NUCLEAR WASTE TECHNICAL REVIEW BOARD

Full Board Meeting

Open Morning Session

Crystal City Marriott
Arlington Ball Room
Arlington, Virginia

October 10, 1990
10:00 a.m.

EXECUTIVE COURT REPORTERS, INC.
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BOARD MEMBERS PRESENT

DON V. DEERE
Chairman

CLARENCE R. ALLEN

DONALD LANGMUIR

JOHN CANTLON

D. WARNER NORTH

MELVIN W. CARTER

DENNIS L. PRICE

PATRICK A. DOMENICO

ELLIS D. VERINK

ALSO PRESENT

WILLIAM D. BARNARD
Executive Director
Nuclear Waste Technical Review Board

ROBERT BERNERO
Nuclear Regulatory Commission

ROBERT SHAW
Electric Power Research Institute

LEON REITER
Senior Professional Staff
Technical Review Board

ROY WILLIAMS
Consultant to the Board on Hydrogeology

AUDIENCE PARTICIPANTS

MAXWELL B. BLANCHARD
Chief
Regulatory and Site Evaluation Division
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DAVID C. DOBSON
Chief
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Yucca Mountain Projects Office

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Washington, D. C.

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Eckert, Seamans, Cherin & Mellott
Washington, D. C.

TABLE OF CONTENTS

P R O C E E D I N G S

CHAIRMAN DON V. DEERE: Good morning, ladies and gentlemen.

First I have an official request to help in our recording. We wish you would wait to be recognized by your last name before speaking or state your name clearly before you speak. Anyone in the audience who later wishes to make a comment we would appreciate it if you would go to the nearest microphone, wait to be recognized and then state your name and affiliation clearly. Last, leave some identification such as a business card or fill in the sign-in sheet by the microphone.

This will help us to get the most accurate transcript possible.

My name is Don Deere. I am Chairman of the Nuclear Waste Technical Review Board. On behalf of the Board members I want to thank you for coming to the autumn meeting of the Board.

We have an ambitious agenda for this two-day meeting which I will outline shortly. I would like to take this opportunity, for those of you now familiar with the Nuclear Waste Technical Review Board, to provide some background information.

The Nuclear Waste Technical Review Board was created by the United States Congress on December 22, 1987 in the Nuclear Waste Policy Amendment Acts of 1987. Although established by Congress, it is an independent agency within the Executive Branch of the United States Government.

The first meeting of the Board was held in March of last year.

It is the Board's responsibility to evaluate the scientific and technical validity of the United States Department of Energy's efforts to evaluate the suitability of the Yucca Mountain site in Nevada for the permanent disposal of the Nation's commercial spent fuel and defense high-level waste.

The Board, also, is charged with evaluating waste packaging and transportation activities undertaken by the Department of Energy. We report our Findings, conclusions and recommendations to the United States Congress and the Secretary of Energy at least twice a year. The second report of the Board is scheduled to be released in mid-November.

Members of the Board are selected by the President from a list of nominees prepared by the National Academy of Science. There are currently eight active Board Members of a total of eleven. One former Board Member is awaiting appointment. The term of appointment is for four years.

I am honored to have been selected by President Reagan to serve as the Board's Chairman. I would like to take this opportunity to introduce the other Board Members.

They are, in alphabetical order, Dr. Clarence Allen, Professor of Geology and Geophysics Emeritus, California Institute of Technology: And I think you will recognize that this word Emeritus has just been won;--

[Laughter]

CHAIRMAN DEERE: --Dr. John E. Canton, formerly as of one month ago Vice President of Research and Graduate Studies, and Dean of the Graduate School at Michigan State University: and I understand they play Michigan this Saturday;--

[Laughter]

CHAIRMAN DEERE: --Dr. Melvin W. Carter, Neely Professor Emeritus, Georgia Institute of Technology and an International Radiation Protection Consultant; Dr. Patrick A. Domenico, David B. Harris Professor of Geology and Geohydrology, Texas A&M University, College Station Campus;

Dr. Don Langmuir, Professor of Geochemistry at Colorado School of Mines; Dr. D. Warner North, principal, Decision Focus, Incorporated, Los Altos, California, Consulting Professor, Stanford University, Palo Alto, California and Associate Director, Stanford Center for Risk Assessment;

Dr. Dennis L. Price, Professor of Industrial and Systems Engineering, and Director, Safety Projects Office, Virginia Polytechnic Institute and State University, Blacksburg, Virginia; Dr. Ellis D. Verink, awaiting re-appointment, Distinguished Service Professor of Metallurgy and former Chairman, Material Science and Engineering Department, University of Florida, Gainesville, Florida.

The day to day activities of the Board are managed by our Executive Director Dr. Bill Barnard.

We established several panels to facilitate activities. The current panels are Structural Geology and Geoengineering, Hydrogeology and Geochemistry, Engineered Barrier System, Transportation and Systems, Environment and Public Health, Risk and Performance Analysis, and Quality Assurance.

At meetings of the full Board we try to have speakers who deal with broad topics that cut across various disciplines, and topics that the Board Members need to understand.

Today we have been particularly successful in having two speakers who will make presentations which I think we will all find of great interest. We are please that Mr. Robert Bernero of the Nuclear Regulatory Commission had agreed to brief us today on the Waste Confidence Proceeding; and the Electric Power Research Institute will also make a presentation on their performance assessment activities: very detailed and very broad. I will introduce their speaker later.

Tomorrow we have scheduled a technical exchange with the Department of Energy of our Panel on Structural Geology and Geoengineering to discuss the Department of Energy's Calico Hill Risk Benefit Analysis, Surface-Based Prioritization and Dry Drilling.

The Electric Power Research Institute will also provide more details on their performance assessment in tomorrow afternoon's session.

Before we begin I want to remind you that we are a diverse group of individuals. Thus any comments made by a Board Member during these proceedings reflects their personal point of view and does not necessary represent the opinions of other Members, nor of the Board as a whole.

In addition, I would like to point out that lack of comment on a presentation does not necessarily indicate the Board agrees with what is being said. The Board's position on issues is formulated only after careful consideration and discussion by the Board, and is presented in its reports to and testimony before Congress.

If you are interested we do have copies of our October 2 testimony before the Subcommittee on Nuclear Regulation, Committee on Environment Public Works, United States Senate.

We would like to begin now with Robert M. Bernero. He is Director of the Office of Nuclear Material Safety and

Safeguards of the United States Nuclear Regulatory Commission. He will address the topic of the Waste Confidence Proceeding.

WASTE CONFIDENCE PROCEEDING

Robert M. Bernero, Director, Office of Nuclear Material Safety and Safeguards, United States Regulatory Commission

MR. BERNERO: Thank you, Mr. Chairman. I think I have enough of a tether here.

It gets harder every year. I am 60 years old and I have never been invited to be a member of a prestigious board; but at least I can talk to them.

[Laughter]

CHAIRMAN DEERE: When do you retire?

[Laughter]

DR. CARTER: Bob, all of us have to go through a probationary period.

[Laughter]

MR. BERNERO: There is on the ballot in the State of Oregon an initiative to shut down the Trojan Nuclear Plant. One of the principal reasons contained in the initiative for shutting it down is the lack of high-level waste disposal.

Not long ago, just a couple months ago, the National Research Council Nuclear Waste Board published a report that said, among other things, "The U. S. high-level waste program, as presently conceived, is doomed to failure."

Exactly eight days ago Dr. Deere and I had the pleasure of testifying in the Senate on: Where are we going in high-level waste? I this chorus of gloom, in this chorus of voices saying "We cannot get there from here," the Nuclear Regulatory Commission has just published a Waste Confidence Finding. It was in the Federal Register just a short time ago.

On the surface that might sound absurd. However, today I would like to explain what the Commission did and why it did it.

First of all, the Waste Confidence Proceeding--and there are copies of this outside of which you should all have one--is a generic environmental Finding. In other words, as we engage in major Federal actions, like issuing licenses for major facilities or things like that, we often have to make decisions or take positions on major issues from which there will derive a substantive series of actions like licenses; and these generic Findings are really environmental impact statements or the equivalent.

That is what we are doing here. We are making a Finding that resulted from reactor litigation--I will explain that in a little bit--and the Commission made this Finding once before: in 1984. At the same time, it pledged to review it every five years.

Therefore, in 1989 the subject came up again.

