1 That's no easy task to develop a code that runs that
2 way.
3 Analysis of data or intermediate and final
4 results, visualization of intermediate and final
5 results, again are things that do require work.
6 They're legitimate needs, in my opinion. And in terms
7 of this idea of tell the story that you need to tell
8 to prioritize work, to evaluate DOE coming in with a
9 particular barrier, whatever, knowing how to deal with
10 the data and knowing how to show it is important.
11 Next?
12 Other PRA work to consider. Well, what is
13 the scenario? They're trying to group FEPS into
14 particular categories. How do you keep something in?
15 How do you keep something out? I know that NMSS
16 staff, as well as the Center, have been actively
17 looking at what DOE has proposed for Yucca Mountain
18 here.
19 They are generally doing a pretty good
20 job, but, again, I see some things that are ruled out
21 for they call them low consequence reasons, and
22 they're not really low consequence -- low risk
23 reasons. They're being thrown out because they may
24 not have -- they may reduce the overall risk by
25 eliminating them. And to me that's probably not the

1 way to rule them out.

2 And then just defining a scenario. How do
3 you define a class, so that you can actually tell
4 somebody what you expect to happen. What would be the
5 evolution of the repository as you go through the
6 system is not so easy when you look at the
7 preponderance of the data so to speak where things are
8 not necessarily linear.

9 Assigning probabilities to groups of FEPS
10 again is something that needs to be thought through.
11 And my question here: is it valid to compare with
12 reactor failure event initiation trees? John can
13 probably best grapple with that kind of question, at
14 least from people I know in this room.

15 In other words, should we ask, what will
16 cause an unacceptable outcome? Or, rather, how will
17 the system evolve? Which question do we ask might
18 imply different research that we might want to do,
19 depending on whether we're asking this first question
20 or the second question.

21 That got to some of the discussion this
22 morning, that sort of general research might be going
23 after the "how will the system evolve," as opposed to
24 more directed research that may be going on "what will
25 cause an unacceptable outcome."

1 And then, it was also mentioned this
2 morning, comparison with what others get. That would
3 be useful to participate in more round robins,
4 predicting in advance, things like that.
5 For the NRC Research proposed ranking
6 criterion, they have a rating factor here on whether
7 the process proposed for study has a potential to
8 cause an order of magnitude effect on dose estimates.
9 I mean, that sounds good, but we need to carefully
10 consider the method used to determine this. It should
11 be done, first of all, using a realistic PRA or at
12 least as realistic as you can afford and develop.
13 Okay. So maybe what they had in mind here
14 was this is a one-off study. That is, you take a
15 particular component out or you -- you maybe minimize
16 its effect. And then, is that the order of magnitude
17 effect, or is it something else? Maybe you want to
18 look at some sort of potential supporting barrier if
19 another barrier fails, and maybe that's how you would
20 want to look at this in terms of prioritizing.
21 I've got a couple examples here, again,
22 just to be controversial but to make my point here.
23 When we did a one-off study on a particular branch we
24 got dripping through the repository, and we took
25 basically cladding out -- and a lot of people are

1 saying, "Well, gee, you know, we don't take credit for
2 cladding. We don't really want to do any work to
3 support cladding." And they'll say with a one-off
4 study, "We'll see. It doesn't really make much
5 difference." Okay? You know, why should we spend
6 money on it if a one-off study doesn't show us much of
7 an effect.
8 Okay. But let's take a couple more
9 barriers out here. So now we've got no drip shield
10 and no container for this curve, and you can see,
11 well, okay, things are higher. But now, if I take the
12 cladding out, after those two are gone, you can see
13 that the cladding actually is helping you a bit more.
14 Now, how much you want -- make these
15 drastic assumptions. Gosh, I'm taking three barriers
16 completely out. But it's only when you take these
17 other more dominant barriers out do you actually see
18 that cladding might actually get you something. What
19 this suggests to me is while maybe we won't want to
20 spend a lot of dollars on cladding, we might not want
21 to forget about it all together in terms of doing
22 research on it.
23 Another way that we've been looking at is
24 to identify barriers using what I call full
25 neutralization, and I mean full. You start with

everything out, and we successively add barrier after barrier here until we've added them all. We're looking at a bunch of different ways of doing this, but I'll try to explain real quickly what I'm going through here.

I'm not sure the scale shows up very well. But I'll start with what's called a hazard index, and this is for Neptunium-237. And what we've got here in our model is this is 10^{14} up here, and what we're assuming is take all 70,000 metric tons, dissolve it in six-tenths of a cubic meter of water, and the person drinks it in a year. Totally ridiculous, I know.

But the idea is we want to see what all of the components of the systems do to get us down to the doses at the end. Where is it that these things actually get knocked down? So what we do is we say, "Well, there is a series of engineering and natural features that actually only cause some smaller fraction of the repository to be wet." So that knocks it down to 1.25 percent.

Then we'll say that the fuel doesn't really instantly dissolve. It actually takes maybe 3,000 years to dissolve and now we've got this curve here and we've knocked the dose down by that amount.

1 Now we'll throw in solubility limit for
2 Neptunium-237, which actually doesn't change it by
3 much, because right now we're thinking that Neptunium
4 solubility limit is fairly high. Cladding fails over
5 time. You can see that that does actually, in our
6 model, knock the peak dose down and certainly extend
7 out the time of release.
8 Now, if we throw in containers failing
9 over time, you can see that containers failing over
10 time really has a significant effect. But if you look
11 at its effect on peak dose, well, maybe it's not that
12 much. Certainly, the time of the peak is dramatically
13 changed.
14 We throw in drip shields now, and you can
15 see that after the waste packages are on there, the
16 drip shields don't do too much more. But if we change
17 the order and put the drip shields on first, you would
18 have seen a much different result. And we're going to
19 look at that a little bit more.
20 For the next one, we look at waste
21 packages dispersed throughout the repository, which is
22 way down here. It's not all focused at one point
23 where they're all releasing at one point. They're
24 dispersed over I don't know how many acres, which gets
25 you a lot of dilution by the time you get down

1 further. So that's down here.

2 We add in EBS sorption. Neptunium doesn't

3 sorb much on, say, container corrosion products and

4 the like, so that doesn't really knock it down much.

5 But that's only because we added it later.

6 We finally get now -- we're finally adding

7 in some unsaturated zone and saturated zone rock here,

8 and we are down to this curve. So, again, you can see

9 how much it has lowered the peak, and so on. We can

10 run it through all of these, and we can look at

11 different ways.

12 And, again, while it's not physical what

13 I'm doing here, it gives you an idea where the

14 barriers might be, and also which of these you might

15 want to challenge. We're going to also, in a report

16 that's going to come out in the next month or two,

17 reorder these to maybe put all of the natural system

18 barriers first, then the engineered barriers,

19 whatever.

20 But it's just an idea of maybe also

21 helping you look at which barriers are back there that

22 you may not see or may think, you know, are not that

23 important.

24 MEMBER GARRICK: John, was anything about

25 this a surprise to you?

1 MR. KESSLER: One could say something
2 like, "Well, gee. You know, the drip shields not
3 adding much was sort of a surprise." And then, when
4 you think about the lifetime of the drip shields
5 versus the lifetime of the waste packages, you think,
6 well, maybe it's not that so much of a surprise.
7 Cladding was, I'd say, one that most
8 people are just writing it off. And because we added
9 it, remember, before these barriers, we can see that
10 it does something if the other barriers aren't there.
11 That was somewhat of a surprise.
12 I think the other thing that was somewhat
13 of a surprise was the alluvium. We saw very little.
14 By the time we added all of the other barriers, the
15 alluvium was getting us. And that's when we realized
16 that that's because we had -- we were taking more
17 credit for the fracture matrix interaction and the
18 sorption that was going on in the fractured tough.
19 MEMBER GARRICK: I assume these are point
20 estimate, best estimate?
21 MR. KESSLER: Yes. This is sort of a
22 single branch, and, of course, we needed to look at
23 this a bunch of different ways. But it's just
24 illustrative of --
25 MEMBER GARRICK: Now, if you were to

1 superimpose on that, say, the 5th and 95th, would you
2 end up getting on the -- for the curves on the left a
3 much tighter spread than you would with the curves --
4 than the curves on the right? Would you expect that
5 to be -- to happen? In other words, are the
6 uncertainties much greater on the -- for the curves on
7 the right than they are for the curves on the left?
8 MR. KESSLER: I'm not sure. I think it's
9 going to -- I don't know for sure. But I would
10 venture to say that it just has a lot to do with the
11 ranges. And, again, this is only for Neptunium-237.
12 MEMBER GARRICK: Right. Right.
13 MR. KESSLER: I could bore you with tons
14 of other radionuclide-specific ones here --
15 MEMBER GARRICK: Well, the only reason I
16 say --
17 MR. KESSLER: -- going to vary, along with
18 the particular -- how much it would change if we added
19 particular uncertainties.
20 MEMBER GARRICK: The only reason I say
21 that is that the curves on the left, of course, appear
22 much sooner in time and involve much less complex
23 systems, whereas the curves on the right -- it happens
24 much further out in time, and the system is much more
25 complex. And one would think that the uncertainties,

1 therefore, would follow that kind of pattern.

2 MR. KESSLER: You could be very right

3 about that, John. I haven't -- we haven't looked at

4 that that way.

5 MEMBER GARRICK: Okay.

6 MR. KESSLER: But it's certainly something

7 useful to do.

8 MEMBER LEVENSON: John, what's the

9 significance of --

10 CHAIRMAN HORNBERGER: Microphone.

11 MEMBER LEVENSON: What's the significance

12 of these falling so rapidly?

13 MR. KESSLER: We run out of inventory.

14 MEMBER LEVENSON: It's going to zero.

15 What?

16 MR. KESSLER: We run out of inventory.

17 Basically, what this says is that it takes us 3,000

18 years, and so you'll see about 3,000 years of time

19 step here, and everything has dissolved. It has gone

20 by. So that's all we're saying.

21 MEMBER LEVENSON: So this is a dose at a

22 specific point.

23 MR. KESSLER: It's a dose at a specific

24 point. Basically, you're under there drinking the

25 stuff that's coming by you. It's that silly in a

1 sense. It's not physical. Don't go there. Don't be
2 too physical with this. Okay? What we're trying to
3 do is find -- understand how these -- what these
4 barriers contribute. And from a modeling standpoint,
5 we have to do these kinds of silly things in a way to
6 try to tease these out. So --
7 MEMBER GARRICK: Wouldn't the solubility
8 issue be much more important, again, for the curves on
9 the left than the ones on the right?
10 MR. KESSLER: Well, we chose to add the
11 solubility early. And if we had chose to add it
12 later, I think we'd actually see less of an effect,
13 because we'd have these other barriers that would
14 dominate over whatever solubility is contributing.
15 Gosh, I can't remember. I may have one of
16 these later in my talk here, where I throw in -- and
17 I do, and you'll see how much less of an effect it
18 does -- it has when you've got all of the other
19 barriers in there.
20 So, again, this isn't necessarily what you
21 want to do to make a safety case, although I think it
22 has a role. It's this idea is -- what barriers are
23 buried behind another barrier that you might want to
24 think about conducting some sort of level of research
25 on to keep your knowledge going on that.

1 This is sort of an aside, but in terms of
2 -- if we're -- my perspective is coming from long-term
3 performance confirmation, long-term R&D for Yucca
4 Mountain, where I'm thinking of decades, maybe even
5 centuries, of work here. And certainly there are
6 plenty of analogies to site remediation where we're
7 probably talking about similar time periods.
8 Loss of institutions to conduct nuclear
9 research is a concern. I would say it's going to
10 dramatically affect research prioritization if you now
11 don't have certain work or capabilities available to
12 you. For an example, this is one that's hitting home
13 right now as we're actually working with NRC Research
14 to try to define some more of these, looking at fuel
15 that's been in storage for a long time. There's
16 almost no hot cell capability in the U.S. now for full
17 assemblies.
18 The cost of some of that work now is
19 becoming prohibitive. DOE is pulling out some
20 funding. It's going to require, if NRC wants to do
21 this -- and we're certainly talking to them about
22 potentially doing it -- they're going to have to look
23 outside the U.S., perhaps at direct funding, or what
24 we're trying to encourage NRC to think, along with
25 ourselves, is maybe use some of our foreign colleagues

1 to get the work done there. It involves a lot of
2 elaborate agreements, but that may be what will be
3 required in the future as we have loss of some of
4 these capabilities.
5 Transportation for R&D does need to be
6 examined. It's -- this is in terms of transporting
7 fuel to where you needed to get it to do the research,
8 that aspect of transportation. I'm not saying the
9 other part doesn't need to be examined as well, but
10 that's the transportation I'm talking about, for R&D.
11 It takes up a huge amount of our dollars
12 just to get the fuel to the research facility. In
13 fact, it took all the EPRI R&D dollars we had in one
14 program to move a handful of full assemblies to one of
15 the hot cells where DOE then picked up the work and
16 actually conducted the hot cell work. The only way we
17 could get work done like that was to look for multiple
18 users to co-fund.
19 Role for NRC-sponsored TSPA development.
20 Significant resources are required for the care and
21 feeding of NRC's TSPAs. The purpose -- again, I'm
22 being simplistic here to make a point. The purpose is
23 simply to evaluate the applicant's TSPA, you could
24 say. The alternative is ask the applicant to do it,
25 develop models or incorporate NRC models, or whatever,

1 and perform specific sensitivity studies.

2 You know, the example is EPA chose that

3 approach for WIPP. EPA did not develop their own

4 model, but they did make a lot of requests of DOE that

5 they put in certain models or make certain

6 assumptions, and DOE did a lot of that for them.

7 Now, what I'm -- I'm not trying to say

8 that NRC shouldn't be conducting its own -- developing

9 its own model. But to develop a detailed model costs

10 a lot of bucks. And if there could be a better

11 agreement or something between NRC and DOE on using

12 DOE's model with certain incorporations of NRC's

13 model, that might be money better spent in the long

14 run.

15 That's it.

16 CHAIRMAN HORNBERGER: Great. Thanks very

17 much, John. Questions and comments? Dave?

18 MR. KOCHER: You talked a little bit about

19 scenarios and assigning probabilities to scenarios.

20 MR. KESSLER: Right.

21 MR. KOCHER: Could you fill me in on the

22 current view of how probabilities are handled within

23 a regulatory framework where dose is the desired end

24 point?

25 MR. KESSLER: I don't think that dose

1 comes into assigning the probability of the scenario
2 existing, because that's the consequence side. But we
3 -- they do look at a class of scenarios and assign a
4 probability because it probably wouldn't be -- there
5 is this criterion that if you get below a certain
6 probability, then you don't really have to look at
7 that FEP.
8 And so NRC is very appropriately aware of
9 not dividing and conquering, so to speak, and dividing
10 up classes of FEPs into individual ones, so that each
11 individual probability falls below that. Volcanism
12 would be an example. There's intrusive, blah, blah,
13 blah. Classify them together and look at the
14 probabilities together is what they're doing,
15 completely devoid of the potential dose effects from
16 those.
17 MR. KOCHER: So the basic use of
18 probability, then, is to decide what scenarios are in
19 or out, because, you know, dose just doesn't -- I
20 mean, this was a great advantage, if you will, of the
21 Part 191 approach is that probabilities were explicit.
22 So, basically, you have to say it's either
23 credible or it's not credible. And if it's credible,
24 you assume that it has a probability of one. How do
25 you handle a probability other than zero and one in a

1 performance assessment where dose is the end point?

