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UNITED STATES NUCLEAR REGULATORY COMMISSION'S

ADVISORY COMMITTEE ON NUCLEAR WASTE

NOVEMBER 28, 2001

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UNITED STATES OF AMERICA

NUCLEAR REGULATORY COMMISSION

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ADVISORY COMMITTEE ON NUCLEAR WASTE (ACNW)

130TH MEETING

WORKSHOP ON RESEARCH NEEDS

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WEDNESDAY,

NOVEMBER 28, 2001

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ROCKVILLE, MARYLAND

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The workshop met at the Nuclear Regulatory Commission, Two White Flint North, T2B3, 11545 Rockville Pike, Rockville, Maryland, at 8:30 a.m., George M. Hornberger, Chairman, presiding.

COMMITTEE MEMBERS:

GEORGE M. HORNBERGER, Chairman

RAYMOND G. WYMER, Vice Chairman

B. JOHN GARRICK, Member

MILTON N. LEVENSON, Member

WILLIAM J. HINZE, ACNW Consultant

STAFF PRESENT:

RICHARD P. SAVIO

SHER BAHADUR

JOHN T. LARKINS

F. PETER FORD

NRC MEMBERS PRESENT:

TIMOTHY J. McCARTIN

WILLIAM R. OTT

JACK E. ROSENTHAL

ACRS MEMBERS PRESENT:

MARIO V. BONACA

ALSO PRESENT:

DARRELL K. NORDSTROM

JOHN L. WILSON

JOHN H. KESSLER

STEPHEN RATTIEN

KENNETH ROGERS

D. WARNER NORTH

DAVID C. KOCHER

TOM KING

BUDHI SAGAR

WESLEY C. PATRICK

I N D E X

AGENDA ITEM PAGE

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

(8:34 a.m.)

CHAIRMAN HORNBERGER: The meeting will come to order. This is the second day of the 130th Meeting of the Advisory Committee on Nuclear Waste. My name is George Hornberger, Chairman of the ACNW. Other Members of the Committee present are John Garrick, Milton Levenson and Raymond Wymer. We also have Mario Bonaca and Bill Hinze with us.

In addition, Mario Bonaca, Chairman and Vice Chairman of the ACRS, George Apostolakis is on my sheet here, but George is not going to be here. Peter Ford, I am told, will join us this afternoon.

During today's meeting, the Committee will finish its workshop on research needs and will then discuss the following: preparation for our January 9, 2002 Commission briefing and preparation of ACNW reports.

Richard Savio is the designated federal official for today's initial 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 sessions. Should anyone wish to address the

Committee, please make your wishes known to one of the Committee staff.

It is requested that the speakers use one of the microphones, identify themselves and speak with sufficient clarity and volume so that they can be readily heard.

So we're going to start right in on our workshop on the future of NRC's nuclear waste related research and we're talking about research needs and prioritization today. Again, thanks to all of our invited guests who have joined us. We're looking forward to hearing from you.

Let's see, first thing on the schedule, we have scheduled a half an hour for a summary of previous workshop discussions. I doubt that we'll need a half hour. I think we're probably going to get started before that.

One of the things that I noted yesterday that I wanted to make mention of is that whenever we talk about research, of course, what we do is we highlight things that we don't know. That's the nature of research. We focus on work that needs to be done on problems, issues that are unresolved and I think that perhaps when we get into a discussion like this, an audience could be misled into thinking that problems is all that there are and that we can't possibly make any decisions. And I think that's a perspective that I don't think any of us share, at least on the ACNW. The ACNW really believes that there are lots and lots of licensing decisions that the NRC makes that the information certainly is adequate to make these decisions and as we look forward to some of the future decisions that the NRC has to make, what really we're trying to focus on here is what new research tasks could be accomplished to make those decisions more efficient, better, better informed, more risk-informed, for example.

I had that reaction yesterday and as a university researcher, I'm comfortable talking about problems and issues and unresolved things and what exciting new things need to be done, but I did think about that afterwards and perhaps in listening to it an audience might be misled into thinking that problems is all that there were.

Other Members, do you have any other comments or summary comments?

Raymond -- from yesterday?

VICE CHAIRMAN WYMER: No, other than the sort of platitudinous comment that I thought the presentations were excellent, right on target, and there was a very broad spectrum of coverage from the fairly detailed research ideas to the very broad sort of fundamental bases for -- reasons for the research. So I thought it was a very good kick off.

CHAIRMAN HORNBERGER: Milt? John?

MEMBER GARRICK: I think I have a number of comments, but I think I'd prefer to wait until the workshop is nearer the end to extend them.

CHAIRMAN HORNBERGER: That's fine. That's what I assumed.

Okay, we're also probably, the schedule, as it is indicated, we had some compression of the morning schedule so that we may, in fact, just launch right into what is on your schedule as the afternoon schedule, somewhere around late morning because everybody is here and I think that we can do that.

Greg Choppin from Florida State, everybody, I think, has a copy of the slides that Greg would have presented were he able to attend. He called in because of an illness he could not attend. I don't really think that we're going to take time to go through his slides. I think that everybody can read them. If the Committee or any of the experts have any comments that they think deserve discussion we can do that in question periods.

So I think what we're going to do is proceed right to Dave Kocher's presentation. Dave is at Oak Ridge.

MR. KOCHER: Yes, good morning. I very much appreciate the opportunity to come speak with you this morning on these important issues. I should give you a little background about myself. I worked at Oak Ridge National Laboratory for nearly 30 years and back in the 1980s I had a research project with the Office of Research here that was looking into issues of uncertainty in modeling high level waste disposal. For about the last 10 years or so at Oak Ridge, I was part of teams that performed performance assessments for about 4 low level waste facilities at Savannah River and Oak Ridge. So I've had some experience in performance assessment on both sides of the fence.

I should also say that I'm a nuclear physicist by training, a health physicist by osmosis --

(Laughter.)

I have absolutely no experience or technical training in geology, hydrology, chemistry or material science.

MEMBER GARRICK: That gives you an advantage.

MR. KOCHER: Which I guess makes me eminently qualified to talk about source term and transport issues.

(Laughter.)

That's the truth.

CHAIRMAN HORNBERGER: So physicists tell us.

(Laughter.)

MR. KOCHER: Physicists are always seeking the truth.

I did, when I got the call to make this, I was frightened because I don't know these subjects, so I did call some of my friends out there and solicited some input. I got some input from Mick Apted from Monitor Scientific and Don Lee from Oak Ridge and Roger Setz from INEL, but for their benefit you should know that the opinions that I express are entirely my own. They're not responsible for this.

(Slide Change.)

MR. KOCHER: I started -- I took a little bit different tact on the problem. I tried to make a list of what I thought we knew enough about already to begin and I identified basically item 1 is in the source term area. I thought we knew enough about inventories and physical chemical properties of waste forms in the areas that I listed there. Notice that low level waste is absent from that list.

In the transport area, I felt we knew enough about atmospheric transport and transport of surface waters, although there are some surface water issues about partitioning between solid and liquid phases and things like that. But in general, I thought for purposes of assessing waste problems we knew enough, and of course, this is a very short list.

Everything else in my opinion is open season.

(Slide Change.)

MR. KOCHER: I have been a modeler in previous incarnations and I'm probably going to lose my union card by the end of this talk. But I want to make some basic points so you know where I'm coming from.

We write down model equations that we can solve. Now this is obvious, but it is profound because it immediately affects the way we think about a problem. It constrains our concepts about thinking about waste disposal issues because we immediately write down an equation we can solve. And I think one of Jane Long's messages from yesterday is that we need to look beyond. Our conceptual thinking should not be constrained by the models that we can solve and I agree with that and I'll make this point again later.

We'll never be able to model these disposal systems precisely, so to me, the question is how good is good enough?

We talk about models and data as if they are somehow different, but I tend to think of them as being largely the same in the waste area, because we don't have models based on first principles for the most part. So when we say we need better models, that often means we need better or more data.

Item 3 I think is particularly important in regard to decision making. Whenever we model a waste disposal site, we are always going to have significant extrapolations of information, either from laboratory to field, from one site to another and one that I didn't list there that's very important is extrapolations in time, because we take a long time horizon in the waste area. And because we don't, can't write down the Maxwell equations for this disposal system, the validity of any of these extrapolations is always subjective and at any site, including Yucca Mountain, the data will be sparse by necessity.

I think Jane's zebra analogy is very apt for -- we just can't make Swiss cheese out of these sites if we expect to put waste there. So sparse data is a problem.

(Slide Change.)

MR. KOCHER: There has not been much talk about this workshop so far about how standards themselves can influence the kind of research that we find we need to do. And I'm pretty big on the issue of standards and how important they are. So I want to make some totally off-the-wall statements here.

A lot of what's in our current day standards in my view is basically unrelated to this issue of long term safety. We have some things in there that just don't really get at the problem. Number one is this business of time cutoffs. Yeah, if you go out in time, your uncertainty perhaps increases, perhaps not, depends on how you look at the problem. But I want to emphasize in my view a 10,000 or a 1,000 year cutoff like DOE uses in the low level waste area today, these cutoffs are completely arbitrary in regard to the safety of a disposal system. They perhaps help you to make a decision down the road, but I'm not so sure.

The concern about cutoffs in my mind is two. First of all, it may distort the analysis that you do. If you have a time cutoff, you're always looking for higher K_Ds to make the problem go away. Is this real? Is it not? I don't know. But to me, the number one issue is that it can discourage full disclosure of your state of knowledge of how this system is going to perform. I think full disclosure of your state of knowledge is essential in making good decisions.

Drinking water standards is an ox that I gore often and the fact that we have to limit the effect of those from 0.3 mrem/y, anything above that is seen as a threat to public health and safety, of course, this offends the intelligence of every tree stump in the nation.

(Laughter.)

But more importantly, it can -- there's going to be serious money spent, I suspect, on trying to solve the I-129 problem because of this constraint and is that a wise use of resources? I don't know.

Another example that I sometimes point to is there are important radionuclides missing from the classification system in part 61 and this can skew the way we look at low level waste.

A point that is rarely appreciated is that our whole system of regulating waste disposal is based fundamentally on how we started regulating protection of workers back in the 1930s. And so we have fallen very comfortably into standards that are expressed in terms of limits on individual dose and what this requires, as was pointed out yesterday is you have to estimate radionuclide concentrations in the environment at specific times and this is a hard problem. There are easier ways to do this and Bernie Cohen is always asking how many people do you expect to die from this activity? And when you regulate based on individual dose, you can't answer that question. So the standard just makes for a harder assessment problem.

This doesn't really affect anything in the way of research, but just to kind of give you a feeling for how I think about some of these problems.

(Slide Change.)

MR. KOCHER: Now the next two pages are sort of my own personal wish list and again, I did get some input from the colleagues of mine that I talked about. The number one, in my opinion, the number one problem in the low level waste area by far is the issue of what's in the waste. All the radionuclides that bubble to the top when we do an assessment that I listed here are very poorly known in most low level waste.

Why is this the most important issue? Well, if we know what's in the waste, all of our regulations set limits on the quantities of radionuclides that can go into a shallow land burial system and those limits, by definition preclude catastrophic consequences of waste disposal. We can't have really bad things happen because there's just not that much radioactivity in the waste. And so it lessens the need for more realistic models of source term and transport, for example, but to the extent that we don't know the inventory, we have our hands tied behind our back when we do these assessments and we really -- at Oak Ridge for many years, they tried to estimate the inventory of Carbon-14 in waste by doing measurements of external exposure rate of low level waste package. Fairly amazing.

Moving on. Number two, chemical controls on release. That's a general problem for any waste and something that we've routinely ignored in low level waste, but I notice it was in the research program plan was the influence of microbes and organics on releases from a low level waste site. Because I guarantee they're there. In low level waste, we basically don't know the chemical forms of many of the wastes.

Another problem particular to low level waste, we model every disposal facility as if the waste is in place homogeneously and that there's no preferential flow of water through the disposal facility, but of course, that's not the way a real facility looks. The wastes are very heterogeneous and we will have preferential flow paths, except in cases like well, the salt stone facility at Savannah River. I mean that's going to be a grout monolith if it works and so you'll have cracks, yeah, but it's not like putting barrels in a trench.

And the reason this is a problem in low level waste is because we typically put our compliance point about a hundred meters from the location of waste and so you don't have a lot of environmental buffer to kind of smooth out the inhomogeneous behavior of the disposal facility before you get to your receptor location, but we've never, in my understanding, we've never really tried to understand and model this phenomenon and see how important it is.

I think the rest of my remarks are sort of on waste in general.

Number four, Mal Knapp talked about yesterday and I think this is really important. I don't see that we've done very much work on what happens when we have multiple contaminants and multiple foreign substances in a waste repository of some kind. We do all of our studies one substance at a time. But what happens when you put 40 different chemical elements out there and they're competing with each other for whatever is going on? What happens? Whether this is important or whether unusual things would happen, I don't know, but I don't see a lot of attention being paid to this.

(Slide Change.)

MR. KOCHER: Well, flow and transport of the vadose zone is a clearer problem. How do you determine moisture characteristic curves at specific sites? Do we even know how transport depends on moisture content? My understanding is that it could either be inversely proportional to moisture content or directly proportional to moisture content. Well, that's a big difference. What is it?

(Laughter.)

Take your pick. This is a basic problem. Flow and transport in fractured or highly structured media; when you combine 5 and 6 together, it's not even clear that we know what the conceptual model is that we ought to be trying to describe for some of these things and I think that came through in Jane Long's presentation yesterday where her student had a new way of looking at the problem. Do we even know what the right concepts are in these areas of complex environments and unsaturated zone?

In the low level waste area we have tended to take the unsaturated zone and throw it away. Because we don't know how to model it, we take no credit for it. We show compliance by assuming that the release from the disposal facility goes right into the aquifer and if we can meet that, we're fine, but you know, Yucca Mountain is a different problem.

We've all talked about this. We all know this before. Extrapolating laboratory data to field scale and I just gave a couple of examples. We basically don't know what the scaling relationships are in many cases, but things like longitudinal disparsivity are extremely important, say, in addressing this iodine problem. Accelerated testing, you all know this. And one that I'm probably totally wrong about, I wonder if there have ever been any serious field studies to investigate the validity of Darcy's law and the linear sorption isotherm which we all use. Thee things certainly work in the lab. They certainly work in sand boxes, but I'm not so sure they've really been tested in the field.

We have preferential flow paths at many sites and how do you interpret the kind of hydrologic tests that we do in the field when you have these flow paths. And underlying all of this is something we talked about yesterday, I totally support. We absolutely have to study historical disposal sites for what they can teach us. That overrides all of this stuff, except for what's in low level waste.

(Slide Change.)

MR. KOCHER: My charge, as I read it, was to talk about how NRC would use research for their purposes and so Ken, I did not borrow this from you, but I completely agree with two points that you made yesterday. In addition to getting data to use in models, there are two fundamental things that NRC has to do with the research and number one is getting at this issue of what are the right conceptual models to describe the behavior of different parts of a system.

And the more research you have, the better you are able to constrain credible alternatives and Bill Ott raised a question yesterday, what do you do if you have several alternatives that you can't choose between and my answer to that is you don't choose. You address the consequences of both and you use both of those approaches to define your state of knowledge about this system, what the uncertainty is. You don't choose one or the other if you can't.

And to me, the investigate alternative conceptual model is just a key to obtaining reasonable assurance of safety. Now I worked on an IEA committee several years ago where we tried to define different elements of what went into reasonable assurance and we had a list of about 15 or 20 different things that you needed to do, but at the top of the list was that you had to embrace the world of alternative conceptual models and really follow through on it.

And number two, this was talked about several times yesterday, we're going to end up with extremely complex models of complex systems and somehow we have to be able to still this down to the essence of how this facility works and describe it to the public in simple terms. If we can't do that, I don't really think that we'll gain acceptance of decisions for very visible projects like Yucca Mountain.

This has not been much of a problem in the low level waste area because in the DOE system permitting of low level waste sites has kind of gone under the radar screen. The commercial area may be a little different, but certainly for Yucca Mountain, you're going to have to reduce this complex system and its descriptions as something that is fairly simple that captures the essence of this thing.

So I think these are really the two important uses by NRC. They will, no doubt, have many challenges in reaching a licensing decision, but I would say the good news for the NRC, and I'm going out on a limb here, the NRC will not be -- probably not be the final arbiter in Yucca Mountain. I'm guessing that the final decision about go or no go of Yucca Mountain will either be made by Congress, the President or the Supreme Court because whatever decision NRC makes is likely to be challenged by somebody, but the NRC has to make a confident, a very confident decision with a good basis and what are their challenges in doing this?

