around, can you -- does our model retroactively predict their durability? Have you validated against anything like that?

DR. GARBOCZI: We've tried to do some work looking at the 4sight model, and there we found that not knowing the service environment well enough -- and then, if -- let me back up. Not knowing the initial structure of the material, what went into the material in the first place, really hampered us in making this kind of comparison.

We tried really hard to compare predictions of the model -- the 4sight model with the real -- the real cores from dams out west and -- but the lack of information about the initial material really hampered us there. So we weren't able to get a real good comparison, because we just didn't have the data.

Now, I think there are some -- several

places around the country that are long-term exposure sites, where pieces of concrete have been well characterized and out there for 30, 40 years. So that's a better possibility for looking at. We haven't looked at that yet, though, but that kind of data might be more helpful.

MEMBER WEINER: How about shorter timeframes? 10 years? If you've been working on this model for 10 years, you certainly must have some experimental work to go along with it?

DR. GARBOCZI: Right. Like I said, that is there. We've not done durability so much. We've been concentrating more on getting the model, so you mix concrete and the new concrete has the right properties. We've done some work on chloride transport through older concrete. And, you know, that does agree pretty well with the models, but we haven't really pushed hard on durability -- collecting durability data. It's just -- it hasn't been our emphasis for the last 10 years.

MEMBER WEINER: Is there a publication or are there publications of the kinetic equations that go in -- the kinetics that go into your model? I mean, I recognize it's too complex to present here, but --

DR. GARBOCZI: Yes, yes, there are. What's available -- I think Les mentioned yesterday there's -- now the thermodynamics and kinetics are available. Not everything has been measured, but there is a lot out there, so it's -- I know Jeff Boller from my group has been working on the HydratiCA model, the new version, which has the real kinetics in it. And he has been collecting a lot of data, what's available for it. So it is available.

MEMBER WEINER: Is it a complex of reactions? Are there -- are they mostly first order reactions, surface reactions? What -- can you give some kind of overview of what the kinetics looks like?

DR. GARBOCZI: Yes. It's complicated. There's various reactions. They're complex reactions. Some reactions depend on other reactions. There's surface things, there's things -- crystalline nucleate in solution, there's chemical reactions at the surfaces, and there's things that we're not quite sure which are sort of in between. So it --

MEMBER WEINER: Do you actually get crystallization as the concrete ages?

DR. GARBOCZI: There's some coarsening. The calcium hydroxide crystals that Les mentioned, there is some coarsening over time with them. The main -- the calcium silicate hydrate which holds it all together is amorphous. I think over a long enough time you will see some crystallization there. In the normal timeframes we look at, up to 50 years, we don't see that so much.

At high temperatures you see more of that. But under normal conditions, you don't really see that. It's metastable, very metastable.

MEMBER WEINER: So just one final thing. I take it from your presentation that when you have a reaction that only involves the surface you can really experimentally reproduce that just with a very small layer. And that's helpful to your modeling.

DR. GARBOCZI: Yes.

MEMBER WEINER: Have I got that correct?

DR. GARBOCZI: Yes, it's -- yes, that's right. That's right.

MEMBER WEINER: Okay. Thank you.

VICE CHAIRMAN CROFF: Thanks.

DR. DOLE: May I amplify some of the comments on using old structures to try to validate? We are currently engaged with the Army Corps of Engineers, who have been coring some of their older dams, 40 to 60 years. We see in our case where we are worried very much about the aggregate interaction, because they have a large impact on the ultimate strength and the utility of a concrete as it ages.

And so the alkalide aggregate, the alkalide silicate reactions are very, very key. And so we're looking at some dam materials that have been shown some defects. Poor choice of aggregate, and we've been frustrated by the fact that the records of what you actually use in the formula are very difficult.

Coming from an NQ181 environment to a standard construction environment, you find that not -- that the rest of the world doesn't operate like we do. And we're still struggling with that. We haven't found -- we haven't found the example, because we, too, would like to find an aged in situ sample that we can use to bound or validate some of our models of interaction.

MEMBER WEINER: Thank you.

VICE CHAIRMAN CROFF: Thanks. Ruth, you asked a little bit about documentation. I think if you go out onto the -- their group's website at NIST, I've been there once and I think there were a number of publications listed, as I remember.

DR. GARBOCZI: We have a monograph, about 300- or 400-page monograph which goes with this virtual lab. They can look up papers and that stuff.

MEMBER WEINER: Thank you. That's very helpful. Yes.

VICE CHAIRMAN CROFF: Okay. Jim?

MEMBER CLARKE: I was going to ask the same thing, if there were particular publications you'd recommend that we look at.

DR. GARBOCZI: Yes. Well, start with that monograph I think. And if you take a look at that, and then kind of get overwhelmed where to start, let me know. I can maybe point out --

MEMBER CLARKE: And I guess the other question is: what are you following with time? What are you predicting? What physical properties are you --

DR. GARBOCZI: Oh. Things like --

MEMBER CLARKE: What does your output look like?

DR. GARBOCZI: These are mechanical properties, transport properties. You know, how fast do chloride ions move through? Because you count on concrete to cover the rebar, say in bridges, to keep the chloride ions out from the road salts, because that causes corrosion. As a barrier, it keeps sulfate ions out or keeps the reactive ions in, some mechanical transport. Those are the main things we're following through time.

MEMBER CLARKE: Thank you.

VICE CHAIRMAN CROFF: I've got a couple of things. First, does the computational or predictive capability, is it able to take into account organic chemicals in cements and concretes?

DR. GARBOCZI: Right now, no. In principle, yes. It's just more chemistry to build in.

VICE CHAIRMAN CROFF: Okay. But the fact that it's organic doesn't cause any fundamental difficulties, if you will. It's just data and this kind of stuff.

DR. GARBOCZI: Not fundamental difficulty. It probably is difficult, but yes -- no, it's getting the right ions in there and the -- the chemical outlook for companies like W.R. Grace, they -- I mean, they care about that chemistry all the time. They use organics to affect concrete. So the knowledge is out there. So, yes, it definitely can be built in.

VICE CHAIRMAN CROFF: Okay. Second, if you continue making progress on your predictive capability at about the rate you have been, how long -- this is sort of a nasty question, but how long is it going to take you to get to the point of, you know, having some predictive capability for waste form types of materials, or dirty water if you will?

DR. GARBOCZI: That -- and, again, prediction is kind of hard.

VICE CHAIRMAN CROFF: Understand.

DR. GARBOCZI: But that would require a concentrated effort, because we're -- and this would have been focused more -- our federal mandate is focused toward the concrete industry, the construction industry. So all our efforts, most of our efforts have been focused around that. We're the management laboratory for industry to -- we have worked with NRC at lower level for years, and developed the 4sight.

To really move more into the predictive capability that you're talking about -- cementitious waste forms -- that's a -- that's a five- to 10-year effort with major emphasis on it. So that's -- that's something we could plan to do on our own funds, for instance.

VICE CHAIRMAN CROFF: Okay. So five to 10 years with increased resources --

DR. GARBOCZI: Yes.

VICE CHAIRMAN CROFF: -- is the estimate.

DR. GARBOCZI: Yes.

VICE CHAIRMAN CROFF: Okay. Thanks.

NRC staff? I don't know about --

(Laughter.)

DR. HAMDAN: I waited hard for this meeting.

(Laughter.)

Since the predictive capability for cementitious waste forms is not there, I thought I heard you say at the beginning of the meeting that the cementitious waste forms you are talking about durability of thousands of years. What's the basis for that? What's the basis for you saying the waste forms should be durable for thousands of years?

DR. GARBOCZI: That's just my partial limited knowledge of the kind of problems that NRC cares about. So it's a very uninformed number. Okay?

DR. HAMDAN: Thank you.

VICE CHAIRMAN CROFF: Okay. Anybody else? Ashok?

MR. THADANI: Yes. Part of this I think might also address what Bill raised earlier in terms of uncertainties. You said that large voids commonly found in heavily reinforced structures like reactor containment vessels -- besides crack, crack sizes -- seems to me this would be another important uncertainty. Certain loads are important for structure integrity.

But it surprises me -- I just want to understand the choice of words. You said "like reactor containment vessels." To the best of my knowledge, we've seen some voids in reactor containment vessels, but they're not commonly found.

DR. GARBOCZI: Did I say "commonly"? That was probably misspoken.

MR. THADANI: Okay.

DR. GARBOCZI: I've been told they have been found. I guess "commonly" is my -- "commonly" was my heat-of-the moment --

MR. THADANI: Okay. All right. Thank you.

VICE CHAIRMAN CROFF: Okay. Thanks. John?

DR. PLODINEC: John Plodinec. Les, this is for you. It seems to me that in talking about concrete durability, chemical durability, we need to think about -- you know, first we need to think about the key radionuclides. I don't want to use those words, but I don't know a better way to say it. But for example, if we're looking like at a Hanford tank, technetium and uranium are the prime drivers for the risk to the public.

How are those incorporated into the grout waste forms? Are they going to be aggregate-like? Or are they going to be intimately connected with the structure?

DR. DOLE: Okay. Just having my entire life's work reduced to anecdotal evidence --

(Laughter.)

-- the answer is we really don't know. We've made attempts at various times to locate the precise phase in which individual nuclides find themselves in a cured piece of grout. And because of the very fine structure of the matrix, we have never been able to isolate a phase, a compound, a nuclide.

Given that, we have all this anecdotal evidence that suggests that the -- in this illuminous silica phases they have ion exchange capacity, and they are kind of indiscriminate. In other words, the -- because they are amorphous, the spaces in which nuclides can fit into the pseudo-lattice, if you want to, is very large, and so they accommodate a broad spectrum of heavy metals and transuranics, very, very easily into these undifferentiated phases many times, or, if they are differentiated, unidentifiable phases.

And that when we do these -- these -- what we think are accelerated leaching tests, we still see very high performances. For uranium and plutonium, I'm pressed to say that I really believe that I've ever actually measured the diffusion coefficient. They're so low that they're very difficult to measure.

Mostly they're a study on the detection limits of my methods, which in the case of nuclides is very, very good. So that -- the answer is we really don't have a precise answer of which compound. You know, by analogy, we can list what we know about the thermodynamics of uranium and those compounds, and there's a list of possibilities. But precisely which one and when we can't say.

DR. PLODINEC: The reason that I make the point is that an easier approach than trying to treat it as if the waste is intimately a part of the cement structure is to treat it simply as the leaching of those species -- let's say, PCO2 conditioned by a cement-water environment around it. For uranium, you'd expect to form the silicates quite readily, which are very low solubility.

In keeping with what you say in technetium, again, what we see is, assuming that you've got the right environment -- again, thermodynamically you don't expect to see much happen. It might be a very much easier way to approach it than trying to deal with it as this -- tied up in the cement intimately as a chemical compound.

DR. DOLE: Okay. Realizing in real life that boundary is to some extent imaginary.

VICE CHAIRMAN CROFF: Do you want to put a horse in this race or let it go?

DR. GARBOCZI: No.

VICE CHAIRMAN CROFF: Okay.

DR. GARBOCZI: It's beyond my capability.

VICE CHAIRMAN CROFF: Dave, you had a question?

MR. ESH: Yes. This is David Esh with NRC staff. And my question may be a little more self-serving. If you were in the position of writing a standard review plan, what elements would you put -- what elements would you want to see -- and this is for all of the cement experts.

What elements would you want to see in order to get from current experience on the order of decades to some of these extended timeframes? With the understanding that there's different functionality that the cementitious materials can play. In some cases, they may be used as a hydrologic barrier. In other cases they may be used for chemical retention to retard the release of the radionuclides.

But just basically, if you could write anything you wanted in that plan, what would you be looking for? What would you put in it?

DR. GARBOCZI: Can I get back to you on that?

(Laughter.)

That's a really -- as you know, that's a really difficult question. I'll throw out a couple of things. Characterization of the service environment, what do you expect to see there for the next years, careful characterization of the concrete. You know, not just say we were going to pull aggregates from here and, I mean, just careful chemistry and what's going in there and related to what use, what we -- what use is the concrete going to be.

Is it -- as you said, different -- is it characterization of materials, characterization of environment. I'll throw that in as the two main things. Without those, I would say it's pretty hopeless for looking at extended periods of time.

DR. DOLE: I would add, you know, the classic problem in the system has been they have spent so much -- they spend a limited amount of money on characterization of the waste. And many times there are some chemical details in the waste that are not presented until you actually get into the job. There's always some surprises.

When you take a cupful of material from a million gallon tank, and you characterize that cup as the whole problem, that gets to be problematic, particularly when you have the stratification.

You know, certainly the fact that, you know, looking back at some of the major difficulties we've had in waste treatment, one of the most difficult ones is the iron scavenger precipitation product from the iron ferrocyanide. It is extremely effective at taking nuclides out of liquid, but it leaves you with a sludge that -- whose rheology is extraordinarily difficult to process.

Borates -- the status of the decomposition of the tributyl phosphate, all of these things grossly affect how the process runs. And so you -- so you always put a few extra hydraulic horsepower in your pumps, and you try to punt out of some of those difficulties when you get into it. But, I mean, you look around the complex, and you can find examples where projects have augured in because of characterization problems.

MR. ESH: And then, in terms of testing, I know there's common tests that are done, say for a waste form, the 16.1 test, but then for other materials -- bridges -- are there tests that you believe would be useful, or are there tests that need to be developed for the extended timeframes as compared to these typical engineering applications for these systems, on the order of decades to 50, 75 years?

Is there anything different when you go to the extended timeframes, in terms of testing that you could do? Or are you always going to be faced with this challenge of trying to accelerate a system that is non-amenable to acceleration?

DR. DOLE: Well, the short answer is yes. But, really, the problems have been is that the -- civil engineering is a lot like medicine. There's standard practice. And so the body of testing that you get out of ASTM is directed at, you know, if a professional engineer prescribes a concrete that meets these tests, and it falls down, he's still no liable.

But those are directed at construction problems and accelerating the expected construction conditions. And one of the virtues of being able to work outside the civil engineering envelope was I modify those tests as I see fit for the -- either the service conditions or the particular strength, because many times a waste form is not a load-bearing member of the system.

You're allowed a lot more latitude and strength, and strength doesn't relate to geochemical stability always. In fact, sometimes it's the opposite. The very fast-reacting, high-strength concretes are the least geochemically stable. So I have a lot more latitude than a civil engineer, and I -- so I use that to wander off the -- off the charts for civil engineering, and I use that as an excuse to modify the ASTM test to suit -- change the time steps.

Like ANS 16.1, if you use the prescribed test, those time steps were designed for cesium and strontium. Okay? With diffusion coefficients in this particular range. But as soon as you get into actinide and some of these enhanced waste forms, those timeframes don't get any results, so you have to be flexible to adjust the time ranges so that you get something -- you measure something.

We typically take ASTM tests and run them under more severe conditions of chemical -- chemical exposure. And we run them for much, much longer times, like three -- sometimes as much as three to six years.

Now, there's other problems that they

never anticipated. You run a phosphate exposure, a sulfate exposure test for three or four years, you get things growing in your experiment that was never intended --

(Laughter.)

-- in the original -- in the original testing protocol.

DR. GARBOCZI: And just to add to that, most of the standard tests that are available that are used empirically by the construction industry fairly successfully would not stand -- would not give meaningful information for long-term usage. So more fundamental measurements would need to be used, more fundamental testing developed for that.

VICE CHAIRMAN CROFF: Sure. Okay. Anybody else?

Okay. Thank you very much.

I'd like to shift gears a little bit. We've gone from defining the problem to retrieving waste to processing waste to talking about waste forms and fill materials here.

Now we're going to move on to sort of trying to put the pieces together a little bit and discuss performance assessment for disposed waste, and how to make decisions on whether such -- such disposals might be acceptable and talk about the processes for decisionmaking.

The first speaker on this will be Dave Kocher, whom you heard from yesterday on highly radioactive. To briefly recap, Dave spent 30 years at Oak Ridge National Laboratory and is presently a senior research scientist at SENES Oak Ridge.

In relation to performance assessment, Dave has been a member of performance assessment teams for low-level waste disposal facilities at Oak Ridge and Savannah River, a member of DOE's performance assessment task team during the '90s, and co-chair of NCRP and NCRP Scientific Committee on Performance Assessment for Low-Level Waste.

DR. KOCHER: I have a short time to give you an overview of PA. This is doomed to failure.

(Laughter.)

VICE CHAIRMAN CROFF: We have every confidence.

DR. KOCHER: A mile wide and an inch deep. What you really want to know ask in the Q&A. I'm going to try to cover a lot of different things, really starting from the basics of what it is, what are the criteria that we work to. It's important to know and understand what they are.

There are important differences between a near-surface facility and a repository that affect approaches to PA, because you all have been mostly working on Yucca Mountain.

Most of the talk is on what are the key technical and policy issues that arise in doing these things, and a few remarks about the problem you're interested in about DOE tank waste.

This is hard for some people to grasp, but it's essential. PA is fundamentally an exercise in subjective scientific judgment. We're basically dealing with the unknown and the unknowable, so you really have to have a certain reverence for the limitations of PA to predict what will actually happen.

But those limitations do not affect or compromise the essential role of PA in regulatory decisionmaking. And this is a conundrum that you just have to get comfortable with.

PA is a process. It's not a one-off item, as people thought it might be 30 years ago. Lots of interactions with other aspects of the disposal facility and lots of interactions with the regulators. The PA for Saltstone is now 15 years old, and still going.

The purpose of PA is fairly clear. First, you want to determine compliance with your performance objectives, and it provides important feedback to identify data that you need, facility design, to meet the performance objectives and what model improvements you need to support what I call defensible and cost effective regulatory decisions. We don't want to spend a lot of money chasing insignificant problems.

What's kind of unique in the low level waste business is that PAs are used to develop and maintain limits on how much radioactive material you can put in a facility. This is kind of different from Yucca Mountain. Basically, the PAs I've worked on, you back-calculate from performance objectives to get limits on what you can put in.

Okay. A real quick overview of the performance objectives. This is what you're working to. NRC, the basic performance objective for members of the public is 25 millirem per year. There's also this qualitative requirement that you have to protect this famous inadvertent intruder, and, of course, I'll say a lot more about what that is.

CHAIRMAN RYAN: Dave, just a quick word if you don't mind, for everybody's benefit. If you'd back up a slide, that top line is what's in 61 now, and that's not necessarily equivalent to 25 millirem per year.

DR. KOCHER: Okay. You know, I have to cut lots of corners. The rule says this. Staff guidance says they'll accept this.

CHAIRMAN RYAN: Right. I just wanted to make sure that --

DR. KOCHER: Yes.

CHAIRMAN RYAN: -- not necessarily equivalent.

DR. KOCHER: No, they are not. Certainly, in iodine-129 they are not equivalent. Good point. The rule doesn't say this, but the staff -- they don't want to --

CHAIRMAN RYAN: All right.

DR. KOCHER: Okay. The business of protecting inadvertent intruders is done by means of this waste classification system, which is basically these Class A, B, and C limits. These are generally -- these are limits that are generally acceptable for near surface disposal. And don't forget that they're -- along with the limits comes technical requirements for how you have to dispose of waste in each of those classes.

Basically, what the NRC did here is they took analyses of inadvertent intruders out of play in licensing specific commercial disposal facilities. All you have to do is meet these. You don't have to do any kind of intruder analysis.

Well, it's also useful to know what these limits were based on. What the NRC did is they looked at some assumed hypothetical scenarios for inadvertent intrusion. The basic scenario that drives all of this, a person comes onto the site, excavates into the waste, spreads the waste over the back 40, puts their house on top of this disposal facility, and lives there.

This is a fairly invasive, intrusive action. And so it leads to relatively high doses per unit concentration of stuff in the waste.

When I refer to a standard NRC scenario, that's what I meant -- this sort of resident construction followed by residence on the site. That those criteria they used were consistent with the old Part 20.

Very important for Class C waste was assumptions about probability of intrusion, and I'll get into this later. There were certain adjustments made for two radionuclides. The iodine-129 number was adjusted down a bit to alleviate concerns about offsite releases, and the cesium-137 number was adjusted up, so that there wouldn't be too much greater than Class C waste.

DOE has certain similarities and differences in how they do their performance objectives. The 25 millirem is the same, and this is effective dose equivalent -- separate criteria here that really are not too much of interest to this discussion.

Where the differences start to come in is how inadvertent intrusion is treated under the DOE system. DOE requires that you assess potential exposures to inadvertent intruders on a site-specific basis, and this is used to establish limits of concentrations at specific sites. And they can vary all over the DOE complex. They can vary within different disposal facilities at the same site.

These limits are established based on using what DOE calls performance measures for inadvertent intruders, and these are the dose limits that are in the radiation protection standards today.

So this is somewhat more restrictive than the limits -- than the dose criteria that the NRC used for some radionuclides. You can also consider the likelihood of scenarios if adequately justified. I don't know whether anybody has actually done this. You either admit a scenario or you don't, but I don't know of anybody that has said there's only a 10 percent chance this will happen.

So this is quite different. Each site has to do their own analysis.

The 1,000-pound gorilla under the tent that nobody has talked about so far is protection of water resources. DOE is required to do such an assessment and is used to -- can be used to establish limits on quantities that are acceptable for disposal at a specific site.

The protection criteria are not specified in the DOE rules, and so it's sort of determined on a site-specific basis. There's this general call in the order that you comply with applicable federal, state, and local laws and regulations.

As you all are aware, neither the NRC nor the DOE are the least bit interested in acknowledging explicitly EPA drinking water standards. That's what this is all -- this is what this song and dance is all about.

But the fact of the matter is that MCLs are probably going to apply, especially at DOE facilities, especially the sites that are being cleaned up under CERCLA. I would say there almost certainly will be performance objectives for offsite releases at all DOE facilities.

And these -- these MCLs are very, very important for the fission products. They are more restrictive than 25 millirem per year for every radionuclide that's in the fission product spectrum, but they're not important for the alpha emitters. The 25 millirem per year is usually more restrictive for the alpha emitters, but the MCLs really -- and for iodine-129, the reduction in allowable releases is huge, depending on how you interpret the NRC rules.

Don't forget that MCLs can be important for hazardous chemicals. The central issue in the Saltstone disposal site at the early days was meeting a limit for nitrate in groundwater per agreement with the State of South Carolina. This was radiopassive waste. Nitrate was the problem.

And, of course, states often impose MCLs as enforceable standards. So this has to be attended to -- personal perspective on what near surface disposal is all about. It's essentially achieving a balance between how much you are allowed to release beyond a site boundary to ensure protection of public and the environment, balanced against how much can you leave behind after institutional controls are assumed to be lost to provide protection of inadvertent intruders. It's a kind of a balancing act.

Intruder protection is not an issue in geologic disposal. It's the tough luck approach. So it -- this balancing act comes into play.

On this one, start at the bottom. If you assume the standard NRC scenario that I described previously, excavate into the waste and live on top of it. If you make that assumption, for nearly all radionuclides and at any reasonable candidate site, even where there's a lot of rain, criteria to protect inadvertent intruders are more restrictive than criteria to protect public and the environment.

What does that mean? If you establish your concentration limits to meet the intruder protection requirements, you almost always will be well below the criteria for offsite releases. Okay? This was demonstrated in the EIS for Part 61, and it's been shown in all of the PAs so far.

So what this means is if you protect inadvertent intruders, consistent with NRC assumptions, there are only modest demands placed on the performance of a disposal facility. We're not calling for something really heroic in the low-level waste disposal business. It's not like a geologic repository. These facilities don't have to perform all that well to do a good job.

Of course, what this does, it provides important incentives for using highly simplified and conservative assumptions in PA, highly stylized modeling of how this system -- complicated system will evolve over time. Les' Volkswagen never appears in any PA that I've ever seen. It's, you know, really highly simplistic.

And incorporation of "realism," whatever that means, only is needed to qualify waste intended for disposal. You know, keep it simple, stupid is a powerful incentive provided by this kind of idea.

A few differences between near surface and geologic disposal. PAs in our line of work, they are generally more modest undertakings. Time I'm not so sure about, but certainly much lower budgets. Most of the ones I worked on were one-, two-year efforts, maybe redo it again.

Another important difference that affects PAs is the size of this buffer zone. Where is the member of the public located? It's typically on the order of 100 meters for a near surface facility, but, of course, it's 18 kilometers at Yucca Mountain. And this has a profound effect on how you do hydrologic modeling. We're doing local-scale hydrologic modeling over here, and these guys are doing regional scale.

The role of inadvertent intrusion is completely different in the two worlds. For near surface disposal, the focus is on protecting intruders. For a repository, the focus is on what's the impact of an intrusion event on releases to the environment. Very, very different.

Time period of compliance? Question mark, because of course we don't know what it is for Yucca Mountain.

Okay. I'm going to give you sort of a general overview of what I see as technical issues in PAs for near surface facilities, and it's going to be divided into two parts -- assessing releases to the environment and impacts on the public, and inadvertent intrusion.

But remember something I said before -- an overarching issue in all of this is what kind of balance do you want to achieve between conservatism and "realism" in a PA? And there's no single prescription for this that applies anywhere, and the usual and widely accepted approach -- this is specified, in fact, in the NRC staff guidance -- is to incorporate realism only to the extent necessary to qualify the waste that you intend to put in this facility.

You know, it's only as good as is needed. And there's not a lot of incentive or need to go much beyond that. So this is an important issue is the balance between these two.

Generally, a PA is divided into several components which I have loosely characterized as follows. You have to have some analysis of what water is doing in all elements of the system through the cover, through the disposal unit, the vadose zone, the saturated zone.

Source term analysis -- I have to -- I have a bone to pick with John yesterday when he referred to source terms as things that are remaining in tanks. A source term is a rate of release of something from a disposal unit. What's left in the tanks is inventory. Okay?

And you have to have some analysis of how your contaminants are transported, and once you release to the environment what are the exposure pathways and dose to humans? Overriding all of this is some kind of evaluation of uncertainty and sensitivity.

The norm in the low-level waste business is what I call modeling by modules. I don't know of anybody that has a fully integrated computer code that handles all aspects of PA from soup to nuts. It's done in pieces. And so the linkages between these modules are very important and have to be attended to.

Now I'm just going to say a few words about these. These are a personal view. It could be wrong, so, you know, be questioning.

Performance of covers and infiltration down to the level of a disposal system -- all disposal facilities have a cover of some kind. They're not open to the air. Well, there's really no generally applicable method for estimating infiltration through soil or engineered covers. You sort of have a different approach and a moist environment compared with an arid environment.

What I worry about at most -- moist sites, like Oak Ridge, is, okay, it rains a lot there. But the truth of the matter is that stuff moves only when you have these really episodic intense rains. When you just get a normal shower or drizzle, nothing happens.

Well, we just widely assume that all of these effects dampen out, and we model all of this as a steady-state flow and release system. Is that good enough? I don't know.

At arid sides, the problem is your relative errors in predicting infiltration relative to precipitation incident on the ground surface are huge. My guess is at Nevada that nothing goes down. It just all goes back up. But who knows? The relative errors are huge.

When it comes to engineered covers, most facilities have these, but it's generally difficult to model the degradation of these covers over long periods of time. A typical approach in PA is to assume some instantaneous failure back to natural conditions at some period of time, and this period of time is usually not too long.

In fact, it's generally very difficult to justify an intact cover system beyond the institutional control period where you can actually watch it. If somebody makes such a claim, then it needs to be scrutinized carefully to see if they've really supported it.

I hate to talk about concrete, because I know nothing about it. I'm sitting here in front of the gods of the business. A naive view of it is what follows. First of all, concrete barriers -- this is like a bunker or a vault that you're putting the waste in.

These structures can do all kinds of good things, but it's been emphasized by all the speakers in this area there is really little relevant data to predict the structural integrity and load-bearing capabilities of this stuff beyond the several hundred years. In fact, even this is fairly -- fairly heroic.

But I think it's worth pointing out that even if the concrete can't bear a load anymore, the chemical properties that should do good things for you should last longer. I mean, just because it can't bear a load doesn't mean the chemistry goes away.

So it's difficult to justify in a PA structural integrity beyond several hundred years. So what does a vault do for you? Well, it clearly has benefit in limiting releases of shorter-lived radionuclides. But the benefit in limiting releases of longer-lived stuff is questionable, unless your kind of compliance is fairly short.