2 MR. KESSLER: Once you --

3 MR. McCARTIN: Well, the doses are

4 weighted by their probabilities of occurrence. It's

5 not a zero or a one. So it is very similar to 191 in

6 that sense.

7 MR. KESSLER: Yes. I mean, it doesn't

8 seem like it's different than 191 in that respect.

9 MR. KOCHER: Does 197 allow that?

10 MR. McCARTIN: Yes, absolutely. Well, I

11 mean, part of the rationale is, remember, our cutoff

12 is that we're going down to events with a probability

13 potentially as low as 10^{-8} per year.

14 To not consider explicitly the probability

15 in the dose estimate would be -- you would have a --

16 you know, potentially a very rare event and holding it

17 to an extremely low probability -- or a low

18 consequence, if it was not probability weighted. It

19 would be 15 millirem or something that had a

20 probability of occurrence of 10^{-8}. It is probability

21 weighted, so it does allow a larger dose for a low

22 probability event.

23 MR. KOCHER: Well, this is why you should

24 regulate based on risk.

25 MR. McCARTIN: That essentially is --

1 MR. KOCHER: A probability weighted dose
2 system is not based on risk, because your health
3 effect end points are different depending on the dose.
4 MR. KESSLER: Well, that's true.
5 CHAIRMAN HORNBERGER: Other questions?
6 Comments? I just have one, John, before I let you sit
7 down. It strikes me that I -- I think that I probably
8 can infer correctly from your presentation that there
9 is a suggestion that NRC actually needs to do research
10 on improvement of PRAs to help them prioritize
11 research in other areas. Is that --
12 MR. KESSLER: That's part of it, yes, is
13 that you need to --
14 CHAIRMAN HORNBERGER: That's not all of
15 it.
16 MR. KESSLER: If you're going to look at
17 data inputs and data outputs, and the way you
18 visualize it, part of what you -- you can ask so many
19 questions as to -- to help you make a decision. And
20 the decisions could be about what research to do based
21 on larger decisions.
22 If you've got a tool that can help you get
23 to those answers, then part of the questions you can
24 ask is, will this research project, if it gets me this
25 kind of data, help in any way that helps me narrow my

uncertainty, for example, on doses. And you can use the TSPA to help you prioritize your research.

CHAIRMAN HORNBERGER: Right. And I could also, then, perhaps connect it to the presentation that we heard right before lunch, in that we heard that the reactor side, the people walk around with PRAs in their head, which is hard for me to envision, but -- but nevertheless, in other words, I think the message was that it's built in in some implicit way in their organization of priorities. And you're saying that that same kind of thing needs to be done for the materials area.

MR. KESSLER: And the Center -- the May 2001 report that we received as part of background material, I think one of the things I liked the best about that report was they talked -- before they'd talk about what they propose to do they say, "Here is the assumptions we're making." You know, we've seen these sensitivities in either our own analyses or DOE analyses. This -- you know, so we are assuming that this is important for these reasons.

I think that's absolutely wonderful. That should be behind anything that's proposed to do --

CHAIRMAN HORNBERGER: Good.

MR. KESSLER: -- in NRC.

1 CHAIRMAN HORNBERGER: Thank you.

2 Okay. Warner?

3 MR. NORTH: I want to start out by noting

4 that Stanford University is my part-time academic

5 affiliation. For about three decades, I have been a

6 practicing consultant where my training, background,

7 and expertise is in analysis of decisions under

8 uncertainty.

9 So I bring a decision analysis

10 perspective. I'm going to take a few minutes to

11 summarize it very briefly, and I'm going to give you

12 some references. Just as I hear about books on

13 geology and try to become familiar with this --

14 (Laughter.)

15 -- I think it would be useful for all this

16 discussion about how we deal with uncertainty at the

17 parameter level, a conceptual model level, etcetera,

18 etcetera. There is a community of people that have

19 been thinking about this for many decades.

20 In fact, I'll argue it goes back at least

21 200 years. I first learned about conceptual models

22 and the problem of uncertainty in models when I was in

23 my early twenties, and I keep getting back into that

24 discussion again and again. There really is

25 methodology to deal with this sort of thing. It just

1 may not have appeared in this particular problem area.

2 So let me start out by talking about

3 decision analysis with respect to what makes a good

4 decision. And my colleagues and I will define that as

5 a good decision is one that is logically consistent

6 with what we know (information), what we can do

7 (alternatives), and what we want, our values or

8 preferences.

9 The key phrase here is logically

10 consistent. In complicated problems, most of us can't

11 carry it in our heads, and we need help such as from

12 mathematical calculations, by hand on a handheld

13 calculator, on a spreadsheet, or maybe a huge computer

14 model, and that simply helps us to be logically

15 consistent in processing the information and relating

16 it to alternatives in the context of a decision and

17 our overall goals, values, objectives, or preferences.

18 So what decision analysis is is a mixture

19 of systems engineering and Bayesian decision theory,

20 which goes back several hundred years and which uses

21 the idea that probability is something that describes

22 information, what we know or don't know, as opposed to

23 it's a measurable state of things.

24 The product of all of this, I will argue,

25 is insight. And I think we've had a lot of discussion

1 on how we need to use performance assessment models in
2 this mode. You don't want just the numbers. You want
3 the insight as to what's important.
4 Now, I certainly have subscribed to that
5 for decades with respect to what decision analysis is
6 good for. The formal calculations are only a means to
7 this end, and you don't want people to take those
8 formal calculations as a formula for making decisions.
9 It's not a paint by the numbers exercise. It's,
10 rather, an exercise of aiding decisionmakers who have
11 the responsibility for making decisions based on
12 currently available information and helping them do
13 that in a way that is more logically consistent as
14 best as we can bring logic to bear.
15 One page on decision analysis. Good
16 undergraduate textbook, "Making Hard Decisions" by Bob
17 Cleman.
18 There is a second book, "The Smart
19 Organization," written by my former boss, Jim
20 Matheson, and his son. These are long-time colleagues
21 at Stanford, and this book focuses on the research and
22 development process at the strategic level. It is
23 published by the Harvard Business School Press, and it
24 basically is, again, at a -- I'll call it MBA level,
25 not a lot of complex mathematics, mainly principles of

1 how to organize the information.

2 Now, I want to turn to my substance. I

3 have three points I want to make. The first is the

4 importance of the long time scales for waste disposal

5 systems. The second is, how do we combine scientific

6 judgment from specialists with valuation by others

7 with I'll call it decision responsibility, in order to

8 carry out setting R&D priorities?

9 I'm going to talk about this in very

10 general terms and then come back on the last point to

11 talking about NRC R&D in the context of R&D by other

12 parties.

13 On my next slide, I am quoting words from

14 page 19 of Matheson and Matheson. And what this is is

15 a set of heuristics for how R&D is often managed in

16 organizations that have very short feedback cycles

17 between making decisions and getting results.

18 I think perhaps what they may have had in

19 mind in writing this section were activities in

20 Silicon Valley, firms such as Intel that make chips.

21 And they're on a very tight schedule to get the next

22 version out, Pentium N+1, as opposed to Pentium N.

23 And they're in a big race with their competitors as to

24 who can do this quickly and without compromise -- a

25 mistake in the chip -- which is very expensive to fix.

1 How do they do it? You can read Andy
2 Grove, as I have done, or a number of the other people
3 who have written about this, and you find they follow
4 principles such as the four that are written down
5 here. Again, the words aren't mine. I'm quoting from
6 a book.
7 One, attend to details and follow through.
8 The devil is in the detail. The key to the
9 improvements is to make sure that one is in control of
10 all of those details and tracking very carefully what
11 happens in process changes. You know, people who
12 write computer programs and iterate them often use the
13 same kind of guidance.
14 Monitor near-term performance. Quality
15 control real counts. So the near term is what
16 matters, and you want near-term actions, and they will
17 lead you to a good result.
18 Ignore uncertainties. May be heresy for
19 a decision analyst. You can learn about it by doing.
20 Try something, see if it works, and you deal with
21 uncertainty either using statistical techniques, or
22 you think about it as obstacles to be overcome. I
23 don't understand that. Let's find out about it, and,
24 if it's not working properly, fix it.
25 And then, don't get bogged down in

1 alternatives. This is a situation where you point and
2 shoot, as somebody said. You're in a fire fight. You
3 don't take the time to aim and to construct elaborate
4 alternatives. If it doesn't work the first time, try
5 again. Contemplating alternatives can lead to
6 unnecessary delay.
7 Now, I won't dispute this in terms of
8 these are good heuristics for an operational decision
9 situation such as the management of nuclear reactors.
10 I will simply contrast it with what they have two
11 pages later, page 21, with respect to long-term
12 feedback situations where you must make decisions
13 without being able to observe results over a period
14 of, for example, many years, decades, maybe centuries
15 or even millennia, which I would argue is the case
16 that we have with respect to disposition of high-level
17 waste.
18 In this situation, we reverse the four
19 points pretty much. We focus on the important issues.
20 The landscape is so vast that we have to figure out
21 what's important and put our effort there. We have to
22 consider the long-term horizon and think about getting
23 you where you want to go in the long term, and taking
24 short-term actions that are consistent with that
25 pathway forward that we want.

1 Accounting for uncertainties becomes
2 extremely important because now you're not getting the
3 data to resolve them as we go along. So maybe the
4 riskiest path is formed by being conservative, going
5 the low-risk route on many individual steps. You have
6 a potential fallacy of composition here.
7 So if you really understand the
8 uncertainty, then you can select the best direction.
9 We have a quote from Francis Bacon which is 400 years
10 old that I often use. "If a man should begin with
11 certainties, he will end up in doubts. But if he
12 starts with doubts, he will end up in certainties."
13 That's the whole idea of the decision analysis
14 approach.
15 And then the last point is, in these kinds
16 of situations where you have to make your choice
17 without resolving what will happen for a long time,
18 you want to be very careful in taking the time to
19 construct alternatives. If you have only one short,
20 you want to aim very carefully.
21 If you have the opportunity for
22 flexibility and correction, perhaps a ways down the
23 path, you want to think very carefully about how
24 you're going to use that opportunity for corrective
25 action later. And the tradeoff between making

1 decisions now based on less information or waiting a
2 while so that you may be able to make a better
3 decision because the information has changed and you
4 know more.
5 I'm leading to a caveat, and the caveat
6 with respect to NRC R&D planning is, "Be very careful
7 about applying processes that you have evolved in
8 operational contexts such as reactor safety management
9 and applying those to the long-term waste management
10 issues."
11 The difference between these paradigms is
12 really quite extreme, short feedback versus long
13 feedback. Maybe we all ought to be in the middle, but
14 we ought to be very careful about taking operational
15 paradigms for a short-term situation and applying them
16 in a long-term situation.
17 Let me go on toward point two. What I've
18 tried to do is to be as simple as I can possibly be
19 and yet useful. If we consider an uncertain result
20 from R&D, we can label that X. The standard Stan
21 Kaplan/John Garrick paradigm says we look at what
22 happens, the probability, and what's it worth.
23 So for much R&D management the focus is on
24 success, and we ask about the probability of success,
25 and we ask about the value of success, if it can be

1 achieved through successful R&D.

2 And if, for example, you are a
3 pharmaceutical firm and you're developing drugs, and
4 you know that the statistics are very few of the
5 things that emerge early in the laboratory as
6 promising actually make it into human clinical use,
7 well, you try lots of bets and you try to manage them
8 very carefully, looking at the probability that you
9 will, in fact, get a drug that is successful in
10 providing beneficial therapy with acceptable side
11 effects. And you look at, what's the market for that
12 if it's successful?

13 And companies manage on that basis. This
14 book has a lot of case studies. Much of the work
15 reported in this book was done by Michael Manky and
16 colleagues. Ray Wymer knows Michael from an Academy
17 committee we were both associated with. And a number
18 of other industries are covered as well, but that's
19 primarily in the context of planning for success in
20 the private sector.

21 We're in a different context. For us,
22 maybe X equals failure or flaw or adverse outcome or
23 unacceptable outcome, in John Kessler's words. And
24 what we're trying to do through R&D is to learn about
25 the Fs. If we want something that's robust, we have

1 to assure ourselves that the probability is low and
2 the consequences are acceptable.
3 So how do we do this? We want to look at
4 the product of the probability of the flaw times the
5 value of the flaw. And I think we can -- at least I
6 will choose to call that risk-informed R&D
7 prioritization.
8 I think it's very useful to bring to this
9 expression the phrase from Tom Pickford that John
10 Garrick has been quoted, "If we want to learn about
11 something, try to calculate it." Yes, this may be
12 very hard to calculate, but it may be a great learning
13 experience to do so, realizing that what we are doing
14 is, should we say, an informed sketch. It is not
15 detailed science. It is, rather, a process of
16 bringing together our information to aid a decision
17 process.
18 And these two terms, I will submit,
19 generally come from two different groups of people.
20 The probability of a flaw often comes from people that
21 are very specialized in their knowledge of particular
22 aspects. I think we've seen examples -- Jane Long,
23 Kurt Nordstrom, in their discussions about the vadose
24 zone and flowthrough fractures, this morning's
25 presentation by John Wilson.

1 I would certainly want to have judgments
2 such as theirs brought to bear on issues of, are we
3 going to be surprised by the amount of water transport
4 through Yucca Mountain looking ahead for the next
5 10,000 or maybe several hundred thousand years? But
6 then there's another issue, the question of the value
7 associated with the flaw. How bad is it? When do you
8 find out that you might have this problem? As the
9 repository is being constructed, or not until after
10 closure?
11 It makes a difference in terms of what
12 corrective actions can be taken, and we need to know,
13 are we concerned about a minor design change, or maybe
14 even living with one less barrier but not really
15 changing anything? Or do we have a showstopper such
16 that the whole system is declared unacceptable and you
17 have to start over?
18 This kind of judgment, I submit, does not
19 come from the scientific experts on something like
20 transport in the vadose zone. This is really where we
21 bring to bear performance assessment as a tool to try
22 to understand all the interactions in this very
23 complicated system, so we can draw a conclusion, how
24 bad is it?
25 In the regulatory context, it may be

1 driven by, have we exceeded some dose regulation
2 because all of a sudden now we're in an area that the
3 law has declared unacceptable. So this is a very
4 simple scheme, and I'm not sure it's totally
5 incompatible with what NRC is now doing.
6 But it seems to me it's a different kind
7 of communication process than what I heard described.
8 It's a matter of back and forth between the leading
9 scientists who have the best knowledge and ability to
10 judge potential flaws or failures, failure modes, and
11 the systems integrators, modelers, etcetera, who are
12 trying to put all we know together and understand how
13 sure are we that the repository or the system is going
14 to perform as we need it to perform.
15 Let me go to an illustrative example on
16 this issue -- thermal loading on Yucca Mountain. In
17 my five years with the Nuclear Waste Technical Review
18 Board, I became familiar with this issue, and now 10
19 years later it's still going on. I'm quoting from
20 their letter of December last year in which they say,
21 "DOE has not yet demonstrated a firm technical basis
22 for its present high temperature base case repository
23 design." Oh, they judge we have a problem here.
24 The issue is overall thermal loading in
25 terms of the amount of heat released in an area. But

1 you get there by two design parameters. One is the
2 spacing of the drifts. The other is how close
3 together you pack the containers in the drift. And
4 then, of course, there is fuel burnup and things like
5 that in there, too.
6 But think of it simply as it's a two-
7 dimensional design. Well, some people worrying about
8 water recondensing and flowing back into the
9 repository have suggested, gee, that's a big problem,
10 we don't understand it very well. And so the
11 conservative reaction, I understand from DOE, is let's
12 make the drift spacing wide, and we'll put the
13 containers really close together. And that I
14 understand is the base case design.
15 Well, I worry about that. And, obviously,
16 TRB does, too. They might judge that the performance
17 -- that the probability of a flaw is high. I won't
18 try to quantify that, but I think it might be useful
19 to do that. And what's the flaw? Well, we can think
20 of possibilities. I'll think of one. It may be
21 naive.
22 Suppose with this heat and over time you
23 loosen some rocks, you have structural problems, and
24 you get rock fall, which might interfere with your
25 ability to retrieve or maybe damage containers or the

1 drip shields. Or there may other problems, such as
2 you're exceeding temperature limits on some of the
3 components in the drift. Things happen that you
4 didn't expect.
5 You're doing something that really hasn't
6 been done before, and we don't have much in the way of
7 long time scale testing to find out. That might be
8 really valuable. What are we going to do if we find
9 we've got a problem? It's pretty hard to change the
10 spacing of the drifts once you've excavated. It's
11 much easier to change the spacing, given you can get
12 back to the containers, which we now have agreed we
13 need to do because of requirements for retrieval.
14 But maybe we ought to consider the merits
15 of the step-wise approach advocated by myself and
16 colleagues in this recent Academy study. And that
17 might suggest that a program of drift-scale testing to
18 understand this complex problem area better, and then
19 stage construction of the repository where we
20 determine what the drift spacing is after we have
21 better understanding might be a better approach than
22 declaring we're going to build the base case design
23 and go ahead and start excavating all of those drifts.
24 So, I now want to turn to my last point,
25 which is the interaction of NRC R&D and other R&D.