(Slide Change.)

MR. KOCHER: No matter what we do in the way of modeling or site characterization, there's going to be a substantial amount of irreducible uncertainty in our knowledge of the outcome of these activities and how to gain acceptance of -- as Mike Ryan said, binary decisions, yes or no, go or no go, in the presence of this uncertainty which we really, we can get a handle on some of it, but certainly not all of it. This will be a challenge.

Budhi mentioned the unknown unknowns. I think this is an important area of thought in high level waste. How to take into account what it is that we don't know and to remember that because we're using geologic environments as our friend, hopefully, some important issues of long term safety don't have engineering solutions. We can't entirely engineer our way out of this problem, I don't think. Unless we just decide that the geologic environment is kind of like a warehouse and we're going to store it some way using an engineered system to store it. We could do that.

Mike talked about this third point, how to judge compliance with fixed standards when confidence intervals at the end of the day are going to span several orders of magnitude for sure. And I've seen ideas in the NRC that well, we come up with a probability distribution function and if the 85 percent confidence limit of that distribution is below the standard, we're okay.

This is nonsense. You can't quantify this decision by a rigorous statistical analysis. We are not going to be able to make objective comparisons of model outputs with standards. There's judgment in the modeling and there's judgment in the decision and the question is how much technical support do you have to marshal to make your decision, but you can't do it objectively on statistical grounds.

(Slide Change.)

MR. KOCHER: There's been a lot of talk about the realism versus conservatism and my thoughts here were not well posed when I wrote this slide, so I kind of want to backfill a little bit here.

I think conservatism in modeling is essential, but as has been pointed out correctly, to be confident that you've made a conservative decision is not an easy problem. So I think what I believe realism is about is gaining enough information on the behavior of a disposal system so you can be quite confident that you've made a conservative evaluation of the system and that's not an easy problem, not an easy problem at all.

I really get worried about people who insist that the question we should be answering is how safe is it? I think the question is is it safe enough?

As Mike said yesterday, if our state of knowledge suggests that the outcome is well below some standard, I don't really care what the outcome is. It's good enough. Because I worry about when you get into a court of law, how can you defend -- you may not be able to defend a statement about "I know how safe it is." I don't know. I'm probably being unnecessarily alarmist, but I can't think of any other NRC program and radiation protection that really tries to answer the question how safe is it. Even in radiation protection of workers in 10 CFR 20, we're not estimating real doses to real people. Never have. So this may be a slippery slope.

Scientific judgment underlies everything we do in the waste business and we should never forget that. It's how we establish the models. It's how we establish the inputs and therefore, it determines the outputs. We hope that this judgment is informed by good science, but there will be limits to that. So judgment is everywhere.

A point that was made yesterday, modeling doesn't determine the outcome of this system. We have to have the right concepts at the front end and as my friend Gary Roles used to say, paper is not a barrier. It just doesn't determine the outcome.

(Laughter.)

(Slide Change.)

MR. KOCHER: What do we have to do to assure safety of this system and this is an idea that I think has been -- I don't know whether I necessarily support this, but I put this out because I think we're clearly heading in this direction in our national policies. I mean the only way to really ensure long term safety is to maintain perpetual control over these sites. I mean to start out today as if we're going to watch them forever.

That doesn't necessarily mean we're actually going to do it because a future society could change its mind. But we could start out as if we do this and when I read the EPA rule, Part 197, I think that's in there, that you really have to have the mindset that institutional control and monitoring is a barrier. It's part of the multiple barrier system and we should take it seriously. And so I would support the idea of how to monitor long term performance of a disposal system is an important area for future research.

Are you all aware that we've already signed up for perpetual care at Mill Tailings low level waste and hazardous waste sites? We've already agreed to do this. So why not Yucca Mountain as well? The public would seem to favor that and the question is how to do it.

(Slide Change.)

MR. KOCHER: I'm getting just about to the end here. This is sort of a summary of what -- some of the things that NRC should be doing and I talk about all of these issues.

Look at the performance of disposal systems unconstrained in time. Forget 10,000 years, forget 1,000 years. Yes, you have to meet the regulations, but when you do an assessment to support a license decision, to me, you have to bring the full range of information to the table. You have to express your state of knowledge completely and this means going out in time however far you care to do.

I think there's work to be done on developing ways to identify credible alternative conceptual models compatible with existing data, but unconstrained by the kind of models that we immediately want to put on top of the data. I think this is what Jane was talking about yesterday. This is not an easy mental exercise to do.

Point 3, simple ways to describe complex systems. The NRC is probably going to have to improve its skills in communicating licensing decisions in the presence of substantial uncertainty. I mean there's a lot more uncertainty about this than some of the other licensing decisions they've made in the past. Research on monitoring and performance and the last one, I don't know whether NRC is doing this, but especially for Yucca Mountain, I wonder if it would be worthwhile to periodically engage in a serious mock licensing exercise. Say "DOE, bring me a rock." And really sit down and sweat bullets over whether you would license this facility based on the information you were given or not. Maybe you do that kind of thing already, but this gets a point Mal made yesterday about the limited quality of the application.

(Slide Change.)

MR. KOCHER: I got this from Bob Budnitz. Eminent geohydrologist, Robert Frost, I think hit the nail right on the head. And you can just read that as you wish, but I think there's a message here that we do need to be fairly in awe of what we're doing here. These are unusual ventures, unlike anything we've ever tried to do and we're never really going to understand everything and that we will achieve surprises along the way and some of them will be good news. So be serious, but be humble at the same time.

Thank you.

CHAIRMAN HORNBERGER: Thanks, Dave. I'll start this time.

Some of your first remarks struck me as perhaps being in the vein of something that Mike said yesterday, if you're not making measurements you're making conversation, but your final slide seemed to be for what NRC should be doing, seemed to be more oriented toward what I might call paper studies. Do I detect a disconnect there or not?

MR. KOCHER: No, I have wrestled a little bit with the issue of what should NRC do versus what DOE should do and my research wish list was really without regard to who would do it and sort of toward the end I was putting sort of the licensing hat on NRC and saying the next to the last slide is really kind of what, when they get into the licensing arena, what should they do. I suppose there's a role for NRC to actually get out and move direct and do some lab studies and things like this, but I'm not really in a position to say what they should do relative to what DOE should do.

It's the question of what do they need to be doing independently to help them review a license application? An so the research that NRC does could very well be different from the kind of research that

DOE has to do to make their case. I don't know. These are hard decisions, but there's not a disconnect. NRC has unique needs because they have to make a decision.

CHAIRMAN HORNBERGER: Okay, thanks. Other questions?

MEMBER GARRICK: Have we got time for them?

CHAIRMAN HORNBERGER: Yes.

MEMBER GARRICK: I just wanted to pick up on a couple of thoughts because I think they're important from a communication standpoint.

You said something about that you can't make decisions based on statistics or statistical analysis or whatever and I'm troubled by the implication there because that sort of is implying that they've been making decisions on the basis of statistics, that is, the NRC or whomever. I think there's a couple of points there. One is that I sometimes think that people who are not heavily involved in the risk business, for example, have a conception that risk assessment is 90 percent statistics and 10 percent something else. And it's just the opposite.

The 90 percent is understanding what's

going on and being able to characterize what's going on in transparent models and logic diagrams and what have you. And I think it's kind of important that we always try to put that issue in context.

The other thing you said is, and I like this, I like 90 percent of what you did say.

(Laughter.)

MR. KOCHER: My batting average has gone way up.

MEMBER GARRICK: The other thing I just wanted to comment on, you probably are correct, that the proper question is is it safe enough? But unfortunately, that's not our job. Our job is to calculate the safety. That is our job. The decision of whether it's safe or not is not made by us, not made by the NRC. That's made by the citizenry. We put our citizenry hat and we vote just like everybody else as to whether that's enough.

So I think that the challenge here is to do this in such a way, to calculate the safety in such a way that that transition can be made rationally and logically and you're absolutely correct that the NRC's biggest challenge may be to come up with methods of communication that enhance that whole process.

So that's just a couple of thoughts.

MR. KOCHER: Yes. Let me respond to the first one about the statistics. The key word there was an objective comparison.

MEMBER GARRICK: Right.

MR. KOCHER: And I did not mean to imply that the NRC has ever done this, but there was a kind of a trial balloon about this in the low level waste area and I just kind of wanted to shoot it down, that an objective comparison of some probability distribution function with a fixed standard is not the decision. It doesn't dictate the decision.

MEMBER GARRICK: Yes.

MR. KOCHER: It's what went in at the front end in your understanding of the system, so I think we basically agree there and I don't see that NRC would ever do this, but it's just a caution.

MEMBER GARRICK: I understand.

MR. KOCHER: I'll let it go at that.

MEMBER GARRICK: Yes.

CHAIRMAN HORNBERGER: John and then Ray and then Mario.

John?

MR. WILSON: Yes, John Wilson. I want to make a couple of comments and then get your response.

I'm sort of surprised having sat here for two days and being naive, I guess. I haven't bene involved in this business in 15 years. When people talk about conceptual models, alternative conceptual models, that's what modelers do. I mean I've never been in a job where there's been a fixed model or one model and that's the only one that exists.

Models contain the essential features for the system and the problem you face. And if you face different problems, two different people working on the same site with two different issues in front of them might have two different conceptual models, both equally valid for what they're doing.

And then that leads me to this issue that you brought up and was discussed yesterday about what is realism in a model. I guess it's whatever is right for the problem you're investigating and the system you're looking at, but does that make it possible to have just one right conceptual model. And in my experience there are clearly wrong conceptual models, but I don't know that you'll ever find the right conceptual model for a problem. You'll find a whole family of them that can serve that purpose.

MR. KOCHER: Yes, I think the issue is to constrain, in some way constrain the ones that do fit the information rather than find the one. I would agree with that completely.

CHAIRMAN HORNBERGER: Mario.

MR. BONACA: I just was, first of all, I appreciate the presentation because being a neophyte, yesterday I was left to the question of where do we have adequate knowledge and at least you offered -- we had so many presentations identifying what the needs are and I was still left with the question of where do we have adequate knowledge.

Now when you presented a slide, the first bullet, you pointed out that we have sufficient knowledge of radionuclide inventories, physical and chemical properties of waste forms.

Now later, you pointed out that for low level waste that's not the case.

MEMBER GARRICK: That's correct.

MR. BONACA: And I was trying to understand how could you possibly, given the source of the low level waste, how could we characterize in fact and develop adequate knowledge? Do you have any idea of how that could be done?

MR. KOCHER: Well, it may turn out that proper characterization of the inventory by itself would solve a lot of problems because if you -- the fact that the limit, the inventories are limited for shallow land disposal systems allows you to get away with a lack of knowledge in a lot of other areas because you just -- bad things just can't happen if you don't have much radioactivity. But some of these long lived ones, their environmental behavior could very much depend on the chemical form.

I mean there is plutonium and Americium in low level waste and I know enough by osmosis that the environmental chemistry of plutonium is very complex, so that in selected areas you may be able to gain some knowledge, but I mean I have to repeat that I know knowing about chemistry, but waste manifest, as far as I know, you don't really have to have any information about what's the chemical form of the stuff that's in there.

MR. BONACA: And I imagine right now for the incineration will bring many elements, just mixed together in the incinerator, so it's very hard to do the tracing --

MR. KOCHER: Well, incineration is probably good because you get a -- you might often get a predictable chemical form of a radionuclide that way, but it's just a mixed bag of junk that we put out there that good God, what's in it?

MR. BONACA: That's right. That's what

goes into it.

Thank you.

CHAIRMAN HORNBERGER: Other questions or comments?

MEMBER HINZE: I guess I would like to ask about the -- your third bullet on what NRC should be doing and develop waste that describe disposal systems using simple models. I'll leave out "credible" assuming that they will be credible.

That seems to me to be a very difficult problem, especially in view of the complexity that we see in our current TSPAs where many of us do not understand all the subtleties that go into them.

How does one go about simplifying this so that they are really understandable, even to the entire Agency?

MR. KOCHER: Well, if I knew how to do this, perhaps we wouldn't be sitting here.

MEMBER HINZE: Do you have any examples of how we can start to approach that?

MR. KOCHER: No, because it depends on -- I think it's going to be a site specific, waste specific thing. It would be different in the low level waste arena from Yucca Mountain.

My general answer to that question is the same as I would respond to John on the question about is it safe enough or how safe is it? We're going to learn as we go here, but it really comes down, I think, in the end to focusing on do you have right concepts up front. I mean we've known for the last 40 years about how these systems are supposed to work if they work well. We have a waste package. We have a waste forum. We have dirt as our friend. And we have to somehow distill all this information to show that those basic components are doing what we think they are. But is there a prescription that I can write down for how to do this? No, no. I can't do that.

CHAIRMAN HORNBERGER: How about just to push it just a little farther, take a specific example. And I'll take one of the low level waste sites that you've worked on. Did you -- were you successful in coming up with a simplified way to present results?

MR. KOCHER: Yes. Well, low level waste disposal site at Oak Ridge was a good example, because there the key was what was the behavior of water in this very unusual environment and the fact that the hydrologic regime there was fairly well characterized and understood was really the key in identifying how this facility would work, all things being taken together. Because we took very little credit for much of anything else.

MEMBER GARRICK: I don't want to get into this subject too deeply because it's something I've been preaching for 7 or 8 years on this Committee that we need simplified models, but trying to pick up on Bill's question and speaking from experience, the way we did this in the reactor field was through the abstraction of complex models to simpler models that were also very physics based in terms of the way things were described and the way things were defined. And it worked very well.

These simplified models, as a matter of fact, have become the basis in many plants for something approaching a risk meter for the plants. And they've had a tremendous impact on -- if nothing else, in elevating the consciousness of the operations people of what's going on, somewhat globally, because of their simplicity. And as long as you appreciate that the simplified model is not the big model and that the big model is not the plant and never forget that, as we've been reminded several times, then these things can be quite useful.

MR. KOCHER: Let me give an example that I did think of. It's not waste related. It illustrates how this process works. We've been studying the global carbon cycle since the 1950s and the very first model of the global carbon cycle was developed, it was a linear compartment model and it had two boxes, the atmosphere and the oceans.

And right away this model was realized that it could describe a lot of details about what the global carbon cycle was doing and so over the years the model got more and more complicated, more and more detailed, but as the knowledge of the details of carbon behavior in the environment increased, the ability to develop a simple three compartment model to describe this system increased to the point where based on good understanding of the system we could define an adequate simple model.

So I think really the answer to Bill's question is as we develop the level of detail of understanding of disposal systems, then the essential elements will emerge that we can then use to have a simplified description. But that's very site specific.

CHAIRMAN HORNBERGER: Okay, thanks very much, Dave. We'll move on to in our presentations of what research is need and Kirk Nordstrom is going to do our next presentation.

(Pause.)

MR. NORDSTROM: How's that? Well, normally one says it's a pleasure to be here. I wasn't sure whether that was the case.

(Laughter.)

When I was first invited to come here, I gave it serious thought because I didn't really want to do it. I had spent a lot of time on the Board of Radioactive Waste Management and that sort of did me in. I didn't go all the way to alcohol, but also a thought consideration. But when I learned that George was Chairman, I said well that has a definite plus.

But actually, I have to say that I'm glad I came and the reason is because of all the presentations I heard yesterday and one this morning. There's a consistent set of themes that's not hard to figure out. It's running through these talks. They're very similar and I think this is going to help the ACNW in their deliberations. And that also helps me that I don't have to spend more than the allotted time to try and say what I want to say. I'm going to have "something to say" messages. I'm just going to beat them home in a different way, perhaps.

One comment for David, I liked everything that you mentioned. I just was going to say in the

USGS we have a lot of work going on in unsaturated zone as well as saturated zone is a very complex topic. Some of that work has been supported by DOE, but not very much. I don't think any of it has been supported by the NRC, but it's an important area. So that's sort of the good news. The bad news is that a lot of these points that have been made have been made before, sometimes many years before. And it's astonishing to me that it takes so long for some of these points to strike home with the DOE.

So moving ahead.

(Slide Change.)

MEMBER HINZE: Could you clarify that?

MR. NORDSTROM: I'm sorry, I don't have enough time. I would.

(Laughter.)

So the issues for this meeting are radionuclide transport and source term. We have the hydrology of the system. We have the geochemical reactions. You have to know hydrology and then the geochemical reactions are an overprint on that. They may attenuate or sometimes enhance the mobility of certain radionuclides and trace elements. We use modeling to improve our understanding and hopefully predictability of how the repository system will function.

Next, please.

(Slide Change.)