The origin of this was in 1976. In 1976 the Natural Resources Defense Council filed a petition that said in so many words, "You should quit licensing reactors or amending the licenses of reactors since you don't have assured disposal of the high-level waste generated by operating those reactors. Since you don't have that disposal you should not authorize the further generation of such waste."

As it turned out, at that time there were two nuclear plants that filed for amendments to their licenses to enlarge the storage capacity of their spent fuel pools. The spent pool is a very large swimming-pool like structure; and the racks in those pools were designed very conservatively with a lot of spacing: far more space than was really necessary for safety reasons.

In the days when the storage of spent fuel began to be a problem, reactor owners looked at their pools and said, "Gee, if I only changed the metal rack I could get 50 percent more fuel," or twice as much, "in there"; but they had to amend their licenses to do that.

Two reactors, the Prairie Island Reactor and the Vermont Yankee Reactor, applied at that time for such amendments. They became the focal point of this in the legal forum; and it went to court.

The United States Circuit Court of Appeals did not vacate the license amendments, but it did remand to the Nuclear Regulatory Commission an issue in 1979. Notice that three years had passed in this litigation.

What they said is the NRC should determine whether there was reasonable assurance of high-level waste by the years 2007-2009. That very precise window of two years, one year plus or minus one, is derived from the expiration dates of the licenses for those two reactors at that time.

There is no precision of such nature associated with the storage or disposal of waste. Those were legally-derived dates and they were associated with the then-valid operating licenses of those two reactors.

The second issue was: Is there reasonable assurance that one can safely store this fuel in the meanwhile?

Two issues: Are you sure there will be waste disposal? And, can the waste be handled safely in the meanwhile?

The Nuclear Regulatory Commission held this Waste

Confidence Proceeding, and this is a quotation from what they said, ". . . solely to address those two issues generically to validate or invalidate licensing proceedings in reactors."

In doing this, in 1984 the Nuclear Regulatory Commission issued five Findings. I have three on this page and two on the next page. The first one is that disposal in a repository is feasible based on a technical evaluation that it is feasible to have deep geologic disposal.

The second Finding: At least one repository will be available; and notice those dates: 2007, 2009. They derive from the court case, and actually derive from the then-current expiration dates for two licenses. And, the repository capacity will be available within 30 years to dispose of all spent fuel and high-level waste generated.

There is an implication in that that not only will a repository open, but capacity to handle all of the spent fuel will be available within 30 years.

Thirdly, spent fuel and high-level waste will be safely managed in the meanwhile. Fourth, spent fuel can be stored safely for at least 30 years--that is to accommodate the time delay--and sufficient storage capacity will be available if needed.

So you see the Nuclear Regulatory Commission's Finding covered the spectrum of it is feasible to have deep geologic disposal; we expect that it will be available in a timely way; and we expect that the matter can be managed safely in the interim.

DR. CARTER: The clock started running in 1984. Is that right?

MR. BERNERO: That statement was dated in 1984 and the revisit was, therefore, pledged for 1989, which we did do.

DR. CARTER: But the 30 years originally was from--

MR. BERNERO: The 30 years was a judgment call about how long after it would take. There is a great deal of debate about what is the right planning basis.

There is an unfortunate tendency--you see it here already--that due to the expiration dates of the licenses and the magical appearance of 2007-2009 there is almost the implication that one is dealing with something that is true to a certain year plus or minus one; and that thought process is wrong.

You are not dealing here in weeks, months or years. You are dealing in decades. You can fairly and honestly speak of decades as the time scale of interest; and you will see that as we went through the 1989 Finding.

I am going to skip over. There are rulemakings--these are administrative detail in Parts 50 and 51--that are necessary for us to administratively deal with that waste confidence.

Basically, in 1979, we went back to look at the issues that were considered and said, "What do we know how, five years later, that is different?" Certainly a lot happened in the high-level waste program between 1984 and 1989; or, to put it more accurately, a lot failed to happen between 1984 and 1989.

We looked at the issues: technically acceptable sites in a timely fashion, and you know that was the period during which we went from a group of sites down to three sites down to one site, and then stopped doing anything on the one site; timely development of waste packages and engineered barriers; institutional uncertainties; continuity of waste program management--all troubling issues; continued program funding, and you know that the Nuclear Waste Policy Act got into the 1 mil per kilowatt hour right at the beginning of this time period; and the Department of Energy schedule for repository development.

All of these issues were looked at again. We then said, "Well, let's look at those Findings and let's see if there is anything different."

The original Finding Number One is quoted right here: "The Commission finds reasonable assurance that safe disposal of high-level waste and spent fuel in a mine geologic repository is technically feasible."--we went over all the aspects of that decision, you can find that in our analysis which was published for comment and later published as the final version this year in the Federal Register--and we could find nothing that said that geological disposal is not feasible.

Basically we reaffirmed this Finding. It was technically feasible to have mine geologic disposal, and still is.

DR. ALLEN: This is the exact wording?

MR. BERNERO: Yes; and we stand by that.

We considered the packaged waste forum processing, back fill sealants. If we look at that, we don't find anything that discourages us other than programmatic aspects. We would certainly like to see more.

Original Finding Number Two: Will it be available? That 2007-2009.

In 1989 we had not yet received the November report to the Congress. Remember when the Department of Energy wrote what was called the 60-day report? They were responding to the congressional challenge: What are you doing about these endemic schedule slips?

They said: This is what we are doing about the endemic schedule slips. We are giving you a whopper all at once; and the schedule slipped to 2010 for repository operation.

We looked at this and we said, "First of all this

Finding focused unduly sharply on the 2007-2009." The 30 years was kind of pulled out of the air. What is a safe time scale for storage?

What we have is an analysis that supports not this original Finding, but a revised Finding that says this: that there will be at least one repository by the end of the first quarter of the next decade. That also happens to be the beginning of the next millennium. It really gives you that sense of time.

I would reiterate the point I made before: that to be rational one must think in terms of decades. This is not to say to put off generation after generation, but it is not accurate, it is not necessary to think in terms like 2007-2009.

So the end of the first quarter, 2025, and then 30 years beyond the licensed life of any reactor entails the possibility, which is very real, that reactors originally licensed for 40 years will have their licenses extended by 10 or 20 or 30 years--we don't know: it depends on the technical merits--and then one is faced with the prospect that a reactor can be generating spent fuel for the initial 40-year life for perhaps as much as 30 years life extension and maybe 30 years after shut-down, speaking in institutional terms.

You can easily add up numbers to get 100 years.

When one looks at this sort of scale and looks at the program, we recommended and changed the Finding to this revised 2025 date.

DR. CARTER: Bob, do you anticipate that license extension for commercial reactors will be blocks on the order of 10 years, 5 years?

MR. BERNERO: Oh, yes. I would say at a minimum 10 years.

The planning and construction time for new generating capacity has to be at least 10 years. You cannot realistically go up to a power company and, in just a few years, expect them to put new capacity on line unless you would constrain them to something like gas generators, gas turbines: the peaking power units.

For base load or major capacity one generally thinks in terms of 10 years. Therefore, 10 years before a license expires the utility needs to know this is or is not going to be extended because otherwise they have to make plans in accordance with that.

In general, it is probably not worth considering unless it is 20 years. You would be constantly at the end of the trail with the 10 years, and 20 years would be a reasonable block.

There is controversy extant right now on one of the first to apply for license extension. It is a pressurized water reactor and there are metallurgical problems with the radiation embrittlement of the reactor vessel that may forbid plant life extension for that plant altogether and may even effect its license life.

However, we expect that plant life extension, where justified, would be in blocks of 10 years. So in here what we have assumed is 30 years.

Remember, you are trying to get a mental frame of mind: What is the right expectation for the extent of waste generation by any one reactor and the length of time involved? It really is a bit conservative but I think reasonable to use 40 plus 30: a 70-year operating life.

Also, our basis included what had happened to the Department of Energy program. What we said in here really, if you look at all this debate, is that we had slippage in the Department of Energy program. There was a 5-year slippage from repository 1998 to 2003 in the program plan, and then the November 1989 report dropped it from 2003 to 2010. It was another seven years.

That is a dramatic change.

The near-term milestones have slipped and slipped and slipped and slipped.

DR. CARVER: Bob, I would like to ask you a question about the previous slide.