1 And I think with the exception that I'm advocating a
2 focus in terms of probability of flaw times value, if
3 there is a flaw, I'm saying very much the same things
4 that John Wilson said earlier today.
5 I think the reality is that NRC has a
6 modest R&D budget, and it has the responsibility as
7 the regulator. So, in my judgment, NRC R&D perhaps
8 has its greatest value in terms of building the
9 scientific capabilities of the staff and the
10 contractors for carrying out its regulatory mission,
11 which is evaluating the applicant's safety case.
12 It has to know enough to be able to do
13 that job well, and it seems to me that should have a
14 very strong influence as NRC decides what research it
15 wants to sponsor. But I think an even bigger issue is
16 that NRC has very strong influence through the
17 regulatory process over the R&D that the applicant,
18 DOE for Yucca Mountain, will carry out. NRC can say,
19 "We want you to do this" as part of performance
20 confirmation or perhaps under another label. And
21 there is very strong pressure for DOE to satisfy that
22 request.
23 There is also the potential for R&D to be
24 carried out by other interested parties, of which EPRI
25 is certainly a primary one. And we also have the

1 Nevada Counties represented in this meeting, and there
2 may be some others as well.
3 So it's the issue of leverage or influence
4 where NRC, in its understanding of R&D priorities, may
5 be able to provide leadership or guidance or
6 requirements or requests that will get additional R&D
7 carried out that may allow us to do a better job of
8 planning for waste management.
9 So I'll reiterate my three points. The
10 time scale is important. I think it's very valuable
11 in prioritizing to foster communication between the
12 scientific experts that may have the best information
13 about where flaws or failures could occur with the
14 I'll call it systems planning safety case experts who
15 may find that total system performance assessment is
16 a very good aid to understanding the safety case.
17 And, finally, we need to consider NRC's
18 role as a shall we say leader or requirement-setter in
19 the R&D process, not just the R&D that NRC itself may
20 wish to carry out.
21 CHAIRMAN HORNBERGER: Thanks very much,
22 Warner. It strikes me that even in your last point
23 where you say that the greatest value for -- of NRC
24 might be in influencing the research done by others,
25 that it would presuppose something that I think John

1 Wilson alluded to, and that is that the NRC would have
2 to have sufficient expertise themselves and with their
3 contractors to be able to know how to provide such
4 influence.
5 MR. NORTH: Yes. Backing up one slide,
6 that's what I mean by my term "scientific
7 capabilities." I think they really have to be in a
8 position where they're highly knowledgeable on the
9 issues, and they are, therefore, able to influence
10 others and manage a process. If the issue is not
11 well-known to them, they are going to have a very
12 great deal of difficulty doing that.
13 MEMBER HINZE: I think a good illustration
14 of just that is what has happened in the last six
15 months in the igneous processes, where the Center and
16 its staff have identified the intersection of the
17 repository by an igneous intrusion, brought in people
18 from the University of Bristol and University of
19 Twente, to look at some of the dynamics of this.
20 And with very simplified assumptions, made
21 a few calculations which has been now leveraged into
22 DOE doing a much more extensive piece of I'm sure what
23 they call just technical work, but it's really
24 research. And I think that's just a good example of
25 how having the right people in the NRC who identified

1 this and could identify the right people to do the
2 research in this case in academia, because the
3 expertise just was not there in NRC.
4 VICE CHAIRMAN WYMER: I think another
5 excellent example of that is in the chemistry area
6 where the last couple of years has seen a great deal
7 more attention paid to chemistry, and a lot of work
8 has been done by DOE under the stimulus of the staff
9 and the Center here.
10 CHAIRMAN HORNBERGER: Other comments or
11 questions?
12 MEMBER HINZE: I'd like to go to your term
13 "capabilities" there and ask you to explain that a
14 little further. Is that knowledge or capabilities?
15 Are you trying to solve problems or to get a general
16 background of scientific expertise?
17 MR. NORTH: I don't mean laboratory
18 capabilities. I think what I really mean is
19 understanding an overview of the field and perhaps,
20 taking up from your example, ability to find the
21 resources they need to go further to understand the
22 issue in more depth.
23 MEMBER HINZE: Okay. A question about
24 this long term/short term. Certainly, high-level
25 waste is a long-term problem. But if I were sitting

1 in the NRC's seat, I would think of that as being
2 extremely short term, because they have to make
3 decisions in the very near term regarding the
4 licensing action. Is there a breakdown here?
5 MR. NORTH: No, I don't think so. I will
6 refer you to this report for a much more lengthy
7 exposition. But, basically, the decision that's
8 coming up soon is taking one step in a process
9 involving many subsequent steps. The decision NRC
10 will make is whether to give a license for going ahead
11 with construction, which may lead to emplacement,
12 another decision, which may lead after many decades to
13 a decision to close.
14 Or possibly the decision will be, as Dave
15 was suggesting, maybe it will be ongoing monitoring as
16 far as we can see, that closure won't happen. I think
17 various people have argued that one of the good
18 aspects of the Yucca Mountain site is you don't have
19 the situation that the Swedes will have of being well
20 below the water table. You may be able to keep it
21 open indefinitely for monitoring at a very acceptable
22 cost.
23 And as my colleagues and I put this report
24 together, we got to thinking, well, what's more
25 secure, especially against threats posed by

1 potentially malevolent humans? Having your high-level
2 waste on the surface in a storage facility or having
3 it deep underground in a mountain where there is at
4 least some degree of dryness, and where access, shall
5 we say, both can be controlled and is relatively easy
6 if you need to go down and look at it or fix
7 something?
8 CHAIRMAN HORNBERGER: Yes. It strikes me
9 that -- at least what I inferred from your slides, the
10 key is short-term feedback versus long-term feedback,
11 not short-term decision versus long-term decision.
12 MR. NORTH: Yes. The word "feedback" was
13 very carefully chosen by Matheson and Matheson. And
14 I think of it as, you know, what's the time schedule
15 between when you make a decision and when you start
16 getting results from that decision? In many R&D
17 situations, you're basically going back into the
18 laboratory to see if something will work.
19 You know, in my previous life as a bench
20 physicist, I got a lot of experience doing that, and
21 it was, oh, it didn't come out as I expected. I'd
22 better go calculate and see if I can figure out what
23 went wrong.
24 MEMBER GARRICK: Yes. One of the messages
25 I sort of aggregate from your presentation, Warner, as

1 well as all of the others, is that maybe the best
2 strategy here for NRC is to be thinking in terms of
3 reaching out more to the centers of excellence in the
4 areas of interest to making decisions about waste
5 management.
6 And when you talk about building the
7 scientific capabilities, that maybe what I'm reading
8 between the lines here is building R&D management
9 expertise. Obviously, that comes best from somebody
10 that's an experienced research and developer, and
11 somebody that knows where the best work is being done.
12 And to be sure, maybe have a cadre of in-
13 house expertise, but in order to assure that these
14 problems get the best possible science, that part of
15 it is probably only going to come from some systematic
16 and deliberate effort along the lines of these kinds
17 of sessions and meetings, but with maybe -- maybe with
18 a little more well-defined products.
19 So I sort of am sensing that it may not be
20 realistic for NRC to -- totally independent of the
21 rest of the world to achieve a world-class research
22 capability. We heard this morning that at the world-
23 class meetings there is not much of a presence of the
24 NRC. So the history doesn't indicate that they've
25 been very successful so far in establishing themselves

1 at that level.

2 So I'm sort of hearing that maybe another
3 paradigm or a different kind of approach here in order
4 to do an end run is possible. I once ran a company
5 and we were very worried that we were falling behind
6 in developing probabilistic, finite element processes
7 in our analyses of certain passive systems that were
8 in the plants we were analyzing.

9 And so we decided we had to do an end run.
10 In the end run we did, we said, "Well, where is the --
11 where are the best experts in the world on this?" And
12 we went to them, and we hired them for a while, and we
13 soon became an engineering company that was as good,
14 if not better, than just about any other engineering
15 company in doing dynamic finite element analysis on
16 passive systems.

17 So am I -- is your message kind of --

18 MR. NORTH: Yes, that's very much
19 consistent with my message. I think many
20 organizations have confronted -- should we try to
21 build the capability in-house, or should we find a
22 place and acquire it, buying it, contracting with
23 those people, or other institutional arrangements.

24 MEMBER GARRICK: Yes. And I think the key
25 -- a key part of this is the in-house cadre of genuine

1 expertise. And that expertise may not necessarily be
2 all disciplinary. It may be primarily R&D management
3 expertise.
4 MR. NORTH: Well, it seems to me that NRC
5 has a natural leadership role in this area because as
6 the regulatory agency it has the responsibility
7 essentially to judge, given the application submitted
8 by the applicant, do we know enough to proceed ahead?
9 MEMBER GARRICK: Yes.
10 MR. NORTH: And, therefore, they have to
11 think long and hard about, how much do we need to
12 know? Details will have to be left for what's in the
13 license application. But the process leading to the
14 license application has been underway for a long time,
15 and there have been a great many meetings back and
16 forth between DOE, the probable applicant for Yucca
17 Mountain, and NRC as the agency that will grant a
18 license to Yucca Mountain, if the site recommendation
19 is positive and the license application is submitted.
20 CHAIRMAN HORNBERGER: Ray, could I just --
21 I just wanted to make a comment following on John's,
22 just so we don't let something up in the air. And
23 that is that we had an example that both Bill gave and
24 Ray gave of absolutely world-class research being
25 conducted by NRC and the Center, and that there are

1 selected areas where these people really are at the
2 forefront. And it's clear -- that doesn't detract
3 from your --
4 MEMBER GARRICK: No.
5 CHAIRMAN HORNBERGER: -- message. What
6 you're saying is they can't be world class in
7 absolutely everything, and, therefore, they have to
8 also know how to manage other people. And I think
9 that's consistent with everything --
10 MEMBER GARRICK: Yes.
11 CHAIRMAN HORNBERGER: -- that's been said.
12 MEMBER GARRICK: Yes.
13 VICE CHAIRMAN WYMER: Yes, I agree with a
14 lot of what you said, John. But you went a little bit
15 farther than I would have gone.
16 MEMBER GARRICK: I usually do.
17 VICE CHAIRMAN WYMER: Yes, that's right.
18 (Laughter.)
19 I would point out, besides the really
20 first-rate chemistry that's been done, in the
21 chemistry area in particular, that I don't think that
22 the -- what I consider to be important areas of
23 natural analogues and of second phase formation would
24 have been pushed nearly as aggressively if it hadn't
25 been for the push from the Center and from the staff

1 here. I really think that they were responsible.

2 MEMBER GARRICK: And I think I can say the

3 same thing about the TPA effort. I've been extremely

4 impressed, given the small size of the group and the

5 resources available, what they have been able to

6 achieve in that arena. And I think it -- and as far

7 as these kinds of systems are concerned, it has put

8 them pretty much at the head of the class.

9 CHAIRMAN HORNBERGER: Wes, did you have a

10 comment or a question?

11 MR. PATRICK: I think a third party saying

12 it, as you have all -- thank you -- have done, is

13 great. I'd only add two things. One, no matter how

14 many people are doing work on behalf of NRC, there is

15 a real limitation in resources that are available.

16 Second, there is a real difference in mission that NRC

17 has taken upon itself to fulfill. Third, we perhaps

18 don't do enough at publicizing as opposed to

19 publishing the results of the work that we do. We

20 take a rather modest approach in that regard.