MR. NORDSTROM: Now I've drawn on quite a bit of quotations from Naomi Areskes because among all the people that I've met and talked to she articulates some of these problems most clearly and again, this is sort of repeating in a different way some of the stuff that you've heard already. "In recent years, there have been great advances in mathematical treatments of complex systems and staggering advances in computational power, but these have not been matched by comparable advances in data from the natural world. Like any chain of reasoning, computerized models are only as strong as their weakest link."

Next, please.

(Slide Change.)

MR. NORDSTROM: This is, by the way, from a book that I recommend to all of you called The Earth Around Us, edited by Jill Schneiderman and she chose that title because it mimicked Rachel Carson's title, The Sea Around Us and all the chapters in this book are excellent. Naomi's chapter is called Why Believe a Computer? It seems a little bit appropriate for some of the discussions going on in this meeting. She continues by saying "Without evidence from the physical world, both as input to models and as a check on them, we run the risk of constructing a computational house of cards."

And I think this has happened, especially with respect to radioactive waste research. "And without discussion of the criteria by which we judge our models, we run the risk of obscuring the profound social issues at stake in the shadows of our edifices of computational prowess."

(Slide Change.)

MR. NORDSTROM: So what can we do? Well, one obvious consequence and this has been said before, get more and better field parameters. How does the actual chemistry, for example, I'm a hydrogeochemist. I mostly do field work.

I also do modeling, but from my own experience I know that if I want to understand how an environmental system works, the only way is to get out there, get my feet wet and muddy and make measurements. I try to measure rates of reactions. I try to see if solubility is really controls something in the real world by getting as good a data from the field as I possibly can and then I do calculations, not modeling calculations to predict what something is going to happen in the future necessarily, but to test hypotheses about processes that are going on in a groundwater or sometimes in surface waters.

So how does the actual chemistry change along a flow path, for example and why. These are questions that we struggle with in the survey. We've had programs for many decades looking at both large scale and small scale aquifer systems and we've developed tools to help us interpret those.

Some of those have been used, but a lot have not been used in the radioactive waste environment and they should be.

To improve the reliability of the physical and chemical data bases, this is the other part of the input data. For example, for geochemical modeling, you need thermodynamic data, sometimes you need kinetic data. Thermodynamic data has been discussed by a few people and some of that is known very well, probably as well as we need to know it for the purposes of regulatory requirements and understanding solubility. It's things like uranium.

There's a whole book thermodynamic data of uranium that I reviewed. It's evaluated the data very well. There's another one on Americium. I believe there's one coming out or may have come out already on Neptunium and these have been done by some of the best experts in the world and they're fairly reliable. The weak link here is that solubilities are one thing from laboratory studies and evaluating thermodynamic data.

The other question is when this stuff leaks and gets out, how does it react with a particular ground water with a specific pH in chemistry and so forth? Now that's sort of what we study an awful lot of, but we haven't done so much of that. When uranium actually gets out and gets in a particular kind of aquifer, is it more mobile or less mobile and what causes that?

And as was mentioned before, there's lots of sites where this has happened, but it hasn't been studied and monitored. Like there's a uranium plume at Rocky Flats. That's a perfect place to find out how fast and what are the reactions that attenuate uranium in that environment. We know it's been attenuated because there was nitrate solutions that were poured into these solar ponds along with the uranium solutions. The nitrate has far outpaced the uranium, but we haven't quantified that difference in partitioning, if you will, and what factors have led to that.

Next, please.

(Slide Change.)

MR. NORDSTROM: Model utilization is another factor. We're using models, but why? Have we developed criteria for the applicability of those models? We need to think about that. We need to think about when is a model likely to be useful and when not. Which model should be used and why?

In some cases, there's a half a dozen perfectly good models that will give you the same kind of calculations, the same kind of results, given the same initial conditions, but there are other models that won't. Some models are built specifically for sorption and work well for that and there's other models that have been built for just speciation and mass transfer reactions and are good for that.

The previous experience with the same model indicate that the objectives will be achieved. That's another way of looking at models and developing this criteria for aquiferability.

Next, please.

(Slide Change.)

MR. NORDSTROM: Now here's -- getting back to Bill's question and some of the discussion, here are some other things that you can do to improve the applicability and to look at model evaluation. Have an independent modeler, these are things that the NRC could do -- have an independent modeler run the model for the same conditions, say that somebody in DOE has done, but apply his interpretation or even better, have an independent modeler run a different model that supposedly does the same kind of thing, say groundwater flow or geochemical modeling for the same conditions and see if he gets the same kind of answers or not. Then you have the problem of well, if they kind of match, are they right? They might not. They might both be wrong. This is a problem with models. If they don't agree, which one is wrong? Again, you have a problem. You don't really know because you don't know the truth. You don't know the final answer, what you're after.

And this is sort of the problem when it comes right down to it, it's a problem of science in that we approximate, hopefully better and better, the real processes in the world, but they are approximations and we have this non-uniqueness problem of a model and all we can say is hopefully the more consistent the information is, say from different models and different field constraints and laboratory experiments that we do, hopefully, the closer we are to what the correct answer is.

So it's a matter of consistency and the better you can hope to approximate reality or approach better accuracy is by doing it more -- looking at the same problem as many different ways as you possibly can.

This one needs a little explanation, "use standard reference systems to test models." I'll explain what this means in geochemistry. I think most of you are somewhat familiar with speciation. We know that, for example, plutonium or uranium, uranium is a good example, exists in many different ionic forms. We know that in carbonate solutions, uranium is very soluble and that's because uranium ionic forms a complex with carbonate and tends to stay in solution.

How do we know the speciation models are any good? Well, I've done a little bit of evaluation of that. They're not real great. You can get significant differences, especially for trace elements sometimes, depending upon the thermodynamic data.

What we did back in 1979 is published a paper in which we took seawater, average seawater composition and we made up an average river water composition and we asked about a dozen different people who use speciation models to calculate their results and then we would compare them and discuss the discrepancies. And we did that and most cases for the river water example the results were pretty similar because river water is very dilute and you don't get much complexing going on.

It was a bit different for seawater, but the point of it is that we set up two sort of standard reference water samples that could be run through and we encourage people if they build a new speciation program, to run these standards through and see how they compare with this round robin evaluation that we did. Some people have done that. A lot of people have ignored that. We've built some more standard reference systems for groundwater since that time and one could do the same thing for mass transfer reactions. This would help us a lot in knowing whether those kinds of models really have anything to do with reality.

Next.

(Slide Change.)

MR. NORDSTROM: Vocab and meaning, this gets to communication. There's been some discussions on that. This is my personal opinion. Please delete the phrase "model validation" from your vocabulary. The reasons have been clearly stated by Naomi Oreskes and several other people, Andy Konikow, John Bredehoft, a host of experts.

The problem is it gives the implication that when you validate a model, the model is okay, it's good, it's reliable and through the discussions that we've had, that's not the kind of meaning or connotation that you want to get across to the public. And that's not really how science works, so please encourage others to do the same.

Next.

(Slide Change.)

MR. NORDSTROM: The other thing and this has been coming up in the discussions as well, it would help to keep the distinction clear between the testable hypotheses of science, that is, you might develop a model for a groundwater system and then predict what it's going to be like 10 years down the road and then 10 years down the road, hopefully, you're still alive and you can test it and see if you're right or not. Versus the intestable simulations from time forward modeling, scenario building or whatever you want to call it over hundreds of thousands of years.

As people have said, these are very uncertain. We don't have any reliable way of knowing if those results are any good. This is not the direction that we should be going. You all know the history of how that's happened.

Next, please.

(Slide Change.)

MR. NORDSTROM: Now here's another one, beware the model complexity paradox. Again from Naomi, "... the more complex a model, the harder it is to refute." This is a very important thing to keep in mind. So we face a paradox. "The closer a model comes to a full representation of a complex earth system, the harder it is to evaluate it.

Put another way, the better a model is from the point of view of the modeler, the harder it is for others to evaluate it." I've seen this happen time and time again. One of the first evaluation committee meetings I was on regarding mine-waste, there was a very sophisticated model that said that arsenic wasn't going to move through this groundwater system in a thousand years to hurt any drinking water supplied downstream. And I consider myself pretty good at knowing hydrology and geochemistry and I didn't understand what these guys were talking about. I didn't know the model. I asked them questions about it. They couldn't really explain it and so we had to say, we don't know if this is garbage or not and the end result is this is one of those intestable simulations anyway.

So there's a trade off between representation and refutability. There are limits to how far we can go on this.

Next.

(Slide Change.)

MR. NORDSTROM: For low level sites, well, some of this applies to high level sites too. Hydrologic and geochemical measurements and modeling can be done. A monitoring system can then determine and compare real time series data with the modeling predictions. That's the kind of information that we need and we actually have done a lot of this in the survey. A lot of people from academia have also done this. This is a very straight forward use of science.

Apply young groundwater age-dating techniques. There's a lot of work that's been done on the CFCs and tritium and helium, improvements for dating young groundwater as well. A lot of this stuff has been done both in the survey and outside the survey and they could be applied to low level sites for getting a better handle on transport times and we're just about slipping off the slide here, but I'll read it out.

The last one is an important one and Jane Long mentioned this. Apply mass balance and inverse modeling methods to sites where monitoring data is available. In geochemistry, what this means is you collect groundwater chemistry data, along a flow path and then you know something about the mineralology and you back out of that what minerals must have dissolved or precipitated to give the end results that you get in your water chemistry and of course, the more chemistry and isotopic data that you have, the more you can pin down the processes.

Next.

(Slide Change.)

MR. NORDSTROM: And finally, we have to accept the limitations of the real world. Naomi put it like this, "Because of the heterogeneity of the natural world, uncertainty is inherent in the very nature of the systems we are studying."

Now this is hard to get across to the public sometimes too, but we have to do this. We have to be honest. "The required empirical input parameters are fundamentally inaccessible, not because our instruments are not sophisticated enough, our budgets not big enough, or we are not clever enough, but because the data do not exist in a form required by the equations at the scale to which we have access."

Just a couple of more comments that I'll make. First, just a footnote on this. I wouldn't say that it's impossible to figure out all the heterogeneities say in a groundwater system. You can do this, but to do it you would make Swiss cheese of the place. You'd have to get so many samples that it no longer has the original hydrologic geochemical properties that it did. So there's a little bit of an analog to the Heisenberg uncertainty principle there.

I've been involved with natural analog studies. I think these are extremely useful as people have pointed out. I'm not sure of the extent to which they have -- the information from that has been synthesized, boiled down and then applied to the radioactive waste disposal problems that we have. And if that's been done, fine. I'm very glad. If it hasn't, it urgently needs to be done because there's a lot of very good information there and I'm not sure it's all getting to the right people.

In addition, the natural analog data, I would say we have these anthropogenic analog sites that we've talked about: spills, leaks, uranium plumes. There's a lot of information, for example, on plutonium and plutonium colloids that's been worked on at Rocky Flats and Los Alamos and other places. Just to mention that I was on the Rocky Flats Advisory Panel for almost 3 years and one of the big discussions that came up was with regard to the distribution coefficient for plutonium. I'm sorry that Greg Choppin is not here because he's been on that Panel longer than I and has some very strong opinions about that.

The problem was that they wanted to take some distribution coefficient, put it in the RESRAD and use that for regulatory purposes and we kept trying to say that plutonium from every bit of research that has been done, including research on site in the field, it is extremely insoluble. And the partition coefficient they were using didn't make sense, didn't apply. And if you've tried to put in something that maybe approximated it, it didn't work in the program. We ended up wondering exactly what RESRAD did and we actually had a whole meeting to understand RESRAD.

But there was a conflict here between real data, experimental data that's very, very reliable and trying to use that in RESRAD and we kept saying you don't really have to use RESRAD. That's not the goal here. But they said no, regulatory requirements say we have to. And so we had a problem and I think maybe this is typical of a lot of problems that the requirements and the policies that are put into place are not necessarily reflecting the real issues that we want to deal with.

I'll stop there. Thank you.

CHAIRMAN HORNBERGER: Thanks very much, Kirk. Questions and comments?

John?

MR. KESSLER: Kirk, you mentioned you thought a lot of the thermodynamic data bases were pretty good. Did those extend up to the higher temperatures for Yucca Mountain, thermodynamic temperatures, somewhat above boiling and there's a lot of uncertainty there about whether there is a decent thermodynamic data base up in that range for everything that would need to be considered.

MR. NORDSTROM: That's a good question. Above about 50 degrees the uncertainty increases a bit. Above 100 degrees, the uncertainty of thermodynamic data increases a lot and that's one of many reasons why -- not just me, but the survey has said that it would be much better to have the temperature limited to 100 degrees, rather than something higher.

MR. KESSLER: Would it be just an insurmountable obstacle to attempt to improve the data base rather than decrease the -- look at it the other way. If we had better data, is that better data obtainable at any time soon with anything that's possible with joint research programs or whatever, to get some of the more critical data base there?

MR. NORDSTROM: There is some data and I would say it's not that difficult to get more data, say in the range of 100 to 200 to 250, even degrees. That would easily be done. There are some very good labs. There are some of the national labs have been doing measurements in that temperature pressure range, so that could be done.

I think the bigger uncertainty is with the physical and geochemical aspects of the site itself it's undergoing this high temperature change, so -- and I'm glad you brought that up because there's another point that I want to make and that is you know even in an unsaturated system like most of Yucca Mountain, there is some water and there is going to be movement of gases when you start heating this thing up. Presumably some kind of convective cells may be set up, but geochemically, you're going to start dissolving silica in some places and precipitating at others. Likewise, you're going to dissolve calcium and precipitate calcite in places.

There are some analogies to this kind of a system in mineral deposits which people have been studying for a very long time and again, this has been pointed out a few times before, but we could use those analogies to help us understand a repository environment much better.

CHAIRMAN HORNBERGER: Bill?

MEMBER HINZE: Kirk, following up on that and following up on your theme of lessons learned and analogs and so forth, one of the severe problems, one of the severe uncertainties I think is the coupled process area and chemistry is paramount to that. Is it possible that we can get a warm fuzzy feeling regarding coupled processes by investigating some of these other analogs? Are there lessons that we can derive from studies of mineral deposits or radioactive ore deposits, etcetera, that will help us with that coupled process area? As a geochemist, I'd like to get your ideas on that.

MR. NORDSTROM: The simple answer is absolutely. There is a lot of information there. The people who have studied some of these natural analog sites have considered the coupled processes. They have to because they see, for example, zonation of trace elements and certain minerals with distance that has to do with a cooling effect of some high temperature source.

And they've looked at groundwater flow paths and how that relates to radionuclide migration and there's -- I don't know how many natural analog studies there have been, but I'm sure there's over a dozen, world-wide. These are all in different settings. Some are in granite, some are in sedimentary rocks, some are close to the surface, some are deep, some are at repository depth or depths of say 400 or 500 meters, so there's a whole range of conditions and in every case that I know of, they brought in both a hydrologist, the geologist, geochemist, geophysicist and so forth to understand how this site operates.

And they've also done some things I didn't like too much, but it's along the right thinking of blind prediction tests where they analyze the groundwater samples along the flow path for a range of trace elements, but they kept the trace element data secret, okay?

And they told two or three or four modelers to go out and model what ought to be the uranium concentrations, nickel concentrations and other things and then this one guy who had the data would compare that, the modelers results with the actual concentrations in the groundwater and there was a criteria which was pretty reasonable that if they agreed to within an order of magnitude then they felt like they understood the processes.

That sort of testing is sort of helpful. What I didn't like about it is the same problem as I mentioned earlier, a model could have comparable results, the real world data, but for the wrong reasons. Maybe they were just doing mineral solubilities, but in fact, attenuation had to do with sorption. So that's the kind of problem you run into. It's a start anyway.

CHAIRMAN HORNBERGER: John, you want to --

MEMBER GARRICK: Real quick follow-on to that. Is this particular example amenable to inverse modeling? Was there enough data to do that?

MR. NORDSTROM: Yes. I was involved in one site, Pocos de Caldas in Brazil, and I did the inverse modeling of that site and showed how it worked and it helped us a lot in quantifying the relative proportions of minerals that were dissolving and precipitating and what were the driving forces.

CHAIRMAN HORNBERGER: Ray.

VICE CHAIRMAN WYMER: I want to make an observation which is perhaps obvious, but I've noticed over the years people make good livings by stating the obvious, so I'm not going to be any different.

(Laughter.)

It seems to me that there's some ambiguity or possibility for some in the use of the model. Some people mean it to be a very limited process. Some people mean it to be like the TSPA which is certainly not a very limited process. And it seems to me that we ought to be a little bit more careful when we discuss models and say what we mean by the word model.