MR. BERNERO: Sure. I am going to go over each of these sub-elements. There is a subsequent slide.

DR. CARVER: The question pertained to item (b), there.

MR. BERNERO: I am about to turn that up.

Yucca Mountain. What about Yucca Mountain? There is no site characterization going on now. Basically we said that the only way to deal with Yucca Mountain is to take a pessimistic approach and look at the time scale that would be associated with arguing about Yucca Mountain: whether arguing about the ability to characterize the site or arguing about the suitability of the site once characterization is underway.

Let's postulate it is argued until the year 2000, and then somebody throws up their hands and said, "That is it. That tears it. We walk away from it"; and we make that assumption here in order to look at the span of time. Then, if you haggled until the year 2000, another ten years, gave up on Yucca Mountain at that late date then, we said, it would take probably another 25 years--that was an estimate the Department of Energy made some time ago--to start over: to persuade the Congress and the states, and so forth, to start over with another site.

That is where we got the 2025.

DR. CARTER: My point was, and I think I understand, the dichotomy. The generic slide says Yucca Mountain will prove suitable. I think it should be unsuitable.

MR. BERNERO: Oh, that is an error.

DR. CARTER: Either that or you argue both ways.

MR. BERNERO: I should have proofed that. Yes.

These slides were changed in format on the computer and I did not proof read them. I should have. They were just changed yesterday.

The key thing is this 25 years for another site after 10 years of futility on this site. Certainly this is pessimistic. Some might argue it is realistic in light of the history of the program.

One of the problems we considered is that the second repository is an important aspect. For political reasons, active work on the second repository was suspended by the Congress.

This is a factor that influences the length of time. There is no back-up site. It was one of the real program losses, when the focus was narrowed from three sites to one, that you don't have multiple sites working at one time for A: an assured first repository, you know, higher likelihood of a successful first repository; and the Congress was adamant, don't even talk about a second repository. New England granite. You must not.

I think the authorization bill still forbids the Department of Energy to do research on granite. Crystalline rock is a no-no for technical reasons. Most of the northeastern United States has it underneath.

That is where the Department of Energy made their estimate, the 25 years; and that is what got us into this.

Let me turn to the issues of storage and the safety thereof. We looked at confidence in the storage, and I constructed for you a little while ago in response to the question about plant life extension, 40 plus 30 plus 30. You can institutionally get to 100 years; and it is not unreasonable to say that, institutionally, if you have a licensed reactor you have people for the surveillance and control, and that if you have a life extension of 30 years you still have those people.

As we said before, we thought it reasonable from an institutional point of view to say: For 30 years after the shut-down it is not unreasonable to expect institutional control in a full-fledged manner. That is where we are getting 100 years of institutional capability.

Then, of course, you get into technical considerations. "Well, if, institutionally, it is okay for 100 years, how about physically, technically?"

We have licensed dry storage in the time period since the prior waste confidence Finding. We have spent fuel pools now that have had 25, 30 years of storage in water. We are now in a more passive, more inert environment.

If you have ever looked into the dry storage designs being authorized right now, basically they are sealed cans containing inert gas. They are very passive, very low temperature, very simple robust designs. When you go in there you don't find failure modes of significant concern, whether wet or dry. They have time scales of even tens of years.

Looking at that technically, we are able to voice confidence that these things are not going to come apart: with surveillance, they have a stability we can safely say, with the institutional control, they are safe for at least 100 years. I don't even see 100 years as being anything magic in that.

The combination of technical and institutional controls is acceptable.

The other Findings will go along with that. The original and continued Finding Number Three is: Reasonable assurance that it will be managed in a safe manner until it is available.

You can look and the program gives assurance of that. There are different designs now, but the institutional process is basically the same. We don't have an MRS, but we have what I call the lower case MRS: monitored retrievable storage at a reactor site. We have three of them licensed now and more in the pipeline.

The original Finding Number Four was that it was going to be at least 30 years, but at that time we did not have plant life extension in mind; and this is a revision to recognize the longer time scale. So there is a change to Finding Number Four.

Lastly, Finding Number Five is what it was before: that spent fuel storage on-site or off-site will be made available if such storage is needed.

At the time the original Finding was made, in 1984, I think you could have safely asked the Commissioners the question: What is that off-site storage?; and they probably all would have said, "The MRS, obviously." The MRS is now forbidden by law in this sense that it is linked to the repository, but we have had, under the Nuclear Waste Policy Act provisos the development, the authorization, the construction and licensing of dry storage and dry storage capacity taken in conjunction with wet storage capacity; and, for that matter, recognizing the possibility that the Congress may free up the MRS after all.

All taken together reaffirm this Finding.

Basically what you have here is the Nuclear Regulatory Commission has taken the position that in spite of all the complaint and the comment on the destiny of the program--and there are problems in the program--if you look through the fundamental issue still remains: Can this society continue to generate more high-level waste knowing that it needs to have a practical way of disposal? Can it be confident that a practical form of disposal will be available in a reasonable time, and that the matter can be safely managed in the meanwhile?

Basically the Nuclear Regulatory Commission Waste Confidence Finding is yes. Yes, it can be assured that disposal will be achieved, and it can be safely managed in the meanwhile.

I will be happy to answer any further questions you have.

Questions and Discussion

DR. LANGMUIR: I am sorry I am a little after the fact on this important point.

You point out that the 1984 Waste Confidence Findings included the statement that disposal in a repository is feasible. You then went to the 1989 reaffirmation.

I look at the list of issues you examined at that time. Nowhere in that list of issues is any assignment of the geologic environment giving it credit for part of that safety.

You list waste package, waste form, and it talks of some back fill sealants; but never does it look as if the Nuclear Regulatory Commission has considered that geologic environment would have any effect or any importance in protecting the environment from the waste.

The implication, of course, is that these considerations are sufficient from the viewpoint of the Nuclear Regulatory Commission without giving any credit to the environment.

MR. BERNERO: I think that is a flaw of presentation rather than coverage and consideration.

The consideration was entirely in the context of geologic disposal; and both by the terms of the Act and by our own expectations in regulatory space the geologic disposal assurance is to be enhanced by all the man-crafted, the human-crafted things: the waste package, form, sealants and so forth.

But we definitely did this in the context of the geology: that the isolation in geology is required as a fundamental part of it.

The way the presentation comes across, I do recognize, it seems to slight over that. We did not make the decision solely on the basis of engineered safety.

DR. LANGMUIR: But you must have assumed some characteristics to that geology, and there are certainly obviously very wide variations in properties.

MR. BERNERO: Oh, yes: many different media and the peculiarities thereof, the controversies.

This was considered in the first Waste Confidence Finding and re-evaluated now; but, of course, the peculiarity now is that the program is actually focused on only one: the tuff. It remains to be seen whether or not that site as well as that media is compatible.

In our re-evaluation we were actually looking for evidence that would say geologic disposal is not acceptable to technically feasible. We are not looking to find acceptability, but looking for evidence that it will not lend itself to a Finding of acceptability after due investigation.

We found no indication of that.

There are, of course, recommendations. The National Research Council, in their report, has said repeatedly something like at key steps in the way you ought to be considering alternate disposal forms, like don't dispose it all for many years or deep-sea disposal, or something like that.

That is always tantalizing. Some of the best lectures I have ever heard are for disposal in the abyssal plain. Is many ways this is like the grass on the other side of the fence: it is very green. It sound wonderful.

However, our focus here is on the technical feasibility, the confidence that deep geology in the United States is going to be achieved.

DR. LANGMUIR: It is still an interesting point, that given all the variety of properties of those geological media we might encounter, some with much less ability to retain things than others, you come up with a conclusion that regardless of what that might be we can still comply with regulations.

I think that is an interesting observation.

MR. BERNERO: No. What we are saying is that an acceptable site can be found; and we make the assumption that tuff may be an acceptable site. It may not be. We make the assumption that for ten years we will argue the point and then shift, most likely, to another geologic media.

I doubt very much that another tuff site is even likely to be considered. We are not tying ourself to the tuff site.

We are saying: Is there evidence that says an acceptable geologic repository is not feasible? It is not necessarily a tuff site. It could be salt, it could be granite or basalt, or whatever the alternate would be turned up.

DR. CARTER: What sort of criticism did you get as far as the Waste Confidence Proceeding is concerned from those that might have participated? I presume you had a lot of experience at that stage of the game with water storage at reactor sites, but not necessarily that much experience with cask storage.