21 To help address that, at least the

22 immediate situation, I'm committing to send to John a

23 list of our publications, presentations, and hopefully

24 that will assist at least in a local way of making a

25 little better known the quite extensive involvement,

1 not just of the Center but of the NRC staff in these
2 areas of work.
3 They have -- they, the NRC staff, have had
4 a number of opportunities presented, and they've leapt
5 at those opportunities to come down to the center and
6 do -- and this speaks to Warner North's comment on
7 capabilities.
8 Although NRC as an agency does not have
9 the facilities or the capacity to develop its own
10 laboratories and field sites, a number of NRC
11 employees have come to the Center, have gone with us
12 to the field, and as a result of that work they have
13 had the opportunity to be co-authors on some real
14 world-class publications in hydrology, in volcanology,
15 in natural analogues, in chemistry, and so forth.
16 And I think that is something that NRC and
17 we have worked very hard together to do, to bring that
18 capability in the broadest sense of the world
19 available to the staff, so that they're able to defend
20 the positions that they're called upon to take in
21 these meetings with the Department of Energy, who has
22 the full force of multi-billion dollar laboratories
23 available to them.
24 CHAIRMAN HORNBERGER: It is interesting,
25 because, Warner, as you said, you've been involved in

1 this for many years, and you hear some of the same
2 things. And I can remember when former Commissioner
3 Rogers was still Commissioner Rogers and meeting in
4 his office and having a conversation on maintaining
5 research capabilities at the NRC.
6 And with the recognition that it -- it is
7 not -- it's not easy because scientists typically --
8 the best scientists typically want to go where they
9 have fantastic laboratory facilities, etcetera,
10 etcetera. And so I think that it is something to keep
11 in mind, and I -- I, for one, take this as the
12 consistent message.
13 As Kurt Nordstrom said, there are
14 consistent themes, and I think that one of your things
15 -- one of your messages here, that you have to have
16 the scientific capability to even allow you to not
17 only evaluate the applicant's cases but to influence
18 the appropriate research to be done by others with
19 more resources.
20 MR. NORTH: I would very much hope that
21 this is part of at least the informal planning system
22 in the waste area, even if it doesn't get into the AHP
23 process. It seems to me that NRC has so much
24 opportunity for leadership in the high-level waste
25 area that it really ought to work hard to continue to

1 accomplish the successes that have just been
2 illustrated.
3 I think the problem I would see, from my
4 outside point of view, is you have a lot of area to
5 cover. And your resources are modest, and I think
6 you've done extremely well with those resources in
7 providing the coverage that you've accomplished.
8 But as we look forward in the process, I
9 think the challenge is great, and especially picking
10 up on the theme from the performance confirmation
11 workshop at EPRI where I was part of the panel working
12 with John that just produced a summary report, it is
13 very important to start now having a dialogue between
14 NRC and DOE to figure out what further performance
15 confirmation and long-term research we need to think
16 about getting done now, so that it will be done and we
17 don't get locked into silly things like we've decided
18 on one design with drift spacing and we're locked into
19 that.
20 CHAIRMAN HORNBERGER: John? Somebody
21 else?
22 MR. WILSON: Would it be -- I want to make
23 sure I haven't leaped to a conclusion that's
24 unjustified. But would it be fair to say that the
25 idea of stimulation of research by others or influence

1 leveraging research by others using NRC's position,
2 would it be enhanced by spending a certain amount of
3 the funds available for research at NRC doing research
4 by panel?
5 Sort of marrying what the Academy does, we
6 have an issue -- and I think we heard an example of
7 this regarding igneous intrusion. We have an issue.
8 For a relatively modest amount of money, we can get
9 together some of the best people in the world to come
10 to grips with that issue, do something to initiate it,
11 and then, through one kind of influence or another,
12 stimulate work by DOE and others.
13 And, I mean, you could end up with some of
14 the best work in the world on a particular topic
15 through that kind of stimulation.
16 MR. NORTH: The Yucca Mountain proposed
17 repository is going to be a colossally expensive
18 undertaking if we decide to go ahead to do it. It
19 seems to me that gives us an economic context in which
20 the value of better information, to avoid mistakes and
21 having to correct it at great expense later, or
22 finding out you can only put half as much waste in the
23 repository as you thought you were going to put in it,
24 is very high.
25 And the cost of involving more scientists,

1 maybe in a peer review capability as opposed to a
2 large team doing the research, but getting the minds
3 engaged, getting the judgment brought to bear, it
4 would seem to me that this would be very much worth
5 doing.
6 And if for a small amount of incremental
7 funding we can get more of the experts involved in
8 reviewing the plans, making sure that the potential
9 flaws have been identified, and that appropriate
10 research is being done to understand the important
11 processes better than we do now -- I mean, it's the
12 creativity of thinking of that experiment.
13 I think Jane Long's work picking up on the
14 kapitza thin film flow work was terrific. I would
15 hope we can have many more examples like that and the
16 hydrogen bubble and the bentonite. But this involves
17 getting very creative scientists to think about it.
18 Now, my judgment, having been to Sweden
19 and participated in several reviews of their program,
20 is they have just about everybody in the country with
21 competence in the related areas involved in their
22 project in one way or another. They're either part of
23 the research team, or they're part of the regulators,
24 or they're peer reviewers and kibitzers.
25 Now, that's great. I think that would be

1 very valuable in the United States to get the experts
2 in the academic community -- perhaps you were a good
3 example -- to be attending the meetings with some
4 regularity and familiar with the issues, so when
5 somebody asks you, "What do you think about the safety
6 case for Yucca Mountain?" you are well positioned to
7 give a thoughtful reply.
8 And it hopefully will be along the lines
9 that I can give for the Swedish program. Having
10 reviewed their safety case, I ran out of good
11 questions where they hadn't thought through ahead of
12 me. How could this possibly fail? And what's the
13 answer?
14 Now, I didn't think of the hydrogen bubble
15 and the bentonite, and I'm glad somebody else did.
16 But if we go through this process of challenging the
17 thinking with our best people who are best positioned
18 by virtue of their disciplinary backgrounds, it seems
19 to me the program is going to be much better off.
20 What will make the program a lot worse off
21 is if such people are brought in at the point of
22 decision and they say, "No, I don't understand that.
23 I don't trust that. I think this is likely to go
24 wrong," and they haven't included it in their
25 analysis. I think that will be a disaster for the

1 program. I'd like to follow the Swedish model.

2 CHAIRMAN HORNBERGER: Thanks very much,

3 Warner. We're going to move on. Steve Rattien has

4 not yet made it, so he's probably either in San Diego

5 or on his way from --

6 (Laughter.)

7 -- from San Diego. But, Ken, we're going

8 to move on to hear from you.

9 MR. ROGERS: Well, I have to begin with my

10 usual disclaimer of not knowing anything. But I have

11 some thoughts. I thought a great deal about research

12 at NRC over the years, and, as you know and have

13 heard, had the privilege of chairing a panel to look

14 at the long-term aspects and the value of research at

15 NRC as it was taking place in the RES area, not NMSS.

16 And so most of my thoughts were focused on

17 what to do about work in RES and its relationship to

18 the rest of the organization. And so whatever I say

19 will -- is largely coming from that direction.

20 I just heard a number of things here from

21 John, from Warner, that I really very much resonate

22 with. I'm going to refer to our panel's report in a

23 few minutes. But let me, just at the risk of being,

24 you know, simplistic, suggest that the view that I

25 have of NRC's role in the whole general scheme of

1 things as a regulator -- we heard about world class --
2 I think of the NRC as being the umpires in a World
3 Series game.
4 They have to call safe or out. They don't
5 play the game, but they certainly have a big influence
6 on whether it's played safely, properly, according to
7 rules. And they have to be able to know what those
8 rules are. And they're -- just as the umpires in a
9 World Series game have got the whole world on TV
10 watching them, NRC has the whole world watching what
11 it does.
12 So I come to the point -- the view that
13 the whole purpose of research at NRC is to make sure
14 that the regulatory decisions that are carried out by
15 NRC are as soundly based technically and
16 scientifically as they can be, and that the whole
17 purpose of research is to ensure that the quality of
18 the knowledge base at NRC is what it should be for
19 making those decisions. And that's the whole reason
20 for it.
21 Now, then, how do you pick research
22 topics, and what do you do? And so on and so forth.
23 But I think it's very important to try to understand
24 what your basic purpose is. I can tell you that
25 that's not always so easy in organizations, because

1 everybody sees multiple purposes.

2 At the risk of boring you, I was a college
3 president for a while and -- 15 years -- and in
4 putting together a strategic plan for the organization
5 I had a terrific battle with the faculty when I wanted
6 to make sure that the mission statement of the
7 university was as simple as possible and it said that
8 the purpose of this organization is education.
9 Period.

10 Well, what kind of education? That's the
11 next question. Undergraduate? Graduate? Well, if
12 it's undergraduate, you have to have good teaching.
13 If it's graduate, you have to have good research. And
14 so all of these things flow ultimately, but you've
15 decided on what the kernel of that organization is and
16 what its main purpose is, and then don't let that be
17 distorted by how you carry out that purpose.

18 And what I'm concerned about here in
19 setting research priorities is that it's kind of mixed
20 up. It seems to me that if I were looking to set
21 priorities for research projects I would not start
22 with the four program goals of the organization as the
23 basis.

24 They can serve as a screen when you're all
25 done. Then, when you've made your decisions on the

1 kinds of research that you think should be supported
2 and conducted under the auspices of NRC, then you can
3 apply that screen and see whether they get through it
4 all right, but not to start out with those. If you've
5 chosen wisely and carefully, it seems to me they'll
6 get through that screen.
7 But to start out with the four program
8 goals, as fundamental as they are, in setting this
9 prioritization rating factors for research topics at
10 NRC strikes me as the wrong way to go about it, to be
11 perfectly frank.
12 And I can understand the reasons, and I
13 can understand the pressures, and it's so much nicer
14 to be able to hold up this nice rating scheme and say
15 here we are. We do things numerically, and, of
16 course, everybody knows that numbers are really
17 sacred, right? I mean, if you've got numbers, you've
18 got -- you've really got the truth, except the numbers
19 that spew out of wrong computer programs are -- we
20 drown in them.
21 So I'm -- I'd suggest, you know, really
22 thinking -- rethinking this thing through from a
23 different basis on how to prioritize research at NRC.
24 And I thought that the actual topics that were listed
25 in the RES program that I referred to the other day in

1 the report on proposed research program -- I thought
2 they were excellent. I thought the whole thing was
3 thought through very well, but it didn't give a basis
4 for prioritization.
5 And it seems to me that when you come down
6 to the question of prioritization, you've got to
7 address what it is you need to know to make regulatory
8 decisions now, in the near term, or in the longer
9 term. And once you have sort of gotten your ideas
10 together on those, then you can start to look to see
11 what it is that you really need to do to make good
12 regulatory decisions, what's happening out there in
13 the world.
14 You've got to get involved enough with it
15 that you're not just academic in your familiarity, but
16 you're thoroughly familiar. And so a lot of things
17 will flow, once you've got your general direction and
18 general emphasis set correctly, that will be more
19 natural I think, whereas I'm afraid that trying to
20 impose these conditions on picking research leaves out
21 the most important thing and that is, what effect do
22 these research projects have on the ability of NRC
23 staff to make proper safety regulatory decisions.
24 Not what happens out in the industry. NRC
25 has got to be the umpire, not the player. And that

1 doesn't mean that you get to be an umpire without
2 being a player. I mean, you know, we know that if
3 you're going to have that knowledge you have to have
4 had experience in involvement, in carrying out these
5 things. It doesn't just come out of a book.
6 So I think that the problem is one that
7 requires going back to deciding on what has to be done
8 and how can a research project -- done in the way of
9 making regulatory decisions, how can a research
10 project contribute to that?
11 NRC should not be doing the work of the
12 licensees. That's their job. But it has to be in a
13 position to be able to knowledgeably, fairly, and
14 objectively judge the quality of that work, the
15 quality of those presentations. And, again, that has
16 to come from the disciplines of involvement over some
17 period of time in the details. The devil is, indeed,
18 in the details. And if you haven't ever been exposed
19 to those devils, you won't be able to recognize them
20 when they come up.
21 So I -- my suggestion is that somehow the
22 top priority for research should be its contributions
23 to the technical knowledge base of the regulators, not
24 the licensees, not the community at large. That all
25 has to take place. They have to come as petitioners

1 in a sense to NRC for its approval and endorsement of
2 the quality of what they are presenting, and that's a
3 different proposition from actually doing it.
4 The two are not totally disconnected, but
5 they have to be connected. But they're different.
6 And so that's what led me to the thought that I
7 included in my remarks in our report "Role and
8 Direction of Nuclear Regulatory Research," which is
9 available as a NUREG, 1802, now. And if you'd just
10 permit me to read a little bit from that, because I
11 wrote it and I think that it was okay then, it's
12 probably all right now. I haven't changed my mind.
13 "The greatest challenge, and arguably the
14 highest priority of NRC senior management, and of the
15 Office of Regulatory Research, should be to maintain
16 and utilize a cadre of top-notch technical experts in
17 each of the core technical disciplines NRC employs in
18 making regulatory decisions. It's absolutely
19 essential that the technical quality of NRC's
20 regulatory decisions be unassailable."
21 That doesn't mean that nobody can question
22 them. It means that if you question them, you're
23 probably going to fail because NRC has done the job
24 right. These experts should be as broad gauged as
25 possible, but also should possess deep technical

1 expertise on a worldwide basis.

2 The core technical areas and the world-
3 class individuals should be identified through a
4 consultative process that uses experts from both
5 inside and outside of NRC. The resulting cadre of
6 senior technical experts should be seen as NRC's in-
7 house referenced resource for all technical decisions
8 that break new ground.

9 They should be charged with knowing where
10 the deepest expertise in their disciplines resides,
11 where the best work is currently being carried out,
12 and what the latest results of that work are, and with
13 assisting access to them by NRC's regulatory
14 decisionmakers.

15 Together, they would provide an in-house
16 technical quality safety net essential for evaluating
17 potential contractors and the work of existing
18 contractors and for validating all substantially new
19 NRC in-house technical decisions.

20 Now, that was -- that's maybe a little bit
21 far out in the -- in what might actually come about.
22 But I think such people exist and can be attracted to
23 NRC, if you go about it the right way. They are not
24 researchers. They may have been researchers, probably
25 should have been researchers, but they have reached a

1 stage of their lives where they've got a mastery of
2 the field.
3 And you get them here, either on a full-
4 time or a part-time basis, and they form a tightly
5 knit group that acts to validate and question, where
6 necessary -- Ashok Thadani has said, you know,
7 research should probe and poke. Well, those are the
8 probers and pokers, not gadflies.
9 And so I'm just suggesting that in looking
10 at research prioritization that it be looked at from
11 the point of view, as I've said, of strengthening
12 NRC's in-house technical capability.
13 Now, that is not to say that NRC should
14 not reach out, do -- all of the things that were
15 suggested here this afternoon I thought were
16 excellent. Use contacts and the expertise of the best
17 in the world -- should be brought here and called
18 upon.
19 And the purpose of establishing this in-
20 house -- which would be a small group, in my view, of
21 experts in particular disciplines that are necessary
22 for NRC, would be the link to those experts. They
23 would know them. They would be the people that you
24 see when you go to a world-class conference.
25 And NRC has had such people in the past.

1 It may have some right now that, you know, I'm not --
2 I wouldn't rule that out. But I'm saying in the past
3 NRC has had world-class experts who were not
4 necessarily grinding out world-class research papers
5 at the time, but they were seeing that the work that
6 was approved by NRC was world class.
7 And I think that that's the approach which
8 I've come to, over the years, as the way that NRC
9 ought to face its research responsibilities. Not as
10 just contract administrators -- contract
11 administration is important, and it should be done by
12 people who are knowledgeable -- but I'm talking about
13 an in-house group of experts who have the
14 responsibility of being absolutely up to date, to know
15 everything in their discipline that's going on
16 anywhere in the world, and be on a first name,
17 eyeball-to-eyeball basis with the folks that are
18 actually carrying it out.
19 That's my image of what I'd like to see at
20 NRC. But in the meantime, we've got to -- we're faced
21 with the question of how to select from the wide range
22 of interesting things. And we've heard a lot of
23 interesting things that might be looked at by NRC --
24 how to make that selection.
25 And my own feeling is that be very careful

1 that you don't do the work of those who should be
2 doing it for themselves, not because it isn't a good
3 thing to do, but you simply can't afford to do it.
4 With the research budget that NRC has right now, every
5 penny has to be spent in a way that brings in a real
6 return.
7 And that isn't to say that some of those
8 pennies shouldn't be spent in universities. I think
9 the question has to be in choosing research topics,
10 how do they contribute to NRC's ability to make a
11 decision on what is going to be presented to NRC, not
12 to do the work for the licensee themselves. Now, that
13 means a high degree of expertise to do that properly.
14 So those are my comments. I like very
15 much what I've heard here particularly this afternoon.
16 Not in disagreement with any of them in any violent
17 way; although, I don't have some details.
18 CHAIRMAN HORNBERGER: The devil's always
19 in the details, Ken.
20 MR. ROGERS: So that's all I have to leave
21 everybody with.
22 CHAIRMAN HORNBERGER: Thanks very much.
23 Questions or comments?
24 MEMBER HINZE: Let me ask you a question.
25 How do you envision these experts to interface with

1 the prioritization process? What is their real role
2 in this?
3 MR. ROGERS: Well, they would be directly
4 involved, in some way, in commenting on proposed
5 pieces of research or proposed regulations.
6 MEMBER HINZE: Even identifying --
7 MR. ROGERS: Oh, certainly. Oh,
8 certainly. Certainly, yeah. I don't know that it's
9 possible to take that step -- this big step of
10 creating the cadre -- but the words "cadre" have come
11 up time and time again now in the last couple of
12 years, and I think the concept is there-- small,
13 tightly-knit group of really first-class experts that
14 owe their allegiance to NRC. I'd like to see them
15 here full-time, but maybe they don't all have to be
16 full-time. But at any rate, on tap. So in other
17 words, the kinds of folks that make up the ACNW and
18 the ACRS, only here all the time, not 80 percent of
19 the time.
20 CHAIRMAN HORNBERGER: Other questions or
21 comments?
22 I think what I'm going to propose that we
23 do is break for 15 minutes. And then what I'd like to
24 do is reassemble and continue on to our final mission
25 here, which is to have some summary discussion, and

1 that's for everybody.