That's it.

MR. NORDSTROM: Is that something that people want to discuss or not?

(Laughter.)

Because I've thought a lot about it, exactly that question too.

CHAIRMAN HORNBERGER: John?

MR. WILSON: I think that's an incredibly good point. I tend to use the word model from a conceptual point of view, not from the point of view of computer code, for example. Freak is not a model. It's a code to me, for example.

(Laughter.)

But I was involved 15 years or 20 years ago on some studies which were quite interesting that you alluded to the concept that if you had different people work on the same problem without talking to each other, what kind of results would you see? Would there be similar results or significantly different? Is this some measure of uncertainty?

And what we discovered through that process is that if you have almost no information, you find that people come up with pretty much the same model because if they're good engineers and scientists, they go back to everything we all know and that leads to pretty much the same kind of model. Very parsimonious model, quite simple.

On the other hand, if you have an incredible amount of information, you condition the result so that everybody has to come with the same result as well.

This in between where you have some information, but not everything, in other words, before you get to the Swiss cheese stage and then you get complete variance in model results, everybody working with the same thing. The funny thing is if you talk to modelers about whether they get the same thing or not, no. They say things like I used the finite difference model. He used the finite element model.

Of course, we had different results. And they misunderstand. They look at the numerical methods and so on instead of the conceptual aspects. It's a conceptual thing that the variance grows at in this intermediate stage. And for most of the problems we deal with, we're in that intermediate stage.

MR. NORDSTROM: That's a good point.

CHAIRMAN HORNBERGER: Kirk, I had a question that I wanted to at least pose for you. You outlined some things that you see as need doing including getting, for example, better field parameters and a host of other things. And I was just wondering, do you have any thoughts on what the role of the NRC is as opposed to other people who do this kind of research should be and we'll set aside the fact that the NRC should be funding the University of Virginia and the U.S. Geological Survey and others.

(Laughter.)

MR. NORDSTROM: Yes, I thought a little bit about that. It seems to me that the role of the

NRC is a bit analogous to the role of regulators who are licensing, permitting sites and dealing with hazardous waste sites of a non-radionuclide type. And some of these regulators that I know have some of the same problems that I think the NRC has. And so my recommendation is going to be very similar which is that it seems to me that DOE constructs some kind of a model or group of models and the NRC needs to understand that.

They need to understand that to the point that they can see the flaws in it, the problems with the assumptions, and then that can be worked on. That's why I put up that slide about get an independent person, somebody who's independent say of the DOE that the NRC asked to investigate a model and its conclusions and how reliable and relevant they are and use a different model to investigate the same set of conditions and same problem. And make it real clear, spelling out for everybody just what those assumptions, what the differences in the model results are and why and then how relevant is this to the whole problem of licensing a site and so on.

And a lot of it is communication. It is hard to understand a sophisticated model. Maybe sometimes impossible. I mean you've probably encountered the same thing where somebody presents a model that you should understand, but you don't and you keep plugging away at it and that's what we have to do. If we can't understand the model, then there's no point in doing that work. It just shouldn't be done. I think we can if we use the right kind of approaches and right criteria for testing it.

CHAIRMAN HORNBERGER: Milt.

MEMBER LEVENSON: A question out of ignorance, just because you mentioned hazardous waste dumps other than radioactive. In the administration of that program, do they -- what do they use as a time of compliance? How long?

MR. NORDSTROM: You mean like at a Super Fund site?

MEMBER LEVENSON: Yes, or just hazardous waste dumps of any kind, municipal dumps, all the others.

CHAIRMAN HORNBERGER: Dave is offering to help you out here.

MR. NORDSTROM: Yes, go ahead, David.

MR. KOCHER: They have not wrestled that bear for even one round yet. The way the law is set up at the moment, you put the waste in the ground, there is no calculation of any kind about what the long term impacts of this will be. You basically monitor ground water and in 30 years you make a decision about whether to watch the site forever or let it go. But the kind of assessments we do in the waste business have never been done for hazardous waste.

CHAIRMAN HORNBERGER: John?

MR. WILSON: I think if you go back and look at the original Super Fund act and the first set of amendments, the idea was that you could clean up these sites, that there was no long term issue because at the end of the day it would all be gone and then it was the obvious discovery over the last 15 or so years that that's not the case. You're in this intermediate stage where now people are just sitting on it. And EPA and Congress has finally admitted that they can't clean these things up.

MR. KOCHER: Well, I was thinking about RCRA where you're deliberately putting new waste into the ground. There was no -- they had some kind of idea that they might be able to meet groundwater standards if the facility were engineered properly, but the kind of forward looking performance assessment that we do in the radioactive waste business is not done under RCRA.

MR. NORDSTROM: Getting back to reality, the answer to that question is coming out from trial and error and hard experience. The Iron Mountain site in California that I've studied a lot, a very nasty acid mine water with very low pHs has been settled, the beginning of this year, for $862 million. The settlement is such that all the EPA costs have to be recovered now. The companies that have liabilities spend some more money and they take a remaining $500 million which is something that they start building on and at the end of 25 years, if they have not completely cleaned up the site, then they pump that big chunk of money into it and finish it off.

That gives you some idea of the complexities, how hard it is. They're not really completely cleaning up these sites. They're not walk away sites yet. At that site they have a neutralization plan in that removes 95 percent of the metal loading to the Sacramento River. But they don't have the final answer.

At Summitville, they're in the process of settling. They're almost done. They've admitted that they can't completely clean up that site. It's a mine site in Colorado and they've said that it may take 100 years which is sort of another way of saying they haven't got the permanent effects and they don't know how they're going to do that. So that's how it's coming out because they didn't have any time line.

MEMBER LEVENSON: I was thinking not so much of the sites you're cleaning up, but if you clean up a site, you move that stuff somewhere else like the RCRA sites which are somewhat closer to the sites we're talking about and the context of the question is that the waste going into many of those sites, things like lead and arsenic and cadmium have even longer half lives than plutonium.

(Laughter.)

And from a standpoint of risk to society, and diversion of resources, are we going too far with de minimis risks compared to what we're doing elsewhere.

MR. NORDSTROM: That's a very good point.

CHAIRMAN HORNBERGER: Is that a point in need of research, Milt?

(Laughter.)

Okay, we're going to take a 15-minute break and we'll continue 10 past 10.

(Whereupon, the proceedings went off the record 9:54 a.m. and resumed at 10:10 a.m.)

CHAIRMAN HORNBERGER: The meeting will

reconvene, please.

Okay, we're going to continue with our presentations on needed research. And our next presenter is John Wilson.

MR. WILSON: Now is it working? There we go. We got all the buttons on it.

I actually wrote two talks for this meeting. The first one is sitting in a hard disk drive, on my desk at home, torn apart with the rest of the computer that went down the other day. And then, as I looked again over the agenda for what we were supposed to be talking about, I reread the thing and came up with a slightly different talk than what I originally developed, which is more like the kind of very good talks we just heard from Kirk and Dave.

There was a question in there that said something like this: How can research be used to develop the needed knowledge and technical tools? In other words, the question I reread was not, what is the important research? But, how can we used research to develop the things we need? And I approached this, then, a little bit differently.

I kept my focus on transport and source terms. And since I know very little about source terms -- I'm a groundwater hydrologist, by the way, with an engineering background, and I chair an Earth and Environmental Science Department, so I do some geology. But, I really don't know about source terms in this kind of problem, so I'm going to focus mainly on the transport part.

This, by the way, is a view of cesium-137 underneath the tanks at Hanford, if you're not familiar with that. And this little gadget in the lower right-hand side, to remind you, is the effect of heterogeneity on fluid flow and transport -- multiphase flow, as a matter of fact; something that we're quite interested in.

So, I asked the question, what kind of knowledge and tools do we need? This kind of thing we've been talking about, knowledge of processes -- physical, chemical, biological processes. We need characterization and monitoring tools for doing any number of these things; basically, gadgets and methods. And we need modeling tools, to do synthesis, data assimilation, and to prediction and decision support.

For example, risk assessment -- the kinds of things we've been talking about. I'm not going to spend my time talking about these today.

Except, in the next slide -- where every time I push the space bar, the thing sort of spits back at me here -- I want to mention my favorite process studies, my favorite characterization and monitoring studies and modelings -- Processes. You keep hearing this. Trust me. Multiphase flow in a fractured rock is something we know very little about. And given that that is, in fact what goes on at Yucca Mountain, I find the whole thing very disturbing.

Characterization. Scaling -- we know very little about how to scale processes in time-space, particularly scaling up from the measurement scale to what we might use in a computer code or a model of some sort, or the synthesis of measurements made at different scales. This is true for the geochemistry, for the flow physical parameters, and a variety of other things. Biology -- if we don't understand it very well for physics, we have no clue when it comes to biology. So scaling is my second favorite topic today.

Modeling -- quantifying and reducing uncertainty -- particularly, this conceptual model uncertainty. And when you throw in heterogeneity, I really think of that as part of the conceptual model issue, as much as it is a parameter issue. We really need to do a lot of work on that.

I have been on the NSF panel for some years now reviewing proposals, and in all of this time, we only had one proposal come in that talked about trying to quantify model uncertainty, and do something about it, to do an inverse problem on it. And you know who that was? That was from somebody who submitted a proposal to do parameter estimation.

And then after they were rejected, the panel suggested that they retest this new idea. So, they responded to the panel's request, in so many words, and a year later came through with a proposal that was funded.

There's not much work going on in this. I know of two projects now -- Shlomo's project in Arizona and the NSF-funded project at UCLA. It turns out, in other areas of modeling, besides the one we're talking about here, there is work in this field of quantifying and reducing conceptual model uncertainty.

Okay. Well, here's the question, again. How can research be used to provide the needed understanding or knowledge and tools. And I'm going to give you a very naive point of view. Although I get lots of funding from DOE and others that has something to do with the subjects here, I'm so far removed from the practical side, I haven't thought about it in years.

But, 15, 20 years ago, I did a lot of performance assessment modeling on all the salt sites. And the last time I was here, 15 years ago, I was here to show a videotape for the $15,000 that NRC gave me, of multiphase flow and fractures, to demonstrate my hypothesis at that time that there were fast multiphase flow paths through the fractures at Yucca Mountain.

We showed videotape of little drips and drops moving down through fractures. And that was the last funding for that topic for ten years, until work was done about five years ago on that same topic, leading to the kind of thing that Jane talked about last night. Fifteen years ago, nobody wanted to hear that story.

So my naive point of view comes about from the fact that I went on to other things. However, we all know what really will work in this situation. We wouldn't be here if this wasn't the problem. Notice, that's not my handout.

(Laughter.)

MR. WILSON: Dollars -- if we had lots of dollars, we could probably figure out what's the most important research -- which you need to do anyway --

prioritize it, and we could come up with a whole scheme of different mechanisms to approach it through contractors, through extramural funding to universities through consortia, through all sorts of things. But, in fact, you don't have much money, so let's see what else we can do.

I'm going to break this up into two types of research, what I'm going to call fundamental, God forbid, "basic research". I don't mean that in the sense that NSF means basic research. I mean it in the sense that you've probably been talking about it here as anticipatory.

And then, I'm going to break it into a second category, which I'll call "site-" or "problem-specific research". The second of these things is sort of the thing I do as a consultant; the first of these is the thing I do as a scientist and a researcher. Okay.

Fundamental research is going to be on processes, particularly risk-significant processes, but also on characterization and modeling methods. Building new computer algorithms, for example, might fall into this category. It's the kind of stuff that allows you to go in and do site-specific work, problem-specific work.

The kinds of problem-specific things you deal with are conceptual model issues. How does something work? What's going on, on this particular site? It's not the process that you don't understand; it's the site. And, it's all conditioned by what kind of decisions you have to make.

You might call this confirmatory because it's in the regulatory context that you have that comes up in response to something that someone else has brought to you. I'm going to talk first about this fundamental research, and probably the term "basic" is not the one I should be using.

Others, especially other agencies, are sponsoring lots of this kind of work. Can NRC leverage this? Yeah. We all know that. Can you rely only on that? The answer is, no matter what somebody else might tell you, no. And I think everybody here feels that way anyway.

But why is it you can't rely only on this work? A couple of reasons. One is, if you don't have your own in-house expertise, you don't know what's going on. You really can't understand what's happening. And that is one of the reasons I'll bring up below.

What should NRC do in this area of more

fundamental work or anticipatory work? Keep enough work here at NRC, and in the contractors, so that you're on the leading edge. You know what's going on, so that somebody's not informing you for the first time about an issue, that you've recognized it on your own.

For some of these most important issues, I would say you want to be in a leadership role in science. You're not. Curiously, if I go out in hydrology, I don't ever see anybody from NRC there. I don't see NRC contractors there. I'm on all sorts of committees and consortia. NRC has no presence on those things. Where are you guys?

If you're going to be involved in doing this kind of stuff, if this is that important, you should be in a leadership role. Where is it? I don't see it. Some of your researchers are. Schlomo's in a leadership position, but that's about it. And I see Tom Nicholson from time to time.

What else should NRC be doing? Important work that others are neglecting because they haven't bothered to do it, or it's too hard -- that's usually the case. It's much too hard; we heard some of that yesterday. That's a good reason to work in those things. It's important for the kinds of risk-based decisions that are being made here, and it's being neglected for any number of other reasons.

And I heard yesterday about the EPRI model of, work in those things, get it stimulated, get it started, and bring others into it. Okay, make sure that you're doing that. That makes a lot of sense to me. So, the second thing here to do is important research that is otherwise being neglected.

And the last thing I've listed down here at the bottom, if you can see it, in terms of what NRC should be doing is the thing we've heard all along the last two days, is form various different kinds of partnerships and consortiums. You've been doing a bit of that, and you've lost some of it.

The Apache Leap site, an incredible research site, theirs was some leadership. I guess I haven't forgotten that. That's five years ago. I mean, that's where NRC was leading things. It's gone. Go back to that model.

Let me talk about a couple of these. Interagency partnerships -- DOE has come out with an initiative on the Vadose Zone roadmap. Sixty-two team members, an executive committee, with, among people, like Ed Weeks from the USGS involved, put together this thing over the last year or so.

It provides a science basis for funding research and communicating with stakeholders to address a lot of Vadose Zone issues, primarily related to the kinds of things you're interested in. Some of us see it as going way beyond waste disposal issues and to general science issues related to the Vadose Zone, and they're making a move in that direction.

It's an order of magnitude -- it hopes to lead to an order of magnitude improvement over existing technologies for Vadose Zone processes, which are things like multiphase flow and fractured rock, over about a 25-year time span. It's a long-term planning exercise.

It covers processes, including strongly coupled non-linear processes. It covers characterization, including the development of new sensors and the testing of conceptual models. It includes mathematical modeling exercises -- a little about what I'll talk to you about in a minute -- including the development of a new center at one of the national labs, with its own big, giant computer -- because, if you do anything at the DOE, it has to have at least one enormous computer associated with it.

(Laughter.)

MR. WILSON: I finally broke down and we

put that in there.

It involves a lot of infrastructure and technological support. And there's a website on your handout.

One of those things being proposed -- just one -- under the modeling area, is to develop a problem-solving environment, a user-friendly, integrated software environment for examining subsurface flow and transport modeling. An example of something like this might be Mathematica, MatLab, or if you don't know about those things, something like Excel. Something easy to use.

The problem here is, if you're working with somebody from one of the national labs, they have a code that does something. If you want to use it, you basically have to go there for two weeks, train on it, and maybe you'll assimilate it. Everybody has their own little thing, and it's very difficult to do easy problem-solving.

And so, the idea is to begin to integrate all of that following certain protocols into one enormous, but coupled, system. So it's bits and pieces that can be brought together. You can add what you need to it. There's a whole library of things there, including data-assimilation libraries, visualization libraries, data management libraries -- all sorts of things.

It would unify the mathematical, scientific and engineering idea in one framework, allow for much faster testing of hypothesis and evaluation of decisions, and inspire greater confidence in those decisions.

The kind of thing we're talking about, our proposal is, it would have aspects available over the web to the public to allow this whole thing to go easier. This is just one of those things in that Vadose Zone roadmap, which, as you can tell, is rather ambitious. But it's a 25-year plan.

Here's something else to consider. Former university consortium in this area that we're talking about, transport and sources, modeled on the NSF Science and Technology Center; EPA Hazardous Substance Research Center, some of which have been remarkably successful; the NIEHS Centers, superfund centers that pick little bits of the system and look at it for superfund-related work.

I think there are lots of things. For a few million bucks a year -- which is more than you have, I found out --

(Laughter.)