MR. BERNERO: The corrosion environment of spent fuel in a cask and inert at relatively temperatures was rather easy to extrapolate.

Most of the criticism I have seen and we have received in the Waste Confidence activity is institutional. Just look at that program.

How can you be confident? That is very difficult to answer. When you look at the Department of Energy high-level waste program that John Bartlett is now in--and I wish him well--the leadership has been a problem. The funding has been generally there, but controversial.

Even as we speak there is constant argument about it and there is litigation associated with even trying to characterize the site. There is constant political interaction about: Should be put in Nevada because we outnumber them? At the hearing we heard Nevada offer it to Wyoming, and Wyoming left the room.

Programmatic or institutional uncertainty is far and away the biggest issue that most people raise. That makes it very difficult because you look there and you say,--I have often thought this myself when I get in a gloomy mood--"How can I possibly endorse that I was involved in giving this opinion to the Commission for them to promulgate?"

I look at that and I say, "How can I believe our society can do that?" But I am old enough I remember when all my classmates were falling at the wayside from polio and the March of Dimes was going to cure polio. That sure looked like a bigger problem to me that getting a high-level waste repository.

That easily worked in my lifetime, and what used to be a terrible disease is no longer one.

I was involved in the space program, and that was a major technological thing. We could accomplish a great deal there; and in that there was a lot of institutional opposition and institutional problems, and there still is today.

It basically boils down to a judgment. If you look at the technical facts, is that the obstacle? Can one point to difficulties with the geologic media, difficulties with the waste package, difficulty with how to drill a hole or an entrance ramp as the critical path as the real problem? They are not.

The critical path for the development of a high-level waste repository in this country is institutional. And so, the judgment that one makes is going to come again and again to that; and the comments and the criticisms will come again and again to that point.

They are not technical issues. It is not because we discovered that carbon-14 leaks out of tuff, or may leak out of tuff depending on the model you use. No. It is that you may not even be able to investigate the site, or you may not even be able to investigate the eastern repository sites.

DR. CARTER: How many current reactor sites now are storing spent fuel for multiple reactors?

MR. BERNERO: The only sites that are storing spent fuel for reactors other than--

DR. CARTER: Their own.

MR. BERNERO: --the reactor on the site can easily be numbered.

First of all, there is a private MRS facility: the General Electric Morris plant, which is an old reprocessing plant that ended up storing 700 tons of spent fuel that belongs to a number of other reactors, mostly very old reactors that had contracts for reprocessing.

DR. CARTER: I presume its capacity is full.

MR. BERNERO: It is full. It is a water pool. It is right across the road from the Dresden Nuclear Power Station in Morris, Illinois; and it is full.

In the southeastern United States you find two utilities which own multiple sites: Carolina Power and Light, and Duke Power Company. They are adjacent utilities. They have both used a system approach to the optimization of spent fuel storage and handling.

They have shifted spent fuels from one to the other of the sites they own. In particular, Carolina Power and Light has shifted between the Brunswick Plant, which is on the ocean at Wilmington, North Carolina, and over to the H. B. Robinson.

They had authorization to shift to Harris. I don't know if they have done it yet; but they have three plants: Sharon-Harris, H. B. Robinson and Brunswick. They even own their own rail cask. They now have dry storage licensed at H. B. Robinson.

It is a real checker game of how they are managing their spent fuel, but basically you can treat them as a system: Carolina Power and Light's three sites. Similarly, the Duke Power Company has a system approach for McGuire, Catawba and Oconee; and, now, Oconee has dry storage licensing.

Those are the only ones I know of.

There is another consideration. I believe the Shoreham fuel may be shipped to another site because Long Island Lighting Company owns a large part of Niagara-Mohawk's Nine Mile Point Unit 2; and they are similar reactors. They may be able to use the fuel.

DR. CARTER: Is there any prohibition as far as licensing is concerned against companies exchanging these fuels or storing it? Could Duke Power, for example, store fuel for Carolina?

MR. BERNERO: Legally and environmentally in licensing space, yes, they can certainly do it; but lots of luck.

The Rancho-Seco people, the Sacramento Municipal Utility District, has whistled in a few ears to see if they could find space. They would just like to get it off their site. To my knowledge, no one has offered yet.

It is an institutional burden that a utility taking a systems approach can handle their own. General Electric took that only because they had contract obligations. They certainly were not interested in the storage business.

DR. CARTER: In 10 CFR 70, part of the title refers to independent storage. What does the "independent mean"?

MR. BERNERO: The original intent of that regulation was that it would be independent of the reactor facility itself. To put it simply, when you go to a nuclear power reactor the spent fuel comes out of the reactor under water, because it is very hot and you don't want to hang it in the air for more than a moment, and it is transferred by manipulators, racks or some mechanical devices in the water shielding, and cooling, to storage places in a spent fuel pool. It does not go into a cask.

An independent spent fuel storage facility is one to which the fuel comes in a shipping cask. The shipping cask may just go across the yard, but it is nonetheless a shipping container. It is a movement of the fuel out of this water environment of the reactor itself and its water pool into a container for transfer to an independent facility which, itself, could be water shielded, like the General Electric Morris facility is, or it could be dry.

DR. CARTER: But at a different--

MR. BERNERO: Yes, but at a different location.

DR. ALLEN: If, in fact, we did fritter around so that we are approaching 100 years and still trying to get rid of this stuff and, at that point, we finally decided that Yucca Mountain was suitable, at that point would there still be a sufficient proportion of young fuel so we could attain the temperatures that, at least currently, are envisaged as necessary for the performance at Yucca Mountain?

MR. BERNERO: To attain the temperature? I think it would more likely be that it would be less than the design temperature. The fuel would be a lot older than the design basis.

DR. ALLEN: The current idea, as I understand it, is that it would be well above the boiling point with some good reasons.

MR. BERNERO: Yes.

DR. ALLEN: Could we still attain that?

MR. BERNERO: Yes, but you would not seek to attain that. You would just seek not to exceed it.

Older fuel is better. It is cooler.

CHAIRMAN DEERE: I think that is true, but in the current design they want to maintain the temperature above boiling point for 300 years, minimum.

MR. BERNERO: To keep the water away and--

CHAIRMAN DEERE: Right, to keep their water away. This creates other problems that people are looking at.

MR. BERNERO: I get the thrust of your question.

CHAIRMAN DEERE: That was the thrust.

I think somebody gave me the figure--you can correct me--that if the fuel is an average of 80 years old it will not attain boiling temperature. I think this was the figure.

MR. BERNERO: If this program dawdled and dragged on for years and years yes, indeed, that peculiar aspect of the design would not be attained. Many people are arguing you should just lower the packing density and plan for that.

DR. CARTER: Bob, I have one other question, maybe a difficult one.

What impacts, in your opinion, does the Waste Confidence Proceeding results have on a schedule for a high-level waste repository? In other words, if we can store it for 100 years why build a repository? Let's wait a while.

MR. BERNERO: That came up in the hearing last week in the Senate. Senator Simpson frequently voices the opinion that storage is an imminent hazard: that it is dangerous.

I strongly disagree with him, and we looked at that very carefully.

In a sense, it takes some of the pressure off; but at the same time that one can legally, in a proper environmental Federal context, make a Waste Confidence Proceeding and justify license extensions or amendments, the political process in our country often works by different ways; and the ballot in the State of Oregon is a good example.

I suspect, certainly the polls now are saying, that the voters will vote to shut down Trojan. Whether or not we have a Waste Confidence Finding is not going to affect their voting. If they vote that way, I don't think it will affect the outcome either because now you have the political process at work.

Will the Portland General Electric Company continue to operate, even though they have Federal authorization, if the voters have voted--one poll I heard was 2 to 1--against continued operation?

The political process of the country will control.

This is certainly an important Finding. It is something we have to do if we are going to do our job and do it responsibly within the law; but I don't see it as persuading the Congress to set the whole program back a generation or something like that.

I think the Congress in the past, if anything, has gone in the other direction. They almost want to get the repository by force: focus on one site, and go do it by rigid schedule.

DR. CARTER: If you had continuing delays, of course the reactors are continuing to produce used fuel. The question is: How long could you delay this and still make the assumption that only one repository is going to take care of the wastes?