2 And we've listed four questions here on

3 the last page of the agenda; people can have a look at

4 them. I don't want to restrict it to these questions.

5 These are ones that we thought of well in advance of

6 even meeting here; there are probably others. But

7 what I'd like to do is get everybody's thoughts on

8 what they've heard over the past couple days. What

9 we're going to be face with, the ACNW, is summarizing

10 this and extracting the lessons learned and the common

11 themes, and in particular, what advice that we should

12 be giving back to NRC.

13 And so while we have our assembled

14 experts, I would certainly like to get their views on

15 any synthesis that they may want to give us on what

16 they've learned over the past couple days.

17 So let's take a 15-minute break. We'll

18 reconvene at about 2:55.

19 (Whereupon, the foregoing matter went off

20 the record at 2:40 p.m. and went back on

21 the record at 2:57 p.m.)

22 CHAIRMAN HORNBERGER: Okay. It's five

23 minutes to three. And as I promised, we're

24 reconvening.

25 What I intend to do is I'm going to go

1 around and offer each of our invited people -- in
2 fact, I was going to invite Wes and Budhi, who
3 participated yesterday, to join us as well, or you can
4 stay there if you like-- wherever you're comfortable.
5 But I'm going to go around and ask people if they have
6 summary comments, last-minute comments. I don't want
7 to restrict anyone to the questions that we posed at
8 the end, but, again -- not to put too fine a point on
9 it -- but as John Garrick said, "We would like to milk
10 you guys for all it's worth."
11 So let's see, we'll start up here on my
12 right. Can we start with John Kessler?
13 MR. KESSLER: So you want my two drops?
14 I'm trying to decide whether I want to
15 answer these questions. Are they focused on the right
16 issues within the current budget resources? I don't
17 know. I don't know. I mean, I haven't read enough,
18 need to look at the scope, to know whether that's true
19 or not.
20 The words I've read are generally the
21 right words. Some of the details -- because I know
22 about what the center is doing -- seem to be generally
23 the right program. Again, from the parochial view of
24 Yucca Mountain that seems fine.
25 Is there agreement or disagreement on

1 technical issues that need to be addressed? I think
2 you've heard enough of the same topics coming up and
3 up again that you have a good idea as to what the
4 general issues are.
5 Can Roger's comments about who should be
6 doing it -- this is, again, something that NRC is
7 going to have to deal with. I kept thinking as he was
8 talking about the MOUs we've got with NRC research and
9 EPRI and the others, and thinking, I don't know how
10 this work would get done without it. There's an
11 interest on both sides to get something done. It's
12 certainly something that needs to be thought about,
13 whether the mix is right between NRC contributing to
14 joint programs and industry. But I see feel that, if
15 NRC is going to contribute, doing it collaboratively
16 is a great way to spend a dollar.
17 Where is there disagreement? I think it's
18 the chicken and egg thing that has to do with
19 performance assessment, and conceptual modeling, and
20 the role for what is exploratory research versus
21 confirmatory research. I don't think we got there in
22 this discussion on what should be done, what's the
23 right balance, whatever, on that issue.
24 Central attributes needed to ensure the
25 best process for prioritization of the research work.

1 I think we had some good discussion this afternoon
2 about what those attributes are. Several of us
3 proposed criteria with which to look at them. I don't
4 think I'd want to add anything particular there that
5 hasn't already been said.
6 One of the things that would make sense to
7 look at I think a bit more is, the when do you need to
8 know what you think you need to know for the decision
9 you need to make today-- looking ahead when you need
10 to look ahead.
11 This gets into a lot of the discussion
12 that we had at the performance confirmation workshop.
13 And I was hoping to hear from Jeff Poole, but didn't
14 hear, which was how one works in a performance
15 confirmation program; what is NRC's part of that in
16 terms of doing work or expecting work from DOE such
17 that the work is staged at the time that NRC needs to
18 make a decision. There are the couple decision points
19 we talked about. But in addition, Part 62 requires an
20 update every two years on data. How can all of us,
21 especially NRC probably, avail themselves of using
22 those kinds of opportunities for DOE to present work
23 to make a decision on whether it's okay to keep
24 loading, or whether you want to change loading, or
25 whatever. I think that that has to be an attribute

1 also-- when do you need to have information done that
2 you need, in terms of prioritization as well.
3 I guess that's it. Those are my milk
4 drops.
5 CHAIRMAN HORNBERGER: Good. I will just
6 note that the plane from San Diego and RAND must have
7 arrived. Steve Rattien has joined us.
8 Steve, if it's okay with you, I've started
9 around extracting the last bits from people as to
10 their impressions. And after we go around, perhaps I
11 will then ask you to have your comments. Even though
12 they're not summary comments, we will revisit
13 prioritization and other comments you want to make.
14 MR. RATTIEN: That will be fine. I'll
15 upgrade whatever written material I provided so that
16 if it's of any use, you'll have it. And if what I've
17 written dovetails in with what's been said, I'll
18 comment appropriately.
19 CHAIRMAN HORNBERGER: Excellent.
20 MEMBER GARRICK: Can we push Kessler just
21 a little more?
22 MR. KESSLER: I'm so sorry I put that --
23 MEMBER GARRICK: As you know, our chairman
24 pointed out that the questions may or may not be the
25 guidance for your comments. I guess I'd like to hear

1 you indicate your views on what you've heard here in
2 the last two days in a very brief and concise form, or
3 highlights, or surprises, or what have.
4 MR. KESSLER: Surprises. Again, it could
5 be the presentation, but it's the lack of clarity on
6 the part of the research prioritization
7 decision-makers within NRC. Maybe it wasn't
8 specified -- we got a lot of generic information -- a
9 lot of general comments that sounded right. And a lot
10 of things sound right. I think we all have good
11 intentions. It's just that when the rubber meets the
12 road on some issues, it's awfully hard to keep our
13 eyes on the prize.
14 I thought Ken Rogers' comments at the end
15 were very good. That's where NRC should be going.
16 Now, how does NRC live it when they make decisions?
17 If they're going to be risk-informed, how do they live
18 it? Show me that you're risk-informed. I didn't see
19 that so much. That doesn't mean that they're not. It
20 may just mean I didn't see it or hear it in this
21 meeting or get it in the material that was sent to me.
22 It may exist somewhere else. A couple people that
23 I've talked to have already said, oh yeah, some more
24 of it's in this document. Well, that's fine.
25 I would hope that there would be some

1 better discussion on -- these are the criteria we use,
2 keeping our eyes on the prize in terms of what are our
3 highest priorities, was something I guess I would like
4 to see more. I'm sure everybody would like to see
5 more.
6 And this issue of how one does use
7 existing knowledge, including what the insights one
8 gets from the modeling tools that are available in
9 their current state to help make these decisions and
10 where one uses other information that might come into
11 making a decision, is something that, perhaps, needs
12 some more fine tuning.
13 CHAIRMAN HORNBERGER: John Wilson?
14 MR. WILSON: Yeah. It's going to be hard
15 for me to respond to this list, but I'll try.
16 CHAIRMAN HORNBERGER: No, no. Listen.
17 Dismiss the list. I'm much more interested in your
18 general comments.
19 MR. WILSON: Item 1, I think we've heard
20 a lot about different technical and scientific issues
21 and their importance. The ones that were repeated
22 most are, obviously, more important, at least in the
23 minds of this group. And I'm not going to go over
24 those. You can recall or read through the minutes of
25 the meeting to see them.

1 But a couple of other things came out I do
2 think are worth reviewing. And one of those was
3 something I mentioned briefly, and Ken Rogers spent
4 some time on. And that is, an
5 under-the-table -- maybe it's an
6 over-the-table -- reason for doing research is to
7 prepare staff, to have them active, to identify other
8 people they can consult that need be to understand
9 what the important problems are, to be able to react
10 to surprise with calmness, to deal with it. And I
11 think that's an incredibly important part of a
12 research program. I didn't see it anywhere in the
13 priority approach. And somehow or another that has to
14 be brought out. To me that's the number one reason
15 for doing a research program in this case. If you
16 have a limited amount of money that's got to be the
17 main reason you're doing research, because you're
18 certainly not going to be able to cover everything.
19 The second thing I'd like to mention is an
20 idea that was brought up by a number of people,
21 including Warner North, and something John has talked
22 about, and several people mentioned yesterday, and
23 that is the idea to stimulate or influence others to
24 do interesting work. And to me that is the second
25 reason you have research program. You can't do it

1 all. You have to figure out how can you leverage what
2 you can do best of let everybody else do it, whether
3 it's taking advantage of your position as a regulator
4 to influence people, or taking your advantage as a
5 scientist or engineer to stimulate people to do it
6 through, perhaps, a leadership role. But you've got
7 to be able to make sure that whatever program you have
8 is stimulating, leveraging, influencing others to do
9 the work that needs to be done.
10 Ken Rogers mentioned rather well something
11 that I was interested in. It's not NRC's job to do
12 the work. It's NRC's job to be the umpire and not the
13 player. I think that's a very important point, and
14 that should come out in the research program,
15 particularly under the limited budget constraints.
16 That boils down to, then, the last
17 question on here, which I will tend to answer. And
18 that is, what kind of process would you use for
19 prioritization? The numbers game I don't like because
20 there are too many arbitrary things, even in the very
21 quantitative system. How do you disaggregate all the
22 activities that are going on if the activities you're
23 going to rate? How do you deal with incommensurates?
24 And so on and so forth, comparing short-term projects
25 when dealing with reactors with long-term things like

1 high-level nuclear waste, something Warner brought
2 out, if it's possible for me to see how you're going
3 to put that in one ranking system that's fair and
4 equitable and achieves the objective.
5 I don't see what's wrong with a classical
6 way of doing it and going back to panels. I think Ken
7 mentioned this in his comments. Have a panel that
8 goes through and disaggregates how much money should
9 go in research in one area versus another, and each of
10 those areas have panels that make decisions about how
11 much goes into waste-package research versus vadose
12 zone hydrology and multi-phase flow and fractures; I
13 don't care exactly what.
14 But I think you can get a lot of bang for
15 a small amount of bucks in doing that, a lot of
16 expertise and a lot of forward-thinking. Big
17 deal-- forward thinking. And you don't get that out
18 of that rating system. Besides, the rating system,
19 every body fills out those forms maybe. I'm assuming
20 they fill them out anonymously. But when you get a
21 bunch of people at the table talking about what's
22 important, you discover things are important you
23 didn't recognize on your own. There's a synergism
24 that occurs, and that can be quite important. Those
25 panels can be NRC staffers, contractors, some

1 outsiders, some people from this committee-- enough
2 different actors that you stimulate the conversation;
3 it's not all closed thinking. And to me that's a much
4 better way of prioritizing research and a much better
5 way of recognizing or anticipating the possibility of
6 surprise than just doing some kind of so-called
7 objective ranking system. That's my comment.
8 CHAIRMAN HORNBERGER: Thank you, John.
9 Kirk?
10 MR. NORDSTROM: Well, I second those
11 comments. And I'll just add a few things.
12 Again, I mentioned this consistent set of
13 themes that keeps coming through all the talks, and I
14 hope everyone picks up on that and goes somewhere with
15 it. I think, generally, there's more agreement than
16 there is disagreement on some of the kinds of things
17 that ought to be done. But, as it's been pointed out,
18 we've been sort of floating around a bit amorphously
19 because there wasn't, at least to me in the beginning
20 before I came to this meeting, a knowledge of just
21 what all the NRC's done in research, what the
22 effectiveness of that was, and even their own ideas on
23 where it ought to be headed.
24 Some information on that would be helpful.
25 Maybe that's something that ought to be done very soon

1 here so that it's clear. And I think some things
2 ought to come out of that, in addition to the
3 information that's come out at this meeting.
4 And this goes back to John Garrick's
5 comment about sort of how much research is enough; we
6 need a global framework for this. Absolutely. And
7 that ought to be an important short-term goal, and
8 then something that's not, obviously, a static goal.
9 It's going to be dynamic; it will go through changes
10 and iterations as it should.
11 One of the things that I mentioned in my
12 presentation is, in the line of communication -- and
13 as I think Jack Rosenthal mentioned -- good research
14 is necessary but not sufficient activity to gain
15 public confidence. And a lot of the issues and
16 problems with DOE has involved using words and
17 assurances and guarantees that's something going to be
18 done and everyone's going to be safe from any kind of
19 contamination in the biosphere, when, in fact, that's
20 not exactly what can be done, realistically.
21 And so the communication involves choosing
22 the right words and the right phrases to get across to
23 people exactly what we know, what we don't know. Of
24 those things that we don't know that are highly
25 uncertainly, that are, in fact, important, what we can

1 do about them. And then get on with the job.

2 We need to be very honest in our

3 communications. It's not always been the case with

4 DOE. I think NRC has done a rather good job on this.

5 I might mention that -- well, it's already been

6 said -- NRC has done a natural analog study. And as

7 far as I know, DOE has not done one. They have

8 contributed some money toward some of the

9 international projects, but that all started in

10 Europe, and sort of kicking and screaming, DOE was

11 brought in to contribute something to it.

12 And that brings me to the international

13 aspects of radioactive waste, research and disposal,

14 especially transport. We have a lot to learn I think

15 still of what other countries have learned, especially

16 what's come out of OECD programs and research. And

17 every once in a while I see a synthesis of this, but

18 that should be an ongoing thing. Every few years

19 there ought to be a synthesis, and then just brought

20 out and say, is there something we've missed here that

21 these other people have done? The European countries

22 are doing this all the time with each other, and it's

23 been very helpful. I don't think we've done enough of

24 that.