MR. WILSON: -- but for a few million bucks, you can get a lot out of this. What can you get out of such a center? You could have it focus, of course, on an issue of special interest to NRC. As a consortium, it would give you a large pool of people available to work, only a small percentage of which would be working on the center at any one point in time. But, if a new issue comes along, you can go out and grab faculty and staff from the consortium members in the area that's new.

But, it also would condition those people to think about NRC problems, so that it's on the front of their minds and it gives you a resource that you otherwise would not have.

It provides this enormous pool of talent and can form the foundation for a center that can then go out and seek additional funding. So, this center, which you might give two-, three million bucks a year; gets another half a million from EPRI, goes out; and gets, oh, two million bucks from DOE in a way that is not inconsistent with the mission to NRC; a million bucks from the Army; half a million bucks from the Air Force; two million bucks from the Navy. Before you know it, this thing is a real deal, not to mention all the private places it can go out and get money from.

And, it can serve as a model for others kinds of consortiums that NRC could sponsor in other areas besides this transport area.

Let me go to site- and problem-specific research -- I've finished with the anticipatory bit -- and take a look at things here. I didn't know how to deal with this. I do this kind of research in consulting, and it seemed to me that the kind of thing the NRC does has some similarities but some differences.

So, I asked myself some questions. What's special about research and support of regulations? That seems to be what you're conditioning on. And I couldn't answer that question until I thought of some examples. These examples may be entirely wrong. I've never done a regulation. Actually, I used to be a commissioner in the Commonwealth of Massachusetts, so I have been a regulator, but all we were trying to do was keep people from building houses in swamps.

(Laughter.)

MR. WILSON: Here's the first example I came up with. And you tell me whether this is reasonable. An issue is presented to the Commission by some advocate -- and I'll say an advocate; if somebody wants to do something, they're an advocate --

where they take advantage of the gray of uncertainty to shade it their way, okay.

We see the same thing in court cases. You don't want to lie, but you want to make it look good because you want to be successful. That's one possibility. I don't know whether this happens but it seems to me it's a reasonable expectation because it happens all the time in the things I deal with.

The second kind of problem you might run into is an unbiased view is based on data, methods, such as modeling and assumptions, that are inconsistent or weak. You look at them and you say, the pieces don't fit together. You know, they've got stuff in there that doesn't work. Or, quite frankly, you look at it and say, well, it's not as good as it should be.

How do you determine what to do about these kind of situations, or even recognize that they exist? And to do this, I had to go in and look at, how is NRC unique. And before I could figure out how NRC is unique, I asked myself the question, what the hell does research do for regulators in the first place? What do regulators do? So, I sat here yesterday and I made this list up.

Research supports the development of

regulations or the implementation of them by making sure that whoever the regulator is understands the industry they're dealing with. Okay, that's one thing.

And then, it supports the ability of the regulator to assess whether a licensee -- probably misspelled -- is competent to assess whether the facility is adequate for the purpose and safe, to make sure it performs correctly. So that's this next one. Constrain the licensee and facility to operate in a safe and reasonable way. Rules, laws, things like that. Investigate and learn from accidents and failures, leading to changes in all of these.

Something that Mal Knapp said yesterday I found interesting, which I absolutely agree with -- you want to make sure that the decisions made by this regulator are robust in the presence of uncertainty. So, anything in here is robust. Now, you probably have your own version of all of this, so I'm just relearning something.

So, if we look at NRC, how does the NRC situation for sources and transport differ from the sort of standard thing that FAA may deal with. You have much less -- I say no opportunity here -- but much less opportunity to learn from accidents and failures. If Yucca Mountain is a failure, first of all none of us are going to be alive to determine that. And secondly, it's really not going to be allowed as a public policy issue.

And we've heard a lot of versions of this. Planning for the future without experience for the past -- we can't take advantage of those problems, and that makes the decision perhaps necessary that they be very robust. And that leads to over-conservatism in the minds of some people for doing these things.

So, what do you do to deal with this situation? I think you need expertise. It goes back to the anticipatory stuff. You need to use the knowledge you develop from fundamental research on processes, characterizations and modeling, to develop the ability to deal with these situations. You need to, basically, train and prepare for it.

Your research has, as a very important component of it, training people who are going to be involved in dealing with these site- and problem-specific kinds of things. If you're not doing that, you're not doing your job. You need to do that so that everybody's adaptable and flexible.

This is like training for war. You don't want to go to fight the last war; you want to be ready for the next one. You train the troops so that they can go out and deal with some new kind of feature that may be thrown at them, some kind of new condition. And that's where this anticipatory research really pays off.

And then, you can do confirmatory measurements, experiments, modeling -- all, perhaps, incremental, learning from prior steps in doing this, to investigate the kind of proposals or applications that are coming to you.

But it still boils down to, the way to do confirmatory work is, in fact, to do anticipatory work. If you're not involved in developing the research that is used to examine these things, then how can you examine them? You're not going to be prepared to do it.

Focusing particularly on confirmatory research, I had a suggestion here. In many fields that we deal with, it's difficult to fund or publish confirmatory research. In hydrology, it's basically impossible. This is the opposite of the situation in medical science, where it's encouraged -- in fact, demanded. Change that tradition, and NRC will get a big bang for the buck.

Talk to other sponsors; talk to the editor of journals; change the tradition. Make it possible, in fact encourage, to do and publish confirmatory work on issues that are important to NRC. But then, of course, everybody else would benefit.

In hydrology, if I measure something and Kirk measures it a year later, that's too bad. Now, my measurement could be wrong.

(Laughter.)

MR. WILSON: It doesn't matter. Of course, all -- some of the dynamic rate coefficients are wrong.

Focus on transport and source. Surprises -- I heard several people mention this, and I think surprises are a real big deal. The tanks out at Hanford -- I don't know how well you can see this little picture down here, but we've been excavating plastic dykes under these -- not under the tanks, thank God, but they are under the tanks and they're all around that area.

Plastic dykes are a geological feature that could have profound hydrologic consequences, particularly for the movement of fluids and waste. And nobody bothered to deal with them until now. I call that a surprise. And, you go to K25 at Oak Ridge It's built on carst and nobody, in dealing with conceptual models at Oak Ridge, said, geez, carst. Duh -- that's different. It has holes.

The conceptual models at K25 thought of it as a porous media. That's really not a surprise.

CHAIRMAN HORNBERGER: Don't say what it is.

(Laughter.)

MR. WILSON: But anyway.

I don't think multiphase flow and fractures at Yucca Mountain is a surprise in five years. I mean, this is something that should have been anticipated. But anyway, what do we do about that? Surprises and disasters? Somebody could say, let's do so much research that there are going to be no more surprises ever again. Not possible.

There are going to be surprises, and the reason for doing anticipatory research is, among other things, the develop the ability to handle surprise smoothly. Something goes wrong, you don't say, oh shit, that's it -- we're finished. Don't record that, by the way.

(Laughter.)

MR. WILSON: Okay, who's doing research on surprises? I don't know. Every time I ask somebody who should be doing work in this area, they look at me like I'm an idiot. You know, how can you do research on a surprise? And I've labeled disasters in that, as well.

There is research going on in the science of predictability. The ability to predict -- it's related to things like chaos theory and the like, particularly in the climate. And I think there's some close relationships to that.

Since accidents and failure play a large role in the evolution of knowledge in a regulatory environment, and the NRC can't wait for these accidents to occur, how do you compensate for that? This is the serious side of conceptual model uncertainty. This is why I feel conceptual model uncertainty is a much bigger problem than parameter uncertainty because this is where you get hit with surprises.

An additional reason for training, preparation, adaptation and flexibility -- by the way, this is where you lose credibility. Let me give you -- do you have enough time for one quick war story?

I had a graduate student a few years ago that George knows, Jaimin Won, who did a study on colloids and the behavior in multiphase flow, the attachment of colloids to gas-water interfaces in the

Vadose Zone. It's a great piece of work.

Well, I get a call from a guy named Naretniks, who some of you may know, who wants to visit and learn about all this work. And he comes into her new lab at Lawrence Berkley, and my labs. And we talk about this.

He spends about two weeks visiting us over and over again, and it turns out that there's a concern in Sweden, at least this was two years ago or so, now. And that is, when they're going to put nuclear waste in these deep granite batheliths and let them reflood. They'd be under water table, except they're dewatered right now. And when those things reflood, they're going to have a saturated zone disposal site.

They're going to have a cask of some kind buried inside of Bentonite, sealed inside the room. Does anybody know about this particular proposal? Okay, if I go wrong, straighten me out. And so, here was the scenario. Eventually, water was going to corrode the outside cask, which I believe is steel --

AUDIENCE: Copper.

(Laughter.)

MR. WILSON: Copper -- and then get to the inside cask, which is steel. Right? Did I get that right?

MR. NORTH: Yes, over a very long time.

MR. WILSON: Over a very long time, okay. So here is the issue. It breaches the cooper, gets to the steel; the steel corrodes, releases hydrogen gas; the hydrogen gas bubble builds up; eventually, it builds up enough pressure, it hydrofracts through the Bentonite, up through the cracks, and you release gas. Okay, no big deal. Except now, the Swedes are thinking Bentonite, and water and bubbles and bubbles, Bentonite; Bentonite attaches to the bubble surface -- because we talked about colloids as attaching to -- so as the bubbles rise, they're carrying Bentonite attached by forces, various forces, various interactive potentials, on the surface of these bubbles.

So, we're now eroding and carrying Bentonite right up to the surface real fast. And then, Bentonite absorbs plutonium. And so, you have an elevator carrying the Bentonite to the surface. That's what Neretniks was quite interested in. And he had gone and made a big deal back in Sweden; this was important, something to do. There's a surprise for you.

The biggest surprise was about five weeks later, there was an airplane flying to Zurich, and there was a guy from Sweden next to me and he was reading a newspaper and I looked at it. It looked sort of strange; it was a Swedish newspaper, stranger letters and stuff --

(Laughter.)

MR. WILSON: -- and I asked him -- there was something on there that struck me. And it was an article about this whole issue, and the proposal that had come up from the scientific community, particularly Neretniks. And there was a furor in Sweden, apparently. And the furor was basically, gee, you guys told us you knew what you were doing. But you don't.

Here, you want new money to do some new work on something you had no clue about last month, last year, and now you want us to give a whole bunch of money. What's going to happen next year or the year after? What other surprises have you for us?

Somehow or another, you have to build into this whole thing the ability to deal with surprises. I think it's an incredibly important thing, and we don't do much research on it because we can't figure out what to do. You know, I don't know what to do on it, but I do know that we have to be prepared to deal with it. That's where the training aspect comes into play. But I'd like to actually do some science on it.

And the last thing I'd like to ask, and this is because I'm incredibly naive, is, whose job is it anyway, when you have an applicant come to you for something? It's always easier to disprove a hypothesis than to prove it. So, if I'm coming in here with a proposal -- maybe this goes to the issue of, not how safe is it, but is it safe enough?

If I come in here with a proposal, I'm really in a position where I have to prove it. All you have to do at NRC is show that it's broke. I don't have to dissect it. If I can show that it's broken up and it ain't going to work, that's all I have to do. That's an advantage because you don't have to do research on everything. You have to just be able to identify the broken bits and pieces, or the main broken bits and pieces.

An example is Yucca Mountain. Water will not flow in fractures until the surrounding matrix is saturated. The USGS -- I hate to say this -- research program -- even the NRC research program on Apache Leap designed every experiment that the only way you could get water in through that experiment is through the matrix. The fractures never flowed because of capillary forces until the matrix was saturated. If you just poured water on the top of the darned thing, water would go right down that fracture to the bottom of the experiment.

But that's not the way those experiments were designed. They were designed to support the hypothesis and prove it -- but it didn't prove it; it just supported it -- that the fracture will not blow until the matrix is saturated.

A slightly different design would have done something completely different, in which what I think is going on at Yucca Mountain today, and for which we've seen a lot more evidence, both laboratory experiments and otherwise.

Design defining experiments. Go in and test applications for the defining elements. We do that. We talked about it. Sensitivity analysis is one of those aspects used to get at this. But surprisingly, to me, we don't take advantage of this enough, and to do that, to focus on those defining experiments, assumptions, data, measurements and models, you've got to go back and have that foundation in anticipatory research.

That's the bottom message. Thanks.

CHAIRMAN HORNBERGER: Thanks, John -- a

lot to challenge all of us, I think, at the NRC. Questions? Comments?

(No response.)

CHAIRMAN HORNBERGER: John, I'm curious about -- I'll do it in two parts. I'm curious, first of all, about the status of the Vadose Zone roadmap. Is it likely to come out of DOE any time soon, in your opinion?

Number two, how do you think the NRC might be a player in that? It's pretty clear, I think, if you looked at the radionuclide transport research plan, that some of the same questions come up, obviously, for the NRC.

MR. WILSON: The Vadose Zone roadmap -- as matter of fact, I left here yesterday for a half an hour. We had a conference call with the executive committee. What we were addressing was our request to the Academy to review that roadmap. So, that request -- and it's already been gone over with the Academy staff, and it was just to finalize the wording. So, that's going in for review status.

I think the roadmap initially came out of DOE in the previous Administration. And that has something to do with the fact that the pace has slowed up a little bit right now. So, that's going forward for Academy review and I'm not sure what the support at DOE is because that's been evolving under the new Administration.

It's to be an interagency thing, so participation by NRC would be very interesting, even if it's just to attend meetings and suggest where priorities lie.

I think if you looked at the agenda for that research project, I think it would fit very closely into what NRC's interested in. And, although it's labeled the Vadose Zone Project, and it does have that Vadose Zone focus, a lot of the work that goes on has spin-offs, I guess, in other areas. Not just hydrology and contaminant transport, but other areas, as well.

MEMBER HINZE: Is the NRC involved in any way at this point?

MR. WILSON: No. Many other agencies have been involved in putting it together, but nobody from the NRC participated.

MEMBER HINZE: You mentioned predictability, and that seems to be a recurring theme, and the need to avoid surprises, if at all possible, and that's part of the predictability situation.

You mentioned the use of chaos theory and non-linear dynamic systems. Is there research going on, on this in the subsurface realm, and what kind of progress is being made?

MR. WILSON: Not much research is going on. In fact, it's part of the Vadose Zone roadmap, to invest some additional resources in that area, and that's principally because in the Vadose Zone, and particularly in fractured rock, there is reason to believe there is chaotic behavior in the flow system, not to mention transport.

And that's based, among other things, on some unsaturated flow tests done up at INEL by Lawrence Berkley Labs and some INEL people, where there's clear evidence of chaotic behavior in the flow. And you can think of the little pools that have to spill, and then another one, and you're sort of like, come on; cascade on down.

And that's one area. But quite frankly, over most of subsurface hydrology, there's little evidence for chaotic behavior, outside of that kind of thing, so there's a need for studies like that and an emphasis on that, but it's not probably the dominant behavior. There's a reason why chaos theory wasn't discovered by groundwater hydrologists.

(Laughter.)

CHAIRMAN HORNBERGER: John.

MEMBER HINZE: I thought you were going to tell us what it was.

MEMBER GARRICK: One of problems that problem solvers, or people that have to make a decision -- one of the problems they have with researchers is that the researchers never admit that the problem is ever solved.

We've talked some about this business about how much is enough. And there hasn't been a great deal said about the kind of analysis you need to go through to define a research program.

Coming from the world of modeling complex systems -- primarily engineered systems -- and thinking about your comments about surprises, one of the fascinating lessons learned in the application of probabilistic risk assessment to nuclear power plant safety was the pinpointing of some of those surprises.

And also, more important to me was the revelation that there's a lot of things, technical questions, that you don't need to know very much about in order to solve the problem, in order to make a decision, which is what the NRC has to do, is to make decisions.

An example I've used many times is, when we started taking a serious look at the impact of external events on safety and started developing the kind of seismic information that would allow one to transform this into a probabilistic format; and started to get seismic risk curves of specific sites; and were able to calculate the frequency and magnitude of different earthquakes, we began to really put some focus on how important it was to continue to study seismic events. Because we found out in many cases, for example, that we could tolerate seven orders of magnitude of uncertainty in the contribution of seismic events to reactor safety because it didn't make much difference whether it was seven orders or two orders, because we had a pretty good idea of what the end points were of these distribution curves.

What I'm really getting to is, as I listen to the researchers, I don't -- it seems as though there is not a real connection between the research that needs to be done and the guidance that comes from the decision-making world of when you have done enough.

And, you know, if I were an outsider sitting in this room the last two days, I would walk away from the room thinking we are hopelessly in a dilemma with respect to ever making a decision on something like Yucca Mountain because of all the uncertainties. And, of course, I don't believe that's true.