You know, if you have an unlimited time horizon, a concrete vault doesn't do anything for plutonium, or at least you can't claim that it does. Sometimes infiltration through a degraded structure can be an important concern. Are you generating preferential flow paths for water to get to the waste once this thing looses its structural integrity?

Moving on to inventory and source term, again, very briefly, a problem that bedevils low-level waste in general, the radionuclides that often are important in predicting offsite releases and doses are the ones that are very, very difficult to measure in waste. And the uncertainties in those inventories can be huge.

However, my guess is that this is a less important -- it's still an important issue, but somewhat less important for DOE tank waste, given their origin and history. I think a lot more is known about these wastes than your eclectic garden variety of low-level waste that comes from all over the place.

Another problem in the low-level waste area in general is how to model highly heterogeneous sources. I mean, you're putting hundreds of individual waste packages into a facility. You may have X curies of iodine-129, but it's all in one waste package. How do you -- you don't model package by package. You just sort of make general homogenization assumptions and close your eyes and hope for the best.

But the good news, of course, about the DOE tank waste is that they are homogenous for the most part. So these thorny little issues that we routinely sweep under the rug I don't think will be all that important.

Grout waste forms are great. You just smear stuff out.

This is worth what it costs you, which is nothing. My opinion on key concerns for your problems. For waste removal from tanks, it's put into some kind of facility like Saltstone. Again, the key issue for the Saltstone facility was the hydrologic properties of this monolithic waste form. I mean, monolithic waste forms are basically a great idea. They alleviate a lot of problems.

And questions about how this waste form degrades over time that were raised in the previous discussion -- there was to me a fairly compelling argument in the Saltstone PA that this huge monolith will weather very slowly over time. I mean, it -- because it's a monolith and it has a very small surface-to-volume ratio.

For residual waste in tanks -- and this is a guess -- a concern to me is that these heels at the bottom have a very -- probably have a very high surface-to-volume ratio. So if they're all leachable, which they probably aren't, you have to worry about this. And there will be concerns I think about the ability of this grout to limit in-flow of water over long periods of time. But I don't know; I haven't done PAs for these things.

A really, really difficult problem in the low-level waste area is modeling unsaturated flow and transport. It's very data-intensive, and it's hard to defend on a site-specific basis. The fundamental issue here is if you have a highly complex and non-linear relationships between your three key parameters, which is moisture content, pressure or suction head -- remember, this is suction, not forcing out -- and hydraulic conductivity.

And these relationships are highly dependent on soil type. So it's really, really difficult to develop and defend a model based on this. As far as I know, there's no model of any kind that can be defended for flow in unsaturated fractured rock. This is a tough research problem that we basically can't deal with it in PAs today.

So given the difficulty of this problem, what do people do? Generally, you'll find what I call a graded approach to dealing with this problem. You may find that it's ignored completely. Just assume that these releases from the disposal facility go directly to the underlying aquifer.

The analyst is basically saying, "I don't know how to deal with this. I'm going to ignore it, take no credit for it whatsoever."

The step up from that is something that we call a unit gradient model. You basically -- it's a steady-state model. I mean, a real model is highly transient. It's a steady-state flow model, and you basically assume that the flow rate is equal to the infiltration rate. It's essentially a Darcy's Law in an unsaturated medium. It has a very comfortable, intuitive feel about it.

The next step up is to try a full-blown solution of the full equation. This has been done in PAs. This was done in Saltstone. But there are concerns about numerical errors, because these equations are hard to solve.

Transport in the unsaturated zone is generally handled in the same way as in the saturated zone using the KD concept. You just apply it to an unsaturated medium. Whether that's right I don't know.

Saturated zone flow and transport is an easy -- in principle an easier problem to handle, because at least you can solve the equations. But it is not a trivial problem.

The central issue in modeling aquifer flow is to generate a spatially dependent velocity field of groundwater. It's important to understand that these velocities are not measurable, and they are not directly deducible from any field measurement. Rather, what you must do is generate this velocity field conditioned on a model that you assume to describe the system. It's the old chicken and egg problem.

This model is conditioned on hydraulic head data from wells and data that you get from pump or core hydraulic conductivity tests. But it's important to remember -- well, there are many issues in doing this. An important one is the non-uniqueness of this generated flow field. You may make the wrong guess, and so you have to continually iterate testing against all the varieties of data that you have.

Assumptions about boundary conditions are going to be important. Pump tests are inherently transient things, but you're using that to calibrate a steady-state flow model. Issues of spatial scale and heterogeneity of the system, modeling of fractured media, again, is difficult.

There are all kinds of issues with

applying lab scale data to the field, and you may have incompatible data sources. You may have -- certain data sets are compatible with your model, but others are not. How do you reconcile all this and come up with a defensible model?

Issues in transport -- again, it's the simplistic nature of the KD concept. Everybody uses it, because that's what we can model. I'm not aware of any evidence that this really works in the field. It's a reasonable assumption, but I don't know whether it's been proved.

There is often controversy over the specific KDs that you assume, especially if you assume some kind of heroic value in order to show compliance.

If you're modeling diffusion and dispersion at a complex three-dimensional level, there are all kinds of issues with site-specific data and taking laboratory data into the field where extrapolation can be way off.

A few general observations about this problem. Really, what you have to do is investigate multiple conceptual models that are consistent with this variety of data that you accumulate about the flow field that you're interested in. You have to resolve these uncertainties on a site- and analysis-specific basis.

Modeling fractured media, I would say, could be viewed with considerable skepticism. That is, you should examine the assumptions and the output carefully. The good news for DOE, of course, is that their groundwater systems have been extensively studied. The Snake River aquifer is pretty well known.

Okay. This is the area I work in. When I work on PAs, I'm the dose guy. I'm the health physics guy. And let me say this about what I do. Of all the components of PA, the analysis of pathways and radiological impacts are the best understood but the least important. I mean, we just have -- this is just an irony. It's just the way life is.

It's best understood because there's really no concern over how we model these pathways, and there is tons of relevant data from radioecological studies of everything under the sun for the last 50 years.

Why are these components least important to decisionmaking about whether disposals are acceptable? Well, basically, you can't control the impacts of releases if your facility doesn't meet performance objectives. The last place you look is diddling with your exposure pathways models.

You go back to the disposal facility and the site and see what can you do to improve that. And if MCLs are controlling allowable releases, there's nothing about exposure pathways in those. That's just a concentration that you have to meet.

So we understand a lot about this, but it's the least important to deciding whether a particular facility is acceptable.

This is good news, actually, because the lack of importance of these things serves to focus reviews on issues of how the disposal system performs, which is where the focus ought to be. What are the barriers doing? What's the geologic environment around your facility doing? This is where the -- this is what you should really examine?

These things -- the releases to the environment are controllable by selecting sites, designing facilities, and putting limits on waste disposals. This is where the action is.

The problem is that potential impacts -- we calculate doses to reference individuals, and these are largely independent of where the facility is and how it's designed. There just isn't a lot you can do with this part. This is standard off-the-shelf stuff.

Uncertainty analysis in PAs. Basically, the treatment of uncertainty has evolved over time. Early on everything you saw was a deterministic point calculation of some kind, alleged and hoped to be conservative. When you do this, of course, you are not evaluating the margin of safety that you have, and you are not evaluating uncertainty in your result.

And we're slowing evolving to what's commonly known as probabilistic uncertainty analysis. You all know how this works. You assume probably -- probability distributions of your model input parameters, propagate those to a model, and calculate a probability distribution of output to describe uncertainty.

This is being done, of course, at Yucca Mountain. But to my knowledge -- I could be wrong -- there have been no fully probabilistic PAs for low-level waste facilities. There certainly have been PAs that included probabilistic analyses for parts of it, like Nevada area 3 is an example. But don't know of anybody that has done a fully probabilistic PA of everything from inventory to dose.

In my opinion, the key concept involved in uncertainty analysis is what I call importance analysis, and let me just read this because this is important. Importance analysis means an integration and interpretation of the results of your PA.

All of the doses, the output, and all of the intermediate steps that you took -- modeling steps that you took to get to dose -- integration and interpretation of those to identify assumptions and parameter values which, when changed within credible bounds, can affect the decision about compliance.

We are looking for things that are important to compliance, not important to the actual outcome. So it's important to understand this distinction.

So understood this way, uncertainty and sensitivity analysis is commonly understood in practice, is not really necessary in PA. What you want to do is identify those assumptions which drive your answer about compliance.

Okay. A few words now about this whole -- what we fondly call intruder voodoo. I emphasized before that this kind of analysis is required at all DOE disposal facilities. And, again, we are evaluating impacts on intruders, not impacts on the normal performance of the disposal facility. Of course, this differs completely from what's required at Yucca Mountain, where a person drills through a waste package and you evaluate the effects on releases by the groundwater pathway.

The key to any analysis of inadvertent intrusion is: what is the scenario? What is that person doing? And when are they doing it? Once you agree on the scenarios, the rest is a piece of cake, because, again, there's basically no controversy over how a dose assessment for a given scenario is done. So the key is: what is that person doing, and when are they doing it?

I already mentioned the scenarios used by NRC, excavating into waste and then -- you're basically swimming in the stuff. These kinds of scenarios are generally more restrictive, result in lower concentration limits than other common kinds of scenarios that you might assume, like drilling through a facility is a common assumption.

You discover the facility. You start to dig at a site, and you encounter a concrete wall, say, "I think I'll go dig somewhere else." So you get some external exposure while you're standing there making up your mind.

Or you may reside on the site without excavating into the waste for various reasons. This is what's been done at Saltstone, for example. But the NRC scenario is more restrictive than all of these others.

Something that's possibly important is the role of institutional controls in limiting the type and duration of scenarios. For how long do these exposures occur? This could be important at DOE sites, because DOE guidance calls for analyses of intrusion "for a temporary period." The idea is that they believe that nobody would live on the site for 50 years, or whatever. We'll say more about this in a second.

Back to the Class C business, at a DOE site, if you assume the standard scenarios that NRC used to develop the Class A, B, and C limits -- this is excavating into the waste and building a house on top of that site and living there. If you assume those same scenarios at a DOE site, the concentration limits that you get generally speaking are about a factor of 10 or more below the Class C limits.

This leads, for example, to a requirement that the Idaho site, the RWMC, the transuranics are limited to 10 nanocuries per gram, not 100. Why is this? Well, the Class C limits incorporate a times 10 increase to account for an assumption that the probability of intrusion will be low.

And, again, this gets back to a point I made yesterday. The volumes of this stuff are assumed to be small. The probability of actually reaching it is low. So there's a times 10 increase in all these limits due to probability.

They are also based on an assumption of intrusion at 500 years, and some sites on the DOE complex assume shorter periods of time to delay intrusion than that.

The dose criteria are often more restrictive. The 100 millirem per year is often more restrictive than the number that the NRC used. So you don't get a particularly good correspondence between Class C and what's calculated at DOE sites for these reasons.

Credible scenario is going to be highly site-specific. My favorite example is drilling at Oak Ridge versus Savannah River. Drilling at Oak Ridge is a hard rock exercise. If you put a concrete barrier there and a guy drills through it, the drill bit says, "This is salad dressing. Give me more." No problem.

At Savannah River, you can drill with a straw. If you encounter anything, you go somewhere else. And we have actually sold this at Savannah River. A drilling intrusion into the vaults is considered not a credible scenario, because you will encounter something and decide, "I'm going somewhere else."

So what are DOE sites doing to get around the idea that if you apply the standard scenarios you end up with fairly restrictive concentration limits that we don't like? Well, you play with the facility design. You put deeper disposal. You put a thicker cover on the waste, so that a standard three-meter excavation doesn't get down to the waste.

You put in penetrable barriers to prevent excavation or drilling, like at Savannah River. You make other assumptions. At Idaho, the assumption now is you're allowed -- you admit drilling through the facility, but no one will live on the site.

The idea is that the RWMC is so isolated, nobody ever lived there in recorded history. The idea that an inadvertent intruder would build a home on top of this waste disposal facility is fanciful, so excavation is basically excluded.

As soon as you excavate -- eliminate excavation into waste, your concentration limits go way up. In fact, if you don't access the waste at all, there are no limits for all kinds of radionuclides. There are basically no limits. Unless it's a high energy gama-emitter -- and, of course, if you're -- if you still have a meter or more of concrete between an excavation and the waste, it's still huge. A meter of concrete absorbs most anything.

So you end up with much higher limits than when the standard scenarios used by NRC are done. So this is why I say scenarios are really the key issue. You play with these things, and you play with them a lot.

So I would summarize the key technical issues this way. There's no single answer to what's important to a PA. It's highly site- and design-specific. But you can be sure that whatever drives your decision will be scrutinized.

To me, integrating and interpreting the results of PAs for purposes of decisionmaking is absolutely essential. You have to tell the decisionmaker what has driven your answer.

In general, what will be examined critically in a PA, how you model a source term, how you model groundwater flow, and how you model, and how you model unsaturated zone flow if you assume it's important. Does this apply at every site? No.

Groundwater flow was completely

unimportant in PAs at the Nevada test site, because the infiltrating water never reached groundwater in the time period of concern. So it just didn't matter. But generally speaking, especially at eastern sites, it's important.

If you make assumptions about the integrity of engineered barriers beyond several hundred years, you'd better be ready to defend it. In general, if you have important parameters and you assume values that are well outside norms that are in people's comfort zone, that's going to be critiqued. And you assume intrusion scenarios at DOE sites will always be examined.

Okay. I'm going to give you now a quick run-through of what I consider policy issues that are important to PA. Some of these are interrelated. I mean, there's a strong relationship between institutional controls, the role of the intruder, and issues of future land use. And I'm just going to go through this fairly quickly to give you a sense of kind of what current thinking is.

Where is this hypothetical member of the public located? Well, this person is generally put at the point of highest concentrations of radionuclides at the boundary of the buffer zone.

CHAIRMAN RYAN: Just a quick point. You told us earlier that it doesn't matter because the intruder drives the bus. What do you think? I mean, we put them there, and we calculate stuff, but it's not the critical issue. Is it?

DR. KOCHER: Often no. Sometimes yes, but often no. Well, if you make these intrusion scenarios go away, then of course the offsite releases becomes more important. It was an important caveat on the previous statement.

Part 61 basically says this buffer zone has to be adequate to carry out environmental monitoring and any mitigation activity that you undertake, unspecified. The guidance is unspecific. DOE basically says assume 100 meters as a default, but you can assume a larger or smaller area if adequately justified.

We adjusted this at Oak Ridge because 100 meters ended up in a swamp, and we didn't think anybody would really live there.

A wild card here is importance of remediation decisions under CERCLA and what kind of land use may be implied under CERCLA, which would put your member of the public some distance away. But as far as I know, this hasn't been done in PAs. So the person is close. The person is fairly close by.

Time of compliance is a hot button issue. Part 61 is -- contrary to popular opinion, Part 61 is silent on this issue. Staff guidance says compliance for 10,000 years, but if your projected doses for long-lived radionuclides are increasing at 10,000 years, you should extend the calculations.

And, furthermore, if those extended calculations lead to projected doses exceeding 100 millirem, you ought to use the results to establish site-specific inventory limits. So this is not a hard time of compliance, by any means.

The next one in your handouts has a crucial and important typographical error. This number right here that I changed to 1,000 already -- 10,000 is wrong. So just mark that out. Use what I gave you. DOE uses compliance with performance objectives and assessments of inadvertent intruders for 1,000 years, not 10,000. So a big difference with the NRC staff view.

DOE also has this concept of a rolling present, and I'm not going to try to describe it. But as I understand it, it's 1,000 years from today every day. Chew on that for a while.

They also call for carrying out

calculations to time of maximum dose if beyond 1,000 years, but not to establish site-specific inventory limits. This is just for information only.

How does the rest of the world look at this time of compliance issue? It seems to me you have a fairly relaxed view. They don't worry about specifying particular times. What they do is they look at the duration of the hazard and what are their modeling capabilities in different timeframes. And they don't say, you know, 1,000 years is good enough, 10,000 years is good enough, 1,000,000 years is good enough. they look at what it is and how far out in time do you think you can model.

Institutional controls -- very important in low-level waste, of course. NRC says they may not be relied on for more than 100 years, which is essentially an assumption that they preclude intrusion for this period of time. Nothing happens.

DOE says these controls are assumed effective in deterring intrusion for at least 100 years. But, again, in their analyses of inadvertent intrusion, their reference to a temporary period of exposure implies that controls should be effective well beyond 100 years -- in fact, essentially forever.

The truth of the matter, of course, is

that given the requirements of the license termination rule and the imposition of Superfund at DOE sites that we are in a world of perpetual institutional control here, folks. That's a fact of life. Unless the rules change, we will be watching these sites forever.

What do other countries do? A wide variety of things, all the way from none to several hundred years of effective controls.

This is a contradiction but still makes sense. Essentially, what NRC and DOE do, they regard inadvertent intrusion as an accidental occurrence. They don't really believe that these scenarios are going to happen for a variety of reasons, like institutional memory or active institutional controls.

But for purposes of limiting disposals, they assume that they will occur. It's a hypothetical construct to make decisions about waste disposal. It seems to me that -- and I'm not arguing against this, but it seems to me that DOE is desirous of reducing the importance of inadvertent intrusion by assuming perpetual control over disposal sites. This is the direction they seem to be moving.

So they are focusing more and more on scenarios of short duration that will allow a guy to come on the site and attempt to do something, but within 30 days or so he is found out and evicted. That kind of thing. And these scenarios, of course, have limited consequences, and, therefore, higher disposal limits.

Internationally, the view generally is that inadvertent intrusion is virtually certain to occur once controls are lost. But they don't really -an intruder is a member of the public. Okay? It's not something separate.

In the low-level waste business, we generally do not require assessments of impacts of intrusion on releases to the environment. Again, this is completely different from Yucca Mountain, and NRC had a good justification for this, and DOE has developed its own justification.

At Yucca Mountain, of course, it's different, and these releases must meet the dose -- must meet the 15 millirem per year for normal releases. ICRP has recommended that these kind of scenarios by evaluated, but their dose criteria are much, much higher.

Another thing that we generally do not do is admit a groundwater pathway into intrusion analyses. And this gets back to the balance between allowable releases and allowable residual stuff in the disposal facility. Groundwater is stuff that's being released; it's not residual radioactivity. So we generally have been successful in taking this out of play.

My own view on this very controversial issue is this. That these hypothetical scenarios and setting limits based on protection of things that won't happen, I consider this part of what I call defense in depth, a multiple barrier system, and protecting the public.

An intruder is a member of the public. And defense in depth is a bedrock principle of radioactive waste disposal, and these limits based on hypothetical scenarios are part of that multiple barrier system.

I do not believe that we should rely on perpetual institutional control to protect intruders when intrusion in the absence of controls would be likely. We need a combination of the two if it -- if controls fail, the intruder still should be afforded a reasonable level of protection. But, again, these scenarios can be highly site- and facility-specific, and at some types of facilities there can be virtually no credible scenarios for intrusion.

Future land use has really not been given a lot of thought in the low-level waste business. NRC clearly assumed unrestricted use by the public at 100 years and beyond in setting their Class A, B, and C limits. Even though temporary period is referred to in the DOE manual, most sites in fact have assumed unrestricted use. But some land uses are considered not credible. At Idaho, again, residence at the site is considered not to be a credible occurrence.

There are other kinds of scenarios that are being used under Superfund, the kind of brownfield things, recreational use, that involve some control, but these generally, to my knowledge, have not been admitted into low-level waste PAs yet. But it's certainly an open question, given that many of these DOE sites are being remediated under CERCLA.

Last topic. The $64 question is: how do you demonstrate compliance? And how does a regulator decide what has gone on here? NRC staff guidance essentially is a two-pronged approach. If you do a deterministic calculation, if you just do a point estimate of dose out there in time, what the staff wants to see is that your projected dose should bound the performance, be clearly conservative, and should be less than the performance objective.

If you use a probabilistic approach and

generate a distribution of projected doses, compliance to the NRC essentially means that the highest mean -- the mean value of that probability distribution should be less than 25 millirem per year, and an upper 95th percentile of that dose distribution should be less than 100.

Left unsaid here is what happens when the mean exceeds the 9th percentile, but we won't get into that.

DOE is a bit less prescriptive. They just specify this reasonable expectation that you're familiar with from Yucca Mountain issues, no quantification of what this means. This is a highly reasonable and defensible approach.

What this does is acknowledge that when you make decisions about compliance you always have important subjective judgments that can't be quantified, and your decisions cannot be based alone on a simple comparison of a projected dose with a numerical performance objective. That's an abrogation of responsibility to do that.

Personal view of this issue -- this is a challenge to regulators. These projections are highly uncertain, and how to compare a highly uncertain projection with a fixed performance objective is not a trivial issue.

Even if you do a fully probabilistic analysis, you don't necessarily capture all the important uncertainties. Your models could be wrong. You know, your models could just be wrong, and it's hard to account for that.

The qualitative judgments will always be essential. What did you assume in the PA of a qualitative nature? And this is an essential aspect of decisionmaking, so you can't do these simple comparisons. There is always judgment involved in the decision.

Now, during the Q&A, you can ask what you really wanted to find out.

VICE CHAIRMAN CROFF: Well, I hate to keep you in suspense, but we're running a little bit long. So what I'd like to do is take a break right now, actually, and then get to the next speaker, and then we'll let you both sit there and take the swings and arrows.

So if we could be back at 25 after, take 15 minutes, please.

(Whereupon, the proceedings in the foregoing matter went off the record at 10:12 a.m. and went back on the record at 10:29 a.m.)

VICE CHAIRMAN CROFF: I'd like to move on now. After talking about performance assessment, our next subject is how to go about balancing the results of performance assessment with many other considerations going into a decision on whether a waste determination or maybe similar situations such as decommissioning, whether they're acceptable.

To address this subject, I'd like to introduce Dr. Anne Smith. Anne is Vice President of CRA International here in Washington, D.C., specializing in risk management, decision analysis, benefit cost analysis and economic modeling. She's applied these techniques to issues such as contaminated site management, nuclear waste management, global climate change, air quality and food safety.

Anne has developed and reviewed decision support tools for risk-based ranking of contaminated sites and for making risk tradeoffs in selecting remediation alternatives. She has served on several national research council committees reviewing issues involving risk management within DOE's environmental management program, including the current committee mandated by Congress to review the basis for DOE WIR determinations.

Anne?

DR. SMITH: Thank you. I'm going to move one level up, I think, from the level David was working on. What I'm going to talk about is -- may sound in many ways like performance assessment, but it's working more with the question of specific decisions that I think need to be made under the NDAA 3116 and later, I'll talk then about how I think these ideas mesh together with the performance assessment process. But I'm not going to be talking about performance assessment.

Instead, what I'm trying to address here is this sort of more high level question that are decisions that need to be made in waste determination. One of the criteria and whether a waste determination decision can be made is whether the performance objective is met, whether the performance objectives are met by a plan.

But there are still these decisions that I've laid out here that are sort of very amorphous and I think raise some difficult questions that can't be addressed by performance objectives per se.

These are the key words. Have radionuclides been removed to the maximum extent practical? And that applies both to retrievals from tanks as well as the separations of radionuclides for portions that will be disposed on site.

And then secondly, there's a question, has reasonable effort been made to achieve those that are as low as reasonably achievable, the ALARA provisions. And those have two dimensions, two key dimensions, as occupational exposures and public protection. There is, of course, the intruder issue in here as well, but I think I'd like to leave that as more of a performance objective issue meaning the performance assessment question and less of a decision per se in these are the more critical decisions.

Now there are tradeoffs in all of these and tradeoffs, of course, is what leads to decision making, how do you choose between options when no one of them is ideal and better than all the others in all the dimensions. The tradeoffs that we'll be talking about obviously as you reduce risk further and further in any one dimension, you drive up the costs. But there are also other tradeoffs such as whether you are increasing occupational risk as you are reducing future public risk.

And another issue that I think is kind of interesting is how are you trading off risks in time? There's severe term risks to maybe associated with a tank's leaking. By risk there, I'm talking about things like probability of a leak occurring and the probability of release not just health risk per se. And the longer term risk being more the risk associated with public exposures or intruder exposures.

There are also some interesting interactions between the two questions. Removal to the maximum extent practical. As you push that down, you may be actually increasing or making it more difficult to achieve the degree of ALARA that you might otherwise achieve -- I guess it's a bit of an oxymoron the way I phrased it, but the main point being that there are tradeoffs, as you push for lower and lower, greater and greater removals of the tanks, you may be increasing some of the risk at the occupational level, so -- and then the same, you've got a tradeoff between occupational and public protection as well as the maximum extent versus the ALARA.

It's important that these tradeoffs, one thing that makes these very difficult decisions, these tradeoffs are being made across very distant points in time as well which adds to the -- not only issues of equity among the different parties, different stakeholders over time, but also the uncertainties that you have and how well you understand risks later in time than in the present.

So that's the decision context. There's a lot of complexity here beyond just those tradeoffs that I was describing, which are complex in their own right. First of all, who is the decision maker is I think even a little bit unclear, but it's very complex and a process that's still being understood and developed which is in part why we're even having this meeting and why I'm speaking to you.

It's complex in a regulatory sense or a legal sense as well, there are many parties involved from the regulatory side. It's not just DOE and NRC. It's States and EPA as well, all of which have a role in the overall process and even NRC's role is different with respect to the sites, Hanford not falling under the NDAA provisions in the way that SRS or INL are. And I'm purposefully leaving West Valley out of this. I think we're really speaking mostly about the NDAA and we'll focus on that. Obviously, NRC has a different role there to and some similar kinds of decisions to make.

The decision criteria which are largely unique performance objectives, but there are some other decision criteria, they exist, but well, they exist, but it's very qualitative in the sense of the decisions here. There's nothing specifying how these decisions should be made or how to draw the line on them and that part is extremely qualitative. It's just words like maximum extent practical.

Obviously, there are many, many stakeholders and people think of stakeholders as the public around the sites and the Tribes, but there are also, I think, as you keep in mind that justices may be a stakeholder too. Eventually, I think, some of this will end up in Court and whatever is decided, whatever approach is taken, will need to be -- possible to articulate clearly in a legal setting too. So I wouldn't put that aside as a stakeholder or another party. It should be kept in mind.

Alan asked that I come to the meeting to talk about what are the standards for good decision making that can be brought to bear in this setting. I want to draw your attention to two reports, the National Academy of Science has done a whole sequence of reports over the years since the early 1980s on risk and decision making. You may have heard of the Blue Book and the Red Book and there is another color, I don't remember.

But what I'd like you to think of is really the best source document for describing a process that would work here is what I call the Orange Book, the one "Understanding Risk" which is one of the most recent of the set, in 1996. And also a report that came out this year through also National Academy, "Risk and Decisions" which I'm sure you all have heard of.

The Orange Book, I'm going to call it that because I'm going to refer to it again later without referring to the title, it's useful in a very general sense. It talks about how to do decision making in a public domain and in a situation where there are multiple stakeholders involved, not a corporate environment where it's a little easier to define the objectives and the values.

And in doing so, it describes an analytic deliberative process which then when the -- I was -- I admit I was on the Committee that wrote the "Risk and Decisions" Report, so I'm sorry I jumped into first person here. In "Risk and Decisions", it really is largely an articulation of those broader concepts directly targeted towards the question of making waste determination decisions for high-level waste at the DOE site. So the two are very, very consistent with each other.

The "Risk and Decisions" Report was not purposely built on top of the Orange Report, but you will find if you read the two that they're quite consistent in the themes and the approaches that they outline.

But the first one is far more general, in nature, and the second one is far more specific. The other important thing to know about the "Risk and Decisions" Report is it was written without the benefit of there being a National Defense Authorization Act. Thee was no Section 3116 available at the time. So there are elements in there that don't necessarily apply exactly to the situation of Savannah River and Idaho, but I do think it still applies quite well for Hanford.

The key themes in both reports, the key themes are first of all that a risk-informed approach is needed to make decisions and that the process be participatory. Those are really the two most fundamental elements.

By risk informed, I'll summarize by saying you start with risk considerations. You center the discussion around risk issues, but the decisions in the end are driven by a wider range of concerns that are brought to bear in public, by public interest and stakeholders' specific concerns.