MR. BERNERO: At the hearing in the Senate last Tuesday a representative of industry made the comment to the effect that no new reactor or significant change in reactor programs can be expected unless the solution appears more likely: that it is not going to be politically or technically acceptable to the utilities.

I forget his exact words, but it was something to that effect: that unless there is a repository there will not be another generation of reactors.

CHAIRMAN DEERE: I think I see on the slide you have there the Nuclear Regulatory Commission staff has no basis for decreased confidence in technical feasibility.

MR. BERNERO: Oh, yes.

CHAIRMAN DEERE: You have been emphasizing the problems may well not be technical, but institutional uncertainties.

MR. BERNERO: All we can do is look and ask: Is it reasonable to say we are confident? You can argue about Yucca Mountain for 10 years and then take another 25 years to find another site.

I think that is certainly a very pessimistic assumption. And, can it be safely handled in that context? I think the answer is an overwhelming yes.

But, again, it is the institutional issues. The technical issues have not surfaced as the critical path. There are rich important technical issues to be addressed in this program, but they are not the impediments, they are not the roadblocks.

CHAIRMAN DEERE: Yet, again, many of the technical questions have been taken as institutional reasons. In other words, the technical questions have been answered in the negative and then used as a reason why a site would not be suitable.

MR. BERNERO: Yes. This is the argument Nevada makes with Yucca Mountain. They go, really, into 10 CFR 960, the site suitability, and argue that the Department of Energy's own regulation says these are disabling features: that they should not be seeking to develop the site. It is not suitable for site development as distinguished from it is not licensable.

I think John Bartlett also said, Tuesday, he has a definite response to the State of Nevada on that issue. I have not see it yet.

MR. ROY WILLIAMS: I did not understand your answer to Don Langmuir's question and I think it deserves pursuit.

Exactly what geological technical issues did you consider to reach this reaffirmation?

MR. BERNERO: We looked at the media that had been evaluated in the past--and you know the spectrum of media--and we looked at the media that are currently being evaluated, not just tuff, and the assessment thereof--we are deeply involved in the idea of performance assessments to determine whether or not repository performance is acceptable--and the range of issues that are available in that: the fracture flow and so forth.

I am not sure how to answer you beyond that point.

We are not Finding an acceptable site. We are trying to ask: Is there something in the technical feasibility in geologic media that undermines the scientific expectation that one can find an acceptable medium at an acceptable site?

MR. ROY WILLIAMS: What kind of conditions did you look at?

MR. BERNERO: I think I am going to have to send you the analysis. I am not sure what you are seeking.

Did we look at fracture flow or ground water transport in an unsaturated medium?

MR. ROY WILLIAMS: Did you?

MR. BERNERO: That is certainly an issue we are looking at. I don't see how that would enter into the Finding of: If it is unsaturated, therefore it cannot be an acceptable medium. It may be a difficulty with the medium, but not with geological disposal as such.

I was trying to make the distinction that we are not trying to find and say anything even remotely like Yucca Mountain is the basis of confidence whether or not Yucca Mountain is unacceptable. It may be unacceptable because tuff is unacceptable: that it is too fractured, you cannot predict the ground water transport, and a whole bunch of reasons like that; just tuff itself.

Then it may be that that site is an unacceptable site for tuff. It may be the wrong place. The Calico Hills may swiss cheese or the ground water table pumps up and down too much, or the vulcanism or seismicity are too threatening.

We are not trying to find acceptability of a site. We are trying to address the concept of geologic disposal. Is there some information that has developed in the last five years that undermines the basis of confidence that says geologic disposal is technically feasible?

It is almost like proving the negative.

DR. PRICE: On your Finding Three, that the Nuclear Regulatory Commission finds ". . . reasonable assurance that high-level radioactive waste in spent fuel will be managed in a safe manner," did you have any difficulty with respect to that Finding regarding the issue of whether or not there is an overall system integrator or system manager that cuts across institutional barriers and institutional lines?

MR. BERNERO: No, but we talked about that a good deal. In fact, in two respects we talked about that.

One, it has been assumed for a long time that the MRS program managed by the Department of Energy would become the system manager in the sense of taking possession FOB your loading dock of all the spent fuel by date certain, and then having an integrated program to collect it, store it, package it, do whatever is done with it.

That is not working out institutionally. We have considered individual reactor owners treating their own plant or, as in the case of the two reactor owners I mentioned each of which has three sites, having a smaller system management with a narrower horizon: just safe storage.

The secondary aspect we looked at in that was: Is there a system criterion that would mandate, perhaps, or militate toward: You ought to have compatibility between your storage systems and your transport systems, and for that matter your disposal systems.

There is a concept. If you go to the Oconee Nuclear Plant you find they are licensed to dry store fuel in a stainless steel can that holds 24 fuel assemblies. It is certainly a tantalizing prospect to go in the reactor pool and put in 24 PWR fuel assemblies, seal-welded, inert gas--nice and dry, and all that, pick up that canister in a shielded shell, take it out in the yard, store it in a concrete bunker for 100 years, and then have a railroad car that is a hollow shell: slide the canister into it, close it ship it to a repository, and put it in a hole right in the repository.

It is a very attractive prospect, but the system engineering, the system management that goes toward taking all of those things into account at the front end before you license the storage is not done.

However, what we have looked at is: What am I losing in radiation safety, in risk and in cost by not taking that system management into account?

We have done that and it is not a whole lot. It is pretty hard to justify that compatibility because of the radiation exposures involved and the costs involved with saying this canister is good only for storage.

Those costs are not that high. Therefore, it may very well be, even by good system engineering, uncoupled from the requirement.

DR. PRICE: Is there a report on this?

MR. BERNERO: We did a Commission paper about a year, year and a half ago right at the beginning of this mode of storage. I could check and see what we have on that.

DR. PRICE: I would sure like to see it.

MR. BERNERO: It was done in conjunction with the code licensing primarily. One of the reasons we were looking at it: I mentioned that Carolina Power and Light owns their own shipping cask?

They have that same dry storage concept in a smaller diameter at the H. B. Robinson site and they use the rail cask as the shipping shield. They slip the canister into that, move it across the yard and pull the canister out.

So you already have apparent compatibility in that one case.

DR. CANTLON: Since it is the institutional problem that is the problem, not the technical one, the absence of this total systems approach to it is the public's concern. They don't see the system operating in a perceptibly safe way.

Maybe that's part of the difficulty we are having that some of the other nations who have looked at it in a somewhat more ordered, systematic way are avoiding.

MR. BERNERO: If you would say that, the system of storage is the same here as it is in Europe. We are a little farther ahead because we have more spent fuel. We are in dry storage whereas the Europeans have not done that. The Germans are more or less into something similar to it.

The system of transport is the same. The only difference there is, once again, the Germans are willing to certify nodular cast iron and we will not. The system of disposal: their programs are on no earlier time scale than ours, some later. Most of them are now more or less the time scale as the Department of Energy proposes.

I do know the public perceives transportation as a horrendous risk. The British spend 2 or 3 million pounds to destroy a train. They got the Flying Scot going 100 miles and hour and hit a cask. I asked them before they did it, "What are you going to prove? We have gone that route in the United States." They put on television, and it did not persuade a soul.

If you put a radiation propeller on anything it creates fear and it is an almost hysterical reaction, like irradiating food. 50 percent of the chicken in the United States is contaminated with salmonella and you have to cook it out. Probably--I am just guessing--a third of our 24-hour flu bugs are salmonella poisoning, and yet if anyone proposes to irradiate chicken to kill the salmonella you will get shot.

DR. PRICE: The future looks as if there is not immediately available an MRS and a repository that the amount of handling that is going on, such as you described by South Carolina Power and Light and so forth, is going to be increasing; and that there will be shuffling back and forth to find room and find space, and such things, that without such a systems integrator the overall view is one of some unnecessary handling which can be perceived by the public as increasing the dangers of this: the more you handle it, the more chances you have for accidents.

MR. BERNERO: That is the thing we looked at: the unnecessary handling that we postulated was going from the spent fuel pool into the dry storage, and then having to come back to the spent fuel pool for transfer to a shipping cask; and then the shipping cask going the way it would go in any event.

What we analyzed was that unnecessary step. We did a close look at the time study. How many millirem minutes do you get doing this or that step of seal welding and transfer; and it is not that much.