I guess I'm really concerned that we have not done a very good job, in my opinion, of focusing on this issue of defining a research program and defining what we need to do. You said that you need to do research to define the experiments, the assumptions, the data, the measurements and models, and that leads us back to anticipatory research.

It seems to me, we need something up front that's a frame of reference that sort of tests how far we need to go in these areas. One of those frames up front, and I keep coming back to it, that's been enormously beneficial to us in the reactor safety field has been a large-scope risk assessment. And the pay-off has been marvelous. The pay-off has been in the bottom line.

Twenty years ago, there were no U.S. plants on the top ten list in the business of performance of nuclear power plants. Now we dominate it. And while risk assessment hasn't been the primary reason, it's been one of the major reasons because it's focused what the real issues are.

We now know the role of different contributors to risk, when we did not know before. We now know what kind of contributions we're getting from our different mitigation systems. Before we did not know. And I'm sensing here, on Yucca Mountain in particular, and on what NRC is up against, that, you know, the performance assessments are a step in the direction to provide us with this, but I don't see the researchers doing a very good job of making a connection to what I would call a global model here that's going to test the validity here of what is being suggested.

I know that's a tough thing. But in the interest of looking for convergence and in the interest of realizing that the only reason we do research, the only reason we do risk assessment and the only reason we're here is that somebody has to make a decision. And the more I listen to this, it sounds like a totally open and weakly constrained system. There does not seem to be a template here that forces some sort of convergence. And I think the NRC needs that.

MR. WILSON: Well, first of all, I'm fundamentally an engineer and done a lot of consulting. And there's always enough information to make a decision. You know, no matter what it is, if you have to make a decision, you have enough information.

That's not NRC's position, I realize, but I'm simply saying that if you go out into the real world and ask a lieutenant over in Afghanistan who has to make a decision, now, what he's going to do; is he going to wait for more information? The answer's no.

We always have enough information. The question is, what do we do with that information in terms of making a decision.

Researchers live in a whole different world. You're absolutely right. And I don't think you should expect them to necessarily do what you just said. That comes into the response side of, what do you do with the research once you have it kind of thing.

In that last slide I had -- would it be possible to put that up?

I'm not saying it's the answer, exactly, to your question. But what the research does is prepare you to deal with that issue. It is not that you do the research only with that issue in mind; if you don't have anticipatory research -- I know this is not what NRC wants to hear -- that looks at things that may become important or might become important, then when a surprise occurs, you're not ready to deal with it.

MEMBER GARRICK: Yeah, then let me tell you what bothers --

MR. WILSON: Let me just finish one thing.

MEMBER GARRICK: Yeah.

MR. WILSON: That is, you want to be able to go in when something like this comes up and be able to do defining experiments or tests or models that allow you to determine whether what is being proposed makes any sense. I'll throw it out from DOE's point of view -- and I've worked on performance assessment for DOE.

Too many times, it was due to the performance criteria are not met; change the conceptual model. That's one of the reasons BWIP died years ago because that was actually in their -- in the loop of how they were going to make decisions. Change the conceptual model if you don't meet your performance assessment criteria. It doesn't make any sense.

What we have seen in Yucca Mountain and other things is an approach to engineering those systems, as if it were building an airplane. If we can't make it fly with this element, we'll add another element to it.

The idea of rejecting Yucca Mountain has been so low on the priority list at DOE, in my opinion -- and this is speaking out of a large degree of naivete, I admit -- it's been so low in the priority list that everything's designed to make it work.

And the reason we're in trouble on Yucca Mountain is we've spun our wheels a lot trying to make it work without making sure that it was done on a sound scientific and engineering basis. In other words, we didn't allow the possibility that it would fail; therefore, we never really were able to do a good, objective job on it until more recent years. And I think that's where the problem lies.

MEMBER GARRICK: Well, the only analogy I'd give, and then I'll stop, is that when we first started to utilize the reliability technologies in the safety field, of course the first thing everybody thought of was data, and the first thing they all tried to do was go out and define a comprehensive data program.

In most cases, it turned out to be a disaster because the guidance for what constitutes a reasonable data program was also very weakly constrained, and so they tried to do everything. And all the budgets and all the money and all the enthusiasm was consumed in trying to get a database to support the kind of analyses they wanted to do.

When they started doing the analysis first, when they started trying to calculate what it is they were interested in calculating, then they began to bring some focus on what the data requirements really are. And it was a remarkable change that took place. And if you look at where the reliability projects have been most successful, they have been where it was preceded by a very genuine attempt to calculate, on the basis of the information that existed, the parameters that you were interested in.

And that's what I'm sort of missing here, is there does not seem to be anything effective and scientifically based to telegraph what we really need in the way of research.

And while all these brilliant researchers are here, we want to press you into sharing with us your experiences on how this is done because I don't think we'll get anywhere in terms of a defensible and high-quality research program until we somehow, up front, have some sort of mechanism to keep us focused and to keep us from trying to solve all the problems of the world.

MR. WILSON: And I thought that's what we heard from David.

MEMBER GARRICK: Yeah, and they did. Dave made the comment that you kind of do it as you go along.

MR. WILSON: Yeah.

MEMBER GARRICK: As a matter of fact, the "Rethinking Radioactive Waste Management" document that was written many years ago had that theme, as well. And I think that, as a theme, is rational. But we're kind of way down the road here, and I'm just curious as to just, given the circumstances that we face, how do we do this? How do we rationally evolve a research program on the basis of where we are and what should be the guiding light in doing that.

CHAIRMAN HORNBERGER: Okay. We have a bunch of questions. I have Budhi, and then Ray, and then John Kessler.

Budhi. You need a microphone.

MR. SAGAR: Come over there?

CHAIRMAN HORNBERGER: Wherever a microphone -- wherever you can get to a microphone.

MR. SAGAR: Well, I had a real question,

but before I get to that, I want you to comment, John, on your comment on BWIP.

MR. WILSON: Uh-oh -- a BWIP guy.

(Laughter.)

MR. SAGAR: And there is another one here.

(Laughter.)

MR. SAGAR: I frankly don't think the impression you're giving is because of a flow chart where the conceptual model was related to the data. There was a question, of course, asked -- and that's a good question -- is, for making a decision, when do you stop and say I have enough data? And you have to make a flow chart to figure out how you would go in the loop.

But that wasn't related to whether or not you met the compliance criteria, but whether these calculations that you were doing stopped changing, is when you said, okay, I don't need any more data for that particular decision -- site selection, for example. And these criteria change as you move through steps.

In my view, the reason BWIP died was, one, that was a site about which we knew the most, among all the sites, and data is not always good, frankly, because you know too much. You know the weaknesses.

Others, you say, oh well, Yucca Mountain is dry. Right?

MR. WILSON: Yes.

MR. SAGAR: And the second, it was seen as very expensive. So, those were the reasons; not because of the flow chart.

But coming to my real question on predictability, I think it was David Kocher who said we should not use the term "model validation" because it has a connotation, to the public anyway, that it's not quite right.

It's the same thing about predictability. I would say that for natural systems, for predicting for 10,000 or a million years, you can't predict. Period. Even after 25 years of research. So, maybe we shouldn't be using that term, either. Estimate is the best; extrapolation; whatever else you want to call it; projection.

MR. WILSON: But predictability is a measure of prediction. It basically -- if you have poor predictability, then you have what you just said.

SPEAKER: It's a semantic thing like validation.

MR. SAGAR: Yeah. The connotation of predictability is -- despite the plume you are showing, the complicated plume, that's not --

MR. WILSON: I think the point is taken. There's a usage of the term. There's a technical usage of the term "predictability" that has grown out of, in particular, climate science. And it doesn't mean what we are assuming that it means here. So I think we take the point that we have to be careful with the semantics.

But in the technical literature, predictability, particularly in the climate sciences, does not mean that something that you can predict for 10,000 years. In fact, predictability in the climate sciences --

CHAIRMAN HORNBERGER: Means you can't predict.

MR. WILSON: -- indicates the limits. So, for example, chaos theory limits the predictability of weather something like seven to ten days into the future. That's the connotation of predictability in the technical sense. But we take your point.

CHAIRMAN HORNBERGER: Ray.

VICE CHAIRMAN WYMER: Yes, John and I think most of the other speaker have done a really good job of identifying what the really knotty issues are, the real thorny problems that we haven't got a handle on. And the thing that disturbs me is that usually, then, they wind up by saying, well, I don't know how to handle this, and I don't know what to do with this.

MR. WILSON: Oh, I do. Money.

(Laughter.)

VICE CHAIRMAN WYMER: Well, there are some that don't even yield to that.

MR. WILSON: Ask any university professor.

VICE CHAIRMAN WYMER: But that wasn't my whole point.

(Laughter.)

VICE CHAIRMAN WYMER: My point is that, I think if I were to trying to decide what a research program is, for starters, I'd go back through the transcript of this meeting. And every time somebody identified a knotty issue and said "but I don't know", I'd take a good hard look at that and decide whether or not that's something that needs to be known as a basis for research. I think the really tough issues have, in fact, been identified, but we don't know what to do about many of them.

CHAIRMAN HORNBERGER: John Kessler.

MR. KESSLER: I want to get back to the earlier discussion on, you know, how do you define what research needs to be done that's targeted toward making a decision?

I mean, a lot of this seems like it could easily fall into the category of, well, we don't know what we don't know. And therefore, let's just go out and study everything.

To me, your example of Neretniks looking at the -- we call it the Bentonite burping -- is an example of appropriately directed research. This is because SKB was going to take credit for a Bentonite barrier behaving in a certain way. And it's a perfectly legitimate set of research that challenges whether that barrier is really going to perform the way that they said it performed.

To me, that's the way you direct research, is that somebody's going to claim a barrier or claim some performance that's really important. And then, that's the research you go after. I'm not saying all of what --

MR. WILSON: No. That's what this is trying to say. I mean, it may not have come across that way, but it's basically figuring out what's important, and then direct your research at that.

MR. KESSLER: Exactly. All I'm saying is that sometimes it needs to be a little bit more targeted than perhaps a 62-panel member Vadose Zone committee might come up with over 25 years.

(Laughter.)

MR. KESSLER: And I don't mean to --

MR. WILSON: No, the research is 25 years.

(Laughter.)

MR. KESSLER: I don't mean to --

CHAIRMAN HORNBERGER: There goes your half-million from EPRI, John.

(Laughter.)

MR. WILSON: Yeah, might be.

(Laughter.)

MR. KESSLER: All I'm saying is, these are national problems that need to be looked at in a more generic basis; and therefore, things like that are appropriate. But it certainly cuts somebody like me out, and it may cut somebody like NRC out, with a limited budget. It needs something a little bit more defined that is really challenging particular barriers.

I would argue that this is where I asked for, can you separate out this confirmatory from the anticipatory because the kind of work I would assume they'd want to do was going to be first attesting, challenging the claims for barriers.

Now, the other thing is, what do you do about this idea that there's always going to be surprises. Yes, there's always going to be surprises, and you answered your own question because you said, you come up with a more robust system. You'd better not be living on the edge, such that if one of your barriers goes or is somehow degraded, the whole thing falls apart.

I think that DOE is taking that onboard. I would say that the final Part 63 clearly talks about multiple barriers and is looking at things that way. So I'm heartened by all of those approaches. But again, this idea of, what anticipatory research do you do to get at surprises where you don't know what you don't know is tougher.

CHAIRMAN HORNBERGER: My suggestion is that we have segued nicely into talking about how one prioritizes. And in fact, that is the topic of the next item on the list. So, what I suggest we do is go ahead with these presentations. And we will then have ample time to come back and have a totally full discussion, but in light of the new information we're about to get from some of the other people on our distinguished panel.

I think Jack Rosenthal has come in, so

we're going to start with Jack.

(Pause.)

MR. HORNBLOWER: Is this a pop quiz, Jack?

(Laughter.)

MR. ROSENTHAL: I assure you, if we all rate the same issue, we'll come out with different ratings.

I've just got a few slides in the package that's being handed out to you, and then just one separate piece of paper.

Let me start by saying, my name is Jack Rosenthal. I'm the branch chief of the safety margins and systems analysis branch. Most of my activities are in the reactor arena, although I do have some in waste associated with storage. The reason that I'm making the presentation is that I'm leading a little team that's looking at prioritization in research, and I've been involved in the past and have interacted both with the ACRS and also with the NRR and NMSS.

I'm trying to have a reasonably uniform scheme. So let me talk about the prioritization scheme. I think we're both proud of where we are today and recognize that we still want to improve the system. So we're very interested in what you have to say. We're part of the NRC, and the NRC has four performance goals. And we rank our activities against the performance goals.

We typically have somewhere on the order of 120, 130 activities which rank within any one year, and then an activity might have several subparts. So, the NRC's goals are to maintain safety, to be efficient, to reduce unnecessary burden, increase public confidence. And so, we feel those should be our goals, too.

The current scheme does not differentiate between arenas, the reactor materials and waste, across the board. And the advantage of that is that you get a 110 ranking of everything that research has to do. The disadvantage, obviously, is that if you don't have the same matrix or the matrix aren't compatible, then you may get distortions.

But I assure you that not only does Waste say wait a minute, we want a little bit more of the pie, even within the reactor arena, if it's your area that's not being ranked high, you say, well, let's change the prioritization scheme so that my area gets ranked high, too.

There's a real advantage of going across the arenas in terms of research being scrutible. The disadvantage is that the matrix may not quite work.

If you go towards a ranking only within arenas, then you will end up making a somewhat -- Ashok would have to make a subjective decision about so many millions here and so many millions there within a budget, and I think it would be harder to defend. But we recognize that there are merits, pro and con.

CHAIRMAN HORNBERGER: Could you -- before you continue, could you just say something for my benefit on how the 130 items that you have to decide upon arise.

MR. ROSENTHAL: Some come from congressional mandate. For example, you will have an annual report -- I'm sorry -- AOs --

CHAIRMAN HORNBERGER: Abnormal occurrences.

MR. ROSENTHAL: -- Abnormal occurrences to Congress, where you talk about over-exposures of individuals, or particularly severe events at nuclear reactors. That's a congressional mandate.

There are issues that the Commission is interested in, and we've received a Staff Requirements memorandum from them.

There's user needs from NMSS or NRR.

And then, there's issues nominated by the branches and the staff. And it's always this question of the -- ideas are precious things. And so, we've gone out of our way to -- in addition to the top-down approach, to try to permeate ideas from the bottom up, or from other sources. And that would include public workshops and soliciting information from a broad variety of sources.

The scheme is quantitative. That is advantageous because it gives you a degree of scrutibility that you wouldn't have otherwise. And it's a weighted score, which I'll get into.

At one time, we had a scheme with, I think it was nine different metrics and a more formal AHP pair-wise comparison method was used to come up with relative rankings of the metrics. And then we applied that to the hundred and some odd activities.

So this starts out in a more formal analytic hierarchy process methodology and evolves into the scheme that we have today, which it would be, I think, incorrect to say is an AHP process, but rather just a simple multi-attribute decision analysis, I mean, in its simplistic form.

And we, in fact, thought about, maybe we should go back to a more formal analytic hierarchy process, which makes you do a lot of pair-wise comparisons. When you start doing pair-wise comparisons against a hundred-some-odd activities, it becomes onerous.

But, our scheme is a weighted score. And I think that is less subjective than any of the offices within NRC. All NMSS, NRR and RES are all using the NRC's program goals.

The way the weighting works out, new issues end up scoring higher than known issues or previously known issues. That's a consideration that we may want to reconsider.

Vulnerability to terrorism will end up as a new issue and displace all other issues. The work -- I have to use a reactor example. The emerging issues connected with control rod drive mechanism nozzle cracking on vessels -- primary system cracking, in a sense -- ranked higher because we knew less about it than other known issues. And I'll get back to that in just a second.

This scheme does not currently have consideration of sunk cost and there's no -- there's a management overlay that you're going to do to whatever ranking you're going to do.

But we don't consider some costs in the prioritization. And one advantage is that it provides opportunity for new issues to come in and displace older issues. So there's pros and cons about considering the sunk costs.

A real strong advantage of what we're doing going across arenas is that we get a one-to-n ranking of all of our activities. Ashok Thadani can bring that before the Budget Review Boards. And you look at where your dollars are and you know where your cuts are. And as I said, it's scrutible.

I'm going to spend a little bit of time on our rankings now. For a hundred-some-odd activities, you would score each activity. This is the scoring sheet that we do. Then you put it into Access or Excel and you're on your way. You can do sensitivity studies to see if your weighting factors are affecting your decisions, and to what degree. And if your decisions are reasonably -- if your one-to-n ranking is reasonably robust, this changes in the ranking factors, then it gives you some confidence that you're picking the right things to work.