And in order to do that, in order to achieve that second dimension which takes risk analysis into risk-informed decision process, you need to have a participatory approach and I suspect some of you can't read these words down here, but the quote from "Risk and Decisions" that I have here is that "in order to be effective, a risk-informed approach must be trusted. A technically credible risk-based approach that lacks participation or transparency would likely not be trusted." And ultimately, although these are decisions where really I guess DOE is the decision maker, it is a public interest issue. It is a process that needs to have public buy-in.

So in order to decide what's the maximum extent practical, you need to have not just the determination by DOE with NRC consultative support, but there does need to be public buy-in to that as well or I think the process will fall apart.

So in thinking about a review process, a standard review process, I think that really needs to be kept in mind, even though it's not officially part of the law, nor is it officially part of the criteria for setting decisions.

Now that means you need to go through a process for decision making. You, DOE, we, the parties involved need to go through a process for decision making and there is some general features that mark a good decision making process which is listed here. I've underlined participatory because I do think it is one of the most important to emphasize, but of course, analytically it needs to be logical. It needs to be consistent with scientific knowledge. These are all of the general features that David was speaking about when he was speaking about how to do a performance assessment.

It needs to be transparent, traceable. We feel, in the Committee, that it is very important that the analysis of decisions be made somewhat independently. That doesn't necessarily apply in this particular situation, but just reiterating what you'll find in risk and decisions.

Peer review is important and when we said that the results need to be believable, we mean by this that they need to have some general measures of what we call lab tests to be colloquial about it. We need to look at the results and say do these make sense? Is there something about the way this analysis has been put together, the calculations and the risks, that just doesn't fit with conceptual formulations of how these processes will work, these processes of waste transport and institution patrol, etcetera.

I'll come back to the last point in a second, but it needs to be framed to recognize the needs of the decision process. All those are quotes from "Risk and Decisions" but if you read the first 10 pages, the 10-page summary of the Orange Book, you'll basically find the same ideas there, stated slightly differently.

I also looked through the NRC's regulatory analysis guidelines, the September 2004 version. And there are a couple of extra points in there that I thought were useful to bring out as good decision making attributes, process attributes. First of all, the analysis should avoid after-the-fact rationalizations to justify decisions that have already been made. That was certainly a very important point and an important issue in terms of getting public buy-in. They often feel that performance assessment is just a rationalization for a decision that has been made. I'll come back to that in a minute about how to capture that in the decision-making process for 3116 rules.

And also, we must not let the process

unnecessarily delay decision. Process must move forward. It shouldn't become hugely cumbersome. And that's actually very tricky because these are very complicated processes. The process is complicated. The analysis is complicated or can become very complicated. How do you prevent the process from becoming just the delay scheme. And that cuts both ways. It's not just DOE making the analysis too cumbersome to delay a choice. It's also stakeholders who potentially try to use that as a way to delay a decision.

I think an important point on the participatory is how do you get that into the process. There's nothing in the law that says this is how we make it participatory. This is what must be done. Nothing in the law says it needs to be participatory, but the important question here and I just leave it as an open question is in creating -- should NRC be making an open process in its review plan? Should that be where the participation comes in? Or should NRC simply in its review plan request that DOE or expect that DOE may prepare a more participatory process than could otherwise occur.

It's just food for thought there. I don't have an answer.

This is a figure from the Orange Book and I think the important thing here is to recognize that this is conceptual. This is not a flow chart. I'm not going to show you flow charts or step-by-step processes. If you want that, go to the books and read them. Rather, what the point of this figure is and then I'll move on quickly is you have many stakeholders. They interact with each other during the process of analysis at different -- all the way through.

The process of analysis, this is the flow chart for doing, for instance, a performance assessment. That goes on inside this process, but that's only part of the process, the decision making and there's a lot of interaction. There's deliberation meaning not just doing analysis, but talking about what the analysis is telling us about our choices and how we might alter the problem, how we might alter the actions that can be taken.

And a lot of feedback, learning and feedback. You see lots of cycling. So that's the conceptual issue. Lots of iterative process, lots of interaction among the parties and it continues even after a decision is made that continue on with that deliberative process.

So that's just a conceptualization, but it makes the point that the analysis and the process have to be blended together. The analysis doesn't stand on its own and the process separate from that.

So I want to move on to the -- how you structure that analysis. Some of the key principles then, rather than doing a flow diagram, do this, this, this and this as the steps, there are some key principles underlying what needs to go into a decision or into the analysis.

First, you want to structure the analysis as a comparison among alternatives. So I'll come back to this second, but if you're trying to think is something having been retrieved to the maximum extent practical, you've got to compare what you're proposing to do to something else to say is that more? If you do more, is that better or not? You have to have a comparison. Don't come in with just a single plan and do a very, very detailed performance assessment of that single plan. Structure an analysis as a comparison between the choices you have for different levels of retrieval, different levels of risk reduction.

Additionally, you need to identify early on all of the parts of outcomes that are necessary for all the affected parties in order to choose between those alternatives. Identify that up front and then structure the analysis to provide that information and don't do an analysis that provides additional information that you don't need. So very early on you need to understand what all do you need to know. I argue it's more than just what you can get out of the performance assessment per se. Performance objectives are not sufficient to meet all of those informational needs to get by.

And finally, while a performance assessment can and usually will get very, very complex by the time it's done, you really only want to be adding complexity as needed in order to say this is enough. So the performance assessment may need to be quite complex in order to assure that you're meeting performance objectives, but it may not need a lot of complexity in order to compare, among alternatives, to make a determination that you've removed enough waste to call it maximum step practical.

So there are two different kinds of ways the analysis needs to be used. One is to make a decision, the decisions about maximum extent practical or ALARA and the other is to demonstrate compliance with performance objectives for whichever alternatives you want to choose.

And I just -- "Risk and Decisions", Chapter 4, does go through the steps that show almost in a flow diagram way how to implement that. But this is a more important question. As we get to the waste determination decisions themselves, to demonstrate the maximum extent practical or to demonstrate ALARA, I think the best way to think about that decision or those decisions is to ask the question if this is our plan that we think is best, what is the next most -- what more can be done? What's the next increment of level of effort at further retrievals or at further risk production?

And right there now you have a comparative analysis. So I don't think you need to have a little bit less -- the preferred plan and something less and something more. I think it's better to think in terms of this -- do we think this is as far as we can go, now let's demonstrate why we believe this is as far as we should go. What's the next most degree of reduction that we can achieve? What would it take? And there are questions that go with that such as how much longer do we have to wait to achieve that lower degree of reduction?

In doing that, next most stringent

alternative, how much more does it actually reduce risk? And I'm assuming, by the way that whatever the plan is up here, that it does meet performance objectives, that that's not the concern. I'm assuming the question is should we do more than that? Can we do more? Should we?

So you've already reduced risk quite a bit and as David pointed out, a lot of these performance objectives are done in an extremely conservative way, so you probably have a -- if you've met those performance objectives with a fairly conservative analysis, you probably have very little risk left in the first place.

So the question is how much more can you reduce whatever risk is left by going to the next most stringent alternative. And of course, how much does it cost, but also in addition to whether it changes the time line for risk, are you creating some new risks as you go further? And this is where you get into the interactions between the retrievals decision and the ALARA decision. You retrieve more and more. You may be increasing occupational risk. And in some sense undermining the ALARA part of the decision.

So these are all interrelated and have to be thought of as packaged and the way you set them up as packages is you compare alternatives and they give you package of outcomes of risks of different types that you play into the ALARA question.

Now I do want to get to the performance assessment question and this is simplified. I apologize, but I've simplified my statement of what a performance assessment does for purposes of making my point here. Is a performance assessment sufficient for making these decisions? And my feeling is per se, the way they're performed, no. Part of the reason is that really a performance assessment is a bit of an, I think, a bit of a numbers game. It really tends to focus on identifying the risk at some point in time and space and we heard about that a minute ago. The point of compliance may be 100 meters. Why isn't it 10 meters? Why isn't it 18 kilometers? Where do you choose that point of a compliance? Of course, that's whether you're going to need to performance objectives or may affect that and also you tend to look at the peak exposure or if not the peak exposure, at some point a thousand years out, a thousand years out, but it's a specific point in time and a specific place. And if you meet the objectives there, by and large you say good enough, we're done. But that doesn't help you say should I do more? Because if you're comparing two alternatives, the next most stringent alternative will also meet performance assessments. How do you choose between them?

Essentially, the more stringent one may meet the performance objective per se better, but it may actually have created some different, some changes in the whole risk profile. I call it a risk profile. By risk profile, although I've yet to figure out how to draw one for you, but I will one day. Risks occur over space and time and there are different types of risks. We've been talking about them all along. But, for instance, if you retrieve more waste sooner from a tank, you may reduce the amount of potential contaminants that would be in the near field, in the near term.

If you wait a while and wait for some new technology to become available, say five or 10 years down the line, you may increase that risk but lower the overall risk of the longer term, because you can retrieve by waiting awhile, retrieve more waste over -- eventually before the closure occurs and before the leaks actually do start to move beyond the fence lines and the like.

And different types of risks, of course, we've got risks to this generation, risks to several, multiple generations out. You ave risks to workers, risks to the public, risks to intruders. All of those differ too, both in time and space.

So I feel that the most important point I'd like to leave here is in terms of thinking about a plan for how to make these decisions, to recognize that performance objectives, just determining whether they are met is actually inconsistent with the concept of a risk-informed process. It doesn't mean they don't address some risk issues, but they're inconsistent with a risk-informed process for decision making and it is that aspect of the analysis that having a process that involves all the stakeholders is necessary in order to get the public acceptance for these waste determination decisions.

And even though potentially, legally, those decisions could be made in a vacuum, absent any input or public acceptance of the decision process, if they are, I think we'll be right where we were a few years ago, several years in the future because there will be huge public debate over these issues.

To summarize then, identifying the maximum extent practical of ALARA, by doing this as a comparative analysis with at least one alternative action. You could have a couple of alternatives that are more stringent, would better serve that goal of gaining a buy-in and a sound decision.

That didn't tell you how to make those tradeoffs, but that falls out of the process. That doesn't -- there isn't a hard, bright line to deciding how to make the tradeoffs.

And I just wanted to end on a few thoughts of how this thing could be broadened a bit beyond the very basic concepts that I've laid out for you. First of all, and I think most importantly, in doing these analyses, you need to never lose sight of the ranges of uncertainty and that's the uncertainty of the comparison to the alternatives which is a little bit different than the uncertainty in your absolute measure of risk.

The uncertainty in what the risk will be at a certain point in time and in a certain location is obviously huge, especially with a very long time line we're looking at. But the uncertainty in how much you will reduce the risk from the planned or preferred alternative to a more stringent alternative that might be deemed also the maximum extent practical and that uncertainty may be narrower, because it's narrowed down to what's different between the two alternatives and they both have huge ranges of uncertainty, but the comparison between the two should generally be easier to analyze and to gain some narrower balance on.

Also, it's important to keep uncertainty along for the ride all the time. Every time I see a presentation on a risk analysis and performance assessment, the uncertainty bounds are gone. I don't see them in presentations. They're always missing by the time someone is trying to summarize. Today, you can't remove them and even if you've done a very conservative analysis, you still need to show which direction the uncertainty is. You need to show that it is conservative and the only way to do that is to have some of the alternative calculations with alternative assumptions presented.

And it doesn't need to be quantified with statistical confidence levels. Error bounds just raises the sensitivity analyses are perfectly acceptable ways to convey that uncertainty. I like to use scatter points. I may have 10 scenarios, based on 10 different sets of assumptions and just show what the outcomes are as dots for each of the alternatives, without trying to say which of these dots is more likely.

You need to be able to trace back which

dot which applies to which assumption, so that people can understand what they want to weight. Or you may able with that, to just simply make a decision based on the worse case of the set of scenarios, the most conservative.

Anyway, I just can't emphasize that enough when we start getting more into the details beyond the very simple points that the rest of the presentation is trying to make.

I think also, in thinking about these issues of waste determinations and clean up plans, if you will, by clean up I'm mean retrieves and stabilization and not clean up of existing contamination of the sites. Try to understand there's a temporal dimension to all of these risks and the different actions you take can sort of shift the risk around differently in the near term without much affecting the long-term risk or perhaps you can reduce the long-term risk, but in doing so you're raising the near term risk.

I think that there are some very important trade offs across time in that and I do feel that the way I've seen performance assessments done so far is that inter-temporal tradeoff is being lost, but really the near-term risks are worth considering and not just the long-term risks which are really the focus of the performance objectives.

I think if that consideration, having been brought into this thinking about waste determinations, it would be helpful and I think we could, in the end, find some better ways forward. Because really, what a lot of people are worried about, I think, when they're debating these issues are the near term risks. I just pick up that thread when I listen to the stakeholders. They talk about the long-term risks and what's left behind and lack of institutional controls, but really what they're worried about is what is happening in the next 50 to 100 years on that site. And that's where a lot of the debate -- get the tanks out, get them away from me.

I really see that as being concerned with near-term risk as much as it is long-term risk. That's a personal opinion. A lot of this is personal opinion. I forgot to say that all the way down the line here. But that's really my personal observation, having to a number of these sites, listened to a lot of presentations by stakeholders as well as regulators in DOE.

And the last point I thought would be worth noting and I know this is a very controversial one, maybe not even worth mentioning, even though there's been an order to avoid any controversy it will create, but I tried to phrase it not as can we assume institutional control for a long period of time, but rather to what extent can post-closure monitoring actually reduce the risks that are associated with releases?

It's very likely there will be releases some time, once the tanks are closed and left behind. After enough years, I think it's very likely and I don't think I hear anybody debating it, that some of that material will be released and start to move through the environment. It's just more of a question of when and how much.

With long-term monitoring, let's say the release starts at some point where the monitoring is still continuing, if you observe a release, action can be taken. It doesn't have to just keep the leak or if the contamination doesn't have to just keep traveling towards the point of compliance, towards the receptor. If you know it's there, even if it occurs after the closure, you can stop it. You need to know it's there. You need to have institution control. It does, I think this factor, which is not acknowledged in a proper, formal performance assessment, it does reduce the risks associated with the site.

And so while it's not allowed for determining that compliance, I think it merits consideration and the extent to which the performance assessment is conservative. And one may question to me is to what extent does effectiveness in that monitoring really depend on their being the length of institution controls that you can retain and the length of time. In other words, can you reduce the risk a lot by monitoring out another 100 or 200 years or does that additional risk reduction that can come from detection, early detection, actually is that associated with the release of 900 years out, a 1000 years out, at which point it's much less likely that you can expect monitoring to be effective or useful at that point in time.

But still, this is an important part of risk management, outside of the DOE world, outside of the performance assessment world. It's an important part of risk management is understanding what actions can you take after you've chosen your course of action to offset risks, if they occurred to offset the bad outcomes, if and when they occurred in the future. It does allow a good deal of difference in the choices that are made at a corporate level of risk management, whether it's financial or environmental.

I think it would do us all well to try to capture some of that issue in the DOE setting as well. So those are the main thoughts I had. I was hoping it would mainly lead to discussion. If there's no discussion, I'll just sit down with him.

VICE CHAIRMAN CROFF: Okay, that's fine. And that's a great lead in to what I expect to be maybe about a half hour of discussion here on the performance assessment and decision making topic. So Mike?

CHAIRMAN RYAN: Thanks. Two interesting presentations. Let me start at the end with Anne. I think -- I couldn't agree with you more on your point about how does monitoring fit into this whole scheme.

My own view is that monitoring does play a role in several aspects. One is in creating the modeling system in which performance assessment is done, whether it's geological cores or hydrological understanding or other kinds of things. And I guess my own view is that particularly in a lot of operational facilities, like low-level waste sites across the country, that's on-going. Many of them, in fact, and this might be a departure from your view, but many of them have monitoring plans that go out for a hundred years or more.

So there is an element of that going on. Maybe it needs to be a more important element or maybe a more formal element, but I think it's a great idea and it was really sort of the question that I had for Dave is that a lot of times people talk about performance assessment as something that starts and ends, once you decide to run a facility. But my own view is not only is monitoring a compliance demonstration activity, whether it's a groundwater concentration or a barrier integrity parameter of some kind or that kind of thing, it's also a way to improve your model as time goes on.

One hundred years of data on geohydrology or groundwater levels or surface water evaporation or transpiration, whatever you want to talk about, that's 100 years long, sure makes for a better model than the one that's three years long.

So I guess I'd offer that comment and will ask you in just a second to both comment on it, but let me start with maybe an easier question to Anne and it's a challenge to your comment --

DR. KOCHER: Us old folks can't remember old questions.

CHAIRMAN RYAN: Oh yes you can, Dave. If

you can remember all that stuff you presented in 30 minutes, you can remember anything.

(Laughter.)

I guess in my reading of the risk-informed literature or in risk literature, in general, that you said that a performance assessment is inconsistent with the concepts of a risk-informed process and I guess I've always thought that a performance assessment which is, in essence, a risk assessment is the first step in a risk-informed process. So just think a little bit about that and take it in whatever order you guys decide to take it.

DR. SMITH: That's worth clarifying. What I meant is if you simply do a performance assessment and determine you've met performance objectives, do that in a nonparticipatory method, meaning without a deliberative process that involves other parties, then it's inconsistent with it.

CHAIRMAN RYAN: Okay, that is not a risk-informed process.

DR. SMITH: I'm sorry.

CHAIRMAN RYAN: Then, it is not a risk-informed process if you just do it by itself. I agree with you there.

DR. SMITH: That's what I was trying to

say.

CHAIRMAN RYAN: What I'm referring to in my mind is that the NRC has published a risk-informed white paper. It's a SECY paper and I think it recognizes the very distinction between a risk assessment which is kind of the numerical science part and then how you go from there to risk-informed. I guess I see it as the first step in a process, but by itself, not so sufficient.

DR. SMITH: Actually, I would argue that it's not even the first step. It is --

CHAIRMAN RYAN: A step.

DR. SMITH: It is an element of the process and that was actually why I wanted to put in that diagram from the Orange Book, in that you can -- that was the one with all the arrows every which way and different colors.

CHAIRMAN RYAN: I'll have to study that a while.

DR. SMITH: Don't.

(Laughter.)

DR. SMITH: And don't use it as a flow chart. I actually disagree with sort of a lot of sequence in it, if you try to interpret it as sequential. But it is important that the risk analysis should not even be started until after you've started a process. It involves discussion and deliberation about how the decisions will be made because you'll do a different kind of risk analysis as a result.

So I agree, it's a very important part of the process. And it's all of the analytic content of the process, but how you craft it will be quite different, depending on whether you start through participation and discussion and deliberation or whether you do it and then try to start the discussion. If you do the analysis and then say now let's take it to the public and start speaking, describing it to them, first of all, the analysis may hit the wrong mark, but secondly, I think you've put yourself into that world where you appear to be justifying a decision after the fact.

CHAIRMAN RYAN: I appreciate that clarification. Thank you.

DR. SMITH: That's the important part.

DR. KOCHER: Back to your comment about PA over long time frames, something I did not talk about because I ran over as it was, my apologies for that. An essential aspect of low level waste business in the DOE world is what they call performance assessment maintenance. These things are living documents. For as long as you are doing anything at a site, it is susceptible to inputs of new information. It's not ever a done deal. It's a living document. Simple as that.

CHAIRMAN RYAN: And again, I think there are two aspects that the monitoring side of that question which is that there's a compliance demonstration structure of some kind and then there's a -- how do we improve our PA question. And I think that when you design a monitoring program, when one does that, you ought to think about both because sometimes they're at odds and sometimes they're real easy to get together on.

One of my favorite examples is where do people's sample streams when they have a very large area to sample? The simple answer is where the roads cross over the stream. Now that man may not be from a modeling standpoint the ideal place to get a stream sample. So you might be able to demonstrate compliance, but is it helping you do a better mode. It's maybe a trivial example, but it's one where if you just think about how you're designing monitoring at the beginning and then update as you go along, you might find that monitoring points come and go or compliance points evolve over time. There are other aspects. I guess maybe next session we'll hear a little bit more about a practical example of that. But I've taken that you both concur with the idea that getting something like that in your thinking is a good idea.

DR. SMITH: Yes. And I'd like to add, I think the monitoring should be designed as an early warning system and so therefore not necessarily monitoring at a point of compliance, but rather monitoring closer in to where the leaks may first start, so that you have time to take action, if there is a need.

CHAIRMAN RYAN: Well, said.

DR. KOCHER: Typically, what you'll do early on at a waste site is say does the cover have a hole in it yet?

DR. SMITH: Yes.

DR. KOCHER: Things like that. You're not measuring a concentration in a well some place. You're looking at the physical system and saying is it staying put.

DR. SMITH: Yes.

CHAIRMAN RYAN: And to me and everything you've both said, the key word is system. It's a system.

DR. KOCHER: Yes, it's a system. A complicated system.

CHAIRMAN RYAN: Thanks. Ruth?

MEMBER WEINER: I'm afraid I have more questions than the time allows, but I'll try to start with Anne.

You mentioned public trust and when you talk about stakeholders and public, there is the implication that you're talking about all those people who come to hearings and object to whatever actions the DOE is taking.

A comment, I'd like to point out that the stakeholders who are people who have a stake in the outcome of the decision involves the people who work at a site, the people who run the site, the people who finance the site, the people whose waste goes to the site. They're all stakeholders. Now how do you measure, how do you assess trust? Because somebody can always say you to you, I don't trust you. I don't care what you say, I don't trust you. And I've heard that said at meetings.

DR. SMITH: Well, I don't think it's important to measure trust. What you have to do is strive to gain it.

MEMBER WEINER: How do you know when you're there?

DR. SMITH: When you get a decision that people accept and it doesn't go back to the Court.

MEMBER WEINER: Well --

DR. SMITH: And so the question is not -- there's no way to assure that you will get that, but there are ways to improve your chances of achieving that outcome by having a process that gets a discussion going early on and finds out what the real criticisms are and you're right, you can never prove that you have someone's trust and ideally -- I mean not ideally, in the real world, people are going to game the system if they don't get exactly what they want which may be some very self-interested outcome, like more funding and more jobs, rather than really managing a public risk concern.

But still, if you get them talking early, you can understand what their arguments are and head them off as best you can, by trying to incorporate those considerations into an analysis and certainly to the extent that you don't do that, you are definitely increasing the chances of creating outrage that's real.

I do agree with you. Stakeholders are

much broader than just the people who come to the meetings and complain about things. That's absolutely right and in the interest of speed and talk, I kind of summarize it in those terms, but really we have to recognize stakeholders are anybody that's affected by the outcome, anybody.

MEMBER WEINER: I'd encourage you to look at the history of the opening of the waste isolation pilot plant as a case study and I can talk to you about that later.

On another point, we operate by regulation and regulations are made in a very public process and having been a member of that public to try to influence regulation, I can tell you it really is a public process. So when you say you have to do more, what kind of -- how do you know when more is enough? And aren't you basically undermining the regulatory system itself? In other words, if a regulation isn't good enough, there is a public process for changing it. But if you say, okay, you meet this regulation, but we want you to do more, that's open-ended and how can you make decisions on that basis?

DR. SMITH: I think the more that I'm talking about is more early on and what I've seen of the regulatory process, I won't pretend to be real familiar with the NRC regulatory process. I work more in the environmental field with the EPA regulatory process. What you usually see is a decision being made, even if it isn't announced yet and then the analysis starts to be presented. It's already been crafted to drive in a particular direction.

Then you get to comment as a public on it. It's a bit late at that point to feel that you're doing anything but being in any mode other than adversarial, if you're on the outside, not the regulator, but on the regulated side.

So if there's a sincere interest in trying to just make good decisions here, I don't see any reason under the 3116 issue specifically, because that's the only regulatory process I'm trying to address, I don't see any reason why it couldn't start with a little bit more participation, early on, to identify issues and concerns and it's not like we're starting in a vacuum. A lot of this discussion has already occurred, but to explain how you're going to go about doing the analysis. And the "you" here is really probably more DOE than NRC, to explain a little more about how the process will move forward, so that people will have a chance to comment before the wall has really been rolling for a while.

That's where, I think, the more is needed. There's definitely a regulatory process that was intended to allow for iteration. But those processes are very rigid and you get your 30 days to make your comments and they need to be to keep them moving along, but these are kind of one-off decisions. There aren't a lot of them to be made. This isn't something that's going to be going on forever and ever. This is just a few sets of waste that needs to be addressed. And they're highly contentious decisions and I just think that the process could be more effective it it starts a little bit differently.

MEMBER WEINER: Thank you for the clarification. I think that clarifies my concern. On your slide 5, the one with the figure, are you equating natural and social science input? Are you making them, giving them the same weight?

DR. SMITH: Are you talking about the picture that I had that's not in my hard copy?

MEMBER WEINER: Yes. Because I --

DR. SMITH: I drew it last night --

MEMBER WEINER: There's one little arrow that said natural and social science.

DR. SMITH: I copied it straight out of the Orange Book and I was not an author of the Orange book.

MEMBER WEINER: Well, does the Orange Book equal natural --

DR. SMITH: It's just making the point that you have the analyst community and you've got a regulatory community. They just lumped them together. I'm not trying to equate them one way or the other.

MEMBER WEINER: Okay, thank you.

DR. SMITH: Think of it as a spectrum and you've looked at three strips of it.

MEMBER WEINER: If you tried to play this out in your decision which is removing the waste to the extent practical, I think you've recognize that there isn't any quality among social science and natural science.

Do you advocate multi-attribute utility approach, the decisions with multiple objectives?

DR. SMITH: Personally, for this setting, no. I do not because it's, in fact, a point I wanted to try and make somewhere in my remarks.

The problem with that is it drives everything down to a single metric and it ends up hiding a lot of dimensions to the question and I think here the more important question is how do you choose between a couple of alternatives. We're not optimizing a system. We're trying to say here's a plan that we think is workable. We think it meets objectives and we need to demonstrate that we don't need to do more, that this is maximum extent practical and ALARA.

MEMBER WEINER: I think this is a very good example of where that kind of mechanism would actually help the decision because it makes the trade offs very, very clear. I mean if you can -- you can even almost quantitate the tradeoffs.

DR. SMITH: Well, that's the problem is it's trying to quantify some things that people will never agree on quantitatively. You can play around with waiting systems and really multi-attribute utility is nothing but a waiting scheme for saying what am I going to give more weight to in my decision, which of these attributes -- so certainly, you can play around with -- once you have some various measures of risks at different points in time and space, you could play around with waiting schemes, but I would not argue that you should try to -- and when I say play around with, you can set up the method and play with different numbers, use alternative variables, weights and see if it changes the decision you would take much.

That can be helpful for demonstrating --

MEMBER WEINER: Isn't that what you're doing anyway?

DR. SMITH: No, unfortunately, multi-attribute utility is a whole theoretical construct and starts to get into issues, you know, people's utility functions and it starts to prevent that it's a little bit objective and honestly, even though I'm an economist and I come from that field, I feel that that has been more of a detriment to decision making and policy planning in a public policy framework than it's been of help.

MEMBER WEINER: That's an interesting point. Not to take up any more time, I have a question for Dr. Kocher also.

Is there any -- how would you put probability or risk into the intrusion scenario?

I'm asking the question from the basis that when we were working on the waste isolation pilot plant, there was a data base from which we could draw some conclusions about intrusion. We could say well, this is an area where people have been drilling for oil and gas, so we have an idea of the drilling frequency, annual drilling frequency and so on.

How would you do that in this case of

shallow land disposal site or even the WIR case?

DR. KOCHER: My preference is either to say a scenario is credible or it is not. And when you get into -- attach a number to a probability, WIPP is a kind of one off situation -- a repository in that sense is a bit different from WIPP or Yucca Mountain and the NRC did assume in round numbers that basically a 10 percent chance that intrusion into Class C waste will occur. That's a defensible decision in the sense that if you assume that intrusion at a low-level waste site has a 100 percent probability, there's still only a small probability that you'll actually reach that small volume where the Class C waste resides.

So it's all defensible. We've tended not to do that kind of thing in the DOE system. We've tended to say this is a credible scenario or it's not credible and leave the numbers for another day.

I'm comfortable with that. You just need some way to try to limit this in a way that people will accept.

MEMBER WEINER: By the way, I also want to thank you for a very clear presentation of the differences between the geologic disposal PA and what you're doing for this situation.

DR. KOCHER: Thank you. This whole issue

of should at a low-level waste site, should the impacts of intrusion on off-site releases be considered, that could bubble to the surface and get ugly.