25 I'll mention something. I don't know

1 exactly how this would be applied. And maybe it's
2 been, again, done, and I just don't know about it.
3 But something like this I think would be helpful both
4 for research and also for a particular site, what
5 don't we know and we need to know.
6 We did this exercise at Rocky Flats when
7 we started discussing, again; what kind of research
8 needs to be done; what kind of issues need to be
9 discussed; what's most important, we only have a
10 little bit of money?
11 We put together a flow chart, a one-page
12 flowchart, that you could call a flux reservoir chart.
13 We said, what are the main contaminating
14 radionuclides, where do they reside, what are the
15 pathways to the environment? And we considered every
16 conceivable pathway that there was-- through the air;
17 and there were fires years ago, and that transported
18 some stuff; through the water; through the soils and
19 erosion, for the whole site.
20 And that was a neat iterative process. We
21 had to go through several versions before everyone
22 agreed that we got it. And what we ended up with were
23 box and arrows with all the pathways. But each line
24 was given a different symbol. There would be a heavy
25 solid line, and a light solid line, and a dash line,

1 and a dotted line. And the weaker the line, the less
2 important that pathway was, based on all the knowledge
3 we had. And then we designate lines where we knew
4 there were important, but there was some missing
5 information. And say, hey, that's it. We've got to
6 go after it.
7 I think something like that could be very
8 useful in research as well as site characterization
9 and executing a research and decommissioning closure
10 and so forth. We also had several pages of
11 explanatory text that referred to those lines and
12 those boxes to give a better description of what they
13 mean.
14 I very much like Ken Rogers idea of
15 improving or getting a cadre of experts. That's a
16 very clear path that would, I think, help in the area
17 of research at NRC. And I think there has been
18 something like that, and it's worked rather well, and
19 sounds like it needs to be beefed up or enhanced.
20 The last thing I'd like to do is just read
21 a little bit from a letter that is public information.
22 It was sent by the director of the USGS to Robert Card
23 in response to Card's request for the opinion of a
24 survey on several other agencies about the current
25 status of radioactive waste disposal because it also

1 sort of brings together some of the discussions and
2 thoughts that we've had.
3 "As the final design of the repository is
4 prepared, the USGS strongly supports the inclusion of
5 three design considerations. First, maintaining the
6 surrounding rocket at a temperature less than boiling
7 at all times will minimize potentially negative
8 effects of the repository and the site's natural
9 attributes, and, therefore, lower uncertainty in its
10 predictive performance."
11 My own footnote to that is simply that the
12 whole rest of the world has done that. What happened
13 to us?
14 This is quoting again, second, "Force to
15 natural ventilation should be used to improve
16 repository performance by lowering temperature and
17 removing substantial amounts of moisture from the
18 mountain."
19 Ant third, "A period of retrievability and
20 monitoring preserves the options of future generations
21 to make alternative disposal choices."
22 From an earth science perspective, the
23 survey leads pretty strongly in these three viewpoints
24 of design considerations. And I think those ought to
25 be kept and focused as we look out for the future

1 research.

2 And then, finally, again, this just

3 confirms or corroborates what we've been talking about

4 how we should have a phased implementation.

5 "Recognizing that uncertainty in the future

6 performance of the repository remains, the USGS

7 endorses a step-wise, decision-making process, and

8 phased implementation of the repository program. This

9 approach allows for future decision-makers to select

10 alternative options if necessary, based upon

11 additional information, different societal needs or

12 changing priorities." Thank you.

13 CHAIRMAN HORNBERGER: Thanks, Kirk.

14 Ken, anything to add?

15 MR. ROGERS: Well, the only thing I'd add

16 is just sort of a disclaimer. I really didn't focus

17 at all on the Yucca Mountain issue. And I didn't say

18 anything about the center of which my contacts in the

19 past have always led me to believe is of the highest

20 quality and really good work. So the fact that I

21 didn't get into that at all was just idiosyncratic on

22 my part.

23 CHAIRMAN HORNBERGER: Warner?

24 MR. NORTH: I'll add a few thoughts here,

25 and then I'd like to read a paragraph as well.

1 I really like the idea as a methodologist
2 of getting away from methodology in the abstract as a
3 basis for selecting R&D and moving in the direction of
4 seeing the whole exercise as communication among the
5 appropriate people with the expertise to support this
6 decision. The methodology ought to be an aid. It
7 shouldn't be a numerical system that drives the
8 process.
9 So I think what we really need to move
10 toward is a system of involving the right experts,
11 which we could call panels, and assisting them in
12 understanding how their judgment about the details of
13 the science and what needs to be done interacts with
14 the needs of the program, and in particular, the
15 regulatory mission of NRC.
16 I'm not sure I can sit here and comment
17 more intelligently without seeing more about how the
18 process is coming along. I think it would be very
19 useful, for example, to go through a series -- I will
20 use key technical issues, which is I think Yucca
21 Mountain speak, I'm not sure whether that relates to
22 other areas in the waste part of NRC's domain -- but
23 essentially a systematic covering of critical-issue
24 areas in which the questions are addressed-- which
25 research is going on by the licensee applicant

1 community; what research is currently going on within
2 NRC? How do capabilities stack up against the
3 perception of NRC's regulatory support needs for the
4 future; and, therefore, where does something need to
5 be done either within NRC or by NRC working with the
6 other interested parties to fill a need that is not
7 now being filled?
8 I think it would be really useful for the
9 ACNW in future meetings to go through a review of this
10 kind, especially when I look at Jack Rosenthal's
11 presentation and see this line about enhancements
12 intended for the FY 2004 budget cycle. I believe
13 that's a ways away. It would seem like a very useful
14 time to figure out what enhancements will make the
15 most sense and develop the process for doing this.
16 Now, Yucca Mountain may be a bit of an
17 outlier because big decisions are coming up in the
18 system, we think, and yet there are other steps in the
19 process further down the line. Many of us who are
20 close to Yucca Mountain feel quite passionately that
21 now is a good time to be thinking past the
22 site-recommendation, license-application process
23 toward what should we be trying to learn to subsequent
24 decisions, such as emplacement of fuel, such as
25 details on the design, which maybe ought to be changed

1 as we learn more, and then finally the big decision
2 coming up far in the future, do we know enough to
3 close it.
4 I think we should not view Yucca Mountain
5 as a construction project. And I orate a lot as a
6 member of the Nuclear Waste Technical Review Board,
7 and then in the next five years as a member of the
8 Board on Radioactive Waste Management, the deal-- we
9 really thought of the Yucca Mountain repository as a
10 big construction projects. All it has to do is jump
11 over the right hurdles and through the right hoops to
12 satisfy NRC, and then go up and build the thing. I
13 think that's absolutely the wrong way to look at it.
14 And I want to close by quoting from the
15 paragraph of the findings and conclusions chapter from
16 a disposition of high-level waste and spent nuclear
17 fuel. All of this material is in italics.
18 The heading is, "A Step-Wise Process as
19 Appropriate for Decision-Making under Technical and
20 Societal Uncertainty." I might reiterate, this report
21 was written by citizens of seven different countries,
22 representing many nuclear waste programs, not just the
23 USA. Text is as follows:
24 "Some, but not all, of today's
25 uncertainties in predicting the future long-term

1 behavior of a repository system can be reduced or
2 eliminated by further research and development.
3 However, for a very long time 'proof of safety' in the
4 usual sense of these words will never be possible.
5 This is also the case for many other technologies
6 employed by today's civilization. For waste
7 management, the long time scales give an added
8 dimension, especially since the actions of future
9 societies, particularly those in the distant future,
10 cannot be predicted. Nevertheless, both are
11 fundamental knowledge, and our ways of dealing with
12 uncertainty are advancing and will continue to do so
13 during the course of a repository development program,
14 which can less for many decades. A step-wise decision
15 process can utilize this evolving knowledge to make
16 sound decisions about repository siting, design and
17 operation."
18 CHAIRMAN HORNBERGER: Thank you, Warner.
19 David?
20 MR. KOCHER: John, I wanted to go back to
21 the comments you made before lunch. If I understood
22 them right, you felt a little uneasy about what kind
23 of framework do we have, what kind of basis do we have
24 to make decisions on prioritizing research? Is that
25 sort of the idea or was it something else?

1 MEMBER GARRICK: Take off on that for now.
2 Yeah, go ahead.
3 MR. KOCHER: Is that the right idea?
4 MEMBER GARRICK: Sure.
5 MR. KOCHER: And you used development of
6 reactors as a case in point. Because you said
7 something like, we had analyses of reactor performance
8 that we could use to guide.
9 MEMBER GARRICK: Yeah. And I thought that
10 was timely in that a little later, Rosenthal said,
11 "Behind all of this, we had a bunch of PRAs in our
12 head." So that was the precursor, framework, that was
13 the basis for letting out their particular
14 prioritization scheme. And that was the point I was
15 making-- what comes before.
16 MR. KOCHER: Well, I would suggest,
17 probably naively, that we have done exactly the same
18 thing in the waste business; that we started with
19 generic assessments of hypothetical waste-disposal
20 systems, where we identified what we believe would be
21 the key elements in isolating and containing waste for
22 a necessary and sufficient period of time.
23 And then the big leap was to go from a
24 generic situation to putting a real disposal facility
25 in an actual site. And I think this is kind of where

1 waste departs somewhat from reactors. I am no kind of
2 expert in reactors, so I may be totally wrong here.
3 There certainly are site-specific issues
4 about reactor siting and design, like tornadoes and
5 earthquakes and floods and things like this. But by
6 and large, I think of a reactor as an engineered
7 system that will function more or less the same
8 wherever you put it. I'm sure that's naive, but
9 that's the way I think of it.
10 But waste disposal is a different breed of
11 cat entirely. When the rubber hits the road, the
12 issues are site-specific, but the framework for
13 deciding what you have to do is the analysis of the
14 performance of a system at that site. I think that's
15 sort of the way you said that the reactor business
16 developed as well, is by analyzing system behavior.
17 And I think we really do the same thing. And the
18 issues are site-specific.
19 For example, vadose zone flow and
20 transport has never been a major issue for low-level
21 waste disposal east of the Mississippi because we just
22 have decided that we're not going to take much credit
23 for it. There's too much pain for too little gain to
24 try to do something about that. But Yucca Mountain is
25 clearly different. So site-specific issues of what's

1 important in research are much more important in the
2 waste business than in the react or business. But I
3 think, in general, we start with the same framework.
4 And that's some kind of assessment of what we think
5 this thing's going to do and what's important to
6 overall safety.
7 CHAIRMAN HORNBERGER: Thanks, Dave.
8 Budhi?
9 MR. SAGAR: A couple of comments. I think
10 there are two givens that are true all the time. One
11 is that everybody has limited resources, not just NRC.
12 NRC's research has 2 million -- I know that DOE's
13 basic design -- research -- has 500 million a year.
14 That's because research is endless. It's infinite.
15 There's no point, as far as I remember in my 30-35
16 years of professional life, and I don't think it will
17 be true in the future that people will say this is all
18 done, I'm done, this is it.
19 That will be the case on any topic.
20 Anybody that's written a Ph.D. thesis knows that the
21 last chapter -- you solve one-half of a problem, and
22 the last chapter has 500 problems defined for future.
23 In other words, you don't get your degree. And if you
24 combine all those 500 of the previous theses, only
25 one-tenth or 1 percent have been tackled or even

1 touched. So the search will never end.

2 Therefore, prioritization is a necessity,
3 whether you do it numerically or you don't do it
4 numerically; do it subjectively, do it objectively;
5 whether you build your models in your mind, PRA or
6 otherwise, you do it, one way or the other. We have
7 to live to whatever the budget is. And you decide on
8 the scope of work you're going to do based on
9 something.

10 In the center, for example -- even though
11 we don't do "research" there. And I appreciate CNWR's
12 comments and John's comments here that the work we do
13 is good. And some of this is even published, and we
14 go to meetings and all that. It's not still research.

15 My point is going back to Ken Rogers'
16 idea, that the work you do has to somehow impinge on
17 the regulatory decision you're going to make. Well,
18 the regulatory decisions -- the ones that you're
19 looking at -- change from year to year to year. There
20 was feasibility analysis, there was EIS, then there is
21 the site sufficiency, then there was BLA, then there
22 will be performance confirmation. It's not a static
23 system.

24 We do resource allocation. We have never
25 called it prioritization; we call it resource

1 allocation. We are given budget, and you have to
2 decide what you shall do in that particular year. We
3 sit down, mostly to explore elicitation. We actually
4 go low, medium, high; we don't give all the numbers.
5 But in the end we average out, and then the managers
6 sit down and say, well, does this make sense or should
7 we adjust it a little bit. But you still have to say
8 500K goes here, 1 million goes here. And there's
9 always objections from people whose budget gets cut.
10 And they will come and explain to you why theirs is
11 the most important thing. "You're not considering
12 risk information here. Look." Okay. Well, in the
13 end you have to settle down and say this is what
14 you're going to do.
15 But another important thing, at least at
16 the center anyway, is that we have to be extremely
17 flexible. We can do the planning at the beginning of
18 the year. Two months later it's all changed because
19 something else came from the area, something else
20 happened. In that sense, whether it's research or
21 not, I don't know.
22 When we had a program with the Office of
23 Research we had at least a three-year project plan
24 developed. At least we knew that something will
25 continue for three years. So we could set up an

1 experiment, we could go out to the field, and we could
2 say, well, this is what we anticipate to do at the end
3 of three years. Well, that's not true anymore after
4 the budget cut because the image here is things have
5 to be done first. That's the highest priority. So
6 sometimes the priorities get forced on you.
7 I was hoping that what we were discussing
8 here is not the kind of work, really, what we are
9 doing, but the research where you did, indeed, have a
10 time horizon of three to five to seven years, and you
11 could sit down in peace and say, look, let me define
12 how I will do this, what experiment I will do, what
13 analysis I will do. And this would take you from
14 point A to point B.
15 Should it impact our regulatory decisions?
16 Eventually it will. But it may not. There should be
17 some risk. If I call something research, there has to
18 be some risk in it. It may succeed; it may not
19 succeed. Because if I already know everything, well,
20 then what research am I going to do?
21 And, Ken, although I agree that you should
22 have a cadre of people, any one person in one
23 discipline who knows almost everything about that
24 discipline I think is almost impossible to find such
25 a person these days. So much is published these days

1 that it's very hard to keep up on any one
2 subject-- multiphase flow and fractured media. There
3 are at least 50,000 papers on it. But we don't know
4 enough. Well, maybe not.
5 But how do you keep up with all those
6 things? It's really not an easy thing. But the
7 training of the staff I think is an important
8 objective. And the best training is to dirty your
9 hands at least once in a while. So if you're just a
10 contract administrator or you have done research in
11 the past but you never do research now, I don't think
12 that works very well. I think it's easier for
13 academicians, for example -- and I was a teacher at
14 one time -- because of the students that keep coming.
15 You stay in touch. Even if you're not doing it
16 personally, you're doing it through your students. At
17 other places it's not easy. It becomes very
18 expensive. And as I said, you have to meet the need
19 of the day. That becomes the most important thing you
20 do.
21 So I don't dislike the prioritization
22 process. I mean, you have to do it somewhere. And
23 now it being a federal agency, the more transparent
24 the decision is, the better it is to be able to
25 present to people. Not every decision will be right.