Now, if you look, in general, what you do is, if -- stop for a second. We keep track of which items came from the Commission, which are formal user needs, which come out of technology or operating experience. But we don't use that in the ranking. That's another consideration that we do. And we just plain add up the budget and say how much came from one source or another.

If you look at safety, efficiency, burden and public confidence, it really now goes five, three, one and one. You are ranking safety in the broadest sense at five; efficiency at three; public confidence at one; burden reduction at one. As I've said, we've done some sensitivity studies. I don't think that our one-through-n ranking is overly sensitive to those.

People have raised the issue, including some executives of the Agency, that in the NRC strategic plans, we have four performance goals and we don't rank those at the Agency level. We say we have four goals; why shouldn't they be one, one, one and one.

MR. BONACA: I just had a question. Where do you evaluate your current level of knowledge? What I mean is, you made an example of the CDRM cracking. There is an area where it's safety-significant and it challenges our current level of knowledge. That would be a multiplier that would apply on that item.

We might have a safety-significant issue for which you assess and determine that you have sufficient knowledge to support your criteria upon which licensing actions are based, and also to support the technologies that the licensees have to use to demonstrate that they meet the criteria. In that case, you would have a lower multiplier.

At what level do you use this kind of assessment? I mean, in a certain way, this points to the issue that John Garrick brought up before. I mean, in fact, it does because I could just echo his statement regarding, you know, where do you come from and how do you put a measure of that into this ranking?

MR. ROSENTHAL: Well, we're really doing it at some level of analysis. It's not looking at safety, but rather a delta safety. And you say, well, what do I know? And then, if I knew everything, how would it change that answer? And that really becomes -- addresses the new safety issue and it gets the highest ranking of all things.

Just to give a comparison -- again, from the power reactor arena -- diesel generators are very, very important, but we already know that they're important and we know a level of importance that we want, and we monitor that they're at that level of importance. And we accept the risk associated with a certain level of performance.

So we would not rank that as high as the new issue, where we don't know, and we look at what is the increment in what I'm calling safety, associated with the knowledge of that issue.

MR. BONACA: The reason why I raise that issue, by the way, was because in all the presentations that I saw, which were extremely informative -- and again, I am one of those neophytes who doesn't know much and was really confused at the end of yesterday, knowing what's important and what is less important.

At the end of that, you know, I looked at the wish list on what each presenter saw that they would recommend, and they were somewhat different, which would lead me to believe that each one of them would use a different multiplier in their mind to support one task or the other. And so, how do you set some order unless you make it explicit in the listing so that you can tackle that.

I understand you make a judgment. Who makes a judgment? I mean, how is it made? Is there a basis?

MR. ROSENTHAL: For the judgment about how you're going to give it this --

MR. BONACA: No. I'm saying the judgment

of how much is needed in what area. I mean, how much do we know that supports a decision versus what we need to know in addition, and what items are more important than others, you know, from a perspective of what needs to be done first?

CHAIRMAN HORNBERGER: How do you justify a high number in the safety?

MR. ROSENTHAL: The reactor arena, which is the arena for me to speak about -- it would be those things for which we have a large change in a delta core melt or a large delta risk public health consequence, from your base knowledge level.

CHAIRMAN HORNBERGER: So, if somebody does an analysis --

Let's take our base knowledge level as NUREG 1150, let's say, in which I have some assumed primary system break frequency. And now I get a new emerging issue, like this control rod drive mechanism issue, and I say, that has the potential for significantly changing my small break -- actually, it's large break event frequency. I know that's important. I'm going to give that a high ranking.

Now, that doesn't -- because we're so reactor-oriented and because we have 1150 so imbued in our consciousness, it's reasonably easy to go that way.

When I get into the waste arena, it becomes -- well, clearly harder for me because it's not my arena. And there's, again, the question of the transition. You know, what was built in the reactor world -- how good is it in the waste materials world? That's how we derive in the rankings.

MEMBER LEVENSON: I'm sorry. To follow up on that question because I interpreted Mario's question slightly differently. And that is, suppose in the reactor case, you have two issues that pop up.

You're evaluating -- well, you say you evaluate for the potential impact, but the other half of that is, two new issues pop up, one of which you know quite a bid about. You're still surprised, but you've got a lot of background. The other is really a big surprise; you have background. Your scoring system would not differentiate between those two -- is that correct? Because they both have --

MR. ROSENTHAL: Actually -- wait. But in one case, my level of knowledge is greater than in the other case.

MEMBER LEVENSON: Right.

MR. ROSENTHAL: In that case, for the high-risk one, for which I have knowledge, I would give it a ranking. Actually, what happens is that my safety significance would get multiplied by a .6, while, for the one for which I have less knowledge, my safety significance would get multiplied by a three times a one.

As I say, what we're willing to do is, where we know -- where we know of a safety issues -- and presumptively, we run through the generic issue process and we have made a decision about whether it is a significant safety issue and it's cost-beneficial to change, or we decide to live with that risk, at that point it's a question of maintaining where we are.

MEMBER LEVENSON: So, you're saying, you're not using the words are there for your rating because -- the case I posed, they're both new issues so it's not a matter of maintaining the current level of safety; they're both new issues that challenge safety. There's a different amount of background. Is that the issue?

MR. BONACA: I think so. I can see how you can use this system probably. My concern was, essentially, you're making certain judgments on the inspection of CRDM. You always have axial cracks.

MR. ROSENTHAL: Yes.

MR. BONACA: The judgment was, you know, periodic inspections and monitor that at a certain pace. Or you discover that those, now, result in circumferential cracking. That's really the new issue. So your criteria, your licensing criteria for inspection frequency, wasn't a problem.

Now, that becomes, suddenly, a very different issue because your level of knowledge has changed there. And if it is different, you have different expectations for how the licensees will demonstrate that they can live with that kind of crack. So, I guess you could use steps two and one to measure how far, and determine that you've moved from step two to step one and now you need to --

MR. ROSENTHAL: Yes.

MR. BONACA: -- it doesn't say it, but I think that probably that's how you do it.

MR. ROSENTHAL: Now, things may have high-risk achievement worth but are well-known, and they'll receive a lower score than new stuff. This is biased towards where you don't know -- now, I actually wrote these words on this chart that we use. And of course, you only have so many words to fit on a little chart.

At one time, Bill Morris, former deputy

office director in research, wrote out a long essay on what the factors that we were using at that time would be. And what we found is that by the time you went up to about 20 pages, nobody was reading it, and they were just doing the scoring. The other end of the spectrum is to just have some cryptic words on a piece of paper, and people don't know quite what they mean.

The reality is that there are only seven branches in research. We talk to each other. We propose the rankings. And then we do a scheme where two senior people will sit down and try to normalize what's going on in the office.

MR. BONACA: Just one last statement. On the other hand, on the presentations that we've had to date, since I really cannot make personally a judgment between which item one of the presentation Mr. Nordstrom, and item one of David's presentation is more important, I would rate them both three-one because they seem very safety-significant to me.

The criteria that determines which one should be above should be, do we have enough information about each one of them that says, yeah, but the priority of that issue is less important because I can make an informed decision on the issue today without further work, while in that particular area I'll need to spend more time. That's really where the ranking comes from. That's why I brought it up, and I don't see it here, really, in those terms.

MR. ROSENTHAL: In the reactor area -- as I say, we walk around with these PRA models in our heads, so we're doing a ranking effectively, in terms of the metrics, of man-rem, etc., and I don't know how to make quite that same transition to the materials arena. But it ought to be in terms of dose to the public.

What we do is simply -- you rank this and then you go three times your multiplier plus two times another multiplier plus two times another multiplier, and you add up a score and compare scores. And that's how you come with the one-through-n list.

And let me just spend just a moment or two -- how many licenses involved was clearly a reactor concept? Although, at one time when we were working with -- we had a similar material side, clearly NMSS has thousands of licensees. So there are many, many radiographers, and that would receive a high score, too.

I've got a problem where there's one repository, and I'll get to that, when I talk about how we're considering change.

Yes, sir.

MEMBER LEVENSON: If I look, taking at face value, at the ratings that are there, I would guess that most of the congressionally mandated things rate very, very low. How do you cope with that?

MR. ROSENTHAL: An example is the AO report. You do it. You do it. An example that we had was the abnormal occurrence report to Congress, which dates back to a time when the congressional committees want information, but really predates things like the internet and just the way we disseminate information today.

If it's small cost, you do it, and if it's big cost, then you have to go back to the congressional committee.

CHAIRMAN HORNBERGER: John.

MR. KESSLER: A quick question. I don't see a scope in cost on here. I mean, I know that as a small funder, I'm always messing about with the scope to get it within lines of my cost. And then, you know, if I've still got enough left to make it a really worthwhile endeavor or not, how does all that fit in with your prioritization?

MR. ROSENTHAL: That -- the current scheme, for better or worse, does not consider, as I say, either the cost or the sunk cost on something that you've been studying. You do this one-through-n ranking. You have so many million dollars, and you go down your list and it gets cut at a certain point. Now that may be --

MR. KESSLER: You can reduce the scope. You can reduce scope for things above the line to get some things below the line up.

MR. ROSENTHAL: Sure.

MR. KESSLER: How does that work?

MR. ROSENTHAL: It's not part of our formal prioritization scheme today. I recognize it as a consideration, area that we can improve. I think that it is part of our informal prioritization scheme, in the sense that issues that are put forward are reviewed by management at various levels, and that we can come up with how much resources do you need to reasonably resolve an issue? But it's not currently explicit.

MR. KESSLER: So you're assuming you're going in with the leanest, meanest proposal that you've got?

MR. ROSENTHAL: That's the presumption.

MR. KESSLER: You might want to look at that.

MR. ROSENTHAL: That's the presumption. We even had another discussion, and it goes something like this. Once you put forward a scheme, whatever the scheme is, you can game the scheme. And you just have to accept that and that we're all honest brokers and that there is management review. And then we do an office-level normalization.

The bang-for-the-buck part is not in the current scheme, and maybe it should be. The leverage was easy to put in as something that we thought was an appropriate attribute. At one time, two years ago, we actually didn't have public confidence in our weighting scheme because we simply didn't know how to put in public confidence. And then, last year we did put it in because it is an NRC performance goal.

It's a difficult area, and I've compared some of the scores that we get with ones that NRR and NMSS do, because we do talk to each other. Doing good work does not necessarily increase public confidence. So, we've found in some of the schemes, people will rate things that have high safety significance or high risk significance as also of high public confidence. And we decided that, in research, that just wasn't necessarily the case.

And so, what we're really doing is

ranking, in which -- actually, what Professor Sandman at Princeton would call outrage -- and that is, how involved is the public or how concerned is the public with that activity?

And so, you can have -- that's what we're doing. That's a very difficult one for us to capture, though it gets a ranking of one, at best, compared to the risk or safety significance, which gets a ranking of as much as five if it involves a lot of licensees.

So, that's where we stand today, bumps and warts and what not.

As I say, we should be proud because we've gone from in years past something far more subjective to something that's numeric. It does give you 1 through n ranking, it does go across arenas, and the biggest thing is it's scrutable, that we can go and talk to other offices, or other people, and say, okay, here's how we derived our ranking for better or worse.

MEMBER LEVENSON: Jack, I have one question. You explained how you rate the safety because of the background of -- and PRA which lets you assess relative importance. But now you've also said you're using the same system across all other areas, and what do you use as a device in other areas to substitute for that where you don't have that tool?

MR. ROSENTHAL: Let me, let Bill take a turn at this one.

MR. OTT: It's a difficult question to answer, and it's one of the reasons that in the plan that we tried to explicitly lay out some kind of a rationale for making those decisions. As Jack pointed out, the original 20 page version that Billy Morris used did tend to focus on reactor considerations, and I mean it's very difficult for us to do those kinds of reviews.

I couldn't tell you explicitly right now exactly how it was done last year. We did it two different ways and then they were adjusted. We came out a little bit better last year because we started thinking about things in a safety context for waste, as opposed to a safety context for reactor accidents and that kind of thing, and allowed ourselves to get higher rankings in the safety areas.

I can't give you a good solid answer right now, but one of the things we're working on is getting a better answer to that question.

MR. ROSENTHAL: In Bill's plan he's got a lovely table that proposes a mapping from the reactor to the waste arena, and I think that the reality is that that was used subjectively to come up with parallels.

But this is what we're currently doing and, of course, it's an Agency, many of us come out of the RES side. So now where do we go from here? So we formed this time to reconsider what we would do. In the short term I think we'd like to improve what we have, in the longer term, we would be quite receptive to ideas about going to quite formal, maybe to formal methods such as AHP or other and maybe we have to take better use of the operations research industrial engineer type people.

But in the shorter term we'd like to enhance the system. As I say, not only that there are problems across arenas but even within an arena.

The first place that this would be used is the fiscal 2004 budget cycle, which is approaching, so we have to make some enhancements. There is advantage of doing what we're doing, in the sense you get some stability from year to year. You just can't revise the schemes dramatically and have the confidence of the senior management of the Agency.

What we're doing, as I say scoring against the four goals is consistent with what the other officers, and we really do have to face up to the question about should we rank across arenas, or should we arbitrarily set a certain amount of the funds for waste and then rank within the waste. We haven't reached a decision, and we'd be interested in your views clearly.

We end up doing about 90 percent of our budget, 85, 90 percent of our budget is confirmatory research, or research that's been asked for by the user offices. At the same time, we would like to be out in front; the Chairman has spoken about us being forward looking. Ashok Thadani spoke about that at the beginning of the session, and we all think that we have bright ideas so, of course, we would like to put effort into anticipatory research, on the other hand, confirmatory research as a known customer. So it's just a constant struggle with what's the right balance between those two.

One idea that came forward came from our generic issue process itself which has been longstanding on the books, and that is that we look at ultimately incremental ManRem and you get that by how many sites would be affected, how many years would be affected and what's the incremental individual risk and you multiply it out.

The advantage of doing that is that for reactor areas we typically thing of, well, okay, there's a hundred reactors and they might have 20, 30 years to go, so you end up with 30,000 reactor years and then if instead in the waste arena maybe I put in one repository, but then I put in all the years that it would have to go to give it some sort of equivalence. And so that's a concept that we're thinking of.

Essentially, instead of where on the prior slide I had what's the safety significance of the issue, maybe what we should be saying is what's the risk significance of the issue in a much more formal PLG triad of, you know, what could happen, what's the likelihood, and how bad could it be, to go to a more, as I say, a more formal risk approach would be an idea, a consideration. It works in the reactor arena and it would be an easy transition for us, I don't know how it would work in the materials arena. But that's the concept for consideration that we have now.

And, as I said, because of the expected duration, that itself, or if it's in the materials arena, the number of people, radiographers, etc. might put things on a more even footing.

So we're struggling with that. We don't have an answer.

Other ideas in the thermal hydraulic

arena, in the fuel arena, in the fission products source term arena, we use PIRT, Phenomena Identification and Ranking Tables, which is a very formal expert elicitation process of independent experts where you ask how important is an issue. And you have to define what you mean by important, and you have to define what scenarios you're talking about in order to do a good PIRT. So the thermal hydraulic area.

Well in the fuel area, for example, there were separate PIRTs for Loca and ejected rod and ATLAS, and you come up with somewhat different rankings of what phenemonon are important.

But that's a way of science basing where you'd like to go, based on a very formal expert elicitation.

Ashok keeps talking about, and I was here the first day, there's a fair amount of talk about realism, put a fairly high bonus on realistic evaluation and knowing what the uncertainties are, as distinct from doing bounding analysis and just how shall we consider it.

The sunk cost is really an issue that, as I said, we haven't formally grappled with it but we've got projects that are very near completion sometime that simply won't score high enough to complete. And, you know, well on one hand maybe you should say I did 90 percent of the work, I know 90 percent of the answer, I'm done. Okay? It's good enough. And let it drop. And then the other argument is, no, let's write it up, let's finish it up, let's complete it. So we'd be interested in your ideas.

We would like to be more forward-looking, this concept of prepare the Agency for the future, and we continue to struggle with public confidence. So that's where we are.

CHAIRMAN HORNBERGER: Thanks very much, Jack. We may have asked our questions as we went. Ray?

VICE CHAIRMAN WYMER: One question, yes. I recognize that you're coming mainly out of the reactor arena. The thrust of this meeting, of course, is on waste and I just wanted to see what you really do as opposed to what you'd like to do or what you're presently doing.

Let me ask Bill, did you put the radio isotype transport program, which is a significant program for you, through this process to select it?