It's as good a reason not to do it, but whether our reasoning will survive, who knows? Why put in engineered barriers and then assume they won't work.

CHAIRMAN RYAN: Well, Dave,that gets back to the question you raised a bit in realism. And other speakers have talked about it. If we're going to go realism, then the bounding scenario tends to fall apart. I mean there are further kind of goofy things in the intruder scenario like you have to unemployed because external exposure occurs for 18 hours a day. And you have to be a fabulous horticulturist, because you have to grow your food in Class C ground up irradiated hardware and whatever all else. So it's very much a very stylized construct.

DR. KOCHER: Absolutely.

CHAIRMAN RYAN: In a lot of ways, it kind of -- it's not a realism question.

DR. KOCHER: The whole purpose is from time immemorial in the waste business, we've had this concept that there are certain things that should not go into a near surface facility because they're too hazardous, however you want to define hazardous. And we've evolved this fairly elaborate construct to help make that line of demarcation between it's okay in your surface facility and it's generally not okay. But it's hypothetical. No doubt about it. Highly stylized.

VICE CHAIRMAN CROFF: Jim?

MEMBER CLARKE: A couple questions for Anne. And I'd like to -- I think you made some very good points and they have wide applicability, much beyond WIR determinations and I'd like to ask you a couple questions within that broader framework.

The way you approach the risk comparisons, different populations, workers, residents, ecological in some cases, if you go into a bigger arena, near term versus short term, very consistent with the CERCLA criteria, balancing criteria. But I think part of the problem wrapping this up into a decision and I'm going to follow up on something you said, you said the way you compare these risks comes out of the process. And these are difficult risks to compare. They have different end points. They're different in time.

Did you mean by that that the group that's going to make this decision in a participatory way is going to decide how they're going to compare those risks, they're going to take a qualitative approach or semi-quantitative or what -- two questions, I guess. What did you mean by that and number two, what's been your experience as the best way to do this risk comparison.

DR. SMITH: All right, I think it's really up to, there's not a decision process defined, so it's sort of to be determined what the process is. What was I thinking about? I think I was recognizing this was really a decision that will be made by DOE and maybe NRC will have a strong hand in which way it pulls, that they would be -- that DOE and NRC combined would be informed by a lot of discussions which is what I mean by a process. Informed in those discussions about how other parties, which of the tarde offs really catch their attention and which are the concerns if there are parties that really don't like the proposed plan, what is it about the next most stringent alternative, they do like. And to use that information, to decide well, are we going to take that into consideration, are the tradeoffs that we see and we, DOE, have to accept, like the higher cost worth is concerned.

And so I see it more as one of understanding the setting that the decision is being made in, than one of a group decision. I don't think this would be a group decision. I don't think I would recommend that it would be a group decision. But hopefully, by the time a decision is made --

MEMBER CLARKE: May not be the ultimate decision, but maybe the way the risks would be approached, the way they would be -- the currency that would be used. If you have two different alternatives, you want to compare them. You want to use a qualitative ranking or risk, you can do it that way.

DR. SMITH: What I was envisioning here is that you really, because I did not suggest having six, seven, eight alternatives, that's what I meant when I said we're not doing an optimization.

MEMBER CLARKE: Just two.

DR. SMITH: Just two and you have a lot of dimensions, you characterize each of the two choices in terms of all of those dimensions. Somebody has to take the time to learn those, but to read through the details to understand how they differ. But in doing that, they will probably drop out a few ways, just a few ways in which they really, the two alternatives differentiate themselves and in other dimensions there may not be as much sense that they really differ. That will help narrow down the focus of what the tradeoffs really are that need to be made. And at that point it really becomes just a judgment and it's the sort of thing the government has to do is make those judgments.

Hopefully, in making those judgments, they can articulate the reasons for them, clearly enough, based on that analysis process that it at least does not appear to be arbitrary.

MEMBER CLARKE: Okay, and just another quick one. Do you think you could improve the limitations that you have identified in basing a decision on a performance assessment which I agree you don't want to do either. But do you think you could improve the process by framing the performance objectives differently or is that not going to get us there.

DR. SMITH: I kind of live with them because they're written into law. I accept them for what they are. Could they be altered and changes --

MEMBER CLARKE: May be added to?

SMITH: Added to. I'm -- again, I'm not sure that that would be helpful. By the time you sort it out what the additional objectives would be. You could just as easily have made the decisions. Again, because it's not a process that's going to go on at infinitum, thousands of times over and over into the future, I sort of wonder if this isn't one where it's better to just look at the choices and make good decisions that everybody is informed about.

DR. KOCHER: I would say that the performance objectives address really only a part and probably a fairly small part of this issue of maximum extent practical. It's just addressing A, the post-closure time phase, all the operations and all the engineering you do to get stuff out of the tank. Performance assessment has nothing to do with any of that. It's just a post-closure time phase, protection of public health and the environment and I'm still fairly convinced that it's a small part of this maximum extent practical debate. It is a factor, but it's probably a minor factor. It does play a role in ALARA, but I think -- and DOE requires ALARA evaluations for post-closure PA. But the experience has been that with whatever option you choose, the projected doses are so low that doing an ALARA analysis on top of that just doesn't buy you anything that you can base a rationale decision on, ALARA for exposure to the public in a long time frame.

CHAIRMAN RYAN: And I think, Dave, the other part of that is you have got to be always mindful of the statistical power of comparing a teeny fraction of a microrem to another teeny fraction of a microrem to three significant digits. That's often a problem.

DR. KOCHER: It's a joke.

MEMBER CLARKE: Thank you.

MEMBER HINZE: Anne and Dave, these were great presentations that will make us think and have made us think.

Anne, what I found in your presentation was that there was a rope that went through it entirely and that was the term reasonably or the principle of reasonably. And I wonder if you can help me a bit with -- in the cost benefit world and the adjudicatory world and the civilian population world, what is this term reasonably?

DR. SMITH: That's a good question. I actually was unaware that I was using that word over and over.

MEMBER HINZE: No, but I thought that was the thread that went through it.

DR. SMITH: That's a good point. To me, when -- okay, this is not something I've thought through before. This is a new question for me, but when I think of the word reasonable, I think of the word rationale. And rational in the economics sphere is making decisions that are consistent, show a consistent ranking of preferences that you're not spending more to increase risk instead of spending more to decrease risk.

There are points of diminishing returns where eventually you will stop going further. So that probably isn't even beginning to get to the thread that you're picking up on, but that's the first thing that comes to mind. In the cost benefit field, really, the concept here is there's a point of diminishing returns and what you're seeking is the place where you have an increase in the marginal costs are equal to the marginal benefits and so it makes no sense to go further.

But reasonable, I think, as it was probably used, if I were to go back over the transcripts and see every place I had used the word, I was probably thinking more in terms of the deliberation aspect that there is a process of doing analysis and then deliberating what it says and what it's telling us about the risk and reasoning their way through that process with input from others as to where do you draw the line. To me, reasonable is just deciding as a group that there's a -- that you've heard all the parties' opinions and that you can reason your way through to the right answer or to a sound balancing.

MEMBER HINZE: We have uncertainty in reasonability? That's not a question.

But certainly a line is going to vary, depending upon where you come from and how close you live to a site as to what your approach is.

DR. KOCHER: These are tough questions. My answer is you can't define in advance what that means. You just can't do it. Twenty-five years ago, I was working with NRC staff here on the Part 60 rulemaking for a geologic repository and they were getting ready to put this language about reasonable assurance into the rule. You know reasonable assurance of compliance and DOE was continually asking, what does that mean?

And the only answer that the NRC could give that I thought made sense was reasonable assurance is when we give you a license.

(Laughter.)

You can't -- a differential equation doesn't cut the mustard. I agree with her answer completely in the sense that we're talking about a process and we hope that a reasonable fraction of the people participating in the process play fairly by the rules.

MEMBER HINZE: That's why I said it was all the way going through because what you described was a process, if you will, and involvement of the process. My Chairman here, to my left is kicking me at this time, but I want to ask Dave one quick, quick question.

You discussed the fact that the key technical issues are site specific and design specific. And I'm wondering have you thought about whether the regulations should be site-specific and design specific?

DR. KOCHER: One big difference between low-level waste regulations and what is in Part 60, for example, but Part 60 is, as we've envisioned it, is not going to be used to license anything. But it had subsystem performance objectives and this basically was considered and rejected in the low-level waste business. And what that does is it gives the operator maximum flexibility to design any way he or she sees fit to meet the site conditions. It's a very flexible system and I think at the end of the day some of these subsystem performance objectives probably don't prove all that useful.

CHAIRMAN RYAN: Just to add to your point, I mean and again, as a former licensee, the license is where the site-specific requirements get spelled out. I mean you can have a license with 120 license conditions and I just want to react to the idea that regulation should be site-specific. I don't know how it can be.

MEMBER HINZE: Well, let me give you an example. Perhaps the use of my terminology is incorrect here, but let me give you a case in point. You were talking about timely compliance as being 10,000 years or 1,000 years. Should that be site-specific? Should that be dependent upon the design, the robustness of the design or the geological conditions?

DR. KOCHER: I don't think so. That's a public policy issue to me.

MEMBER HINZE: Okay, thank you.

VICE CHAIRMAN CROFF: I don't have any, so NRC staff, anybody have any questions? Okay, what time have we got? Well, we're miraculously close.

I'm going to make a Chairman's decision here. We have a choice between a relatively early lunch or a rather late lunch. And I'm going to fall on the side of a rather early lunch and we'll do the entire monitoring session after that. So if we can get back here by let's say 12:45, quarter to one and thank you very much for this panel. We'll have a new panel after lunch.

(Whereupon, at 11:39 a.m., the meeting was recessed, to reconvene at 12:45 p.m.)

VICE CHAIRMAN CROFF: I'd like to come back into session if we could, please. We're going to move into the fourth part of our meeting concerning monitoring. Here we've sort of talked about performance assessment and we're down to, I guess, the final sequential stage, if you will, of the stuff is in the ground, presumably, and how are we going to watch it, and can we find out whether things are going bad on us or not. And the speakers in this session are going to address three different aspects of monitoring, sort of moving from the outside in, if you will.

First, I'd like to introduce Dr. Vernon Ichimora, seated up front. Vernon is the Senior Manager for Environmental Programs at Chem Nuclear

Systems, Barnwell Low Level Waste Disposal Site in South Carolina. By training, he's a geohydrologist, and he's responsible for the environmental monitoring program at the site. Vernon, take it away, please.

DR. ICHIMORA: Good afternoon. What we're going to do today is talk a little bit about some of the changes with technology in environmental monitoring, and in-between I'm going to be talking a little bit about status of environmental monitoring technology. And in doing so, I'm going to talk about some of the changes we see in environmental monitoring, and also address environmental monitoring as to how we address compliance.

I have an overview slide. What we have to kind of focus is whenever we talk about environmental monitoring, we always have to focus on the performance objectives as the rules by which a facility is governed by, and I'm going to use Part 61 as an example.

We're going to focus on what appears to be the most important pathway, at least from my perspective, after a facility is closed, and there happens to be the groundwater pathway. I'll say something about Department of Energy sites, and I'll give you a kind of an overview of what the current ground water monitoring process looks like. And I think that in itself is fairly old, and many of you might be familiar with it. And I'm going to try and couple what we see in environmental monitoring results in terms of how its use in a dose assessment methodology. And finally, when I talk about ground water, since it's kind of an area that I know a lot about, I'll also conclude talking about other pathways, if time allows, and then I'll make some concluding remarks. Along the way, I'll make some comments about technology, as we talk about the various aspects of the environmental monitoring process.

One of the things you cannot lose sight of is the fact that whenever you do any kind of monitoring, you need to look at the risk-based results that you can get from environmental monitoring information. So, in other words, what you collect in the field typically are concentrations of radioactive material, so what do the performance objectives tell us? And this is kind of a mouthful but it says, "Concentrations of radioactive materials which may be released to the general environment in ground water, surface water, air, soil, plants, or animals must not exceed an annual dose of a equivalent of 25 millirem and take that to any member of the public." Those are the things that we always keep in mind whenever we do any kind of monitoring and we look at the results. Okay?

The implications of that statement and the performance objectives say a couple of things. Whenever you do collect environmental media, your results are typically in concentrations. In a few cases like direct gamma in the airborne path where you might be able to get -- you will get information in terms of directly in terms of millirem. I would have to say when we look at the ground water monitoring pathway for coal sites, I'll make a kind of a judgment here, ground water appears to be the most important, and I'll explain why I say that.

Surface water, if the site is clean, surface water is probably not that important. Air, probably important during the operations aspects of a facility. If air is cut off, soil, animals, plants are probably fairly minimal.

Because we have concentrations of information, for example, concentrations of radioactivity on various kinds of media, we would have to take the concentrations that we measure at various locations and transport it to a location where a receptor is located. A receptor also implies that he would have to be at a location that is reasonable. And what I want to say is we transport it to a receptor located at a real public location. And again, that's a great debate on what is real, what is a public location. And in a lot of cases, you would have to negotiate what exactly is a public location. That also can change as a function of time.

So what does that look like in a slide? Basically, you may have an area where you have waste disposal. On a clean site, you will not have any contaminated substances on the land surface. Surface water may wash over the site, and if the site is clean, of course, surface water is not of concern. In the process of closing a site, I might add, the tendency is to take away this particular inventory or source term, depending how you want to look at it. The direct gamma would probably be nil. And, of course, one of the most important remaining pathways would be the pathway through which ground water travels which will eventually meet a surface water body. And in this particular case, I'll give you an example; it is a stream. This particular scenario applies primarily to human sites in the east.

Again, for closed sites, I'm going to

elaborate on why I say it's most important, appears to be most important. This is based on environmental measurements, and basically the level of effort that one would take in environmental measurements. It appears that the amount of effort, making a characterization of environmental information in the ground water environment I think would be greatly emphasized at existing disposal sites.

It is also based on hypothetical dose rate assessments, and also population dose assessment. If you take away the air pathway, clearly ground water would stand out by itself. On a closed site, that would be a reasonable assumption. And, of course, when you look at the annual environmental reports that are published by Department of Energy facilities, you can clearly see the level of effort. This also applies to commercial low level waste facilities.

Okay. I want to make a couple of statements about the ground water environment. And again, I'm going to focus on it, but again, the processes that we use for environmental monitoring in the ground water environment also applies to many other pathways.

In the ground water environment, contrary to what a lot of people think, gravity is a very powerful force, and it drives water, basically, from the high point to the low point, and eventually ends up at the lowest point. One of the things about ground water, it moves relatively slowly, and it's not as dispersive as one would think.

This process in itself gives us a very, very good history. And again, I cannot emphasize this process gives us a history of the site. To the extent that we know, we can take information that we can extract from monitoring wells that might be closest to this facility, meaning what we might consider an earlier monitoring well, might be one that might be a boundary well that might be closer to the facility, might be used for compliance in the ground water environment. Although I don't encourage it, there might be an off-site well just to verify that you don't have a problem. Okay. Again, I want to emphasize ground water moves relatively slowly relative to air and surface water, and it leaves a history.

I used a title slide that says "Radionuclides in ground water at Department of Energy sites", but this also really applies to commercial low level waste sites too.

What we see in the ground water

environment depends upon the quantity and the availability in waste, that determines what radionuclides are seen in the environment. And more importantly, what's the driver behind what we could see is the mobility of the radionuclides. And at the current time, and I want to emphasize at the current time before some of the PA folks tell me that some day in the future there are going to be more important radionuclides. Tritium appears to be most widespread, and it's common throughout many facilities.

Other radionuclides that are present, and again at the Department of Energy sites, I don't think carbon is that widespread, but you see Cesium, Cobalt, Iodine, Strontium, Technetium and, of course, the Uranium isotope, Plutonium isotope, and see some things like Americium. And again, as you get closer and closer to the source, you run into more of the esoteric radionuclides.

One of the things I want to say here - again, going back to the history of the importance of radionuclides is if you were to go to the literature and look at the hypothetical model calculations. And I'm going to take one radionuclide as an example. Carbon-14 appears to be not too important; whereas, what we have seen, Carbon-14 is probably as mobile as

Tritium, and I'll comment and I'll give you a rationale behind why Carbon-14 might be important.

There's some tremendous advantages in looking at sites that are designated for WIR waste. First of all, these sites have very, very good baseline data relative to walking into a site which has never been licensed, like for example, a new site at a new location. And these sites would have reasonable history of radionuclide releases, and these sites will be characterized as some information. It may not be up to the standards that we would appreciate today, but they are characterized. They will have, as implied above, environmental measurements of various quality. And the sites may have environmental transport models associated with it.

What's important to recognize here, because you have a history of this site at this location, you can address some of the hydraulic parameters that are very hard to get to. Like Dr. Kocher mentioned this morning, for example, the combination of hydraulic conductivity and effective porosity. It's very, very hard to measure in the field, and unless you have a tracer, as an example, you cannot get to that information directly. So what you have at these particular sites is a good history, good process by which you can address what has happened to the radionuclides as a function of time.

In addition to that you will have different kinds of radionuclides at various locations. For example, perhaps Tritium is the leading charge in terms of how it's impacted the environment, that is, has spread the furthest. And you're going to have radionuclides that are going to sit in the back, that are coming in at a slower process, kind of like the chromatography process. And from that information, you can extract distribution coefficients, for lack of a better term, and it gives you some idea of what retardation coefficients might be for radionuclide releases.

The other thing I want to point out about a lot of these sites, some of them have 40 plus years of data. That's an important term to remember, so you have 40 years of history. At the site I work at, we have about 30 years of very good history. What this does for you is it bounds the potential outcomes that you're going to have for performance calculations, kind of limits what's the possibility of potential model outcomes.

I'm going to shift gears a little bit and kind of give you an overview of what ground water monitoring programs look like. And, of course, this applies to almost any other pathway. First of all, when you build a program, you're going to have some knowledge of your media; so in other words, you're going to have to have some information about your hydrogeology, and from that information you build your monitoring network. Depending upon your source term, you have an analysis agenda, and I'm going to elaborate on each one of these topics.

And again, you cannot forget what the performance objectives are or is in terms of setting up what your minimum detectible concentration should be. And then finally, for what the trend is, there's more documentation. And then there's some control of all the processing by which we do it through the Quality Assurance Program. And I'll talk a little bit as we go through this set of slides.

Okay. Fundamentally, the site hydrogeology gives you ground water flow directions. Typically, ground water flow directions are very easy to get to. The thing that's usually very hard that's associated with ground water flow direction is the flow rate. Flow rates are pretty hard to get to, and you can really only get that information through modeling, what your conceptual view of the hydrogeology looks like.

Hydrology gives you some idea of ground water and contaminant velocity. Without traces, I think you stand on very weak ground getting that kind of information. Site hydrology gives you ideal monitoring locations, like where the sand layer is. It kinds of gives you well placement information, and perhaps depth and location, and perhaps in some cases construction. You cannot construct certain kinds of wells for -- well in a clear environment or a clay/sand environment, whereas another certain kind of well constructions do better in sand.

About the monitoring network, I cannot emphasize the need for quality wells. And again, this is characteristic. If we were to put in a new site today, the wells would meet certain quality standards. What we're looking at is some sites that have very, very -- a lot of history behind it. Monitoring of wells for different purposes, and consequently the quality of the wells re in question. When you start with junk, you end up with junk, in other words.

And in the monitoring network what you try to do is for compliance demonstration is place it where the contaminants are expected. You have different categories of wells. We have early morning wells that I've showed you on the slide before. At some sites we have what we call water table wells, or water level wells, and these are actually separate from compliance wells, or wells which we sample to get samples of the ground water.

This is particularly important at sites that have a very, very low hydraulic conductivity. What this means if you were to sample a well, the well may not recover in the time period that you make a determination of the water level. So, in other words, if you have a situation like that, you're unable to measure ground water gradients very well. And, of course, you have boundary wells that determine compliance, and then you have off-site wells.

The final thing I put on the slide here, and it's something that we always, out in the operations field, we don't deal with this, well maintenance. Well maintenance is very important at some facilities, particularly in facilities where wells silt up as a function of time. There is small amounts of sediments that come into the well as a process of sampling, and the wells will fill up as a function of time, and you need to keep track of it.

Let me say something about analysis

agenda, and they apply to nearly all sites. Typically, you look for pH and conductivity, radiological indicators such as gross alpha and beta, and gamma ray spectrometry, which is fairly simple, just taking a sample. Where it becomes difficult for different sites, and I think the emphasis should be placed on identifying what specific nuclides you need to monitor because of cost constraints, or looking at the list of specific radionuclides. Certain specific radionuclides might be more important and in other sites they are not.

One item I haven't talked about and I'm going to stay away from, this is the area -- there are other parameters that are associated with an environmental monitoring program, including like I alluded to before, water level, elevation, and the EPA priority pollutants that consist of approximately 130 contaminants. And the EPA priority pollutants sample collection process require very, very elaborate processes to preserve the sample. Typically in the analysis of ground water, we typically get quarterly, annually, or every five years, or as required by special investigations.

I put this alone because in a lot of cases these programs, ground water monitoring programs have been established a long time ago. And I think in a lot of cases, this is not a particular problem, minimum detectible concentration goals in ground water, but you always need to be focused on the environment. In the regulatory requirements, can you see the lowest concentration that's required, and some fraction thereof, and sample that particular media so that you extract a certain amount of sample that's practical. Doing it quarterly and routinely, you have to balance what your regulatory requirements -- what your minimum concentration goals, which is based on regulatory requirements should be, and what can be practically sampled.

About some sample technology, and again, I'm using this as -- I'm giving you a ground water example, but this applies to many of the devices that we see around, and applies to air samples, for example, and ground water. But again, for ground water in particular, the devices that you use for sampling depend upon the depth, the yield, well constructions.

We have generally a requirement to go to three well volumes, three to five well volumes if we build a well, and then we then extract a sample. The tendency at least in ground water sampling methodology, we use bailers, and the trend is that we get into more and more high-tech bailers made out of Teflon. Bailers that may sit in the bottom of the well that have check valves on them that close when you pull the sample up.

There are various kinds of pumps, and over the years they've become more elaborate. We have a lot of electronics, and a lot of controls at the very top. I've given you three categories, the standard or electrical submersible. They're bladder pumps, they're pumps that operate by mechanical devices, the check valves that actually lift water. And the tendency today, at least in ground water sampling methodology, is to go to devices that minimally agitate the water in a margin well to get a very good sample at the sample collection zone, and to go to low flow and low purge volume. The latter is probably in the long run very important because what it does is decreases the cost of monitoring, because of the fact that when you extract contaminated water, you have to treat the water. Again, there are various means to deal with the low purge volume method.

This is a particularly important aspect, documentation. The trend is to go to more procedures, and the procedures drive data collection forms. And what the procedures and data collection forms drive consistency in what you collect. Another thing that has been introduced here in my tenure doing environmental monitoring is the chain of custody. What it does is tracks samples, it provides some kind of who handled the samples, and under what conditions the samples were handled.

When you get the sample results back from the laboratory, and I'm saying very little about the laboratory, the tendency is to get back a certificate that certifies that the sample was analyzed a certain way, and the results are good. And along with that, you may decide there are certain quality control reports to identify how the blanks work with this particular batch. And finally, receive environmental reports.

Quality assurance and quality control, this is something that has become more important as time goes on, and I think there's going to be some changes. There's going to be more rigidity in this process as we progress through the monitoring, as we progress through time. There are programs that ensure good practices. There are mechanisms to assure that the operating procedures are carried out. This is very, very important, and I'm going to talk about and emphasize why standard operating procedures are important.

In the environmental monitoring process, you're looking for trends, and you want to be able to compare today's samples with last year's samples. You cannot make the comparison strictly if you change and you don't adhere to standard operating procedure. You need qualified personnel, people that are trained, that know what they're doing, and a qualified laboratory.

On the very last slide, I want to kind of emphasize that whenever we change devices, and I've seen this with all the new fangled devices, is to use reliable and well-maintained equipment. And reliable, by that I mean - I'll take an example. The standard air sample that we see at many of these sites, the design is probably 30 plus years old, and today we see more and more air samplers that basically look the same, but they have little black boxes next to them. And what they do is they record the performance of the sampler, the function of time, and the question is, whenever you make these changes, are these black boxes reliable, and how will they affect the sampler in the end. So the choice of devices, although they've become very, very good, you need to ask yourself are the devices reliable, because you're relying on samples that are collected quarterly, and you want a result quarterly.

This is going back to performance assessment. Because in a lot of cases in the ground water environment we get results in picocuries per liter, as an example. We're going to have to translate that picocuries per liter to annual dose, and the process that we may use, as one example, is to take the environmental monitoring results, put it into a ground water transport model, transport it to another location. And in this particular example I've given you, I've coupled the stream transport model, and then put the dosimetric model as kind of an all-encompassing lump for the habits of the receptor, and the receptor locations to get annual dose. And again, this process - what's important about this process is as you collect more environmental information, you can repeat this.

I'll make a couple of comments about surface water, air, and soil, plants. Surface water is usually, at least in the human east is a receiver of ground water, and it greatly dilutes ground water, as an example. Usually within a ground water basin, surface water is a place where all ground water meets.

And because of that, when you look at sites or areas that are contaminated, and when you compare the areas of contamination to the whole site, the regional basin, can you imagine how much dilution you get? But generally, surface water at some point in time is consumed off-site, so you end up calculating maybe a real dose at a water treatment plant downstream.

Air - again, this is just an opinion, air does not appear to be important in a closed site, provided the site is closed with the surfaces clean. One of the characteristics of air, it transports very, very rapidly, and it dilutes the contaminants, and it just tends to spread everything out. And it doesn't leave a real good history in all other cases.

Soil, again, if you cut the air pathway off, it probably won't be important. Plants the same way, animals appears to be the same, too.

The tendency that I've seen over the years in looking at environmental monitoring literature tells me that some of these programs, at least soil, plants, and animals, the tendency of the programs are to -- the emphasis on the programs are very, very small.

To kind of summarize what I see in environmental monitoring technology, the fundamentals of collecting samples have pretty much remained the same. There's more rigidity in some of the procedures and standards. There's a lot of new equipment that I would say they look like they're new, but they do the same thing. And then there's a lot of what I consider enhanced equipment, these little black boxes on the side. And the old equipment have become more reliable, they're more and more standard practices. So it kind of boxes you in on what you can do.

I'm going to say something a little bit about compliance evaluation, and just some thoughts about it. It would be very, very helpful if you were to do compliance evaluation based on direct measurements to the extent you have, and you need to look for sensible scenarios. Sensible scenarios would mean if you're dealing with a fish that's swimming in contaminated water and you know, for example, the fish has a certain amount of Cesium, and you ask yourself, it's a little tiny fish, does it make any sense that the fisherman is going to consume the fish, and then that'll be your critical dose pathway to man. If it doesn't make any sense, then you change your scenario.

The other thing that I've seen over the years, is I try to use - this is just a personal opinion - the simplest and most reliable, best standard methods. This becomes important in trying to explain to someone else what method you've used, and say well, oh, I use res rad and that kind of thing. It knocks out a whole bunch of questions with regard to how the compliance evaluation was done.

I want to make a couple of concluding remarks about environmental measurements, and this is something that is quite important. Environmental measurements have shown that the facilities we're talking about have basically met adequate requirements, and seem to at least in your term projection. Again, if the facilities are closed right, if you take out the air pathways, and when you look at the environmental monitoring reports, ground water seems to be clearly most important. And in general, I would want to make kind of a statement, and some people might disagree with me on the last statement, the technology. Make a reasonable determination on Part 61, evaluation on the protection of public from the harm from radioactivity generally available. With that, are there any questions? I guess questions will be taken at the end.

VICE CHAIRMAN CROFF: I think I'm going to take the questions at the end and the panel discussion for this particular session, so thank you very much.

And I'd like to go on and introduce our next speaker. Vernon just finished talking about monitoring around a near surface disposal site. At this point, we're going to move in one step and talk about monitoring engineered barriers that sort of constitute part of the disposal site, if you will. Caps and subsurface barrier walls.