1 Some of the research that will come perhaps shouldn't
2 have been done, et cetera, et cetera. So I don't know
3 of any perfect method that will always give you the
4 right answer in prioritizing research, because there's
5 always some risk whether they should do it
6 numerically, whether they should consider cost or not
7 consider cost. You'll get different answers.
8 The top three or four items I think will
9 always be the same. And you could do that without
10 prioritization. You can even just sit down and speak,
11 and say, these are the four things that must get done.
12 It's always the lower items I think that become
13 controversial.
14 CHAIRMAN HORNBERGER: Thanks, Budhi. I
15 must say that you finally identified for me the
16 difference between technical assistance and research.
17 Technical assistance is research that's reassessed on
18 a monthly basis.
19 Steve Rattien had a comment. But I'm
20 going to invite Steve to come up here --
21 MEMBER HINZE: Could I ask a question of
22 Budhi first
23 CHAIRMAN HORNBERGER: Yeah, sure.
24 MEMBER HINZE: A quick question.
25 How do you prioritize your non-research

1 but pretty close to being research activities? How do
2 you do this, and how would you change it?
3 MR. SAGAR: Well, I call it resource
4 allocation more than prioritization. But, again, we
5 do draw a line, and there are items below the line
6 which don't get done.
7 How we do it? The section leaders and the
8 branch chiefs and Wes and I -- with a set of criteria,
9 and probability of success is one of them. Relevance
10 to regulatory decision is another. The risk
11 information, how important it is to performance, is
12 third. I think we had five criteria basically. And
13 each person then says -- and we force them to say,
14 low, high, medium. And that's the advantage of
15 whether you go numeric or low, high, medium. Because
16 you just can't speak or read a paragraph and say,
17 well -- you have to put something on paper. You have
18 to say why you're calling it low, those reasons are
19 recorded, and so on. And then in the end, we
20 essentially take an average.
21 MR. WILSON: Do you take into account
22 whether others are working on that particular problem,
23 say, somebody at National Lab for DOE?
24 MR. SAGAR: Yeah, definitely. We
25 definitely consider that. In fact, anybody proposing

1 to just duplicate is rejected out of hand more or
2 less. But as I said, we do believe that the staff has
3 to be able to do stuff with their own hands, be able
4 to publish; otherwise, first of all, we will not staff
5 very long. And secondly, they won't be experts
6 anyway. It doesn't take very long not to be an
7 expert. It's very easy.
8 CHAIRMAN HORNBERGER: Steve, I'm going to
9 really turn it over to you and let you decide whether
10 you want to make comments on this. But, then, we also
11 have a presentation that we're happy to have.
12 Wes, did you have something?
13 MR. PATRICK: Well, I thought he was going
14 to comment on something Budhi had, but I did have some
15 general comments to make.
16 CHAIRMAN HORNBERGER: That's fine.
17 MR. RATTIEN: I'm going to spare you my
18 presentation. You guys lucked out by starting early,
19 to be honest. And I wanted to say, I resonated very
20 much with the comments of the panelists that I heard
21 afterwards.
22 What I wanted to say, though, is -- I see
23 a lot of old friends and colleagues here, but for the
24 last four years I've been working with the RAND
25 Corporation. And RAND is very big in metrics and

1 quantification and frameworks for this sort of thing.
2 In particular, I've worked with the U.S. Air Force,
3 NIOSH, Department of Energy Laboratory, and the
4 Federal Aviation Administration to help them try to
5 come up with prioritization for their research agenda
6 or framework for looking at it. And there is no magic
7 answer, and that's very simply true.
8 I will say right up front that there is no
9 substitute for wisdom. And wisdom is best brought to
10 the floor through panels and discussions, and getting
11 people to interact with one another. But I would say
12 that framework without some sort of metrics isn't
13 quite right either. You need something against which
14 to judge what you're looking at and put it into some
15 coherent context. And, indeed, I think context is
16 critical.
17 And what does that really mean? It means
18 that you have to know your business. We call it
19 enterprise modeling-- fancy words that we charge a lot
20 of money for. But, basically, you've got to know what
21 you're trying to achieve, what the goals of your
22 organization are, where you are today, and how you can
23 get from A to B. And if you can't get to B, where can
24 you be that's okay. But before you leap, you ought to
25 have an alternative place to land in case you don't

1 quite get where you want to be.

2 I mean, research, as you point out, is

3 intrinsically risky and it serves multiple purposes;

4 you wouldn't do it if it weren't. So part of what you

5 have to be thinking about is what you need to know and

6 when you need to know it. And that actually feeds

7 back into the goals for your organization. Because if

8 you can't know it in a timely fashion, why have those

9 goals in the first place?

10 That's a problem in a lot of organizations

11 we have, where they set firm, fixed goals and time

12 frames into the future, and then they put in place an

13 R&D program that can't delivery useful information in

14 the time frame necessary to meet those goals. And in

15 the process you lose institutional credibility, and on

16 and on and on.

17 One of the really important areas for

18 research that I think makes a lot of sense,

19 particularly in the context of Yucca Mountain and the

20 regulatory issues associated with that, are what

21 options exist when surprises arise. Basically, how do

22 you get off track A and on to track B, so that there

23 isn't a sense that you're in mid air, and if you fail,

24 you've created a disastrous situation. That strikes

25 me kind of important. I thought I'd read one

1 paragraph. I'm not sure it will fit in to context,
2 but maybe it will.
3 "One area of research that would help give
4 greater assurance to moving forward on Yucca Mountain
5 is, ironically, research on contingency strategies.
6 If we encountered an unanticipated problem, this would
7 enable us to move from a track we now hope and believe
8 is right to another, in what might be called a
9 fail-safe or graceful manner." And so thinking about
10 that I think is useful.
11 "This direction of research is as
12 important, I think, as addressing the closed pending
13 backlog. It will give all interested parties greater
14 assurance that no irreversible decisions are being
15 made." With that, I'll shut up. I think you guys
16 have really hit a lot of good points.
17 CHAIRMAN HORNBERGER: That probably fits
18 into context with the paragraph that Warner read from.
19 MR. RATTIEN: Oh, yes.
20 CHAIRMAN HORNBERGER: Wes, did you have
21 some comments?
22 MR. PATRICK: Just a few. And I'll follow
23 the outline of your four bullets, because I think they
24 do hit a number of central issues here.
25 With regard to, is the current research

1 and TA Program focused on the right issues, I would
2 say yes. But as Budhi spoke to -- I think quite more
3 completely than what I'll take time to do here -- the
4 need to remain flexible is essential. That's true not
5 only because as a regulator we are in a very real way
6 reactive to what the licensees are coming in with, but
7 also because the knowledge level, the knowledge base,
8 the designs for the waste repository system, and the
9 performance assessment results all continue to evolve
10 with time. And it's essential that we remain flexible
11 with the program to be able to respond to those
12 things.
13 I think Warner North's comments are very
14 well taken. And we do in an informal way -- and I'm
15 going to take away from this possibility of doing that
16 a little more formally, or at least recommending it to
17 NRC. We often do what are called "gap analysis" to
18 find out what's currently known by whomever is working
19 in the area, what needs to be known, and where do we
20 really get the bang for the buck in terms of filling
21 some of those gaps.
22 Your second question was with regard to
23 areas of agreement and disagreement. Rather than go
24 through that in any specific way, I would just point
25 out my feeling, that we really did not, in my view,

1 adequately grapple with the question of what do we
2 need to be able to make the critical decisions versus
3 what might motivate us to continue to do research
4 broadly to gain greater assurance, to have more than
5 a reasonable assurance, which is what NRC's
6 regulations set as a minimum standard.
7 I would agree very much also with
8 Dr. Garrick's comment earlier this afternoon that
9 there's an awful lot that we don't need to know. And
10 anything that we don't need to know is probably not
11 worth knowing really, really well. You may still want
12 to probe around the edges as time and resources
13 permit, but this is a program that has both great
14 scarcity in time and great scarcity in resources. So
15 those things, to my way of thinking anyway, have to be
16 lower priority.
17 Your third question about where is there
18 disagreement-- not so much a question of disagreement,
19 but I think we heard over the last couple of days a
20 number of fairly extreme statements made at times.
21 And those are great to stimulate our thinking, and I
22 would not thwart them being used from time to time.
23 But I think we should be cautious because they
24 do -- these extreme statements -- create dilemmas that
25 are really false dilemmas. And I'm thinking of things

1 like simple versus realistic. Well, we're probably
2 going to realistically be somewhere between those two,
3 realistic versus conservative. We're most likely
4 going to end up somewhere between the two. And
5 striving to argue for a position that it must be
6 absolutely realistic or absolutely conservative, I
7 don't think either one of those extremes is
8 particularly helpful.
9 Another one that I think was really beaten
10 on quite a bit yesterday was this question of, well,
11 are we going to have excellent science or merely
12 adequate science. Well, is that really the question?
13 I would say that you need an adequate amount of
14 excellent science, adequate for the purpose. And the
15 purpose is NRC making a regulatory decision with
16 reasonable assurance. So just a word of caution there
17 in these areas where there is seeming disagreement.
18 The fourth one, that's really, I guess,
19 the nub of what you're trying to wrestle with-- what
20 are the essential attributes? And I'll give you my
21 food for thought for whatever it might be worth.
22 I think the two most important ones -- and
23 they're on my list as one and two -- first, what I
24 would call contribution to risk. And that is, to my
25 way of thinking, is to try to answer the question,

1 what do we need to know. The second one gets at how
2 well we need to know it. And I pose that in terms of
3 contribution or degree of uncertainty that is there.
4 Those must be two of the attributes that we consider
5 in any process that's undertaken to try to prioritize
6 the research work that's needed.
7 The third one is really an obvious one,
8 but it's one that I sense is missed all too often.
9 And as difficult as it is -- and I don't know if it's
10 500, 5,000 or 50,000, Budhi; there are lots of papers
11 on lots of subjects -- we are, really, compelled to
12 know the literature very broadly in our areas. And
13 groups like ACNW, NWTRB, other review panels, have a
14 tremendous burden to, by themselves and through their
15 consultants, bring to bear that kind of comprehensive
16 knowledge so that a single voice on a single point
17 doesn't lead the whole program off in some area that
18 is not particularly helpful.
19 An example that came to mind in the
20 discussions here-- yeah, we've known fracture flow is
21 important for a very long time; and, yet, it was posed
22 in this meeting as something that caught us by
23 surprise. It didn't. Russel did work at least two
24 decades ago on Renier Mesa that showed the same thing
25 that Fabrica Martin found, and everyone was surprised

1 by it. She did it with chlorine 36 measurements; he
2 used some other techniques. But he found travel times
3 into the tunnel complexes below Renier Mesa, the
4 Nevada test site, through tough complexes not all that
5 dissimilar from what exists at Yucca Mountain. He
6 found travel times in the order of months, so why
7 would be surprised to find bomb pulse tritium in a few
8 fractures, in a few locations, 500 centimeters under
9 ground?
10 Which brings me to my fourth attribute
11 that I think should come into play. And that is a
12 caution to avoid over optimism or maybe
13 mischaracterization or over-characterization,
14 over-representation of certain kinds of phenomena.
15 I've had the pleasure of working on both the WIPP
16 Project and the Yucca Mountain Project over the years,
17 both of which stumbled upon the same error. And that
18 is that they promoted their sites as dry.
19 Anyone -- and I'm a mining engineer -- who
20 has ever worked underground in a salt mine knows no
21 salt mine on this planet is dry. And that's true of
22 WIPP. And, of course, there was great press. There
23 was a lot of move to shut that project down or to
24 delay it. And I'm speaking back about 15 years ago
25 when it was a surprise that we found that it dripped

1 underground, and that water moved through the
2 formation. And these effluorescences of various salts
3 appeared on the drifts, and water filled up several
4 feet deep in the bottom of the shafts when they were
5 sunk.
6 Anyone who's, likewise, worked underground
7 in any mine, that I'm aware of anyway, in the
8 unsaturated zone -- and pick your favorite geological
9 media -- any country in this world knows that you have
10 dripping fractures. Anyone who was at the test site
11 and worked in the test complexes would know that.
12 Anyone who worked at Climax -- another facility I've
13 had a hand in over the years -- would know that. It
14 should be no surprise.
15 But for whatever reasons, folks overstated
16 or misstated, or simply got sloppy with their
17 semantics, and made a case -- dryness, in these
18 several examples -- that was not sustainable by what
19 had been observed many times over many decades and
20 many facilities around the world. So I think somehow
21 we have to work into this prioritization process that
22 we avoid the over-optimism, and close off things that
23 may be vital areas of pursuit.
24 The last one, and perhaps the toughest
25 one, the fifth attribute would be somehow how to deal

1 more effectively with what I think of the military
2 complex they like to call Unk-Unks, unknown-unknowns,
3 or the surprises, or the contingency factors that have
4 been brought out here in the discussion this
5 afternoon.
6 I think all I can offer there is that we
7 teach and train and encourage in our staffs and
8 ourselves that we keep open and probing minds about
9 all of these things, whether we are DOE and its
10 contractors in a position of advocacy, still need to
11 remain open to avoid these surprises, or seeming
12 surprises, or if we're on the regulator side, or if
13 we're on an intervenor side. In any of those
14 contexts, I think it's important that we keep open and
15 probing minds.
16 And the second thing from a more practical
17 point of view, I think it's important for NRC to give
18 real serious thought about having some reserve, some
19 contingency, some management reserve, whatever you
20 want to call it, that gives enough resources that it
21 can be directed towards some new surprising thing that
22 comes up or some really bright idea or tough concern
23 that somebody brings in perhaps to an unsolicited
24 proposal into the office of research or whatever it
25 might be. But some way to have a way to deal with

1 those sorts of things in a way that it is effective
2 and not disruptive of the overall process.
3 CHAIRMAN HORNBERGER: Thanks.
4 Okay. I'd like to invite the committee
5 members to make any last-minute comments that they
6 wish to make.
7 Mario?
8 MR. BONACH: I just can talk about the
9 prioritization process. I certainly don't like the
10 quantitative model being presented. I think the
11 quantitative model forgets too much credibility, may
12 have a life of its own, may bring out surprises, may
13 really at times defeat common sense.
14 I like the idea of the panel of experts;
15 however, it should be with a well-articulated
16 elicitation process that focuses on NRC's current and
17 future needs to support licensing decision as we
18 discussed this morning. And I think the process
19 should include consideration of work that is also
20 being done by other parties, not just because we want
21 to avoid duplication. And you ought to have a feeling
22 of how much is being done to cover all the grounds.
23 I don't have any other thoughts right now on how that
24 could be done, but, again, an expert panel would be
25 probably the way to go.

1 CHAIRMAN HORNBERGER: Thanks.

2 Milt?

3 MEMBER LEVENSON: A couple of comments.
4 As Ray has commented a couple of times in the last few
5 weeks or months, sometimes some of us from different
6 backgrounds have to remember that it is not NRC's job
7 to solve problems. I retired from Bechtel where,
8 certainly, it was exactly the opposite. If you didn't
9 solve a problem, get out. But we have to keep that in
10 focus. For instance, it's not NRC's job to decide
11 whether a hot repository or a cold repository is
12 better. It's only to decide whether what DOE submits
13 is or is not safe.

14 But one of the things which I think
15 bothered me a little bit -- and maybe I'm
16 misreading -- that I expected somebody would have
17 commented on. In the description of the system for
18 setting priorities, when you ask where do the projects
19 come from, is the various people propose them, and
20 then all the subsequent discussion is how you
21 prioritize that list. And it seems to me that a much
22 more important point is how do you make sure the right
23 things get proposed. And there's been almost no
24 discussion of that. And it seems to me that that's a
25 much more important part of the whole system.