MR. OTT: Essentially, the work is divided up into what we call activities. Each activity has related projects associated with it. Each one of those activities was rated according to this rating scheme. The reason I was waffling was that the rating was done by Sheryl, I didn't do it last year, I tested out our methodology and our methodology came up with higher scores but that I don't think was what was actually used.

But anyway those activities, right now I believer there are six activities. One of those activities lumps processes, for instance the hydrology and geochemistry on work and research addressing uncertainties would be classified in that one area, and that activity would receive a rating and then any projects would be funded out of the resources that were allocated to that activity.

VICE CHAIRMAN WYMER: So this is kind of--

MR. ROSENTHAL: Bill, if I might, Bill, why don't you take that example and like a gestalt experiment we won't, even if doesn't match quite up with what was done, and repeat the activity and explain how you would rank that materials --

MR. OTT: What we have here?

MR. ROSENTHAL: Any example you feel comfortable with.

MR. OTT: Well, let's take one that's

probably more understandable to a lot of people, and that's engineering barriers, okay, which is becoming, there's increasing evidence that the barriers are failing. So it's sort of an emerging issue as something that there really is substantial information and we ought to look at it. And it may cause substantial doses in a relatively short time, so you'd probably rate that as high in terms of a new safety challenge. It's an emerging issue, there's increasing evidence of failures and you need to deal with those failures.

Scope of the licensees. Not all licensees use barriers. In fact, if you're looking at decommissioning, I'd probably say less than 50 percent are actually going to have a specific disposal unit so you must have a two for the scope of licensees greater than 10 percent and less than 50 percent.

For realistic decision making, we'd be trying to develop realistic tools or data to confirm safety. We'd be trying to go out there and do that to evaluate this problem, and that's what we're doing. So we'd probably give it a one.

Industry participation or leverage. This is an area where we had, I think matching effort from the National Institute of Standards and Technology so the leveraging here would have been NRC and external, NRC approximately 50 percent support. So you'd have a one with a .6 on it. Not major because we would have been putting in much greater than 25 percent.

Burden reduction contributors. I don't think we necessarily would have been able to say that we'd be saving anybody any money on this one so you'd probably get the lower score, accumulative savings. This doesn't allow you to give you a zero score, I don't think we had a zero score did we? I can't remember.

MR. ROSENTHAL: We forced -- it doesn't matter.

MR. OTT: We would have forced the lower score in this particular instance so it would have gotten a .4 when in reality it maybe should have gotten a zero.

Public confidence contributors. We'd certainly think it's a response to public concern. People don't like it when they see barrier facilities failing, so we probably would have given it a one. That's the considerations that would have gone into ranking the work on engineering barriers.

MR. ROSENTHAL: And so if, I'm just trying to do the arithmetic here, that would have received a score of 6 or 7, something like that, out of a possible 10 and then be compared to other, the 120 other actitivities.

CHAIRMAN HORNBERGER: Okay. John?

MR. KESSLER: Again, it's not directed at this particular example but, you know, the address is the safety concern. Again, I would ask how you decide well how well does it address the safety concern. I mean the work may be dancing around the edges of the issue or it may be really focused on the core of whatever the safety concern is. And somehow you'll need to distinguish perhaps one proposal from the next on that.

It leads me to believe that obviously 20 pages of description as to how to fill these things out is too much because, of course, it's not going to fit for all circumstances. This is too short, but somewhere, somebody should be writing down how one decides how a score was arrived at, and that implies that the people making this score have to be pretty smart and pretty informed all the way across to make these decisions like that.

CHAIRMAN HORNBERGER: Okay.

MEMBER HINZE: Could I ask a quick question. There are many criteria that are used in peer review of research proposals and research efforts. A couple of those are success of the trend of the research. What has been the previous research, what has been the success of that. Another is your ability to really conduct that research. Do you have the facilities, the personnel, are the people proposing this?

Are those in any way glued into this?

MR. ROSENTHAL: Actually, about three years ago we had as a metric, likelihood of success and we removed it and said, and this is a policy issue as much as anything else, let the four performance goals and the safety or risk significance drive it rather than the, and we stopped using likelihood of success. It's a fair issue. It's a fair issue.

CHAIRMAN HORNBERGER: Is this evaluated on an annual basis?

MR. ROSENTHAL: Yes. And all activities go through the re-evaluation, both new and ongoing work.

CHAIRMAN HORNBERGER: Another question. You've talked about dispersing between arenas, what about within arenas? Is there any attempt to maintain the research edge within a discipline, within an arena, and I presume an arena would be like waste.

Where is this coming into this? Is this part of the, you used the term management overlay. Is the management overlay part of this, of the decision process and distributing over disciplines?

MR. KING: This is Tom King. I'm Jack's division director. Yes, the management overlay sometimes when you come through and do a numerical scheme you find out, for example, waste really got short-changed because of, you know, problems with the ranking sytem, and the management overlay will try and correct that. Say, hey, we just can't go down that low in waste, recognizing that there are important issue that somehow aren't captured very well in our prioritization scheme. And management will make adjustments based upon trying to keep a reasonable program underway.

CHAIRMAN HORNBERGER: I have a follow up to Bill's question because he raised it. Bill and I and others are probably much more used to evaluating research proposals. What I gather is that what you're doing is prioritizing research areas. Is that correct? Or are you actually using this to evaluate research proposals?

MR. KING: Jack used the word activities. An activity is defined as the work that needs to be done to resolve an issue, a specific issue. It could involve several projects at different locations, it could be one big project at one location. We don't really take proposals, for example, you know 189s from the national labs, and look at those as the only documentation that's considered. We basically use our operating plan description of what the issue is and the activity is and go from that level of detail.

CHAIRMAN HORNBERGER: Bill?

MR. OTT: One of the things that we're trying to do is to consider what we would need to do to prioritize at the project level.

CHAIRMAN HORNBERGER: Yes.

MR. OTT: We haven't done that yet, we're planning on working on fairly heavily over the next couple of weeks as a matter of fact. It's obviously a concern of ours.

Balance has always been a concern of ours. It's not explicitly considered in there but it's one of these other factors that have to be considered by the management chain. And so far, all the arguments and all the informal discussions have managed to allow us to maintain balance. That's the good thing that we can say.

The other observation I wanted to make,

Jack made a very good point earlier in his presentation that you could call this an AHP informed process as opposed to a strict AHP process. The reasons are many. One of them is that in the AHP ratings process that we developed this from, the final scale out there is supposed to be an objective measurement scale. And it's hard to say that what we have right now is an objective measurement scale, and possibly one of the improvements that we could make in time is to figure out a way of doing an objective measurement scale.

MR. ROSENTHAL: I see the sense of the questions are going from how does research rank relative, I mean a multitude of activities, what's going to get funded and worked on, as distinct from a little bit broader question about how does research work. And in the sense for major activities we tend to write plans, we tend to get well aired and reviewed.

I know in my thermal hydraulic, in my fuel and in my source term work which is my branch's work, we have PIRTs, which are formal expert elicitation processes, which would give the relative rankings in terms of metrics such as risk. And then activities would be formed from those tasks and then activities from that effort, and then I bring that to the budget prioritization process.

So I mean it's a rich nomination that's brought forward.

CHAIRMAN HORNBERGER: No, I understand and I think that I understood that beforehand. I just wanted to point out for the benefit of all involved that when Bill says he and I will look at an NSF proposal and prior track record as a big criterion, you're not at that level. There's no such thing as looking at prior track level for an activity because you haven't gotten down to the people doing it yet.

I just wanted to make that clear. It may not be. I mean at some areas it might be one big proposal and you say, yes, the the National Lab has done a wonderful job and we can take that into consideration.

MR. KING: Yes, that gets down more to who does the work.

CHAIRMAN HORNBERGER: Right.

MR. KING: You know, if the work is important to be done and somebody that's selected to do that work has a poor track record, that's where that comes in.

CHAIRMAN HORNBERGER: Exactly. But it's

separate from the AHP as Jack presented, right?

MR. KING: Yes.

CHAIRMAN HORNBERGER: John Wilson?

MR. WILSON: Where is the memory in this, you know, most research takes some level of continuity and if you're re-evaluating these areas every year, things come, things go, is there any memory built in?

MR. ROSENTHAL: I think maybe you've pointed out a weakness. We surely have the scores from prior years just in access data base. That's easy. But how you derive the score and the discussion back and forth and why something was a .6 rather than a 1, I think lies in our heads.

Now even more, let me point out again, that we don't consider sunk costs. So you may have funded an activity for four years and be 90 percent done and not choose to complete that effort for all your corporate history because there's a new emerging issue that you consider more important.

CHAIRMAN HORNBERGER: That's fine. Okay. Kirk?

MR. NORDSTROM: Just a quick follow up on that. I really appreciate the discussion because it enlightened me a lot about how things work, or maybe don't work sometimes.

But just a comment that I think it would have better facilitated the workshop and it would have helped the ACNW by having some kind of a summary from the NRC on what's been done, how successful it's been and how does this research affect the PA. Something to think about for the future perhaps.

CHAIRMAN HORNBERGER: Good suggestion. Maybe we'll suggest that.

MR. ROSENTHAL: Let me just close by saying we really are at a formative time because we really want to improve what we're doing and so we really are receptive to ideas that you might bring forward.

MR. BONACA: I just have one more simply just looking at this list, you know, there are so many attributes there and to the central question of, you know, how much core level knowledge needs to be improved. I wonder how much that is diluted by having to go through such a long list. I mean and maybe you're forced by the circumstances to address all those issues but then you know that may be a handicap in the way that -- is there any way I guess you can simplify the process.

MR. KING: I think that's a good comment. It's one that we've wrestled with internally. This is too complex. Do we need it to be this complex?

MR. BONACA: Yes, because I mean at some point you lose sight of really where you want to go. And you're worrying about, I went through some little exercise here where I could take a burden reduction issue, which has nothing to do with -- today and drive it down to a very high ranking because maybe it's good for the public and it has burden reduction contributor sure, I mean and everybody wants it. Fine.

And then, on the other side, I can take one item and I could believe them, you know, mulitphase flow in the fractured rock. It's an important issue, and I can drive it down to a lower ranking with respect to the other one. And then I'm left with the question of what do I need to make the decision today as an organization, and that's maybe a critical issue. In fact, the presentation convinced me of that. And yet it's not going to be ranking high because there are so many attributes to this --

MR. KING: I agree. If you look at NRR scheme for ranking, use their user needs, it's much simpler than this. It's still built upon the four performance goals but it in effect ends up binning things high, medium or low. And the ones in in budget space you spend a lot of time on are the ones that come out low, you know, that you're making the decision, is it in or is it out. And why spend a lot of time on the things that you know are high or come out high in the prioritization process because it doesn't matter in the long run.

What really matters is where do you draw the line when you're down into the low category ones, so I think if the committee has views on the complexity, we'd certainly be interested in that.

CHAIRMAN HORNBERGER: John Wilson.

MR. WILSON: Yes, just one more thing. You're doing this for different activities from increments or areas, which is one of the hard parts. How do you just aggregate something into this individual activities?

You could do it in a way that is another form of making the thing incommensurate, the size, shape can be completely different. And you start ranking them and comparing them, it's sort of like if you were doing the same thing with people. From one group we take the individuals and we rank each of them and we'll take the other group and lump them all together and rank them. And oh by the way, they rank higher because they're more productive.

CHAIRMAN HORNBERGER: There are all sorts of difficult issues and perhaps what we should do is take a lunch break for an hour. Actually, I think we can probably reconvene at one, okay, so if we could start at one, that would be good. And people can think over these difficulties over lunch.

(Whereupon, the above-entitled matter went off the record at 12:08 p.m.)

A-F-T-E-R-N-O-O-N S-E-S-S-I-O-N

(1:03 p.m.)

CHAIRMAN HORNBERGER: We will reconvene the meeting for the afternoon session.

Again, we are on the -- now the prioritization of NRC-sponsored research. And we had heard from Jack Rosenthal about the procedure that the Office of Research goes through, and we have several other presentations this afternoon, hopefully to give us some other perspectives.

Again, the ACNW's view is always to figure out what good advice we can give to the Commission that may help the NRC, and it's with that idea that we are listening to everything that's being said here.

So John Kessler has yet a third hat now that he's going to put on to talk to us, as Research Manager I guess.

MR. KESSLER: Right. Okay. Next slide, please.

I would say that any talk about prioritization has to talk about, what are the criteria? I mean, certainly, that was the discussion we had right before lunch is that, how do these decisions -- how should they be made? And perhaps this is a little too strict, that what we heard just right before lunch in terms of filling out this form, but nevertheless it's a nice thing to shoot at and focus our thinking with. And it's a great focusing tool.

So, certainly, the criteria for prioritization must be clearly stated. And some of my inputs, at least recommended inputs, to developing criteria would be sort of -- it's a reiteration of what I see on this same form. The highest value data should be based on risk information, "value" meaning high probability, high consequence, perhaps significant uncertainty would be something that I would say would be high value data. And anything that could go directly at these issues would be something that I would say would certainly be up at the top of the list of criteria.

I've got costs there. We live in a real world; cost has to be a consideration. And what I was trying to point out about looking at scope versus cost, there's a big interplay between these two, so that you do try to look at the biggest bang for the buck you can get.

Another criterion that we could think about is, when is the information needed? Near-term needs require short-term research. And, of course, if you're talking about the Yucca Mountain performance confirmation program, we may have years and years to do this.

And I think it's kind of amusing that when I think of performance confirmation for Yucca Mountain, I think of the long time that we used confirmatory research NRC does for the short-term stuff, which I find -- going to be interesting, and we'll have to keep our definitions straight here as we proceed.

I talk about something called definitiveness. That is, do we have identifiable, measurable parameters that are going to come out of this that actually address these high value issues up here? And are the desired results achieved in a required amount of time?

That's -- there's some of these eight steps that DOE proposed in their performance confirmation program that really are these two issues. And in the workshop we had at EPRI a couple weeks ago, we tried to get into these ideas as to, what are those identifiable and measurable parameters? And can you actually measure them? Will they get you what you want in the timeframe you think you've got?

And the last part is it supports

development of or challenges existing regulations. Again, I struggle myself with, what does this mean? How is it really different than maybe some of what NRC is now thinking of is their confirmatory research? To me, a well thought out confirmatory research program should, in a way, start challenging things and looking forward to issues that are coming down the pike.

And I would bet that they really do, if you take a look at some of those programs, that there's going to be aspects of something that would be more anticipatory within the confirmatory research work.

So, are basic data available? Here's one we heard this morning, and I think that I can almost skip this now. The chemical thermodynamic database was certainly one that was up higher on my list, which is this idea that sparse data contribute to the continuing hot versus cold controversy, that we may have data down at lower temperatures, but because the data is missing up at higher temperatures this controversy about how important these coupled processes are, and how uncertain they are, continues.

And that's why I asked the question I did this morning about, well, should we just get rid of the hot repository idea? Or, rather, should we try to develop perhaps a better chemical thermodynamic database up at the higher temperatures to support that? Maybe that's a naive view.

But again -- and, again, I don't know exactly what NRC's role in all of this is, but I would imagine that with the many research institutions available to do it, there could be some cooperative research being -- going on. I would imagine with this kind of basic research there would be a lot of ways to piggyback on other maybe DOE basic energy sciences kind of research or whatever that would be getting at this.

Well, we're back again to large scale dispersion and fractured, porous media. It will be another one where perhaps the data is or isn't there. And long-term stability of oxide layers on nickel and titanium alloys would be another one where certainly TRB has already been looking at that. DOE has their peer review panel chaired by Joe Payer that's looking at this issue -- all really good examples of things that need to be looked at, and I know that that is part of the Center's program as well.

Basic data research is well suited to co-funding opportunities, that if you're going to -- if the NRC is going to enter into the realm of basic data, there are probably many opportunities out there to co-fund.

I would still argue that PRAs are going to be a main tool for prioritization, maybe not the only thing you look at, but they will be a main tool. NRC Research proposed PRA objectives, again, seem appropriate to me. Conceptualization of the problem and supporting data seems right. Sampling of complex FEPs seems like it's the right thing to be doing.

Flexible database access -- I can certainly understand, having -- trying to mess about a bit with my simple code, trying to get to some of this stuff is an important aspect of trying to get the right questions answered that you even want to ask using the code or something about it.

The importing/exporting reporting of data results, yes, it sounds rather trivial and a boring exercise. But I guess it's not when it comes down to actually running through these issues.

Incorporating additional models -- yes, I think this is important, that the shell for the performance -- the TSPA has to be flexible enough that if somebody wants to look at an alternative conceptual model that maybe has different data needs and somewhat different outputs, that you can actually handle that.