To do this, I'm pleased to introduce Dr. Craig Benson. Craig is a Professor of Civil and Environmental Engineering, and a Professor of Geological Engineering at the University of Wisconsin-Madison. He's worked on containment systems for the past 20 years. His research has emphasized methods to design, evaluate, and monitor barrier systems, including caps, liners, ground water cut-off walls, and permeable reactive barriers. His recent work on capping has been through the EPA's Alternative Cover Assessment Program, which is a nationwide study on capping systems that include 27 field test sections to evaluate cap designs in various climates.

DR. BENSON: Okay. Well, I'm going to talk about actually a few different things today. I don't think I'll need that. Can you hear me in the back?

VICE CHAIRMAN CROFF: You have to use it

whether you like it or not.

DR. BENSON: Oh, for the -- yes. All right. I tend to talk too loudly as it is, so this only amplifies this. I'll save my voice a little bit. I'm going to talk about the status of technology regarding caps and barrier walls, and then talk about the types of systems that we might use to monitor them, at least the ones that we have been using in some cases.

To begin with, just a little definition. We talk about a capping system, we're talking primarily about some type of containment system on top of the waste. We've got a waste mass of some sort and we're going to put a barrier on top of it that may have several functions. Probably the most important of those functions, though, is to limit the amount of water that would percolate into the underlying waste with the objective of minimizing the amount of leachate that would be generated.

We might have a lining system, as well, not as common in rad waste facilities, but very common in hazardous waste and solid waste facilities, in fact required. Some radioactive waste facilities will have liners, as well.

Talk about a barrier wall, what I'm

talking about would be a vertical slot into the subsurface essentially providing a barrier to movement of contaminants that might migrate out of th facility and move in ground water, so when we talk about barrier walls later on, we're talking about essentially a vertical slot through the ground that acts as a barrier to contaminant transport.

I put together this table essentially to describe the status of these different types of systems. And I think it's important to kind of get the context both in terms of the age of these systems and our understanding of the technology. We look at lining systems, we've been using lining systems for about 30 years, and the same with capping systems. And our knowledge of lining systems is very mature. We really understand lining systems. We can predict their performance pretty well. High level of field performance characterization.

Caps, on the other hand, even though we've been using them for nearly 30 years, our science status is really evolving. In fact, it's evolved tremendously in the last five years. I would say our level of understanding of how they behave at field scale is modest, even though we've been using them for 30 years.

Cut-off walls, probably used as much as 50 years, not necessarily always for environmental applications, but we have used them for a variety of both environmental and civil applications for 50 years. Our science status in that area is evolving at a very slow rate, and our field performance characterization, to be perfectly frank, limited is a nice way of putting it. It's almost nil. We really don't understand how well they work in situ.

Another technology, which is actually - and I put this in for context - called a reactive barrier. A lot of cases where you use a ground water cut-off wall, a vertical wall as a way of blocking the flow of contaminants. Another kind of corollary to that is to rather than having something that blocks contaminants physically, is we'll put a permeable system that reacts with contaminants as they move through ground water and treats them in situ. And that system, which is relatively new, and in fact the first so-called permeable reactive barriers were installed less than 15 years, I think actually 12 years ago. And that technology has just blossomed, and we're really at what we might call a modest level of field performance, so a shorter time period, but a much higher level of understanding. So the time that something is being used does not necessarily mean that we understand it very well, or can be very confident in its performance. Age and understanding are not necessarily equal.

Talk about caps. We can look at caps from essentially two different contexts. We might categorize them in two different ways. One a so-called resistive design, a water balance design, resistive barrier design. There's a system where we put some type of hydraulic impediment, some barrier in place that blocks the flow of water. And we've talked about this a little bit on and off for the last day or so. That might be a clay barrier, that might be a plastic sheet called a geomembrane. Those of us in the geotechnical business, we put geo in front of everything so that we can kind of claim ownership to it, so we have different types of geomembranes, or plastic sheet, typically polyethylene. We might have drainage layers, and then some vegetated surface on top.

Really the differences in these so-called resistive designs is what type of hydraulic impediment is put in place, and it comes in a variety of different scenarios. Clay barriers, the so-called synthetic clay barriers which is a very thin prefabricated clay barrier, and I'll show some of that. And then a composite system normally is a combination of plastic and a clay barrier underneath it.

The conceptual model is that this system when we build it will have a certain throughput or transmit a certain amount of water, and that it will maintain that condition essentially in perpetuity, or it will, as we talked about earlier, it might fail immediately. The question is, do these systems perform very well, and we'll see some of them do, and some of them don't.

Another type of system, the so-called water balance design, completely different approach to capping systems. Water balance design, rather than using a physical barrier to water movement into the underlying waste, is we'll use a natural barrier. We'll use unsaturated soil behavior to store water within an engineered soil profile that stores this infiltrating water. And then we'll release it back to the atmosphere via evaporation and transpiration. Essentially using water balance, balancing the components of the water balance, evaporation and infiltration. And we design these to have a certain storage capacity, and once we exceed that storage capacity, they will leak. And the question then is how do we pick the storage capacity to make sure that the system functions to transmit a certain amount of water. So two fundamentally different approaches, and in some cases, usually we'll use one or the other, but there are examples where they've been combined.

For example, at the Hanford site, the permanent isolation barrier is an example where a so-called water balance cover has been combined with a resistive barrier system underneath it, actually made out of asphalt as opposed to a geomembrane, but an asphalt barrier, and then a water balance barrier on top, so a combination-type system being evaluated at Hanford, a very costly but very effective system.

There is another combined system actually like that. It's been built at full-scale at the Monticello Uranium mill tailing site by U.S. Department of Energy as part of the clean-up activities at that site, has a water balance cover with a composite geomembrane clay cover beneath it.

It was interesting, and I mentioned earlier, we really only in the last, I think, five or six years really started to understand these systems despite their use for nearly 30 years. There have been a number of different studies which have been conducted concurrently. There was Department of

Energy's Alternative Landfill Cover Demonstration done at Sandia that's been completed where they did a number of different test covers and evaluated them side-by-side.

There's the study I've been involved with sponsored by USEPA called ACAP, which is another cover study. There's the study done at Hanford, and there's a variety of kind of small studies that have been done on a site-specific basis for specific applications. But in all these studies, they've had an underlying goal of trying to understand the behavior of these systems, and to collect data for calibration and validation of models.

This is the study I mentioned that we're involved with, the ACAP Program. This is a nationwide study to understand hydrology of covers, and I believe we have 27 different test sections that we're monitoring throughout the United States. Some of them have been decommissioned, so the actual number currently I'm not quite sure.

I'm going to show you some data from some of these. We talked about a conceptual model in the beginning, is that we would build a clay barrier, and it would have certain properties, and it would function that way, essentially in perpetuity, or until it suddenly failed. And the question then is, is that a reasonable conceptual model?

I'm going to show you data from two different sites that kind of bracket the spectrum of climate we might find; one in Albany, Georgia has a fairly characteristic type of clay barrier which would be a presumptive remedy under, for example, a CERCLA project. And then another site that meets California regulations, in Apple Valley, California. Now in Albany, they may get 100 inches of rain a year, and in Apple Valley, California, they typically get four inches of rain a year tops, so very different types of hydrology.

The site at Albany is really interesting because this is essentially a cover that was essentially required by regulation for this particular superfund site, and we instrumented it, and monitored its performance. And this is some of the data that we collected. This is a graph of water balance quantities, and it essentially shows cumulative quantities of applied water, which is precipitation plus some irrigation water used to stimulate the growth of grass, evapotranspiration or removal of water from the soil back to the atmosphere, percolation or what drains out the bottom of the cover, and then some surface runoff shown here. All these are increasing or accumulating over time. We're totalizing the flows over time, and then something else called soil water storage, which is the amount of water stored within the cover at any particular time, stored within the soil that accumulates when an infiltration occurs, and it decreases when drainage occurs out the bottom, or percolation occurs, or water is removed from the surface via evapotranspiration.

What's really interesting at this site is to look at this red line. Percolation is ultimately what we're interested in, what drains out the bottom of the cap? And at this site, essentially the design criterion is about 30 millimeters per year, and that's actually a very high percolation rate compared to most of the sites where we're designing for very small quantities of flow on the order of a millimeter per year, in some cases less. But what you see is this the time line here, and this is classic month/day/year. And you'll notice there's this abrupt change at about six months. There was a short period, relatively short in the overall life span of things, so about six weeks where they didn't have any rain at this site in Albany, Georgia. And within that six week period, the clay barrier dried out, and when clays dry they tend to shrink and crack, and the cracks become preferential pathways. And you'll notice we went from a very slow, gradual, smooth accumulation or drainage of water, to a very rapid and stair-step-type of transmission of water later. We transitioned from something that was a fairly good barrier to flow, transmitting about 30 millimeters per year to something that was transmitting on the order of 25 percent of precipitation. Something that transmits 25 percent of precipitation is a natural system. That's a natural recharge rate, so not a very effective barrier.

MEMBER HINZE: Is that the implementation time of the cap right there?

DR. BENSON: Yes, this is construction right here.

MEMBER HINZE: Okay. Thank you.

DR. BENSON: Construction, April of 2000, finished, and then monitoring after that. And so where within six months we went from something that functioned reasonably well to something that essentially ceased to function in a very short period of time.

Even in an environment with 100 inches of rain a year, which is a very wet, humid environment, we have problems with dessication at that site. We went back and did a pretty detailed study to look at why that occurred, and what was happening within the soil, and we did a series of tests. I won't go into these in detail, but we were looking at what types of features caused those problems. And it was clear to us from that field study that it was a series of cracks that had formed as a result of dessication, and that resulted in essentially a change in the hydraulic conductivity as-built around the 10 to the minus 7 centimeters per second, which is very close to the typical regulatory standard, to something that was about 200-22,000 times higher. All right? So it increased at least two orders of magnitude, and maybe four orders of magnitude within six months of construction. We had a very abrupt change in the properties, so this clay barrier in the conceptual model that would remain the same is not a reasonable model. The clay barrier became very permeable in a very short period of time, far more permeable than one would have expected.

This is our site in Apple Valley, California, and this is a very different type of hydrology. In a similar type of graph, if you look at this site and you looked at the other graph closely, you would notice that the other site there is a deviation between precipitation and evapotranspiration. At this site, the two follow each other very closely. Almost everything that falls on the ground at this site goes back to the atmosphere, and there was somebody who mentioned that about Nevada as being a characteristic process of semi-arid sites and arid sites. But some water does get through, and at this site we constructed in April of 2002, and it wasn't until about two years later until we started to see some drainage from the bottom of this cover. In this last year, we had really a large slug of fluid come through that cover, and larger than we would have ever expected from this barrier system.

Now this is in the Mojave Desert, and so it's under a very highly stressed condition, at least in terms of desiccating and damaging the clay barrier, and yet it did last a couple of years. Ultimately, we had considerable drainage or percolation from the system, so this would occur in a very wet climate, and in a very dry climate. We get that same type of phenomenon. It's not unique to a particular climate.

This is actually a picture of our monitoring area. This is 10 meters wide and 20 meters long in this direction. This is actually taken last week, the 25th of July. It was about 104 degrees at this site in the Mojave, and you can see where there is -- this is the surface of our test section, and there's actually a lot of vegetation on it. And in late July, there's green vegetation on this test section. Why do you think that is? If it's green vegetation, there must be water available for it. Right? Where is the water coming from? Well, the roots of the vegetation are down in the clay barrier, and they're extracting that water and returning it to the atmosphere, so that the clay barrier is being desiccated and damaged by that transpiration and evaporation process.

I mentioned to you synthetic clay liners. These have been considered as a technology that can be used in lieu of clay. They're thin, typically about 10 millimeters thick layer of two fabrics with a granular Bentonite clay between them. It's a Sodium Montmorillonite clay, pretty widely used. Their performance in the field has not been well documented. Typically, one of the nice things about them, at least the selling points is that they have very low hydraulic conductivity to water. That's about 50 times lower than the regulatory standard for clay barriers, about 50 times lower.

The question is, this is what they are when they're new, is how do they stay that way? How long do they retain that level of hydraulic conductivity? These are pretty widely used because they're easy to install. And conceptually, if they stay at two-tenths to the minus 9 centimeters per second they transmit very little water, but they're easy to install. They roll in like a carpet. You can see this being installed at a slope, essentially a cap at a site, comes off the spreader bar very easy, quick. But we really don't know a lot about how well they perform.

I got interested in this because we had a case history where one was being monitored and didn't function as intended in Wisconsin, and so we got a study funded to dig some of these up and examine the hydraulic properties of these over time, and we picked up four sites. And this is actually a difficult thing to do to get people to actually let you dig up their existing barriers. They aren't mine, they're somebody else's, and look at the properties of those barriers after they've been permitted and closed. It was a challenge to get parties to agree to allow us to do this.

Essentially what we did, we went to four different sites, we dug up the barriers, we patched the existing condition and refilled it, and we brought those samples back to our laboratory to look at, essentially just removing the cover and taking the samples out, and then essentially replacing the barrier system before we left.

We looked at a number of things, some issues with regard to ion exchange, which are kind of important in terms of the change in these materials over time, and we also looked at the hydraulic conductivity, essentially the design parameter that's most important in terms of containment. That was our initial condition that we were planning on about two times 7 minus 9, and at these sites, none of which was more than five years old, the hydraulic conductivities are on the order of 10 to the minus 4, to 10 to the minus 6 centimeters per second. There's a few down here at Site O, and then at the one site they were considerably lower. But this is within five years, these materials are several orders of magnitude more permeable due to a combination of both ion exchange and dessication. So the conceptual model of this is not necessarily correct. It's not going to be perform at two-tenths to the minus 9. The question then is, how can we detect that?

MEMBER HINZE: Can you go back to that slide?

DR. BENSON: Sure.

MEMBER HINZE: These were all geosynthetic clay liners.

DR. BENSON: Yes.

MEMBER HINZE: It's the composite versus earthen?

DR. BENSON: Oh, yes, I should have explained that. I had a hypothesis on this. I picked four sites in my original hypothesis, and it was that if I found one where there was a sheet of plastic on top of the GCL, that it would protect the geosynthetic clay liner, and it would essentially look like it was new when I unearthed it, so I was going to have this case to prove my point, to show this is what we need to do, and the GCL is going to be in perfect condition. And all the other ones were just a classic earthen cover where we had a GCL within a cover soil on top of it, but no plastic membrane.

In my hypothesis, of course, that's why we make and test hypotheses - the hypothesis was completely wrong. In fact, this was probably one of the worst of the sites, and it made me re-think the mechanisms that were causing the damage for these particular types of barriers.

MEMBER HINZE: That's the basis for your design, isn't it?

DR. BENSON: That is one of the designs, a composite design. And for GCL, a GCL even with a plastic on top, they've not maintained the integrity of the GCL. This just shocked me, it really did, and the details for why that occurred is kind of a different process that I didn't think of before. It's an upward diffusion process, and there's hydration mechanisms, and the timing of those two, and the timing of the hydration relative to ion exchange through diffusion is what, I believe, controlled that. But I guess that's a story for another day. A good question. I think it's a good example that you can have hypotheses, but it's really important to test them. Now whether we're doing a research project or a full scale facility, because we always have hypotheses and assumptions.

So largely the data showed there were for covers, except for that one site, where we had a resistive barrier made out of clay, either geosynthetic clay or a natural clay that was compacted in place. Let's look at some data for composite systems and water balance systems, and I'm going to show you from Polson, Montana. This is from western Montana, a sub-humid site with about 400 millimeters of rain per year, rain and snow. And at this site we were doing a side-by-side test of two different types of covers, a classic composite, RCRA Subtitle D or RCRA Subtitle C type of design, Modern Subtitle D, and then an alternative water balance type of cover.

And at this site where we - this is the composite barrier. The composite has performed extremely well. The plastic over clay barrier has functioned just wonderfully.

This is again a similar graph, a water balance graph, accumulative water balance quantities. And this one we have lateral flow, we have a drainage layer on top of the geomembrane that's transmitting a small amount of water, not a great amount, some surface run-off, and some storage in the layers over top of the geomembrane, water going in and out of the overlying swells, but this key one down here at the bottom is percolation. So we have some ticks and percolation that occur intermittently, normally associated with snow melt events, that get through that barrier system. But on average, on a field scale value, we're talking less than a millimeter per year of percolation, less than a millimeter per year on average when I scale to field.

I have to put that in a little bit of context. When we built our monitoring systems, we monitored a 200 meter square area, and put in a small hole that was representative of design hole, and that engineers would normally use for design computations. However, we put in one hole, and if actually you do this, the frequency of holes for a normal field scenario, that frequency is about 10 times higher than you'd normally see in the field, because we can only put in a minimum of one hole. So our number of holes is a little higher, so I scaled these by about a factor of 10.

On an annual basis, if you take that number over five years, that'll be about 10 times higher for our test section, our monitored section, but on a field scale it would be 10 times lower, or about .05 millimeters per year. But that's in general what we see for our composite barrier systems. They function extremely well, at least over this five year period or so that we've been monitoring them.

The percolation rates range anywhere from near zero to on the order of a millimeter per year, depending on whether we're in a dry climate, or in a humid climate. That's a very good barrier system, limiting it to a millimeter per year in a humid climate system where we might have a total application of 1,000 millimeters of water on the surface.

The other type of system, as I mentioned, the water balance covers, the other approach to managing water is instead of having a physical barrier is this balancing approach of storage and transpiration, and evaporation. And this type of system can work very well, too. We found that this system can be very effective at the site in Polson, Montana. Again, we have side-by-side test sections. We have less than a millimeter per year total over a five year period, so actually we got slightly better performance, you might say, out of this test section than our composite, which actually has a sheet of plastic in it. Just using a natural balance of soil water storage and transpiration.

Now it's a less in the composite but from a practical perspective, they probably work about the same. But these systems can function very well, but they're contingent on our understanding of our ability to store the water, and then release it back to the atmosphere, the latter being largely controlled by a biological system, the plant, that we're going to put a lot of faith in. Our understanding of the biological behavior, essentially the plant system, is not as strong as it is of the soil physics.

Here is our site in Sacramento, a similar type of system with a water balance cover, where we have a cover that's about a meter thick, and where we rely on natural grasses at this site to remove and transmit the water back to the atmosphere. Sacramento is a little bit north and east of San Francisco, and it's a semi-arid climate, about 400 millimeters of rain a year. And at this site you'll see a similar type of graph. What's interesting here is that -- what's key to these systems, and what's interesting at this site is that we're relying on the plants to extract all the water stored within the cover each and every year. And, in fact, that happened in our first year. Our soil water storage climbed up, and then the plants drained the system back out and sent it back to the atmosphere. In the second year they didn't, for some reason.

Our conceptual model, or our basic understanding was that it would do this every year. In fact, our numerical models predict that. If I went in the design simulations I did for this site, it showed the water storage coming down every year, but there was something in our biological system that we didn't build into our numerical model, or our conceptual model, and we see periodically these plants do not remove all the water. And when they don't, this system which is essentially a store and then release principle, well, it doesn't release one year, and as a result there's no storage capacity the next year. We haven't removed the water and, therefore, the next wet season that comes back up, and the storage capacity is exceeded, and we get a big slug of percolation coming through. And at this site on those occasions, it's about 100 millimeters, about 100 times higher than our design standard, and 100 times higher than we predicted based on our a priori calculations.

So some of the lessons we've learned about conventional covers from our monitoring has been that the composite systems work very well. We can get less than a millimeter of water a year in humid climates, and less than a tenth of a millimeter per year in arid climates with these fairly standard systems.

The systems without a geomembrane that are resistive-type systems relying on clay do not function very well. They do not function well, and really there's an abundance of these used throughout North America, and they're still being constructed today, and I'm not quite sure why that is.

From our water balance covers, this alternative approach we found that we can design these to meet a millimeter per year or less in the western United States. We can essentially make them as effective as our composite covers, but are contingent on two things. We know how much water we need to store, and we can design to store it, and that our plants, our vegetation will provide the capacity to remove the water each and every year.

In that latter part, our ability to understand the biological side of it, is probably our weaker understanding in this area, and really is something that can confound our interpretation of performance.

CHAIRMAN RYAN: Just a little short clarification on that point. Did you pick the plants based on their ability to transpire water, or did you pick them based on their ability that they'd grow in that location?

DR. BENSON: Good question. The philosophy - the question is how did we pick the plants, what was the philosophy behind it? The philosophy is largely that you need to pick something that's native to that area. And conventionally, you look at what the vegetation in the grasslands of that area is, which largely goes through, at least in semi-arid areas will go through a store and release-type mechanism. They've adapted biologically to extract all the water that's available, and then they stop. So we used whatever was at the natural environments. We didn't custom design it. On the other hand, we were relying on our historical information about how those systems behaved in those areas, so it wasn't ad hoc, you might say. Did that --

CHAIRMAN RYAN: Yeah, it's an interesting question though, because if you picked the local plants that may or may not optimize for the purpose of having a predictable cycle, I mean, it's an interesting question. Do you pick it because you want to stabilize the soil from a geotechnical perspective, and so forth. There's lots of variables there. It's an interesting area.

MEMBER CLARKE: Precipitation comes in what form?

CHAIRMAN RYAN: Well, that's the other thing; Montana, which has six months of snow cap is different than California.

DR. BENSON: Very different. Very different sites, yes.

CHAIRMAN RYAN: So it's interesting to

think a little bit about that.

DR. BENSON: The custom design of vegetation is complicated by nature, because nature doesn't like what we want to custom design.

CHAIRMAN RYAN: Exactly.

DR. BENSON: Nature wants to go back to her system. And largely, the philosophy of designing these systems that I've worked with is to try to work with nature as best you can, because whenever you work against nature, you end up losing the battle. She has a lot more experience, and a lot longer track record. So that's kind of some history on what we know about our capping system. I'll talk about how we monitor there performance currently in a minute. I'll talk about cut-off walls, was the other thing that Allen asked me to talk about.

The cut-off walls where we're putting a vertical slot in to block flow. And they're all barrier systems. We have a horizontal system to block infiltration. The other one is a vertical barrier to block essentially horizontal flow of ground water. The big difference between these systems is that we can't really see them when we put them in. When we build a cap, we build it in layers, and we have people walk all over it as we're building it and take pictures of it, measure things. There are a lot of quality controls that can go into that, so it's very visual. And when we build a cut-off wall, I can draw this in Power Point on an engineering drawing and it looks like this, but whether it's actually built like this is a whole other question all together.

We might build these out of earthen materials, a soil bentonite mixture. Some are made with geomembranes, as well, becoming more popular. A classic one would be with a sheet pile steel wall being driven in place, and then there's some composite systems again where we're combining geomembranes and soil barriers, and sheet piles and soil barriers into a composite to essentially get the best of both components.

Those are the more common types. There's also other types of systems that people have experimented with. Some of these kind of slip-form barriers where essentially we use a mechanistic system to essentially carve a slot in the ground and then fill it with an impervious material.

There was a good bit of work on that done by DOE in the SCFA when that was active in the 90s, and then there's work done, frozen wall technologies - the idea that we could create a frozen barrier, as well. But largely, those technologies are only experimental, and they haven't really been verified at full scale. The other ones I showed in my first slide are really the ones that people are using. And to give you a picture about the difference between my drawing and Power Point and the real thing is shown here. This is what they really look like, and this is how they're really installed. This is a vertical or soil bentonite wall being constructed at a site. See the river here, there's a source over here somewhere that's going to be transmitting contaminants to that waterway. They're excavating this trench with a long stick backhoe, and they're mixing the backfill materials over here on the side, and they're going to push those materials into this opaque liquid here, hoping they end up at the right spot. And we blend the materials on the side, and it's actually done. We blend them on the side with a bulldozer. It's not a high precision recipe here, blend it, and we push it back in the trench. And we don't really know where it goes, and that's a little bit of a cumbersome problem, because that can result in things like windows, a window, a very permeable zone that we can't see. We may end up with an excavation that doesn't go deep enough, so that we end up with a gap beneath our cut-off wall and the top of the aquitard, essentially a window of itself due to under-excavation, both of which being permeable pathways.

If we use a plastic wall, they tend to be put in in panels, and they have joints. The joint may not seal, but since it's subsurface, we don't necessarily know whether it's sealed. We may tear it, and we may not be able to key it into the underlying aquitard. I should point out that this geomembrane example is I'm looking into the wall as opposed to this other one, I'm looking downstream.

So the point is that we can have defects, and these defects, even if they're small can dominate the hydrology of the system. This is just an example. There's very little field data on this available. We did some large scale bench scale tests in our laboratory looking at the impact of defects. And just to give you an example here, this is essentially what you might call a flow reduction factor. It's the flow passed the cut-off wall relative to what would occur in the aquifer itself, and so a low number means that I've done a good job blocking the flow.

For example, in this case, the soil bentonite wall that's been keyed into the underlying aquitard has reduced the flow by a factor of about a thousand. We take that same barrier and have a small window in it, that may be less than 1 percent of the gross area, and we render it nearly more than two orders of magnitude more permeable, or two orders of magnitude less effective. We have a geomembrane wall that has a seal defect, and we can see that it's rather than getting something on the order of a reduction of a factor of a thousand, we're getting a reduction on the order of about a factor of five. So these ones with Ds which have the small number are the ones with defects, and these tended to be small defects, less than 1 percent of the area, and yet they're dominating the entire flow through the system. And we've done a good bit of modeling on that, too, as well.

With the powerful computers we have today, we can create these wonderful three-dimensional flow and transport models. And we've built models, numerical models where we've put tiny holes in ground water cut-off walls, and we'll find that nearly all the flow goes through those holes. The ability to find out that that's where the flow is occurring is very poor. For example, if we put monitoring points downstream in the back end of the wall, our ability to pick up those defects is extremely poor. And I'll come back to that when I get to the end here.

That's essentially the status of those technologies. The question is why do we want to monitor them, and how might we monitor them? And I tend to think of the monitoring as falling into three different time frames, and three different philosophies. We do a lot of modeling up front, whether it may be simple hand calculations, or simple 1-D bucket-type models to sophisticated non-linear numerical models in three dimensions for a variably saturated flow and transport. We do a lot of modeling up front, and we often assume that that modeling is correct, but it may not be. And I think one of the things, the most important functions of short-term monitoring is to validate our hypotheses and our assumptions. We don't do enough of that.

I think this is like the 20-year time horizon, where we really do detailed monitoring with the primary objective of validating our models, and validating our hypotheses. And once we've done that, and we can convinced that we know how to predict the system, then we can make forward predictions, and go to a simpler monitoring system where we really just monitor a smaller scope of critical systems which are indicative of overall performance. And ultimately, we can transition that after we gather more information about how the system behaves, and get into even a reduced monitoring scope over the long time where we might really be looking at not so much understanding the system behavior, the function of individual components, but really the overall picture of whether we're protecting the public and the environment.

But to get to here, we really need to do this part first. We need to validate our hypotheses, because we can put in the monitoring system, but we may not monitor the right things if our underlying hypotheses and assumptions that were used to create the system are incorrect. So the time line I see, like a 20-year horizon, and then perhaps a 60 or 70 year horizon, and then the long-term kind of kicks in out there.

I haven't worked in the radioactive waste business that much, so I tend to think in terms of CERCLA and solid waste time horizons, so mine may be a little shorter, but this is, at least -- I think these years do translate it conceptually from one discipline to the other.

One of the reasons that we need to focus on short-term monitoring, a really detailed monitoring, is that model validation. I talked about that site in Sacramento where we assumed that our plants would extract the water each and every year. That was a conceptual model built into a numerical model that's widely used in practice these days, and yet there was a fundamental component to that that was incorrect about the effectiveness of that system. And if we weren't monitoring that system appropriately to look at whether the storage was being extracted, we wouldn't know whether -- we would be assuming it was functioning as expected over time.

If we don't have a model that's correct, we can't use it also for doing a what-if simulation. That's one of the really great things about models, is to say well, what if the precipitation increases by a factor of three over the next 1,000 years, or what if we have a fire and want to evaluate it. We have a fire that strips off all the vegetation - how will that affect the integrity of the containment system? We can only do those type of what-if simulations if the model works right in the first place, so that short-term really needs to get back to looking at whether our underlying assumptions are correct. And when we do that, probably the most important thing is to monitor the most important aspects of your predictions. And in caps, that's usually what drains out the bottom. That's the most important variable. Oddly enough, in about half the monitoring schemes I see, that flux or that drainage at the bottom is not being monitored, even though that's the driving variable for building the cap in the first place. So whatever the program is, it must monitor the key variables, such as percolation from the cap.