1 I think that with a broad range of
2 activities that the NRC has, from waste, to reactors,
3 to all the other things, there isn't any technical
4 basis for allocating resources across that broad a
5 spectrum. I think that's a policy decision as to how
6 much money's available for reactor safety, how much
7 money should be available for waste, et cetera; then
8 you can use technical panels to prioritize within it.
9 But I don't know how a technical panel could decide
10 with such widely differing fields which have different
11 policy matters and different requirements. So I think
12 it needs to be a compound system, part of which is
13 policy driven and part of which is technical panel, et
14 cetera, driven. But, you've got to find a way to make
15 sure you're addressing the right things, not just
16 prioritize what anybody happens to propose.
17 CHAIRMAN HORNBERGER: Ray?
18 VICE CHAIRMAN WYMER: Well, over the last
19 two days we've had a lot of different points of view
20 on this business of research, ranging from the very
21 general and generic and philosophical almost, to the
22 quite specific with respect to research projects,
23 which I think is appropriate for a meeting like this.
24 And in thinking of what we've covered the last couple
25 of days, the things I've picked out to comment on

1 here, they're in the same vein. Some are more or less
2 on target, and some are sort of surrounding the
3 target.
4 The first one I picked up on was modeling.
5 Some of the important things that were said, to my
6 mind, was the fact that a variety of concepts should
7 be explored. I thought that was a particularly good
8 observation, since modeling is in the mind of the
9 modeler and does not necessarily represent the truth
10 or anything else. And so to look at it from a number
11 of points of view is certainly important. And that
12 was said a number of times, and it's true.
13 Another point I thought was made well was
14 we need to simplify these complex models down to where
15 somebody can understand what in the world they're all
16 about. And probably the only way you can do
17 that -- I've forgotten, and somebody said it very
18 well -- is you sort of iterate yourself with a very
19 complex model, which then you begin to have some
20 confidence in. And then you use that, and then you
21 abstract it down into something comprehensible, where
22 you picked out those things which have emerged out of
23 the complex model as being the most meaningful things.
24 Now, you may have missed some, of course, you may be
25 wrong, but at least you've made a move in the

1 direction of making it comprehensible, which it is not
2 at the moment in Yucca Mountain.
3 We also discussions of how to use modeling
4 to guide research. I think that was a very good
5 point. We need to have some way of deciding what are
6 the pinch points and what we need to look at. And I
7 thought that point was made. And I suggested that we
8 need to be more careful in how we use the word
9 "model." It means very different things to different
10 people. And people stand next to each other or talk
11 back at forth, and they're not talking about the same
12 thing. They think they are.
13 The next area I wanted to go to was data.
14 You do have to identify the data needs, and part of
15 that comes out of the modeling. I thought that Mike
16 Ryan made an excellent point, and it was seconded by
17 several people, with respect to going out and mining
18 the resources that are out there. Now, there are a
19 lot of sites out there of various kinds that we can
20 get information out of. And their information, there
21 is some available; some more can be made available
22 through spending a little money. But let's get some
23 facts and not just push the paper.
24 And in the area of data, I come back to
25 what I said earlier. I think that you maybe ought to

1 go back to all the "I don't knows" that appear in the
2 transcript, and do a phrase search, and use that as a
3 basis for a selection list of things that maybe need
4 to have some research done on them. You don't do
5 research on all of them.
6 And finally, the point that I picked up
7 was how much is enough. You've heard that a lot of
8 times. But it is clear in some cases that things are
9 demonstrably unimportant. Well, don't knock yourself
10 out. If the dose is 10^{-2} rem per year, let's not
11 worry too much about that one; that's not a big deal.
12 Spend your money where it counts, if you can figure
13 out where it counts.
14 And then finally, what's needed to ensure
15 safety to the extent practicable? How much is enough?
16 How far do you have to go? How much do you really
17 need? And that, of course, is the key issue that NRC
18 is going to have to wrestle with-- are we there, and
19 how do you decide when we're there. That will not be
20 a scientific decision. That's all I have to say.
21 CHAIRMAN HORNBERGER: Steve, you had a
22 comment?
23 MR. RATTIEN: Ray Wymer made a point it
24 seemed to me I might be able to add to. And that is,
25 there is a data base called RADIUS. It stands for R&D

1 in the United States, which is built by the federal
2 government. Actually, RAND built it. I'm not selling
3 it or anything. It's just that it's available to NRC;
4 you may well be using it. And its keyword driven.
5 All federally-funded R&D over the last decade is
6 embodied in this. You can look it up by keywords, by
7 researcher, by institution, by state, whatever. And
8 it's kind of mindless, but it generates what you need
9 to know before you decide whether you need to spend
10 money in that area. And it would be a useful thing I
11 think for NRC to get involved with.
12 VICE CHAIRMAN WYMER: Thank you. It's
13 worth knowing.
14 CHAIRMAN HORNBERGER: John?
15 MEMBER GARRICK: Thank you, George.
16 Let me first say that this committee finds
17 that these workshops are among the most satisfying
18 exercises we go through because it sorts of nurtures
19 our knowledge tree in the very diverse and life
20 spectrum of projects and disciplines that we have to
21 deal with. And we greatly appreciate the experts
22 coming here and the diverse views that were offered.
23 It really is very helpful. And one of the products of
24 this is inspiration for new workshops.
25 VICE CHAIRMAN WYMER: You've been warned.

1 MEMBER GARRICK: And I am inspired in that
2 direction a little bit because I can see on the basis
3 of what's been said the last couple days some real
4 interesting ideas of new workshops, and we've had some
5 discussions of that among ourselves.
6 I was struck this morning by Dave Kocher's
7 second or first slide that said this is what we know
8 and is in pretty good shape. And, ironically, I had
9 already written down this is something we ought to
10 have a workshop on. And that's inventory,
11 radionuclide inventory. I was very interested in some
12 of the remarks that Mike Ryan made yesterday because
13 it followed a comment I had made about the fact that
14 what we heard mostly was what happens after this stuff
15 becomes mobile rather than what happens in that very
16 critical part of creating the source term, which is
17 very dependent upon the fine structure of the
18 chemistry of the radionuclides and what have you.
19 And there's lots of issues there that are
20 very important. The whole issue of volumetric source
21 versus surface source; the whole issue of
22 distinguishing between mass and curies with respect to
23 the problems and computational methods; the whole
24 issue of classification, that classification was not
25 designed to help us do what we're doing today, it came

about another reason and so forth. It just occurred to me that there are some real interesting questions there.

Another workshop idea that's come out of this and our own previous work is, just as a lot of people like to bash DOE, there's a lot of people that like to bash TSPA, and we're among those. And it seems to me that we need to step back and say, well, what is wrong with it, and what do we need to do to make it have credibility, and to build confidence, and is there an alternative. And I think we ought to be, in a very systematic and deliberate fashion, trying to address the question what do we need to do to our TSPAs to enhance their acceptance and to enhance the whole process of building public confidence.

And then the other thing that came out of this that I found very interesting, we had some excellent discussions of this in some academy work, and that is the subject of monitoring and performance confirmation. I think the opportunity there is extremely great. And I think this is one of those things where the motivation may be 10 to 15 percent scientific and 80 to 90 percent public confidence building. I think it's one of those things where if you can say that you know what's going on under the

1 ground all the time as you put this stuff in there,
2 and especially after you close it, that that makes
3 people feel a lot better.
4 And as a matter of fact, in a workshop
5 that I attended a few years ago in Salt Lake City on
6 waste with the Tribal Nations, one of the reasons this
7 particular group was advocating storage over disposal
8 was principally because in storage you know where it
9 is and what's going on, and you can easily measure.
10 And so I think monitoring and performance confirmation
11 is a very interesting subject.
12 Just a couple of other little things. I
13 want to just make a comment on panels. I agree
14 with -- I think Steve said that there is an advantage
15 in these evaluation processes having metrics and
16 having some numerical content to them. Wes I think
17 said that as well. And maybe we shouldn't be too
18 quick to abandon that. I do agree that wisdom and
19 panels are probably the most important aspect of this.
20 The only thing I would say about panels in
21 reviewing lots of expert elicitation results and in
22 conducting a few panels during the early years on
23 getting insights and information necessary to build
24 large risk models for nuclear power plants is that, we
25 need to make a distinction between the opinions of the

1 experts and the evidence that supports those opinions.
2 And it's been my experience that you really enhance
3 the meaning of the panel process if you behind the
4 expert and try to find out why they're saying what
5 they're saying. And there's a way to do that. It's
6 somewhat laborious at times, but it can be done. And
7 when it's done, the panel process can be extremely
8 effective and powerful.
9 I wrote a few things down here that were
10 in the category of summary things, and I'm not going
11 to cover all of them. I did want to pick up on
12 something, Warner, that I think you said in your
13 reading from your excellent Academy report. And it is
14 an excellent report. And I guess one of the reasons
15 it caught my attention is, the experience that I've
16 had with ACNW that maybe has been the most significant
17 has been the opportunity to interact with the public
18 on the Yucca Mountain situation and to participate in
19 some public forums. That's the good news. The bad
20 news is it's developed a high sensitivity to some
21 words, the whole business of perception and risk
22 communication, and what have you.
23 And I think the language that you read us
24 at the end of your report included a phrase having to
25 do with, "proof of safety is never possible."

1 MR. NORTH: In the usual sense of these
2 words.
3 MEMBER GARRICK: Yeah, in the usual sense
4 of these words. And my only comment there is,
5 inevitably, these kind of phrases will be taken out of
6 context and used to say that this cannot be done.
7 Now, the critical finding of the regulators is, of
8 course, not to prove safety absolutely but to at least
9 address it in the context of reasonable assurance.
10 That division is not always appreciated by the public,
11 but I'm just pointing it out because it's extremely
12 important for us to phrase our conclusions about these
13 facilities as contextually as we possibly can. And I
14 know you intended to do that, and you did that. But
15 I'm just pointing that words like "proof of safety is
16 never possible" can be really badly taken out of
17 context.
18 One thing that I wanted to say here
19 is -- I said some of these things this morning when I
20 made a speech, so I don't need to say them now. I
21 don't recall who said this. Dave Kocher said it, and
22 I know, Kirk, you said it I think as well. And that
23 is, there's this whole issue that we want to be very
24 careful when we do research and when we do assessments
25 that we don't let the standards and the policies and

1 the regulations and the rules stand in the way. In
2 other words, I've often drawn the analogy between risk
3 management and compliance management, and tried to
4 make the point that there's a major difference.
5 Compliance management is setting up a process that
6 will demonstrate compliance, and risk management is
7 setting up a process that answers the question, what
8 is the risk. And, of course, those concepts are
9 converging, and you'd like them to be the same, but
10 they're not.
11 We had a few examples presented to us the
12 last couple of days. And I think, again, Ryan gave us
13 a couple of good examples in that arena. When we are
14 thinking about what we need to do to demonstrate
15 safety, and, therefore, what we need to do to conduct
16 a good research program, we need to look beyond the
17 standards and the rules and the recommendations. And
18 when I was building a company to do risk assessments,
19 I found it was more difficult to train people who were
20 experts on the rules and regulations and licensing
21 process than it was people who knew nothing about them
22 but understood the basic thought processes that we
23 were in employing, because their frame of reference
24 was tainted by always employing the frame of reference
25 of compliance. So I think that's something that's

1 very important in making decisions about our research.

2 This hasn't been mentioned, and this is

3 kind of a Bill Hinze, off-the-wall one. But one of

4 the things that I think is worth saying is, in order

5 for the NRC to, indeed, employ panels and experts and

6 consultants and so forth -- one of the experiences

7 we've had that hasn't been particularly pleasant is

8 the complexity of what I'll call the NRC method of

9 contracting experts. I think that is an obstacle in

10 implementing some of the ideas on the use of experts

11 and expert panels that has been discussed here. And

12 it may be something that we have to address in a

13 slightly different way.

14 I've got a number of things here, but I've

15 used enough time.

16 CHAIRMAN HORNBERGER: Thank you, John.

17 Bill?

18 MEMBER HINZE: Well, looking forward to

19 your comments, George. I think everything wise has

20 already been said. But let me try a couple of

21 questions.

22 I'm a real devotee of analogs. And I

23 think that Ken Rogers' analogy of the role of the NRC

24 as the empire at an all-star game, or whatever, is

25 very appropriate. But also, really, in my view it

1 goes beyond that. Because the NRC also has the job of
2 not only implementing the rules and regulations but
3 also deciding upon them.
4 And I guess my question is the research
5 that one does to make certain that you implement with
6 the minimum amount of uncertainty the same as those
7 that you use to determine the regulations. I suspect
8 there are different types of research, and I think
9 particularly in the participatory area that one,
10 really, has to look forward to what types of rules and
11 regulations are coming along for NRC. I don't think
12 that we've spent near enough time on that topic here.
13 Another question that one could raise, as
14 we've talked about the research around the table in
15 the summary, by and large, it has been focused upon
16 the high-level waste concerns. Now, I'm fully
17 cognizant of Mal's point about the fact that NRC is
18 not here to solve the problems of the sites. But I
19 would suggest to you that there is a difference of the
20 level of research that is needed, and the approach to
21 research that is needed in the high-level waste area
22 where you're interfacing with a $500 million research
23 megalith versus that of the state of Nebraska, who
24 isn't going to put a cent into it. And so my question
25 would be, is there a difference in the approach to

1 research in high-level waste versus that of uranium
2 mill-tailings or the other areas. And I think that
3 some consideration should be given to that.
4 The final point is, one of the themes that
5 has cut through everything is the business of data and
6 information. And we are all interested in making
7 certain that we minimize -- and understand that the
8 NRC can't go out and acquire data for the purpose of
9 regulation. But the NRC does have a real
10 responsibility to make certain that they minimize the
11 uncertainties in their decisions by making certain
12 that the applicants come forth with the right data
13 collected correctly in sufficient amounts; that the
14 heterogeneities have truly been sampled as well as
15 they can without making the Swiss cheese, and that
16 means other methods as well.
17 I guess I'll leave it at that. Those are
18 some of the points that come to mind as being a little
19 different.
20 CHAIRMAN HORNBERGER: Thanks very much,
21 Bill.
22 As Bill has encouraged me, I will be
23 mercifully brief. A thought had occurred to me. I
24 liked Steve Rattien's comment that there is no
25 substitute for wisdom. I think that we all agree with

1 that. Having said that, it's not necessarily a
2 trivial matter to ensure that you inject wisdom
3 appropriately in the decision-making process. I have
4 always been quite amazed at how well the other
5 NRC -- the National Research Council on behalf of the
6 academies -- somehow manages, in my estimation quite
7 consistently, to put together panels and committees
8 who work very effectively to bring wisdom to the
9 table. And I guess what I'm saying is that, we can't
10 lose sight of the fact that very good and enlightened
11 leadership is a prerequisite at the NRC within the
12 management however the wisdom gets injected. And
13 that's probably a very primary component.
14 The last task I have is a very happy one,
15 to thank everyone. I must say that the ACNW really,
16 really, truly appreciates the participation of all of
17 you. And we haven't even bought you a cup of coffee
18 because of government regulations. But that doesn't
19 mean that we don't appreciate it. And I do, really,
20 want to sincerely thank everyone who has participated.
21 There's a lot of organization that has
22 gone on to put this together. And Dick Savio has
23 worked probably some fairly long hours. I didn't keep
24 track of it. But I do want to thank Dick. He really
25 has done a job. And he has been very ably assisted by

1 some other people here on the staff, including Jenny
2 Gallo; Barbara Joe White, Michele Kelton -- our own
3 Michele sitting over there -- and Theron Brown. So I
4 think all of those people.
5 And with that, I will call an end to the
6 workshop session. We will have a 10-minute break, and
7 then the ACNW will reconvene. And we'll talk about
8 other matters, including our presentations to the
9 upcoming commission meeting.
10 (Whereupon, the foregoing matter went off
11 the record at 4:21 p.m.)
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CERTIFICATE

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

Name of Proceeding: 130th ACNW Meeting

Docket Number: (Not Applicable)

Location: Rockville, Maryland

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

Pippa Antonio

Pippa Antonio
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