DR. PRICE: Did you assume that no accidents or incidents could occur?

MR. BERNERO: Oh, we looked at accidents, yes.

We are talking about heavy equipment handling in an optimized fashion. This has been done for many years. The equipment is redundant and the shielding is taken into account. The whole process, as low as reasonably-achievable doses, has been followed.

As a result, we have a pretty straightforward set of activities: very well understood, well practiced and no big risks.

DR. PRICE: No spent fuel assembly hangs up on another while you are drawing it out?

MR. BERNERO: Oh, no. It happens. In fact, right now the Indian Point 3 reactor had a problem that some reactor internals were pulled out. Guess what? Two spent fuel assemblies were hanging under it.

DR. PRICE: That is why I mentioned this.

MR. BERNERO: That happens. They have dropped. Spent fuel assemblies have dropped.

Basically the accident mode when you drop a spent fuel assembly, the mechanical damage can breach the cladding and cause the gap activity to come out. But typically you are dealing with the fuel is either in the water all the time or it is just briefly above water. In fact, in all the plants I know of it is always in the water so you get a natural scrubbing from the water. Fuel-handling accidents are relatively low in consequence, and the buildings are also filtered just for that reason.

DR. PRICE: I think we would very much like to see that study you are describing if you could get hold of it.

MR. BERNERO: Okay. I will get some material together. All the fuel-handling accidents are treated in the reactor licensing in the first place. They are postulated accidents.

DR. VERINK: I was intrigued by your comment about the contrast in views between the Nuclear Regulatory Commission and the Germans with regard to nodular cast iron.

Is that information available somewhere?

MR. BERNERO: Yes.

DR. VERINK: What is the basis for that?

MR. BERNERO: It is a point of some difficulty. There are international standards for the shipment of high-level radioactive waste: spent fuel and the like. The Nuclear Regulatory Commission certifies casks and we conform with international standards.

The casks are generally designed to be so robust that there are no abrupt failure mode, and the failure mode of concern is leakage: a slight breaching of a seal or radiation leakage, leakage of contaminated water, whatever.

The introduction of nodular cast iron was proposed by the Germans quite a few years ago as a compatibility concept: a cheap cask that could be used for shipment and for storage, and possibly even for disposal; and because of its low cost nodular cast iron would be desirable instead of the alloy steel previously used.

The alloy steels previously used have elongations before fracture of 20 percent, 30 percent one can reasonably say. That is ductal steel.

How ductal is nodular cast iron? You go to the Black Magic and you can get 5 percent or 8 percent. The question of failure mode exists, and it is a very difficult one: Does that level of ductility, that reduction in ductility introduce what I would call a shattering or catastrophic failure mode as a consideration?

There is no question the nodular cast iron can pass the letter of the law. It can pass the impact tests and the puncture tests and the fire tests and the beat-up-tests and drop-it-on-the-end, and all that. But the question is: Do you have the same margin of safety that you have with a structural alloy that has 20 or 30 percent elongation?

DR. VERINK: Have you some reason to believe it does not?

MR. BERNERO: Oh, yes. Because the elongation just is not as good. There is an evident difference, and the difficulty is: What is the basis of judgment to say, "I am willing to give up that margin"? Is that an ample margin of safety? That is really the argument.

DR. VERINK: Is there any kind of a report or document that we could look at.

MR. BERNERO: Yes. I will tell Charles McDonald in our Transportation Branch of you interest. Leon knows the man.

MR. VERINK: He has a copy of it?

MR. BERNERO: Yes. This is a long-standing very sensitive argument.

Mind you, here is an arena where we are talking about technical issues the public does not perceive at all. If a truck carrying 8,000 gallons of Shell gasoline comes by their home while their kids are playing and Husband George is smoking a cigarette in the yard, it does not bother them in the least. But if a truck, whether it is stainless steel or nodular cast iron, that has a radiation propeller is in the same situation, it is a source of panic.

The public perception goes beyond any such technical consideration.

DR. PRICE: Is there a suggestion here that for some of these specific considerations the tests are not adequate since it will pass all the tests?

MR. BERNERO: Yes. See this has been a long-standing argument in transportation safety.

Transportation safety is a classic definition of deterministic regulation. The design basis is very simple deterministic tests: a 30-foot drop on an unyielding surface; a drop onto a puncture member of such and so size, so many inches long; a fire for 30 minutes at 1,475 degrees

Fahrenheit; et cetera, et cetera.

These are deterministic arguments or tests that many members of the public have challenged. "Gee, I drive down a road where, if you fall off the side of the road, you can fall more than 30 feet"; these interchanges that go layers high.

As a result, we have done risk assessments of transport risk taking into account the full spectrum of possible conditions in the world. Time after time we have found that devices tested, developed and certified against those deterministic standards have an abundant margin of safety for the realistic spectrum of environments you see in the world, even the worst-case accidents.

But included in that is an insight into the value of 30 percent elongation in a steel. You can learn something from the risk assessment, if anything, one can ask the question: Should I change the regulations and add to it that the elongation of the alloy in question, before fracture, shall be greater than, I will say, 15 percent or 20 percent, or some number.

That is certainly a valid consideration.

DR. BARNARD: Could you briefly describe a bit about the process involved in getting Waste Proceeding out? Is this in terms of time and manpower?

MR. BERNERO: Yes. The Waste Proceeding is a policy statement or a rulemaking procedure whereby the issues are posed, a staff group--in this case it was a mixed group from the rulemaking office, which is the Office of Research, from the General Counsel's Office and from my office, Nuclear Material Safety and Safeguards.

The technical analysis was put together to consider and to modify, as appropriate, the findings. Then that went to the Commission. It was proposed to the Commission that: Here is what we ought to do in 1989 subject to alteration and modification by the Commission. It was published as a proposed Waste Finding just like a proposed rule. We have done that in the past on policy statements, too.

The Commission puts it out as a proposal in order to elicit public comment on the rationale, on the scope, on the justification, on the findings themselves. We get the public comment and then go back. The same team reconsiders the whole matter in light of the comments, and any events that may have ensued in the subsequent months, and then it goes back with a final version of it to the Commission, just as we would with a rulemaking.

The whole process took about one year because I think the final publication was just last month in the Federal Register. I have a copy of it if you want it.

CHAIRMAN DEERE: The Board members have a copy.

MR. BERNERO: We started it fairly early in 1989.

It was a little over a year. But we tried to get the proposed version on the street in 1989 with the expectation it would be into 1990.

As far as the resources, it is hard to say. I don't know an exact count of what it took. I just could not answer the question.

DR. BARNARD: Will you do this again in five years?

MR. BERNERO: No. You will find, in the fine print, we said ten years. If we are going to think in decades, we are going to think in decades.

The five years was an unduly strict thing considering the previous finding took five years to develop. The lawsuit was in 1976, the court remanded it in 1979 and the finding was in 1984.

CHAIRMAN DEERE: I believe we would now like to open up the session for questions from the audience.

DR. REITER: I was just glancing over the Federal Register and I think you alluded to the question both Don Langmuir and Roy Williams asked: Did the proceedings look at the concept of the technical feasibility of repository geological aspect?

If I understand the Federal Register--I think you alluded to this,--the way it interpreted it was: Is there an acceptable site for a repository? I think the words were that the technical feasibility of a repository rests initially on the identification of acceptable sites.

The reason, if I understand it correctly, that they still supported that was the fact that because of the work the Department of Energy has done at other sites, even if Yucca Mountain proves unacceptable or unsuitable, there still will be other sites available.

MR. BERNERO: Yes. In essence that we have not had a pattern of scientific development in the last five years, now six years, that says the potential media are falling by the wayside--that now it is not feasible to consider this full spectrum of media, that only half or one-third of them may quality--that would be a signal.

If we were developing evidence in our program--and we follow the European and other nations' programs quite closely--that suggested that a range of media were not available. But, again, we are being very careful not to tie to the Yucca Mountain tuff bandwagon--

DR. REITER: Yes, that is very clear.

MR. BERNERO: --because that is not a basis for confidence.

DR. REITER: I have two other short questions.

Given the fact that they expect a lot of plants to come in for license renewal, would this automatically indicate that a repository with a 70,000-ton limit would not be sufficient?

MR. BERNERO: Oh, I think that is fairly evident now.