To get the moderate term, I alluded to this already. We could use that validated calibrated model that we believe or we have confidence in, and we can do some what-if simulations and look at what are the key variables that we need to monitor over the long-term. For example, maybe we don't have to monitor the whole thing, but if we have a composite barrier, maybe we need to really pay attention to monitoring the drainage layer on top. That's a key variable that we want to monitor, because that seems to control other behavior of the system. And we can develop less-intense systems with less redundancy, and those systems are easier to implement remotely, as well. But even with this part, we still need to go back and check with our model to see if our model assumptions are correct over time, and we update it and calibrate it, as necessary.

And we can do these things today remotely.

This is an installation we put in at Fernald, low level radioactive waste disposal facility on the Fernald Plant in Ohio, where we actually put in different monitoring stations within the cap to look at variables that were keyed to performance. And this was a web-based system where you could click on these different icons, and it would pop up and tell you what was going on in that cap at any given time. So we can monitor that with existing technology today - web-based is very practical.

This type of output we were looking at, one of the things was the drainage layer at that site. We could look at the temperature in the drainage layer, we could look at pressures in the drainage layer over time and see whether they were functioning consistently with theory, and with the expectations for that site. And we could use that at the time we develop a series of flow charts the site monitor could use to make decisions. Well, the pore pressure in the drainage layer is too high, what do I have to do? Well, the first step is to look at these different variables that may be the rationale or the reason for that elevated pore pressure. If it's functioning as intended, maybe we don't have to do anything, but we can develop a set of flow charts for a monitoring system to allow you to take action in accord with what type of data is being collected.

Even that system I think is probably too complex, though, for long-term. I think ultimately you'd like to get something that's truly remote, something perhaps we could monitor by satellite. And if we gather enough information, ultimately we should be able to get to the point where we have indicators of performance. For example, we may find that by the data we have collected over time, that the vegetation, changes in the vegetation are indicative of a change in the cover over time, and so we can use things like satellite imagery to look at changes in the vegetation, the species composition, or perhaps the water content in the surface layer and how its affecting vegetation over time. We could do that remotely, and there are ways of -- I think that's very practical with existing technology that we can do some of those things right now.

This is an example. This is a photo from Jody Waugh, who works in the Mill Tailings Program, and this is the Burell Uranium mill tailing site in Pennsylvania. This is an example of a photo that shows you the vegetation on top of this cap at this particular site. And they're able to monitor, get a sense for what's happening at the site by looking at the vegetation, changes in the vegetation. The vegetation at this site are keyed to changes in the properties of the barrier over time. So we could use those type of remote images ultimately for long-term monitoring.

In the short-term, probably the best system that we have for monitoring caps, as I talked about before, is what's called a pan lysimiter. And most of the studies that I mentioned earlier on have used this type of technology for both research, but it can be used directly in practice, as well. We can build this into full scale facilities as a monitoring tool, and you can make it as big as you want. Essentially what this is, a pan lysimiter is a large bathtub filled with a cover source, so you build a bathtub that has the exact same cover profile as the rest of the site within it, and we monitor the fluxes we're interested in, what drains out the bottom, what flows off the top, what's going on within the soils. And we can do it with well-defined boundary conditions, and of well-defined control volume.

These aren't flawless systems. They have some problems with capillary breaks and vapor barriers, but they are probably the best system that we have right. You could build these, and they could be monitored remotely, and we can monitor all the key variables we need to monitor. We can monitor drainage out the bottom, soil water status, meteorological conditions with a weather station, we can do the whole gambit, and even with some remote sensing technologies we can monitor changes in the vegetation over time.

This is essentially what one of these would look like during construction, more of this plastic sheet or geomembrane, the drainage layer on top of it being used to collect that drainage that comes out the bottom of the cover. This is one that's coming up, the side walls are coming up and the cover profile is being built around it. This is a water balance cover in California. If you remember those AT&T Wireless commercials, they have that person walking by when they're on their cell phone by the windmills. That's actually in this pass, this is Altamont Pass between Livermore and Tracy, California.

Essentially build it up, we instrument it with a weather station. This is standard gear that you can buy today. You can multiplex all the instruments together. You can monitor them remotely from a server that's on the other side of the world if you want to. You can control it.

So we have pretty sophisticated systems to do that short-term monitoring task, that zero to 20 year horizon. And, in fact, the instruments on that now are becoming much more sophisticated, that we're getting rid of all the wires. Everything is becoming wireless, so we can essentially put nests of sensors in and monitor them remotely with a data logger, and then transmit it back to an office that may be, as I said, on the other side of the world, for all practical purposes. So we can do those type of detailed evaluations of cover systems with existing technology.

The barrier walls is whole other story, in part because it's below ground. And getting the boundary conditions on it, and the control volume is very difficult to get your hands around. There's really no data that we have right now that truly demonstrates that these walls are effective. We have lots of anecdotal data - I heard somebody use that this morning - and we have ancillary data which would suggest it's working properly, but we have very little data that show that they actually functioning as expected. The common method is put two monitoring wells on either side of the wall, and you space them down the wall, maybe at tenth of kilometer intervals, maybe a little longer, and you compare those water levels, and you compare concentrations in those monitoring wells over time. And it turns out, though, that unless you're lucky enough to put the screen of that monitoring well within about five meters of the defect, you don't see the effects of the defect in the wall, and so a lot of that monitoring data that's been collected from cut-off walls, I don't think really tells us the whole story.

There's been some tools developed for that, tracers. There's been some hydrogeophysical methods. They're all largely experimental. They have some problems with detection with tracers, and one of the big difficulties with geophysical methods is an attenuation, because a lot of times we're using a bentonite, which is a fairly electrically conductive earthen material, and we use electrical geophysics, we kind of lose the signal within the wall. It becomes difficult to determine whether defects exist, so while we have what I would consider wonderful systems for covers and caps, our systems for barrier walls are far less sophisticated, and have not gone up through that learning curve near as much. Okay. Great.

VICE CHAIRMAN CROFF: Okay. Thank you very much. At this point I think we seem to be doing pretty well, so I'm going to do something a little bit interesting. Is Dr. Poston here?

DR. POSTON: I'm here.

VICE CHAIRMAN CROFF: Come forward, please. Latif, is his presentation on the computer?

DR. HAMDAN: No, he has -- did you put it in?

DR. POSTON: I thought we sent it on the email.

DR. HAMDAN: It didn't totally come in.

DR. POSTON: No one ever told us that.

DR. HAMDAN: I did send an email saying we received only two out of five pages. We do have the hard copy.

VICE CHAIRMAN CROFF: You don't have it on a CD or --

DR. POSTON: I've got it on my computer, or I can put it on a memory stick.

DR. HAMDAN: Yes, we have the stick right here.

VICE CHAIRMAN CROFF: Why don't we go ahead and take a break here, and let's get things set up. That sounds like the right thing to do, so we'll break until 2:30. We're doing okay on time.

(Whereupon, the proceedings in the aboveentitled matter went off the record at 2:17 p.m. and went back on the record at 2:34 p.m.)

VICE CHAIRMAN CROFF: Our Chairman is here. Let's go ahead and resume, I think. We're talking about monitoring kinds of issues and moving in a bit further.

The final issue that occurred to the Committee is the possibility of nondestructively monitoring the performance of massive grout modelists; in other words, monitoring the performance of maybe a Saltstone or a fuel tank or something along this line.

To address this issue, I'm pleased to introduce Dr. Randall Poston. He's a principal at WDP and Associates, Incorporated, a structural and materials consulting engineering firm in Manassas, Virginia specializing in failure investigations and evaluation, strengthening and repair of existing buildings, bridges, tanks, and civil infrastructure.

Dr. Poston became an expert in non-destructive testing of concrete structures out of necessity when 25 years ago the state of NDT practice essentially consisted of visual observation and coring of concrete. He heads WDP's branch office in Austin, Texas.

DR. POSTON: Thank you, Allen.

VICE CHAIRMAN CROFF: Thank you.

23) STATUS OF NON-DESTRUCTIVE MONITORING TECHNOLOGY FOR MASSIVE NEAR-SURFACE CEMENTITIOUS WASTE FORMS

DR. POSTON: Obviously that was somewhat facetious when I wrote that, but that really was the state of technology in a lot of ways for concrete structures 20 or 25 years ago. We really did not have much to do to be able to non-destructively test and evaluate structures.

We certainly have come a long way. But, as you will see, we have got an awful long way to go to address the issues or problems, such as the low-activity waste structures.

My colleague Mary J. Sansalone I put as a co-presenter of this presentation mainly because she's the one that snookered me into giving this presentation. So I at least had to provide some acknowledgement of her doing so.

I modified the title slightly, mainly because what I am going to discuss today, as I indicated, is going to be pretty far afield from just looking at massive grout and concrete structures.

A lot of what we're talking about is civil infrastructure. And I'm going to kind of go through and give you an historical perspective of where we have been and where we are at right now and at least try to provide some philosophical ideas of what we may be able to do with non-destructive testing.

Most of our work has evolved mainly because of the deterioration of the civil infrastructure. We certainly built a lot of structures, not only concrete, steel but concrete, and masonry structures after World War II. Most of those were probably designed for 30-year service lives. And we're approaching 50-60 years now. And we don't have enough money to rebuild everything.

So that was the reason a lot of the evaluation, the technology has evolved is because of this idea that we have to repair and maintain our structures.

The estimate for the concrete-based infrastructure now is somewhere around $10 trillion in the United States, which is certainly quite an investment that the public and private have made in concrete.

And I always liked this quote. Smithsonian Magazine in 1994 did a short article on concrete. And basically they could sum up the whole thing that a lot of that concrete needs fixing, which kind of tells us what we're having to do as structural engineering consultants.

This is my idea of kind of a massive concrete structure, a large concrete bent holding up a large aerial structure or bridge structure that is clearly showing signs of distress. This happens to be a pre-stress concrete tank that had been in service for about 35 years where they have had leakage through the pre-stressed shell and had lost some high-strength pre-stressing wires due to attack by H_2S gas, or hydrogen sulfide.

So the current technology, as I am going to explain, is mostly limited to what I would call thin structures, say something less than a meter. You know, it's not dam structures that are on the order of 50 and 60 feet thick. We're really limited in what we can do to about a meter.

Most of the types of technology are contact sensors. There are some we'll discuss that are not contact. Limited remote monitoring and sensing capability most often were involved in being at the site doing the work up close.

And then there are a number of techniques that really don't have a -- they may have sound physical basis or mechanical bases, but they're often used on a comparative basis because we're not able to study them rigorously with numerical methods. There is really limited real-time sensing that we can do in terms of non-destructive evaluation of structures.

In the context of the LAW, I think we're looking at we probably need something that may be non-contact sensors, may be non-invasive obviously possibly real-time and remote sensing capabilities, and certainly something that was more quantitative than qualitative in form.

Kind of the Bible right now for explaining what the state of the practice testing of concrete structures or cementitious-based structures. Masonry structures is this document that is put out by the Technical Committee 228 in the American Concrete Institute on non-destructive test methods for evaluation of concrete structures.

I am not going to spend a lot of time going through -- I have kind of a buck list, a litany of examples of things we can do but just to kind of give you a flavor of what we do, certainly a lot of times as structural engineers, we need to know the strength of concrete. And we're able to go in and take cores and/or take physical probes and assess the strength.

Corrosion activity in a structure, mainly because of metal embedments, is certainly a big issue that we are faced with with deterioration of existing concrete structures. There are a number of methods that we're able to assess both the ongoing status and rate of corrosion activity.

Probably one of the methods that might show the most promise in terms of the context of low-activity waste-type structures is probably going to be stress waves.

The old method, false velocity, and then the impact echo method, which I'll describe in a little bit more detail, something that may be fond to people's heart is nuclear methods. Unfortunately, methods like radiography and so forth are rarely used in structures because you have to evacuate the structure. You have to limit access. It's very costly and expensive. And most owners do not want to discuss having to clear a structure out in order to do any type of radiography.

There are magnetic and electrical methods, which worked fairly well for certain types of problems, for infrared thermography and radar.

And then generally what we do, there is not a single method that, you know, is like that will go up and tell us everything. We are using a variety of techniques to give us a bunch of information in order to make our evaluation.

So we start with kind of the easy, go to the more difficult but certainly visual inspection we rely on a lot as structural engineers, stress wave methods, ultrasonic methods, impact echoes, spectral analysis of surface waves, on down the list.

And then there are aspects about geotechnical engineering and geophysical testing that involve procedures called cross-hole sonic logging, parallel seismic techniques, and so forth that are looking at concrete structures buried in-ground, generally more for quality assurance-type aspects for concrete structures like foundations.

Into the realm of the nuclear methods, we have methods here related to the electromagnetic cover meters, half-cell potential and polarization method related to corrosion, and then on down through again the thermography and radar. So that's really the list of the types of technologies that are available today.

Most of you will probably think that this is a hammer. And we don't call that a hammer. We call that a percussion instrument because we're able to charge more money as consultants doing that.

(Laughter.)

DR. POSTON: Basically this is a sounding device. Most of the other techniques, we look at stress wave. We're looking for defects that are deeper in the surface, but the idea is the same. We are going to introduce a sound wave or a stress wave into the concrete. The human ear is pretty good for things that are shallow, but as you get deeper in looking for flaws or defects in concrete, it's much more difficult.

And so we still use it today obviously for some types of defects, but it really was kind of the state of the practice about three decades ago.

We like to make openings when we can, explore things, be able to visually assess as-built conditions, verify types of deterioration. Obviously in the context of a structure containing some level of nuclear waste, that is not going to be possible.

There are often hidden conditions that no method will tell us about. So probing and even verification of the non-destructive testing is very important. Therefore, we like to make probe openings or exploratory openings.

Going in and pouring out the concrete, in this particular case, we don't want to pour too deep. It happens to be a 90-year-old facility out in Arizona that is transporting water. So we don't want to cause a leak, but we can assess a lot of information by looking at the characteristics of the concrete.

That analysis is done by petrography, which really is a geologic method. We can look at the paste and aggregate structure of the concrete, look at all kinds of things relative to its current health or state by looking at it under a low-level microscope.

And in some cases, we can even go to doing things like scanning electron microscopy and using energy disbursive techniques in order to assess certain aspects about the characteristics of deterioration, whether it be from alkali silica reactivity or freeze/thaw type deterioration. Again, you have to have a sample in front of you to be able to do that type of work.

So this would be an example of looking at something petrographically. It happens to be some alkali silica reactivity. And we effloresce using essentially uranyl acetate here. We're able to find this gel product around here that is forms from alkali silica reactivity and creates and expansion. And, therefore, you get a cracking in the concrete. Again, you have to have samples.

One of the most common things as

structural engineers that we need to deal with is often drawings are not available for the structure because they're old. And the question comes in, is it able to take the current loads? Do we need to upgrade it for loads? So we get into the concept of having to locate reinforcement in steel and other embedments to be able to make a structural evaluation.

This is what is referred to as a pachometer. It uses eddy currents, which is one of the two methods for pachometers. And we're able to go to the surface of the concrete and locate steel embedments relatively close to the surface with this device. Within about four to five inches is its limitation.

Other methods include you'll see a lot of the methods we use in civil and structural engineering. We borrowed from our kindred spirits in other areas. Certainly ground-penetrating radar, radar in general, is one of them. It's a very powerful method for us assessing deeper into concrete what some of the physical aspects are, certainly reinforcing steel, post-tensioning tendons and other things.

So we can make a scan in two different directions and locate fairly precisely what the layout of tendons may be or reinforcement may be in a structure.

So we have an antenna. This is generally for concrete applications. This is about a 1.1 megahertz antenna and partly the electromagnetic wave based on the dielectric constant of the materials.

We get a scan here. And you can see fairly clearly that these kind of hybolic shapes here locate where the steel reinforcement is in the structure. So it gives a pretty clear scan relatively rapidly of existing conditions of a structure in terms of its reinforcement.

This is one the side of a beam. And we can scan down. It actually has two types of reinforcement. One is some shallow reinforcement near the skin of the concrete and then something here that is a little deeper representing of in this case a big massive post-tensioning tendon, which are used in certainly nuclear containment vessels and the like.

On the electromagnetic spectrum, these particular methods, the GPR, working in this range here, slight wavelengths between a millimeter and meter and then the infrared technology is really on the high end of the micrometer level in the spectrum.

Infrared thermography is good, again, for very specific technologies. It is non-contact. And we're basically converting the infrared imagery from intensities of lights. And a lot of it is for looking behind walls, looking at moisture or looking at large delaminations and bridge structures and the like.

The only problem is it's very influenced by temperature of the outside. You really need to have a good thermal drop in order for heat to be radiating in order to be able to really see defects very clearly.

And this is the type of equipment: infrared camera, camcorder. We like to put reflective tape. It gives us a marker in which to identify things. This happens to be a grouted masonry structure in which the dark areas here are grouted cells in the concrete masonry block.

This is a grouted what's referred to as a soldier beam in the concrete. And it's a little bit difficult here, but on a large scale when you blow this up or are able to find out areas where there may be voids in this grout here, which would appear as a light intensity, both from a -- we need to know that as structural engineers because of leakage problems and also just because of the strength of the wall is dictated by the grout that is in the wall itself.

Now, the stress wave techniques are probably the technique that may show the most promise for our discussions today. These pulse-velocity and pulse-echo are older methods. They have been used for maybe 30 or 40 years in the concrete industry, but the one that probably is most useful is impact echo and what's referred to as spectral analysis of surface waves.

Impact echo was developed by my co-presenter, Dr. Mary Sansalone, when she was at the National Institute of Standards and Technology, along with Dr. Nicholas Carrino. They developed this method starting from scratch, looked at numerical techniques to find out what might be the best type of transducers and frequencies and so forth, and then came up with equipment and developed the method. I will show you a little bit about how that works. Other methods, known as impulse-response, also might be used on a more global level.

You can see here it looks pretty easy. You've got a concrete beam here. We have some problem in the consolidation in this case of the concrete. We have got a transducer that is a sending and receiving transducer. This is a bad area. This a good area. We simply measure the transit time between these two transducers. And you can determine what the effective wave speed is.

Wave speed in concrete is typically around 4,000 meters a second. So if you're getting something that's, say, 2,800 or 3,000 meters per second, we know that there is a problem. There is some defect. There is something about the concrete that is not any good. But one of the problems with this technique is you don't really know is the surface all that is a problem, are there other defects internally that we can't see that may be a problem. So it's limited in what it can tell. It tells us something, but it doesn't tell us everything.

The impact echo method is more powerful just because of that basis. Not only can we define where a flaw or defect is in the concrete, but we can identify generally where that defect is located. And we use it for lots of things in concrete, locating delaminations and voids and subgrade voids under concrete structures, voids in grouted post-tensioning tendons, and even assessing the quality of bond between two concrete structures.

The idea is you have a transducer here. It's a displacement transducer, piezoelectric crystal here. We introduce an impact, introduce as a stress wave. It goes and reflects and transmits through the concrete, off to some surface, whether it be air surface for if it's thick and then we'll see a case where it's a flaw. We monitor this displacement with time.

And this all looks pretty good in theory, but in practice, you never get a signal that looks quite as nice. And so one of the tricks, if you will, in the development of the method was that they decided that it would probably be best to look at it in the frequency spectrum.

And so just by taking the distance through the slab, one thickness, double thickness, you can make a simple mathematical calculation about the transit time. And when you take that displacement time history and do what is referred to as a fast forward a transform on it, it looks very nice. We get a nice frequency peak exactly where the thickness of the concrete is.

Well, that's nice if you have solid concrete, but what happens when you have a defect? Well, the defect, essentially your stress waves, instead of going all the way down and then back up to the surface, are going to be interrupted by this flaw. You will get some stress waves that go around the flaw, down, and then back around or they will reflect simply off that flaw.

And you see that very clearly in the frequency spectrum from a shift of what is this through thickness frequency to something that represents a higher frequency to the shallower depth of the flaw.

So it works very nicely in identifying where there is something in the concrete, masonry, cementitious structure that identifies a defect. Of course, there is a limitation on what size defect you can find.

We can generally find bigger defects. The deeper you go, it has to be a bigger defect. Near the surface, we can find pretty fairly small defects. As we get deeper, we have to have a bigger flaw in order to be able to assess that it's there.

Just to give you an idea of where this can be used, we use this technology on a seven and a half-mile-long sea wall out in Los Angeles, California that experienced a fair bit of damage over 40 years due to corrosion.

And the main problem was this reinforcement down here through the tidal fluctuations in this basin. Saltwater created corrosion along this joint. And in the reinforcement, we would see delaminations on the back side of the wall.

The problem is the back side of the wall is filled with earth, and the front side of the wall is water that goes up and down as the tidal fluctuations.

But we wanted to see if we could monitor the defects due to corrosion along this joint here. And what we did was use the impact echo to try to locate where these delaminations occurred. And that would help us in deciding structurally what we needed to do to that wall.

So here we're using the impact echo method. Again, unfortunately, it's a contact method. These are the types of flaws that we're able to locate. We're able to do it with 100 percent precision, if you will, on some blind studies. And you see delaminations that occur due to the corrosion activity of the steel.

So it's not a method that we can use to detect corrosion, but, you know, the manifestations of the stress of the corrosion, in this case the delaminations, but we are able to identify those areas in the wall where this was occurring or was not occurring.

Another application where it has been quite successful is looking at the grout that is used in what is referred to as post-tensioning tendons. These types of systems are used in bridge structures.

Interestingly enough, they're not used in nuclear containment structures because in the nuclear industry, they want to be able to go back and look at these tendons and pull on these tendons and see if they have enough stress.

But from a corrosion protection view, having grout inside this duct is a desirable type of protection. So you have the high-strength steel strands. And then you have the grout that is injected in. And it forms a solid mass.

So the problem comes up that grout is not continuous through those post-tensioning tendons. And that has been a real problem in the concrete bridge industry for a number of years. And the last five years or so, the State of Florida has undergone a massive effort to locate these types of voids and tendons because of corrosion problems.

So you have to be able to locate the tendon, which we do by GPR, and then go in and use impact echo method. And we're able to see whether there's a void in the grout.

So this is the type of signal analysis that we go through. In the case we see a slight shift just due to the grout being a little bit different properties in the concrete. This type of amplitude was clearly representative of a frequency associated with the tendon. And when you have a void, it shifts to about twice the level, so 2 times 11 kilohertz, around 22. So 22.6 kilohertz or so, we see a big peak representative of a flaw.

So this is a case where we have a steel duct. We had the concrete surface. We excavated in, looked at it. And certainly the grout looks quite solid. And then we go to a tendon where it looked clearly voided and went in. And clearly you can see a void in this hot tendon. So we were able to successfully find a relatively small flaw in a major structural element of structures.

I think a complementary method in the stress wave that may be quite helpful in terms of looking at the LAW structure considerations, is spectral analysis of surface waves. It's a part geophysical and part stress wave method, but we are going to impact the surface. And it's a system that for purposes of this discussion, we are going to consider it a layer system, like soil, rock, or different types of soil.

We're going to extract information from the surface waves generated by these mechanical impacts and then do some fairly complex signal analysis. And the complex signal analysis is that we take a guess at what it is, we compare it to our captured results, and then we go back and recalculate it until it converges on a solution.

So here we have a system, in this case some kind of a concrete structure. We introduce an impact. It's going to be generally a much lower frequency impact than the top impact we discussed with the impact echo method. And we get a surface wave or Raeligh Wave and have a series of transducers connected to the concrete structure, impact and then the R-wave or Raeligh wave. And then you had the spectroanalyzer.

So the principles are essentially the surface wave contains a spectrum of frequency or wavelengths that should be impacted and has a whole range of these various wavelength components.

The penetration depths of the components are proportional to the wavelength. And we use that information in our back calculation. And the speed of each component is going to depend on the elastic properties and materials, whether it's soil or concrete or other types of material.

We have a system here, layered system. We impact it. You can see the series of waves that is introduced. And depending on the elastic properties, we're going to get different propagation of these waves through this media. And then we have to do fairly involved inversion techniques.

So here's our layered system with various elastic properties. And here is what we call a dispersion curve, which is based on the measured results or the calculated results and our experimental curve. And, again, we have to converge on this dispersion curve. And once we do that, we know that those are the properties of the various systems.

So if you have a multi-layer system, different types of soil, be it clay, sand, or clay, sand, and another layer of clay, whatever you are able to determine the properties of this layered system and I guess, more importantly, what the difference may be over some time.

I think, finally, the last thing that we do in structural engineering because we can't seem to ever calculate anything very precisely -- structures always behave a lot better than we always think they do -- is to carry out a load test or some type of structural test or dynamic test. We take a static or dynamic load on a structural component. And we are going to assess load capacity based on how it performs.

We would instrument the structure for displacements, accelerations. Whatever type of monitoring element that you want to do we can probably measure. And then we're going to see how it performs under a given load, measure the response, and make an assessment about it. The problem is we have already done an analysis. It probably tells us that it should have already failed and it doesn't fail. So it really requires some pretty careful planning and judgment. You don't want to fail a structure during a load test.

So, just to give you an idea, this is just a where we might have to load test a beam in a structure. And we apply some -- this is the beam being tested. We apply some load. And we're going to measure the displacement of the beams using potentiometers or string pots or linear LBDTs or whatever.

We can also do that in a dynamic sense. In this case, we're using the accelerometer on a large cable stay in a bridge. And we know what the properties of -- the structural element acts much like a string. We can theoretically know what it should vibrate at. We do some kind of vibration test.

In this case, I have a big guy that's able to pull that stay. That's our force that we're introducing. And we're able to monitor the vibration and the ring down and the damping of that particular stay and assess its overall load-carrying capability.

So with those in mind, you can see where you're really in the infancy, I would say, in trying to be able to assess something relative to a vault that would contain some low-level radioactive waste material.

I think conceptually what we might be able to do at this stage is have a series of sensors, either out in the far field of the soil or area, geologic formation where they're buried. You might have to have some near field sensors that are in some type of intimate contact with the vault. And you would certainly like to have some that are buried along with the structure as well.

And you would have some kind of stress wave generator. Instead of having a person do it, you would have to have some kind of impact, whether it be mechanical or otherwise, some other type of sensor that would generate a stress wave or a sound wave or maybe even some other type of wavelength wave in which you would monitor those sensors over time.

The robustness of these types of sensors I guess is anybody's guess. We certainly have had sensors in the field, I think in our practice, for over ten years now, but that's peanuts compared to the life of a structure like this in which we would want to know its integrity.

So it would be a combination of these geophysical concepts that I briefly discussed, probably some non-contact and contact sensors and, frankly, other technologies that we don't know much about, to be determined.

So, to summarize, I think the capability of the non-destructive testing of buried structure right now is really quite limited. It's in its infancy with regard to some kind of contained waste management concerns. Generally right now we need to have access, we need to have contact.

The massive size is really a problem. We're limited to how thick or thin we're able to monitor a given structure for structural integrity. We got the idea that a buried system is going to have more than one component, not only the structural component of the tank but of the soil acting as a structure. So you have soil-structure interaction issues that have to be dealt with. And certainly the time scale represents I think a big problem in terms of assessment over a long period of time in regard to this case maybe being centuries.

So, you know, the questions are really pretty easy in this case. The answers to them are more complicated. You need discrete measurements or do we need continuous or quasi-continuous measurements in the remote sensing? I don't know. And then what size of defect or problem requires detection?

If someone in this room could tell me what that is, then we might be able to develop a system that would work for that. I suspect it's probably a fairly small level of defect or flaw.

So in order to help with this problem, we would need to define clearly what the goals and objectives are, quantify this level and size of the flaw or damage requiring detection. From a structural integrity point of view, you saw the types of things that we were able to assess.

We are looking at fairly large defects, something that's really compromising the integrity of the structure. In terms of research, there would need to be significant research looking at the extension of the existing technologies and adapting of those technologies.

I feel pretty comfortable that we could develop some sensors with the types of technologies we have. Whether we're going to be good enough in order to assess the integrity at the level that it needs to be is uncertain. Of course, we would likely have to develop a whole new generation of technologies at this point in order to assess this problem.

You see, I'm not heavily involved at all in the nuclear industry. I've been involved in a number of structures that had some construction deficiencies during construction that were able to locate defects but certainly not of the magnitude and of the size that we're probably going to have to assess here.