Right now in the United States we have about 20,000 metric tons of spent fuel on hand. If you project the operating life of all the operating reactors over their licensed life span, I think you will get 70,000 tons more or less. If you have any life extension, you would go over that.

There is one thing a lot of people forget about. Under the law high-level waste belongs in the repository. The Department of Energy is responsible to put high-level waste there.

If you go up to the Hanford Reservation, there is an old reactor up there, the N reactor, which shut down after Chernobyl. You will find in storage up at Hanford 2,000 metric tons of spent fuel from that reactor. It was used to generate electric power for the Washington Public Power Supply system.

That goes in Yucca Mountain, by law. If it is reprocessed, the reprocessing waste goes in Yucca Mountain. All of the high-level waste tanks at Hanford, the high-level waste tanks at Savannah River, the two high-level waste tanks at NFS West Valley, and the high-level waste from Idaho--these are all defense system or other high-level waste--has to go in Yucca Mountain or wherever.

If you go after the miscellany that people don't normally think of or talk of, you are going to eat up a lot of the capacity of 70,000 metric tons; and I don't know how much.

DR. REITER: Is it evident at this point that a second repository will be necessary?

MR. BERNERO: I think so. I think certainly license renewal makes it.

It is possible that 70,000 tons may require a second one or a modification of 70,000 tons because it is a squeeze.

DR. REITER: Is the independent spent fuel considered an MRS in a legal sense?

MR. BERNERO: Yes. The original license at General Electric, Morris was under Part 50 of the regulations. The license was modified some years ago to be under Part 72.

When we license dry storage--like at Oconee or H. G. Robinson--those are licenses issued under Part 72, ISFSI; and the MRS, if and when the Department of Energy chooses to build one, would be licensed under Part 72, also.

DR. REITER: There is a part in the amendments that says you cannot build an MRS: the MRS is linked to--

MR. BERNERO: Oh, yes. That is a programmatic linkage.

DR. REITER: Is the building of additional ISFIs also linked to that?

MR. BERNERO: No, it is not. Only the MRS with capital letters, the Department of Energy's facilities.

DR. REITER: Theoretically one could build additional--

MR. BERNERO: Oh, yes.

DR. REITER: --spent fuel storages?

MR. BERNERO: They are doing it. We have a line of applicants now--we are reviewing and licensing dry storage--which are "mrss" with lower-case letters. They are individual reactor owners.

MR. TOURTELLOTTE: I would like to follow Leon's question with one pertaining to the plant life extension or license renewal.

Don't you think there is a strong likelihood that if there is not some solution found within the next decade relative to the disposal of high-level waste that could adversely affect the licensability of extension of licenses for existing plants? If not, why; and what can be done about it?

MR. BERNERO: To paraphrase your question, if a solution on high-level waste is not more evident in the near future will not this inhibit the granting or consideration of plant life extension under the licensing procedures we are talking about at the Nuclear Regulatory Commission?

Yes, it can certainly inhibit it. I would say that having heard what the gentleman from Florida Power Corporation said at the hearing last Tuesday new reactors are certainly not going to be forthcoming unless some solution is in the offing. I think that is probably true for a substantial amount of plant life extension.

However, that is speculation on my part. The problem with plant life extension is: If you own a reactor there is a date certain by which you have to consider plant life extension because your license is going to expire in the year 2007 or something like that.

That means that if it is 2007 expiration you better have your ideas all sorted out by 1997; and that is not very far off. I think that is a real problem for utility people to decide whether or not they will go for life extension.

I am certainly not in a position to say what might be done about it. I would just say the program on high-level waste should proceed in an orderly fashion. We are trying to do what we can to see to that.

I don't see anything else we can do.

MR. TOURTELLOTTE: One of the points of my question is that in order to get to the point where license renewal requires added storage area one has to assume that those license renewals can actually take place.

I am suggesting that if the high-level waste issue is not solved within the next decade that may not happen. Consequently the question Leon had asked earlier would not really come to pass.

MR. BERNERO: Let me make a contrast. You used the word "storage".

Adequate storage for existing plant life as well as life extension is already available. That is the temporary wet storage and dry storage mixture. It is disposal I was speaking of to Leon.

That is a nominal disposal capacity arbitrarily selected. If Yucca Mountain is acceptable it may not even hold 70,000 tons, depending on the packing, density and whatever.

MR. TOURTELLOTTE: I actually mis-spoke. I really meant disposal as well.

MR. BERNERO: I don't see additional disposal capacity, a second repository in other words, as being on the table for some time to come.

MR. TOURTELLOTTE: My second question relates to an issue which did not appear on your slides, but it was one which I think came up recently at the meeting at the National Academy.

There is a certain degree of uncertainty about the establishment of regulatory standards and the achievability of those standards which have been promulgated to date. You did not mention those today.

What, if any, effect does that have upon the confidence of the Commission, and what effect does it have upon the overall program in your view?

MR. BERNERO: First of all, I think it would be worthwhile to make a minor clarification or call for clarification in that area.

I have heard many people speak of undue stringency in standards. I wonder how many of those people have looked closely at what the actual calculations and the actual standards are.

In simple terms, if you are trying to assess the performance of a geologic repository the first and simplest calculation you can make is: From the physics, chemistry and the like of package waste form corrosion and transport in the geologic medium, you can calculate the release of a burst of a puff of radioactivity over time scale measured on a logarithmic scale, time scales of thousands of years, where an increment in the analysis is a human life time. A delta X is a human life time.

You don't have a great deal of precision in this because of the uncertainty about exactly how big the plume is and the fracture characterization, and so forth.

A second level of calculation--much less certain, much more difficult--is to hypothesize a biosphere above that geologic repository. That biosphere is not today's or tomorrow's.

It's the biosphere one might expect two ice ages hence or four ice ages hence. Then you are trying to calculate the transport of this stuff into humans living in that biosphere in order to calculate person-rem.

You have a spectrum of people who might live there; you are smearing the average out; and what you are calculating are health effects using the linear hypothesis.

The last and least certain of all is to be so precise that you will go in there and locate the Jones family in their farming and cultivation and food habits, and calculate individual annual doses.

You will have people say the standards are unduly stringent. Are they unduly stringent because the source term is unduly conservative with respect to the next stage? The next stage is unduly stringent with respect to individual dose uncertainty?

Or are they unduly stringent because people are inherently using too strict an individual dose: they are using one millirem a year instead of 100 millirem a year?

Or, last and far from least,--listen carefully--are the standards unduly stringent because someone is saying "I will protect all future generations": that, that is the design objective?

The achievable standards, the consensus of society in establishing these standards in the first place and what has been done is that a reasonable assurance of protecting the public in the future from receiving a radiation dose higher than we would tolerate today can be reasonably assured by some calculational mechanism by selecting a suitably remote geologic site and characterizing it to some sufficient degree.

I think we have debated that issue separately. We are still active with the Environmental Protection Agency. At the work shop we talked about it. We even talked about it further in the hearing last week.

We are in the next round of comment with the Environmental Protection Agency right now on that point. I think that is a solvable problem; but it would help.

If one speaks of stringency, let's be specific. Where is it unduly stringent? What, exactly, does one mean.

MR. TOURTELLOTTE: I would make one point. I did not use the term stringent.

MR. BERNERO: I know you did not.

MR. TOURTELLOTTE: However, the ACRS has used the term since 1980, and the ACNW continues to use the term.

MR. BERNERO: Oh, yes; and many others.

MR. TOURTELLOTTE: The real issue that I wanted to raise was that it is a fairly widely-held view in the technical community that the regulatory standards, quite reasonably, might not be achievable because of the way they are stated.

It seems to me that has everything to do with whether or not we can site a repository and can operate.

MR. BERNERO: The comments we have made in the past, the Nuclear Regulatory Commission has made to the Environmental Protection Agency and made available to the public, have challenged the Environmental Protection Agency standards on its implementability. Great changes have been made.

Subject to further comment we made recently--Bob Browning wrote a letter to Rich Guimand two months ago with our most recent comments, and we are talking to the Environmental Protection Agency on that subject right now--we think the system is usable and doable.

I don't subscribe to the view that we have an uncertainty. We have no way to show acceptable isolation. That would be, indeed, a disabling uncertainty.

CHAIRMAN DEERE: Thank you very much, Bob. We appreciate your answering the questions and your presentation.