And I must confess when Allen asked me to give this talk, I had to go back and learn how to respell radionuclide because I couldn't remember since my freshman chemistry class in college.

So, with that, thank you very much.

VICE CHAIRMAN CROFF: Thank you.

Now it is time for the Q&A on the monitoring. Randy, I think if you could just pull up a chair to one of these mikes? Well, I'm going to ask two more people to come up: Tom Nicholson and Jacob Philip from the NRC Office of Research.

They have been supporting and conducting quite a bit of work on monitoring over -- well, I don't know how long. They're fairly deeply into it and have given us presentations a few meetings ago on a site where they do some considerable monitoring. So I think they may have some insights here. So everybody try to get someplace near a microphone. I think we can -- we don't need the projector anymore, I don't believe.

Let's see. We've got our folks up here. Jim?

24) ROUNDTABLE DISCUSSION OF ONSITE WASTE DISPOSAL MONITORING

MEMBER CLARKE: Three very interesting presentations. Let me start by inviting Randall to come up to Nashville so I can show you some real percussion instruments.

DR. POSTON: Okay.

(Laughter.)

MEMBER CLARKE: You have them in Austin as well, I'm sure.

DR. POSTON: Right.

MEMBER CLARKE: I have to wonder about just picking up where you left off the applicability of many of the techniques that you're using in concrete to other materials, like soil, slurries that might have holes in them. For example, do you think you could use or do you use GPR on a subsurface barrier after construction?

DR. BENSON: People have looked at using GPR in caps. One of the difficulties, we'll usually use fine grain materials for barriers, fine grain earthly materials. They tend to attenuate GPR signal pretty rapidly.

MEMBER CLARKE: I was thinking more of the walls, the subsurface walls, that might have --

DR. BENSON: Particularly in the walls because the walls are really conductive. And so they tend to rapidly attenuate GPR sequence. If you worked with -- perhaps if you're looking at sheet pile wall, that might work better. The defects in that might be -- you'd be looking at smaller localized defects. I'm not sure whether you'd be able to resolve that or through the inversion to pick up those defects.

DR. POSTON: If you have moisture, that's a big attenuation factor. So if you have something with a lot of moistness in the soil or whatever, it kills the signal.

DR. BENSON: The attenuation has always been our big problem. and the smearing of the -- essentially you get two when you try to do the inversion, trying to find the defect. When you do the inversion, you tend to get all the smear.

MEMBER CLARKE: If I could follow up with you? There was a television commercial on before my time. The punch line was "It's not nice to mess with Mother Nature." I agree with you that I don't want to put words in your mouth, but it's always struck me that resistor barriers are always fighting Mother Nature and the alternative designs are more trying to work with Mother Nature. The alternative, particularly the ET caps, are clearly demonstrated, I think, for arid environments properly designed.

What can you tell us about their potential use in humid environments? I know of one that has been installed in Laurenceburg, Tennessee, which has got to have 40-plus inches of rain a year, with poplar trees Have you encountered any attempts to use them in humid environments?

DR. BENSON: There have been attempts to use them in more humid climates. And we have tested them as well in humid climates. It's difficult to manage the larger volumes of water in wetter climates. We have tested, for example, on the poplar trees.

See, I do the poplar tree as it is a big water hog. It produces biomass quickly and uses lots of water. The idea is you could manage large volumes of water with that tree.

It depends in part on where your objective is in terms of how much water you're going to allow to get into the waste. And it's very difficult. That's on the order of a millimeter, a half a millimeter a year, which is not atypical. That's fairly common to be able to manage that in a humid climate with plants.

For example, where I live, the vegetation is never really stressed for water. The systems really work where the vegetation has to scavenge, where it's biologically adapted to seek out all water it can find and use it up.

In the eastern United States, water tends to be plentiful. So the plants haven't had to do that and the systems aren't geared to using everything up that they can. And, as a result, more gets through than you would like to have.

Our tests of those covers in eastern climates have not been very successful, at least in terms of very low percolation rates. We'll be able to drop things down to maybe 100 millimeters or 50 millimeters a year. But that's about as good as we have been able to get.

I have a long answer to your question.

MEMBER CLARKE: No, no. I guess the follow-up to that would be, are there things you can do to make them work in more humid environments, I guess just ways of trying to reduce the water load to the cap, but --

DR. BENSON: In humid environments, it's more difficult. You have to use more manmade types of systems. So that's where the system tends to drain and aerate. You know, it may be several inches per year. So you're always fighting that natural tendency to go to that type of recharge.

There's been actually some work done here through NRC, actually, Ed O'Donnell's work on using some sheets to essentially enhance runoff.

MEMBER CLARKE: Water shedding.

DR. BENSON: Water shedding. All of those are kind of contingent on the mechanical methods, which are fairly high maintenance over time, could be high maintenance.

MEMBER CLARKE: One more quick one, if I could. You mentioned Fernald. I think the instruments there were retrofitted, were they not, for the first cover?

DR. BENSON: The instruments were installed in cell one essentially during construction.

MEMBER CLARKE: During construction.

DR. BENSON: Yes.

MEMBER CLARKE: Do the designs for the subsequent disposal cells feature instruments as well?

DR. BENSON: Not to my knowledge. It was a prototype system. We tried it out. And after that point, they haven't taken it up. But I don't think they've come to closure with the regulatory agencies about what they are going to have to do here.

I'm not an expert on that. I worked on that part of it, but I don't know what they're doing henceforth.

MEMBER CLARKE: Okay. Thank you.

CHAIRMAN RYAN: Thanks, Allen. I'll second the three interesting presentations.

It struck me as I listened to all three that there is kind of a common theme of how do you do the test to get the information you need to evaluate against an objective from all three perspectives?

And then there is a part that I think is a little different. I'm guessing, Randall, that you get called and say, "We've got a problem. Can you come and tell us what it is?" So the cat's out of the bag in a way from a lot of your experience.

And then on the other side, we've got "We'll instrument it as we install it" and trying to get ready to assess problems. I just wonder if in the concrete case or the surface case there is a way to tickle out some value to all the methods to think about if we had a full sheet of paper and are going to design a system, whether it's a concrete monolith with a cap or any combination of things we've talked about that we could design it maybe a little differently to build in the monitoring up front. I think that was Vernon's point of view.

For example, you mentioned that certain techniques only go a few inches, certain techniques go maybe a few feet or something of that sort, same with soils. What if you actually interrupted that monolith with monitoring ports of some kind, you know, that you could preinstall, that kind of thing?

And I guess my challenge might be to the Office of Research folks to think about maybe an expert elicitation or review or something of that sort to think about this in a little bit more detailed way.

Does this idea of maybe tickling this out make sense to you experts who were speaking today? I think there's a wealth of experience here.

And then on the monitoring side, you know, how do you put in a groundwater well that's a monitoring point and maybe a water level monitoring point and by that value for other either geochemical or hydrogeological or geohydrological uses as well as I've got my sample, and it's below X, that kind of thing.

DR. POSTON: Mike, I think you make an excellent point about I am typically more involved in aftermaths and do some kind of assessment where we're looking at the overall assessment. Maybe there isn't a problem. But that generally hasn't been the case. But I could envision, you know, a structure being built.

You know, the buzz word, of course, now is smart structure, smart technology. And whether it's the type of sensors that have been discussed or some other type of sensors, I certainly think it's doable.

I even heard discussions. I know at Southwest Research in San Antonio, they've looked at basically a vitreous layer in a structure that you pass light through. And depending on how you can then pass light through, just like a fiber optic cable, depending on whether some deformation or damages occur, you get different light back or reflecting back from that layer.

That is one thing that designers I know in soil infrastructure only beginning to think about is trying to develop a structure that makes it easier not only to monitor but really even to repair and maintain, which has always been a problem. And, of course, the problem is there never seems to be enough money to build a structure like that, even though you see that the initial cost may be two, three, or four percent more than what the first costs are. But no one ever seems to want to invest I that.

I think conceptually looking at a structure that makes it conducive to various types of monitoring makes a whole lot of sense in this case.

CHAIRMAN RYAN: One example along those lines is kind of my monolith with perhaps penetrations built in it. I mean, in that way I could probably use a radiography source to look at void identification and transmission kinds of measurements to see if it was monolithic or if it wasn't monolithic.

You know, in the case of grouting a heel in a tank or filling a tank, you know, something along those lines, if you design -- I haven't thought enough about it to make a sensible recommendation, but if you thought about it, you might be able to set up some penetrations through the monolith with the grout to either do testing like a gamma ray test or monolith testing or homogeneity or things of that sort or perhaps even other more sophisticated measurements, so maybe even sampling over time, so, you know, things like that.

When you look at all three of these aspects, this one theme sort of jumps out, whether it's a from-the-surface kind of compliance in modeling, monitoring, or whether it's the installation phase of the "Whoops. I've got a big problem. Come help me" sort of phase. So I think that's worth thinking about.

Tom, what do you think? Jacob? Does it make sense to you guys?

MR. PHILIP: I was very interested in Craig's talk, particularly the sense that he had on the engineered barriers, because that's the first thing that goes bad because most of the monitoring we generally talk about is groundwater monitoring or air monitoring, all of that. So if you get nearer to the source where you had the problem, that's a very important aspect.

And then, of course, the next question is, if you have problems, where exactly did it occur in the structure whether you can fix it. So my question to you, Craig, is, in the type of facility that we are talking about, the WIR facility, for instance, what is the proper mix of engineered barrier monitoring and environmental monitoring that you would suggest would be appropriate for a structure like that?

DR. BENSON: That's a good question. We need the bounds on that a little more. That's pretty open-ended. I mean, maybe if you could just could describe the structure a little more?

MR. PHILIP: Then it's a huge tank with grouted waste inside. And you're basically looking at --

DR. BENSON: With a cap.

MR. PHILIP: Yes, with a cap on top of it. You're looking for infiltration from the top. You're talking infiltration from the sides. And you're wondering about the performance of that, how good it is, any cracking of the tank itself or corrosion of the tank, and maybe some cracking of the grouts itself.

And so you have a system were you can monitor that and plus anything on the outside in the environment.

DR. BENSON: It's still a big question, but I think one of the things you can do is you can forward model that and expect how it is supposed to behave hydrologically, if we at least look from a hydrologic monitoring perspective.

And then you can monitor things like water content. And you can put in these newer technologies flux meters to actually measure flux in the unsaturated zone, water flux in the unsaturated zone. And you could then develop a technique where you could monitor that essentially on a continuous basis and compare that to what your expectations are in terms of your models and then look at deviations between predictions and measure being indicative of a change in properties which would change the hydrology.

And you could also look at if you have a good sense for what type of scenarios might occur and how they might change the hydraulic properties of those materials, like, for example, a crack in the waste mass. And you could get a sense for how that might change the hydrology. And you could actually simulate that ahead of time and then look for that signature response, perhaps in water content or flux, from your instruments.

The difficulty is with those type instruments that we have lots of really nest gizmos right now to monitor things in the subsurface but how long they'll last.

Just in the last ten years, they have revolutionized our instruments. I mean, ten years ago we were measuring our 15 water content with these big, bulky time-demand reflectometry instruments, which were $10,000 each. Now we buy them in a $200 probe. So they've changed a lot now, and they'll change a lot in another ten years.

So you have to have some way of being able to go in and replace the sensors essentially to be able to somehow access it if it falls apart over time or you want to change the technology.

Does that answer, in part, your question?

MR. PHILIP: In part, yes. I liked your talk when you talked about, you know, monitoring to verify your performance predictions and where you talk about short-term five years. Make sure that your models are pretty accurate as far as when it's compared with monitored data and then go up to the next 20 years and then go up to the next phase.

I think that's a useful and a nice technique to do, particularly because it gives us a lot more confidence that our models are working as we predicted.

DR. BENSON: I really see that developing confidence in our predictive capabilities as being essential to be able to predict long-term performance and really achieve long-term containment goals.

Our monitoring system is largely defined on what our predictions tell us we think is going to occur. And then until we're really confident in that, our monitoring system is probably going to have to change over time.

CHAIRMAN RYAN: Tom?

MR. NICHOLSON: Thank you.

I have a quick question for Vernon and Craig and also Randall. One of the things that we're looking at is that we're trying to link performance assessment models to performance confirmation monitoring.

So the issue is, how should we do this linking? We're using what we call performance indicators. And because we're looking at this environmental system and it's behavior -- and, as Jake just commented, the idea that we're looking at both short-term and going out in the long-term. What are your thoughts, Vernon, on how to link the performance confirmation monitoring to the models themselves?

DR. ICHIMORA: I have a couple of suggestions with regard to that question. One is there has been some work that had been done, say, at Department of Energy sites. And that's some commercial low-level waste sites in which some what we call remediation, some changes that have been placed on the facilities -- for example, covers comes to my mind.

And one of the things that you can do is before the covers are put in, there was, of course, some analysis done to address how the covers should perform before the covers are placed on -- these cover designs were actually a design basis to implement and justify the covers.

As an example, one could go back and then through standard environmental monitoring change nothing other than to look at what the performance of a facility is after covers are in place.

And that can tell you a lot about taking out one of the variables that you don't really have any idea how important it is, for example, as infiltration as an example. How does infiltration impact the concentrations downstream of the facility?

So you can look at examples like that.

And the case that I have given you is very large-scale. It's like a field experiment. You have some control over it, you know, when you put the covers. You knew what the initial conditions were. And now you're looking at the conditions that are being measured in the field as a result of that change.

MR. NICHOLSON: Thank you.

CHAIRMAN RYAN: Let me add to what Vernon said. I think if you go back to other facilities, whether it's a DOE facility or some other area where contamination has existed, even a chemical contamination, and then some remediation was done.

I think if you went back and said, "Well, okay. How did it work? I mean, did it reduce concentrations in the way you thought at the location you thought?" I think that is Vernon's point. You know, a couple of the older low-level waste sites that have been enhanced -- Sheffield, Illinois and Maxey Flats come to mind. They were very large remediation projects at both of those after closure based on monitoring data.

And now that we're a decade or maybe even two down the line from some of those, I wonder if just looking at their monitoring data might be interesting.

So extent that to the RCRA or CERCLA closures, that kind of thing.

And then on the decommissioning side, from a nuclear facility side, whether it's DOE or Atomic Energy Act-regulated stuff or what states have done, there might be some pearls in that bag to pull out and evaluate to see if we're meeting the objectives we thought we were.

DR. BENSON: Yes. I agree with that. I think you can use what you observe in the field to essentially go back and look at whether what you thought was going to work actually worked.

I think one of the difficulties is things take -- at least in natural systems, they take a long time to evolve. I mean, groundwater might move at a meter per day if it's really cooking along, right, or maybe ten meters. That's really fast, right? So some of our sites are plumes and have evolved over very large distances because they have been there for decades.

And so to see if I make this change now, how it will affect my system may take us decades to run that field test, you might say, to confirm that it's working.

So that the importance of collecting

monitoring data over a very long period of time I think can't be underestimated. There's a danger I think of, you know, we fixed it and we monitor it for ten years or so and they we kind of cut back on the monitoring. And I think in many cases, that is probably done too early.

CHAIRMAN RYAN: And on the radioactive waste disposal side of it, whether it's 61 or DOE, I mean, that's probably the richest area because institutional control and long-term monitoring programs and funding for them is fairly well-established. I mean, that's not something that just came along yesterday. So there are decades of experience anyway, something to think about how to mind.

MR. NICHOLSON: The other issue if you are going to go back and look at some of the DOE facilities and the foreign, international, community also is where did things go wrong? Did they have the wrong conception model? Did they make the wrong assumptions?

I can think of a variety of sites in which the monitoring program was so focused on a single conceptual model that it actually missed important data, information, that proved that the pathways, the exposure pathways, were not considered properly.

And I can think of an example. The Office of Research went to a site that it had licensed. And we had the U.S. Geological Survey put in a series of shallow wells, something to understand the first water system when it operated, how prevalent was it.

In the course of doing that, they found a contaminant plume. It wasn't meant to be a detection monitoring, but it served that purpose. But the idea was that the pathways were not properly envisioned with the original monitoring program.

CHAIRMAN RYAN: Bill?

MEMBER HINZE: Well, in the same vein as Tom was talking, we have Vernon here and also Mike that have had a lot of experience with a many-decade site.

I am wondering, Vernon, what lessons have you learned regarding baseline data, monitoring caps, et cetera, at the Barnwell site.

DR. ICHIMORA: With regard to baseline data, we basically look at trends. These tends are usually very, very small. There are minute changes in radioactivity content in the media. And it's usually so small that you don't see them over a year period, but maybe when you're looking at five or even ten-year periods, you do see, for example, a decrease in tritium concentration. And a lot of it is due to the function of another facility nearby.

About cap performance --

MEMBER HINZE: Well, I think baseline data, though, I take as prior to any possible contamination. Was there baseline data available to any significant degree for the Barnwell, South Carolina area prior to the installation of the waste site?

DR. ICHIMORA: To my knowledge, there was some baseline information. In fact, if you were to look at some of the early environmental impact statements that came through the NRC, there are some radioactivity measurements before the site was put in. And these measurements are actually almost the same as they are today, say, for the gamma-emitting radionuclides.

CHAIRMAN RYAN: Bill, just if you go back in the record, remember that Barnwell is next to the Allied General reprocessing facility that was designed, built, constructed, and not operated.

MEMBER HINZE: Yes, right.

CHAIRMAN RYAN: And there was a lot of interesting corollary data that was kind of lucky in the sense that it was there and available, but it certainly helped outfit things. And, of course, being adjacent to a border of the SRS helped, in part, as well.

The interesting question that you raise, I think I would offer a view that I think clearly everybody agrees you need some baseline data to get started. And I think my view of it is that that means you never really stop.

And, to pick up on Dave Kocher's comment, it's a process of continual update, not one where you say, "Oh, we're done now." And I think you get to a point where you have confidence that whatever system you want to use for whatever activity you want to perform, we're okay to do that activity. But that doesn't mean you should become comfortable that continued monitoring or observation in the very general sense of monitoring or compliance demonstration should continue in some form or fashion because it really, to my way of thinking, adds to the "I've got confidence. I understand what is happening" story.

So you know what I'm saying? I think there is a step where you can say, "We're okay to begin the activity." And then now that we're doing the activity, let's figure out a way to monitor it in a smart way as we go along so we can gain confidence and demonstrate performance that we expect or adjust as we go along.

MEMBER HINZE: That was really my question to Vernon. You know, how has the monitoring at Barnwell been changed? And what caused that change? What information did you have which suggested that you should change the monitoring system or decrease the number of monitoring wells or increase them or whatever?

DR. ICHIMORA: That is kind of a -- there is a long story with respect to how the monitoring program there is developed. Some of it is due to no technical reason other than there is some pressure to put in monitoring well to a location where someone wanted it. So that's one extreme.

On the technical side, as you know, there's a tritium plume that's associated with that facility. And what we have done is in the sense of the monitoring system, the monitoring system has been focused to look at downgrading from the facility and is designed to characterize the tritium from that facility.

So, consequently, there's a large amount of development on the monitoring effort to be able to measure and be able to have good confidence as to what you are really measuring in the tritium plume from the facility itself. So there is a lot of effort in that direction, and it is driven by the fact that monitoring information fed new information and consequently drove the monitoring program in a certain direction.

MEMBER HINZE: I know that there have been shrinkage cracks, investigation cracks in the clay cover at Barnwell. Are there lessons to be learned there from what you have done to mitigate that or --

DR. ICHIMORA: Yes. If you were to look at the facility after a long period of no rainfall, there are desiccation cracks. And that is associated with, you know, any type of clay-type material.

What Dr. Benson mentioned earlier and what that does is enhances the hydraulic conductivity to natural clay covers. In other words, it increase the hydraulic conductivity.

What we have seen at the facility is these cracks are usually limited in thickness, but they do fill up with different kinds of materials as a process of being rewedded.

So in the end, if you were to look at a

very shallow layer of this material, maybe, say, of six-inch thick, the hydraulic conductivity has greatly increased as a result of desiccation cracks and refilling.

What we have done to bypass that situation is we have since then placed a capping system that relies upon synthetic material. So basically we have the natural clay cover followed by a GCL layer, hydricic polyethylene, a drainage layer, and a vegetative layer on the top. So that bypasses the situation with respect to the hydraulic conductivity of a natural clay cover.

MEMBER HINZE: I think that's helpful to all of us to hear that experience.

I'm wondering, Craig and perhaps Jake and Tom, has there been any research done to mitigate the shrinkage cracks that occur in clays by the proper mixtures of clay and sand materials and so forth?

There must be a point where you reach the situation where you're not going to get cracks but you're going to have the lowest possible permeability under that condition. Is there research work that has been done on that to hep with improving the caps?

MR. PHILIP: Yes. Actually, soon after I found out in the literature from journal articles that they were having problems with clay covers, clay barriers, I started a research program with the U.S. Army Corps of Engineers. Basically what we wanted to know is why do these cracks form. Is it a function of the type of clay? Is it a function of the thickness of the clay? Is it a function of how much soil you have on top of the clay?

In soil mechanics, we have a property called the plasticity index, which is it is an index test which gives you a good idea about how good the clay is as far as the hydraulic conductivity is concerned, as far as its swelling capabilities are concerned and shrinkage and all of that.

They did come up with a report which they are now finalizing as a U.S. Army Corps of Engineers report, which basically said they looked through the literature. And they found out that clay covers do desiccate very rapidly -- this is just from the literature -- and that it happens very quickly and it happens through the entire depth of the clay.

And so we have asked them to look at he different variables that could be useful to look at, like, you know, the thickness of the clay, the type of the clay, the amount of soil about the clay to actually understand the degradation mechanisms that are actually why it is happening.

So we are looking at a wide range. There are three different types of clays. You are looking at a clay with a low plasticity, like a silt. We are looking at a clay with a medium plasticity, which would be a combination of maybe an illitic or a colonnade clay; and one which has got very high plasticity, which is mostly like a monopolyillitic clay, which would have a plasticity index of maybe somewhere in the 60 to 70 range. And the other one is about a pi of about 40 and a very low plasticity.

So we are looking at that. And they have done some preliminary tests in the lab. And they have seen wide ranges of cracking depending upon the type of clays. And now they are going to look more at the thickness of the clay, about the amount of soil they put out in the clay, and try to understand the degradation mechanisms in there.

MEMBER HINZE: The whole thing.

MR. PHILIP: Yes. And most clays when they are compacted, they are compacted wet of optimum moisture content. And that has been in the literature for some time that they would prefer that clays, compacted clays, be compacted to optimize the content.

There may be some disadvantages to that

because the more water you have in the clay, the more propensity it probably has to crack. So there are some considerations of maybe we are going to go to dryer optimum or something like Craig could talk about. I mean, do you see any reason to go dry optimum and maybe it gives us a better performance of the clay?

DR. BENSON: Yes. The cracking is largely due to volume change or shrinkage. So if you can control the shrinkage, you control the cracking. And you could do that by using that something is less plastic or increase the sand content that's been studied or increase the density. And it's not so much dry optimum but getting as close to optimum as possible because you increase essentially the solids content. And, therefore, it becomes less compressible.

You have to balance those things with costs because, for example, to custom-make a clay becomes very expensive. You don't say, "I'm going to increase the sand content to some amount. I've got to do really costly blending." It from an engineering point can become impractical at some point.

MEMBER HINZE: Well, I remember doing this with a cratering problem that I had down at the waterways experiment station many, many years ago and making the proper type of clay layer with an admixture. And it wasn't really that difficult with the machinery, and that was 50 years ago. I would think that that would be quite possible.

DR. BENSON: It is certainly possible. People have done it. And it's --

MEMBER HINZE: Is Wes doing this for you?

DR. BENSON: Yes, yes.

MR. O'DONNELL: My name is Ed O'Donnell. I jumped up to the table, Bill, to kind of answer your question to give kind of a smart response. In terms of mitigating, you bury it deeper.

And there was no big mystery to us. We spotted this at Maxey Flats in the late '70s and then the West Valley. NRC did have an experimental program, a bunch of lysimeters, both at Maxey Flats and then out here at Beltsville, Maryland. We always had this mind.

Don't put the clay barrier right at the surface, which we just did with the Albany, Georgia situation, probably with the best. You know, you get that thing, very quickly get it vegetated and everything else.

In terms of Beltsville, we did test two of the clay barriers. We did it reluctantly, but our NMSS colleagues wanted us to do this clay barrier. And we thought a clay barrier is not particularly good for an Eastern humid site based on our Sheffield, Maxey Flats, West Valley experiences.

And, much to our surprise, at Beltsville the two clay barriers have never leaked. And they should have leaked. For a research thing, we want them to leak.

(Laughter.)

MEMBER HINZE: You just have to build all the big caps. That's all.

(Laughter.)

MR. O'DONNELL: Yes. And all it's demonstrated is that in a research project, where you are a little bit more careful than out in the field, that you build the barriers a little bit too well.

Now, the clay barrier at Beltsville is not very deep. It's about a foot down. And there's about a foot of topsoil, maybe six inches of gravel. And then there's one foot of clay.

In about 15 years, there's been no water passage through these things with the exception of around an instrument penetration. But it's not a cover that we really advocate for an Eastern humid site, these multiple layers. We probably give a much better choice.

Now, Craig mentioned about one other type of cover we did have at Beltsville. This is a surface cover. And it was conceived of as remedial action for West Valley or Maxey Flats sites.

We actually have standing water in trenches. And this cover was conceived of as a maintenance cover, but the maintenance was negligible. In 20 years there's been no cost to maintenance on this thing. It's a series of surface panels, which juniper were planted between the panels. If you go out there today, all you see is nothing but juniper. Ninety-two percent of the surface is covered with impermeable panels.

The juniper was selected because it can suck up moisture. So we have a solar-powered pump. So this cover, zero depercolation, and we started with two meters of water, one lysimeter, and drew it down to zero and maintained it at zero.

The other we start with one meter of water. We drew it down to zero. And it's still at zero after 20 years. That's 1985. So these would be remedial action types of covers for the sites where you have subsidence here in the Eastern U.S., very difficult sites to maintain during the period that you have subsidence. Then you do whatever else you want to do.

MEMBER HINZE: Thanks. That helps very much.

DR. BENSON: Thanks. One of the questions is, though, how deep do you need to bury clay barriers? That's often something that comes up. I don't know if we know the answer to that because we subserve this type of cracking in barriers that have at least a meter of fill on top of them in humid climates.

I'm not sure where that depth it, whether if you made it two meters. At some point, though, if you made it deep enough, you will cut that problem off. The question is, how deep does it need to be?

MR. O'DONNELL: One other thing I would like to volunteer in this is that we always see the beautiful drawings with green stuff on top of them. And we see it is obviously clearly a grass cover.

So we're never thinking far ahead to, hey, you know, we are going to have to always maintain these pesky grass things. We'll fertilize them like a golf course, get the ET up, do all that sort of stuff, mow them. Ever think about --

MEMBER HINZE: In Indiana, we just plant corn. It's a great --

MR. PHILIP: I just wanted to comment on the cover that we had at Beltsville. One of the things there which was very important in having almost no infiltration done was that the runoff was really very high. Almost 50 percent was runoff. And that was enhanced by these panels on the top.

So basically what you really have is just a roof on top of the soil. It was basically what that was. So that you would never get any water down, but if you look at some of the slides that Craig presented, in those sites, those are large-scale sites. Runoff was never more than almost about ten percent or so.

So you're talking about a big difference in runoff, which otherwise was just infiltrating the covers.

MEMBER HINZE: I guess the point, too, regarding the non-destructive testing, I think Mike has summarized a very good idea. And that is the installation prior to the construction of sites for study. And this can and is being done with cross-borehole tomography.

And I think that is something, if I

recall, Tom, that you said you were getting involved with or getting the NRC involved with. This can be done with a number of different force fields.

And the beautiful part about this is you don't have to rely if you start at the beginning of the game. You don't have to rely on absolute measurements. But you can look at the change, and then you could do all kinds of interference patterns and so forth, which become much more sensitive, higher-resolution power.

The tomography, cross-hold tomography, the technology of that the computer programs have really advanced in the past decade and are well-available.

VICE CHAIRMAN CROFF: You done?

MEMBER HINZE: Yes.

VICE CHAIRMAN CROFF: I mean, if you're not -- okay.

I guess I'd like I think maybe to start with Vernon. And maybe Jake and Tom will have some input. And I'm not a monitoring person or a geo person. So I'll display a little bit of ignorance.

In establishing the location of monitoring wells around burial ground or whatever you've got, is this art? I mean, ignoring the regulatory drivers, is it art? Is there a science to it that is somewhat reliable? How do you go about doing this and saying it's going to be there or here?

DR. ICHIMORA: I'd like to take a -- you know, it's partly art, and it's partly some science. But what you typically do on a site is you look at the site as a whole. And you begin to characterize the distribution of, for example, hydraulic conductivity. And you kind of come up with what you think is the full pattern for a site.

The tendency, at least from the humid environment, -- and this is kind of -- and some people might disagree with me on how we would go about locating monitoring points.

The focus would be if you can find materials at a given site which has the highest hydraulic conductivity that is continuous beneath the site. It doesn't have to be continuous with respect to its connection with the disposal units. So the disposal units can be in a very, very tight formation, which is, you know, sometimes in a way a good idea.

But if this layer is very, very continuous and it is a higher hydraulic conductivity; in other words, it has a better ability to pass water, it will probably be a likely location to put a monitoring point.

Now, in some cases, you know, again, I've given you more of a simplistic view of it, but there might be many, many layers. And then at that point, it will be more of a challenge.

But what you would want to do in this case is look for the one that's most dominant, has the most amount of water, and is likely the pipeline. If I might describe the process, it's like a pipeline that drains the site.

And, again, what you may want to do at that point if you have the conceptual model and you believe that that is the case, then you go in and test it. But a numerical model might be very crude at this early stage of site characterization. And it's typically what is done in the licensing process of a new facility.

But, again, as Dr. Kocher mentioned this morning, you never are absolutely sure that that is indeed the pathway until you see the tracer in the system.

VICE CHAIRMAN CROFF: Thank you.

We heard an example mentioned over here of where -- I don't remember which site you said, but they obviously missed it because the USGS found it.

Any other --

MR. NICHOLSON: Well, I agree with Vernon. We have research that is going on right now with advanced environmental solutions, a contractor who is looking at this very question. And where you look, how you look, and when you look has a direct bearing on your conceptual models. And I use the plural, not singular.

And so these alternative conceptual models are extremely important because there are some things you know quite a bit about and other things you know very little about. So the monitoring program has to take that into account.

And so where you look and how you look and when you look has to do with not only the regulatory objective but the baseline that you have already collected.

And you're asking the question, okay. What is the highest probability that if a contaminant were to be released, where would I expect to find it? And that's the point that Vernon was making with regard to these so-called high-probability zones. It could be fractions. It could be a variety of heterogeneities that cause a preferential pathway to exist or may exist.

What's more important is you want to also monitor the precursors. And so the idea is you are looking. We did quite a bit of work at the Maricopa site before on unsaturated zone monitoring strategies in which we looked at things like water content distributions.

And Bill brought up the issue before about the salt geotomography. The idea is we are looking for changes. And so, therefore, you do not have a uniform distribution of moisture content.

But the distribution gives you an insight into where these so-called fast pathways may be. And so that's the idea. It isn't an exact science, but it is this relationship between conceptual models and your ability to monitor. And the monitoring becomes extremely important in that regard.

There are different types of monitoring. Obviously the ones you were talking about with regard to the example I just cited, we were not doing detection monitoring. But we were just still trying to understand the system, how dynamic was it.

And that's a question I'd like to ask Randall and the other speakers, the evolution of the new sensor technology. You obviously want to do it in such a way that you're looking at what are the dynamic and significant processes that are going to affect contaminant transport and how do you go about doing that today because in the old days, we talked about quarterly monitoring programs. What good is that if the process is operating so fast you won't see it?

And so the new sensor technology, especially the wireless technology, I think can give you that kind of almost real-time, near-continuous data that is important to understanding.

What are your thoughts?

DR. POSTON: Well, Tom, that is a great question. And I know in dealing not directly with this type of system but recently on some systems that had expansion and some slag material in the soil that created some structural foundation problems for a large industrial facility that we were going out quarterly, making measurements, and we weren't seeing anything.

But, really, in that context, it really was taking more of a year and a year and a half to really see anything meaningful in terms of the sensitivity of the measurements that we were trying to make, which were pretty much the best measurements or best types of sensors that we could use for this particular phenomenon.

The other point I think you were maybe

perhaps making that I see with being able to get real-time data is that you just get an overload of data and really to process that data. And I've seen that on a number of studies, dynamic studies, on bridges.

We can give you just gigabytes and gigabytes of data, but it really isn't meaningful until you look at it in the context of what's happened last year, two years ago with what is happening now and not what happened last week or the week before last.

So certainly -- and that's even for something, structures in my mind that are perhaps much less sensitive to or slow in terms of the types of things you are going to measure. So I think there is a real thought process that would need to go on in conceptualizing how often would you want to take the data and be able to process that data. Otherwise I think you just get data overload.

CHAIRMAN RYAN: Tom, there's a dimension that, too, that comes to my mind that we think about systems. And we have obviously agreed that thinking about a groundwater system as static is silly and thinking about it is dynamic.

I guess my own view is that there are a

number of different time cycles and time horizons that in terms of this monitoring and modeling and getting it all together are important. The surfaces -- you know, you can think of day and night. ET is day/night. You know, a lot is happening there.

And then maybe the first ten meters is week to week. You know, if you have a rain event of a thunderstorm in South Carolina of two inches, you will monitor it soon thereafter, you know, in your near-surface monitoring wells. And then moving on down, there's maybe more like a seasonal cycle, you know, spring, winter, fall, and you can see those changes.

And the temporal aspects of all of these relationships I think is something that when you take a snapshot, you know, of what is happening, it is very hard to take that snapshot and then see that whole temporal line.

And that is Vernon's point, that if you -- and I think everybody's point -- that if you're careful and collect that data in a way where you can see it, analyze it, display it, and understand the picture it is showing you, you can begin to see the patterns that you might otherwise miss.

So it's complicated by the fact that there are all of these different time constants within the system.

MR. PHILIP: I want to go a little bit onto concrete and how it performs. I was really interested in the discussions that we had on non-destructive testing because one of the things that we found out -- I ran the full cycle quite a few times.

What my general feeling is -- and I have talked to our principal investigator, Ken Snyder, about it at NIST. And what I felt was that if you have environmental conditions where you don't have sulfates and chlorides, that could affect the concrete as acids are leeching.

And if you had proper aggregates which you use in your concrete, you did the test, the test for alkali aggregate reaction, alkali silica reaction and so forth, the only thing that could really affect any transport to the concrete would be cracking.

It is very difficult for us to determine where the cracks would form. We know that concrete is weak in tension. So it's only where the stresses are intention wherever you get the cracks. But it's very difficult because the way the structure is built, the settlement of the structures, some other stresses, you could have cracks anywhere.

So it is very difficult for us to a priori try to predict where these fractures would occur. One of the things we did was we did some calculations. And I did some calculations on flow-through cracks. And it was very revealing to us because it started out with looking at some of the problems that we had with leakage in fuel storage pools.

I was in a meeting at NRR. And there was a leakage of about they said something like 450 gallons per day coming out of a spent fuel pool. And there were a lot of questions as to why this was happening.

Of course, you know there is borated water there. We haven't looked at borated water and its effects on concrete, but we felt that, I felt that, there was something there that was causing this water to leak. And so I went back and did some quick calculations. And what I found out was very small cracks.

It could be even a 100-micron thick crack aperture, which went all the way to a spent fuel pool, which is like almost a meter thick and with a 40-foot head would give you something like 250 to 275 gallons per day. So it's a lot of water that you could get for a very small crack.

So my question was, now, 100-micron crack is just the thickness of a human hair almost. And if you really wanted to somehow find out where that is occurring because it's very difficult to even see unless you had an open surface and you could see the water coming out and you could say there's something going on there but if it's underground, as most of these tanks are or even some of our base facilities would be underground.

So how do you really locate a crack small, really small, which can really conduct an appreciable amount of water and to do it where you do not have access to both sides of the structure or maybe to one side of the structure? So you have a combination of a structure under the ground. So you are going through the soil.

You probably could use the SASW spectrum of the surface waves technique for at least part of it and then probably -- I don't know -- maybe use the impulse echo technique, a combination of techniques to really look at where these cracks are so that we know where it is occurring and to fix it somehow.

Now, there is a problem in fixing small cracks like that, but that is another story. So my question would be, what are existing techniques that can look and characterize these small cracks in concrete using non-destructive methods?

MR. THADANI: I think you might just note in that case that they had isolated the drain system, --

MR. PHILIP: Right.

MR. THADANI: -- which they shouldn't have done.

MR. PHILIP: Right.

MR. THADANI: And that's what caused the problem.

MR. PHILIP: Right.

MEMBER HINZE: Good luck.

DR. POSTON: Yes. Jacob, if you and I could come up with a method for that, we wouldn't be sitting in this room. We would be millionaires right now because there's probably enough work out there to assess, you know, cracks of that size in concrete.

It is a matter of scale. Typically as a structural engineer, a crack is not meaningful to us in terms of integrity of the structure unless it's 15,000ths of an inch or 20,000ths of an inch.

And I echo exactly what you said. I add anecdotal information. I was involved in a tank where it was losing two foot of head every day. And people were going, "Well, where is the water coming from?"

We could see a crack down in the floor slab. And we drew down the water. And you get down there and turn off the pumps. And you could see water coming up through all of these cracks. Otherwise you absolutely could not see.

The short answer to you is there is really not good technology on a non-destructive basis for assessing cracks of that size. When you get down to cracks of that magnitude or smaller and those are typically we're looking at some of the deterioration, the delay deterioration mechanisms in concrete-like alkali silica activity, we usually have to take a core sample.

We do things like neutron radiography or whatever in order to be able to look at the micro cracking through the concrete because that is really the kind of scale that you're getting down to when you're getting down to the human hair level.

So I could imagine some systems where you might be able to I say paint but some kind of conductive or electrically conductive or some other mechanism that would have the ability at that level to separate or break that you might be able to measure some electrical property of the type.

I think some of the larger scale type of measurements that you're talking about, maybe impulse response or spectral analysis, surface waste might give you a general sense over an area that you have cracking that you otherwise couldn't see.

But in order to identify an individual crack in the concrete, frankly, we don't have good technology right now.

VICE CHAIRMAN CROFF: John, did you have a question?

DR. PLODINEC: Yes. This is John Plodinec. I'd like to comment on that.

First, in an arid environment, of course, Hanford has demonstrated they can use -- well, I guess the EMT is the acronym for it -- to find leaks of that size into the soil. In other words, they're not worried about where the crack is per se. They're going to find where the leak goes and then figure the crack is around there. And at Savannah River, they have demonstrated the ability to patch steel tanks with salt gel materials.

Sort of related to that, however, there was a solicitation about three years ago by DOT looking at embedded fiber optic cable into concrete.

Work that we have doe for the folks at now Erdic or Vicksburg for the Army has shown that you can both do moisture and strength determination if you embed the cable in the concrete. It's a little more costly, but it gets at what you're asking for. It's an easy, cheap way to be looking at those kinds of things.

And you can also dope your fibers. You can actually pick up contaminants, too.

MR. NICHOLSON: If I could follow up on that? That's a very good point. And the argument is in this whole endeavor, how much planning do you do to monitor prior to the failure the idea that you built into the design the concept of putting in sensors, putting in methods so that you can see these precursors to large failure, the small failures? How much effort would that take?

DR. PLODINEC: Well, basically what you have to do is say, "What's the limit of protection in the technology, and how fine a grid do I have to have?" Roughly what you're talking about is a fiber grid on the order of foot-by-foot squares.

So you're talking about a fair amount of fiber. On the other hand, the price of fiber is going down. It will continue to go down. And with what is being learned about the durability of fiber in these kinds of environments, it has a tremendous potential for these kinds of, if you will, early failure analysis.

I think, similarly, use of simple things like -- as a change monitor, looking at tanks at the cooling coils and doing ultrasonic or sonic pinging can give you good change monitoring in terms of changes in the state of the concrete fills.

Putting notched rods down the risers can, in fact, over time -- you know, if there's delamination of the different layers over time, can it, in fact, pick up those delaminations?

So there's a lot of the technology that's out there that can be used today if the very first step is I think what Randall showed on that one slide. You've got to decide what it is you're looking for. Once you've done that, there's a heck of a lot of measurement technology out there.

VICE CHAIRMAN CROFF: I'd like to I think direct the next one at Craig. And it's a nasty question, but I'm going to ask it anyway.

DR. BENSON: Go for it.

VICE CHAIRMAN CROFF: You showed a number of graphs of performance with caps, a couple of which look pretty good in terms of the percolation and some of which didn't look very good after years or even weeks there in one case.

What I I think need to ask is, for a cap that seemed to be performing well, such as you showed, what would you expect to be the lifetime of that cap before it goes to seed, you know, really starts to leak?

I mean, I'll take a range. I'm not looking for a specific number, but --

DR. BENSON: So how long are they going to last before they leak substantially?

VICE CHAIRMAN CROFF: Yes.

DR. BENSON: That's another one. If we can answer that one, I'll be a millionaire, too, maybe. Maybe that's got a little better to find scope to it. There's been a good bit of work done at least on geosynthetic caps made with polyethylene that suggest that, at least in that -- I'm always careful when we talk about hundreds of years because you look back. It's 2005 now. So in 1905, think about our technology a hundred years ago. It puts things in perspective.

But at least the research that has been done on polyethylene geomembranes and lifetime expectancies is on the order of several hundred years.

So that's at least a time frame with periodic maintenance that is being considered in the literature.

So not tens of years. It would probably be longer because I've got to say I take a little grain of salt because we're always thinking forward that we know more -- we always think we know more than we actually do. And I think if you look backwards in how technology has changed, you get a sense for how our understanding is going to change in the future.

VICE CHAIRMAN CROFF: Okay.

DR. BENSON: I guess one other comment I would add to that, I think if you can design a natural type barrier system that relies more on a natural element or processes to function and you put it in its natural environment, the chance of it performing in perpetuity are much, much higher.

In some of the work that's been done through the mill tailings program and through some of the work that has been done at PNL has looked at that from a perspective of long-term changes in precipitation and other issues in testing out those hypotheses under those stress conditions and have developed designs that are consistent with that natural environment but also are capable of withstanding changes in that environment over time. And I think those systems are likely to last a very long time.

VICE CHAIRMAN CROFF: Okay. And, second, there's been some previous discussion within this Committee about whether one should design a cap that's, say, to last as long as it can, you know, be very robust, if you will, or whether it should be designed maybe not to last so long but to be more readily replaceable and maintainable. Do you have any thoughts on the bounds there?

DR. BENSON: That's a tough question as well. Part of that is economics. If you're going to replace it in the future, you have to have a mechanism to make sure it happens. I think that's the biggest challenge.

So we have something that happens that we need to fix in 30 years. I'm going to be retired in 30 years. So I'm not going to work on it. Who is going to make sure that it gets done and that there are financial resources to do that? That's the problem I see with that scenario.

VICE CHAIRMAN CROFF: Okay.

DR. BENSON: So if you can make something that is longer-lasting or lasts in perpetuity, you're better off. On the other hand, can we do that? Is that really a realistic goal?

VICE CHAIRMAN CROFF: And I think, finally, from my part, I guess this is probably more in the way of an observation and may be more directed at you, Craig, and Randall over here. I can see a lot of benefit in trying to monitor closer in. And you're closer in, and Randall would be closer yet if any of this stuff could really detect what we need it to.

The tradition in the U.S. design philosophy of near-surface disposal has been basically we don't want any penetrations anywhere near this thing and forget about leachate collection, not unlike the RCRA world. So it's basically don't give water an opportunity to get in. Seal it up as best you can.

And what you're wanting to do sort of runs counter to that a little bit in the sense you're wanting to put sensors either in a cap or get something close to a tank or whatever the issue is.

It seems to me there's an opportunity for some good thought here and maybe some development here, development work on how one might go about providing access to let's say the side of a tank while protecting the top of it with a cap or how best to get instruments in and out of caps and replace them over time.

And another thought I don't want to lose here is one I think -- I guess John sort of came up with it here, trying to think a little ahead about this in terms of especially putting sensors inside of either grout walls or grouted tanks, trying to think ahead a little bit about this in terms of is there something that we can put in now that may be more -- you know, is there some way to provide this kind of access so you can put something inside into the future and maybe view caps a little bit more as an umbrella, where you can walk under an umbrella and the water doesn't get on you if you've got a decent umbrella, no wind, but you can -- and so access to the side as long as you're not in the water table isn't necessarily a bad thing.

But it seems to me there is an area that needs to be thought through. It is sort of a whole fur ball here that really needs to be worked out.

With that, Ruth, you --

CHAIRMAN RYAN: I was just going to add one thought, one additional comment. I think I would challenge Tom and Jake to think about, how do we follow up on this and get the right folks together to do either an expert elicitation or workshop or something to probe some of these questions of integration.

And I mean that, how do you take the monitoring questions and the modeling questions and the details of how you would penetrate a system in an appropriate way, as opposed to an inappropriate way, and all of that and liners versus covers and all of these issues.

By the way, at Georgia Tech, they taught us that you weren't supposed to stand in an umbrella. You are supposed to put it over your head. So liners may not be a good idea.

(Laughter.)

VICE CHAIRMAN CROFF: Yes. I've never been a big fan of them. Okay.

CHAIRMAN RYAN: But I think there's a very rich area here where it -- and, again, I'm thinking in the more practical goal of the staff's function here of developing a standard review plan for WIR determinations.

A lot of this, if there were some aspects of this brought forward, it would be real helpful to practitioners who have to deal with these questions.

So thank you.

VICE CHAIRMAN CROFF: Ruth?

MEMBER WEINER: Just if you've already answered my questions, don't repeat it because I do apologize for my absence. For Vernon, have you ever looked at the likelihood or probability that you're missing a major release of some kind? In other words, you say you put the monitors where you expect the stuff to be, but how likely is it that you're missing?

DR. ICHIMORA: At the current time, I think, at least from our experience and from what we have seen over the time frame, we have characterized around the facility a lot.

And from an operating facility standpoint, we don't only look at what we call routine environmental monitoring. It's procedure-driven. And they're specifically located in locations that are predetermined and may be negotiated with the regulator.

We also look at characterizing, periodically going on a campaign of characterizing, and looking for any other possibilities. So I would say with that kind of program in place, it is not likely that we're going to miss anything that is substantial in size.

MEMBER WEINER: In other words, you are minimizing the likelihood?

DR. ICHIMORA: That's correct.

MEMBER WEINER: Thank you.

Craig, have you ever had any experience where new construction has significantly altered the groundwater flow, like if somebody puts in a large construction, a dam or something like that?

DR. BENSON: So have I personally seen that or have there been cases?

MEMBER WEINER: Do you know of cases? And how would you handle that?

DR. BENSON: I think that if you looked in the literature, you could find plenty of documented cases where the manmade systems affected the groundwater system.

MEMBER WEINER: Yes.

DR. BENSON: That's fairly common. So yes, I think that's been observed in many cases.

MEMBER WEINER: Well, in this kind of an environment, where you're looking at long-term disposal site, how would you address that kind of situation? Would you say, "Well, there can't be any buildings for X miles?" Would you have a buffer zone? How would you propose addressing it?

DR. BENSON: So you're talking about if you had groundwater impacts potentially?

MEMBER WEINER: Yes, potential groundwater impacts.

DR. BENSON: That's a tough question. I think what you want to do is try to predict what is likely to happen with the greatest amount of confidence.

And then I think what Tom was talking about earlier about monitoring data and performance assessments -- then you used another term, "performance" --

MR. NICHOLSON: Indicators.

DR. BENSON: Yes, "indicators." Taking that data and then revising your expectations of what you believe the correct model to me, well, I at least make pretty reliable predictions of where that is likely to migrate over time. And that can then provide you with at least the information about where your limits could be in terms of human contact.

I think that is a reasonable approach. I think people do that now. We just don't often update our predictive capabilities as we go. We often do to the front end and then monitor to see how it turns out. I think we need to close that loop a little bit more on the monitoring and modeling.

MEMBER WEINER: Thanks.

VICE CHAIRMAN CROFF: Okay. Do we have any other questions from NRC staff or any other presenters? I think we have all been pretty engaged. Tom?

MR. NICHOLSON: I have one quick question. One of the things that we're thinking about -- and I'd like to hear if anybody has any ideas -- is how do you structure long-term monitoring to capture these future site conditions and system behavior?

I think Craig did an excellent job of talking about short-term, moderate, or medium-time, and long-term. And obviously the long-term you're not going to be as detailed in your monitoring as you were early on.

At the same time, you want to focus on those significant events and processes which may change the system significantly, that either the engineered system fails or your pathway has now changed and your monitoring program has to take that into account.

How would you think about that? How would you think about structuring this long-term monitoring program?

DR. BENSON: Well, I'll start. And then, Vernon, you could maybe follow up. Part of that I see as our ability to predict or get a sense for what the likely response would be to a change in the environment.

If precipitation is twice what it is today or our barrier system fails, if we have a predictive tool that will give us a sense for how our subsurface conditions are going to change in response to that change in environment, then we can design the system to look for those responses. For example, will the groundwater table rise? Will the water content change? Will the concentration increase above a certain threshold?

So we can design a system based on what our predictive capabilities tell us would happen under those changing environmental conditions. But that's contingent on having a model or a proper conceptualization of the problem.

Again, it comes back to that early stage of monitoring, where you're looking at hypothesis testing and assumption testing, which I think is really critical to doing that type of long-term type of monitoring directly.

MR. NICHOLSON: Thank you.

DR. ICHIMORA: I'm going to try and give you a little bit of a more global view and maybe long term, but it could mean in the time frame of like 100 years. That's my understanding.

In the groundwater environment; in particular, when you look at basin-wide scale, I mean, the environment will probably not change very much. That is, you know, what will most likely happen is for a period of increased rainfall, you would have increasing water table elevation. And, of course, that may impact the performance of their disposal unit.

Over the long term, the flow directions would typically be the same. It's basically going to flow downhill. It's just a matter of everything else is going to rise.

The endpoint -- and this is particularly true in humid environments that I have seen. The boundary condition to stream may rise a little bit. So, consequently, that holds your water level down. In a very, very extreme end on the uphill end, the maximum water level that you would ever see in the system is going to be the land surface. And that, of course, is going to be driven downwards by the fact that, you know, as you get up higher, the ET is going to take over. So nature is going to balance itself. So basically the flow direction is going to be the same.

I think what you will see in impact -- and this is something that I don't see evaluated in a lot of cases, engineering impact of what happens to -- you know, the engineering impact to the naturally occurring water table system, as you make vast, huge changes locally around the facility.

As an example, as you cut off the water, what happens? The tendency would be to ignore that aspect in terms of looking at some of the microscopic environmental monitoring programs. In other words, where do you go and look for the changes in water level as a result of cutting off infiltration from, you know, say something like ten inches to absolute zero in ideal condition.

DR. PLODINEC: This is John Plodinec. Could I give a quick comment as well?

I think this is a good time to plug DOE, who seldom gets talked about, which is the Office of Legacy Management. This is something that comes directly under their purview.

My personal opinion is they're not paying enough attention to the issue, but that's personal opinion. But I think this is an area that ultimately the two, NRC and DOE, we are going to need to be engaged on because clearly when we are talking about in the context of the 3116 law, you have this monitoring function. They're going to have to deal with that monitoring function because they're going to be the landlord of the site.

CHAIRMAN RYAN: Okay. Tom, one of the things that might be a separate sort of point is the single events influence. You know, for example, you get some kind of a storm that comes through and you get 15 inches of rain in a day. I mean, that's an event.

And then, you know, it may all run off because there is so much water or it may cause other problems and there may be perturbations. And I think that kind of transient analysis, things like recurrence interval and other things of that sort, how do you deal with the short-term events. And their sort of dampened effect over some period of time is something to think about as well to round out the picture that Vernon mentioned and Craig mentioned.

So it may not be important in many environments, but in some, they might be. So it's the old story that climate is what you expect and weather is what you get.

MR. NICHOLSON: Thank you.

VICE CHAIRMAN CROFF: Okay. Well, with that, I think I have two final things to do here. I have I think some closing remarks and then a little bit on the path forward.

Regarding closing remarks, it may sound a little bit Academy Awards-like, but on behalf of the Committee, I would like to sincerely thank all of the speakers for your time and preparation, traveling here sometimes over a long distance and the presentations and the Q&A in this meeting and answering a lot of our whacked-out questions, I guess.

You have given us a motherlode of information here to mind for the future. And I think we will be doing that in the form of the transcript, which is our motherlode and why we don't have to take so many notes.

I would also like to thank Latif for his efforts in organizing this. Many of you have talked to him and helped him make all of the arrangements as well as the administrative staff up here. There have been a lot of efforts, you know, travel arrangements in the meeting and the copies and all the rest of this. And I'd really like to thank them for what they have done.

So I think with that, the working group

meeting itself I guess is formally adjourned. You're welcome to stay for the rest of this, which will be I think very short.

My suggested path forward to the Committee is much like the letter Jim is working on on decommissioning. Basically the transcript should be available within weeks. We have got the viewgraphs. And Latif and I would take the lead in mining this and formulating the beginnings of some kind of a letter on this subject.

I think, recognizing that on this subject there are a number of events occurring over the next, oh, I would say six months roughly, we're expecting an academy report very shortly.

The NMSS staff has said yesterday they are planning on having a public meeting, which should provide some useful insights, the final academy report sometime in the winter and a draft of the standard review plan sometime in the spring, I guess.

I'm suggesting, I think, that this letter would be again very similar to the kind Jim was crafting, one more of observations and general thoughts to what we have distilled out to help the preparation of that initial standard review plan but certainly not any kind of a comprehensive review.

So if that sounds right to everybody, we'll go ahead and prepare that and see if we can bring it in to the September meeting or not. We'll just see when it becomes available, the information, because this is a big motherlode.

This has been a long workshop. And there are transcripts, blessedly, will be electronic and not on paper.

MEMBER HINZE: I would think that it would be very useful, even before you started trying to put this into some lettered paragraphs, to come up with a summary of the meeting. If you and Latif could come up with a summary, that would give us, then, kind of the benchmarks from which to work and see where a letter develops.

CHAIRMAN RYAN: To that end, Bill, I think I jotted down a few notes for that purpose to give to Allen in kind of a distilled bullet from a couple of talks and things of that sort. That will be the first vent for us to help Allen get that together.

So if you have some key items or thoughts or observations and share it in that regard, that would be real helpful to Latif and Allen.

VICE CHAIRMAN CROFF: Right, and maybe more interested in things that you might not find in the transcript, distillations, observations, and this kind of stuff.

MR. THADANI: Allen, just a comment. If the Committee makes recommendations such as the one Mike made, which makes a lot of sense to me for Office of Research, then the timing of the letter would be important, it seems to me for research so they could make sure that they have the resources to be able to initiate this effort in some reasonable time.

VICE CHAIRMAN CROFF: Oh, sure.

CHAIRMAN RYAN: We have in mind sooner, rather than later.

MR. THADANI: Yes, exactly. Yes. I think it would be useful.

CHAIRMAN RYAN: Thanks, Allen. Let me add on behalf of the Committee, Allen, that thanks for the Committee and everybody on ACNW staff. You did a lot of yeoman's preparation and a lot of detailed work on getting this working group session together along with Latif's help. And the Committee really applauds your effort and appreciates your hard work and the detail with which you put together a fine two days of working group meetings. So thank you very much for your effort.

Is there anything else before the business for the Committee? Again, I think on behalf of all of the Committee members, we appreciate all of the participants. And I second Allen's comments on the excellent work and appreciate the participation by our members of the visiting audience that were here for the last two days.

We'll start our record tomorrow. I think our first information-gathering session is at 12:45. So that's when we'll start that session. Okay? And so we'll go from there. And, again, we'll convene at 10:15 tomorrow morning for some letter-writing discussion. That should be fairly brief and then on 12:45 to the remainder of our schedule that day.

Any other items?

(No response.)

CHAIRMAN RYAN: We'll be adjourned, then. Thank you very much.

(Whereupon, at 4:33 p.m., the foregoing matter was recessed, to reconvene at 10:15 a.m. on Friday, August 5, 2005.)