We will now step up our break. Mr. Shaw would you like additional time for your presentation or is 45 minutes sufficient this morning. That will determine if we return at

11:00 or a little earlier.

MR. SHAW: Why don't we start at 10:45. I could use the extra time if you would like to do that.

CHAIRMAN DEERE: Fine.

Please be back here at about 10:45.

[At 10:23 a.m., the hearing was recessed to reconvene at 10:48 a.m., this same day.]

CHAIRMAN DEERE: May we reconvene, please.

Our second presentation today is by Mr. Robert A. Shaw, who is the Senior Program Manager, High-Level Waste Program of the Electric Power Research Institute. His topic today will be the Overview of the EPRI/EEI High-Level Waste Repository Methodology.

Mr. Shaw?

OVERVIEW OF EPRI/EEI HIGH-LEVEL WASTE REPOSITORY METHODOLOGY

Robert A. Shaw, Senior Program Manager, High-Level Waste Program, Electric Power Research Institute

MR. SHAW: Thank you, Don, for the introduction.

When I had the opportunity to speak with you last December things were a bit different than they are now both in what we have done and in the industry situation. I might spend a few minutes reviewing the situation that existed then.

A few years ago the utilities got noticeably concerned about the progress or lack of progress about the

Department of Energy program on the high-level waste facility. Nonetheless as we entered a seemingly new period with First Admiral Watkins and, subsequent to that, John Bartlett coming into their present roles the utilities withdrew, at least a little bit, and said, "All right, let's give an opportunity for this whole process to occur to see if maybe we cannot improve the situation and get more progress than we have had in the past."

At about the same time, ACORD--the American Committee on Rad Waste Disposal, which is a utility organization of utility executives which establishes policy for the variety of utility organizations regarding rad waste disposal--and EEI suggested that the Electric Power Research Institute might conduct some research that would enhance the Department of Energy's program.

As I mentioned to you when I spoke here last December, we had a research program in the seismicity area which was concerned with east coast earthquakes, in which we made significant use of expert judgment in order to come to some opinions and determinations, uncertainties, predictions, et cetera, with regard to the likelihood and intensity of earthquakes on the east cost so nuclear plants could submit license indications for whether or not they were sufficiently protected from such earthquakes.

This process was accepted by the Nuclear Regulatory

Commission as an appropriate way to deal with some of the changes and the concerns regarding east coast earthquakes. That whole process worked quite well: a process in which expert opinion was pulled together, consensus was determined, models were developed and decided upon, and uncertainties were used.

It seemed to us to fit very nicely within the purview of the whole question of the high-level waste repository.

So on that basis we moved ahead. A the time I spoke with you last December we had just begun he whole process of collecting together an expert team and putting it together in order to fashion a methodology for the analysis of the high-level waste repository.

We had as our objectives at that time--and have continued, through the process, to have as our objectives--first to develop an integrated methodology for early site performance assessment, and to identify and prioritize crucial issues.

The essence is that we would like to have sort of a quick and dirty overview that says: What does our analysis show with regard to a performance assessment for Yucca Mountain? And, particularly, what does it show with regard to the key crucial issues and how you first identify those crucial issues, technical issues; and how do you prioritize those?

I think you will see, as I go through today, that we have a process we believe can do that. We have a framework within which these kinds of calculations can be carried out. I will show you towards the end how we would suggest that prioritization can take place.

Secondly, we wanted to involve the Department of Energy in this methodology development and its implementation. I will return to comments on that as we proceed through our discussion here.

The first step in our process was to assemble a team. I have listed the names of the people here who are on the assembled team.

Our first, possibly naive, approach was to say: Let's collect together experts in various areas which have not been involved with this program so we can have an independent judgment.

But it soon became clear that was really a sacrifice that was not necessary; and, in many cases, it was inappropriate, particularly when you come to issues like waste package.

You will notice also, as you read down the list, particularly with regard to the expertise, that in most cases we have only one expert for each particular area. As we go through this I would like you to reflect upon the fact that the calculations are meant to be illustrative.

This is not a technical consensus wherein we say the parameters and the values we have selected are those that really depict the performance assessment for Yucca Mountain. We tried to get values and scenarios that are reasonable so that, as we look at the overall performance assessment calculations we have done here, one can say, "Yes, that makes sense."

We are not saying the conclusions we have come to are definitive, but rather that there are more steps that would be required, in particular collecting together a larger number of experts in the particular areas where there are questions.

There are the three people from the Electric Power Research Institute--myself, Carl Stepp and Bob Williams--who have been involved in this, who are listed on this. I would also like to point out that the last person on the list, Russ Dyer, is at the Yucca Mountain project office and has responsibility for performance assessment.

Mr. Dyer attended all of our meetings and has been a very valuable participant in this whole program. So that is certainly the first step in us getting the Department of Energy involved in the activities we were conducting.

We had a series of meetings with this group. We started way back, July in 1989, with a brainstorming session at which we said, "Will this whole process work? If it works, how would you conduct it? How would you carry it out?"

So we laid the ground rules at that point. That is where we decided that a group of the nature and rough size of the one that was depicted on the previous viewgraph would be appropriate in order to carry these out.

We had what we call a qualification check in late November where we brought these people together and said, basically, "What is your attitude? How do you feel about probablistic versus deterministic approaches?"

We wanted to check and see if these people integrated in a reasonable fashion. Will they talk with each other? Were they open to new ideas? So on and so forth.

In other words, we wanted to develop a team that would work together even though it would only meet in a limited number of opportunities. Having fixed on that team, we got the team together in late December, a little less than a year ago. At that point we defined the problem.

We did so using such things as logic diagrams, influence diagrams. We tried to, in a sense, brainstorm the technical features that were vital and important as a part of a whole performance assessment that would take place.

Then we met together in January and each of the people came back and said, "Okay, in your area, hydrology, geochemistry," be what it may, "you are responsible for your area. Tell us: What would your model be? Describe the essence of a model for Yucca Mountain surrounding your particular technology; and, as a part of that, tell us what are the inputs that you require in your model from the other participants in order to affect calculations within that model."

At that point, that was sort of the outline of our model formulation. In between these times, people worked on their models, came back with their results and, for the first time in April, we integrated that model and actually did a presentation of the model as a result of the integration of all these various technologies that went into it.

Of course, we found holes and difficulties and areas where there were inconsistencies and so on, and results that did not make sense. As a result of that we went back and worked again, and we collected together again then at the end of July where we completed that model; and it is the results of that, that you will see here today.

As a result of that model completion, we had an opportunity on August 1 to make a presentation to the Yucca Mountain Project Office, members of the Department of Energy and a number of their national laboratory contractors. In addition, of course, one of the members of the Technical

Review Board and a staff member were present for that presentation.

In addition, subsequent to that, in early September, we had an opportunity to present the same presentation to a headquarters group here, to a couple utility groups, and subsequent to that to the ACNW. It is now, hopefully, refined where we are ready to give it to the superior group: the Nuclear Waste Technical Review Board.

CHAIRMAN DEERE: Prestigious.

MR. SHAW: Prestigious, okay.

[Laughter].

MR. SHAW: The basis for our methodology development is a logic tree. I present here a logic tree diagram. You will excuse me if I seem a little elementary in going through this, but I think it is important to go through the structure of a logic tree and how it works, and what it does and what it does not do.

If we look at the structure of this logic tree and we say, "There is some kind of an external impact,"--this could be climate change, geology change, so and so forth--and out of that we say, "There are two different events that could occur," of course there is a whole range, almost a continuum of events.

One of the elements of a logic tree is that you have made the decision that you are going to take this continuous system and put it into discrete kinds of events. In this particular case we have chosen two. Obviously, there could be as many as you wish, almost an infinite number, of different choices out of here.

As you do that, you go from the discrete more and more towards the continuous. We have chosen two here. The P11 and P12 represent the two probabilities of those events occurring. Of course, these must sum to one because we are saying either one or the other of these events occurs.

Subsequent to that there could be, for example, a source term result from that in which there were to different possibilities; and, of course, this source term could depend very much on what the particular interaction was.

So there is a dependency that proceeds along this path with events to the right being dependent or at least having some dependence on events to the left. Therefore, the dependency occurs in this direction.

Of course, each of these paths, as you follow through, would describe a particular scenario that takes place with the release of radioisotopes; and a subsequent end up here would be, in our case, the concentration of radioisotope release from the site boundaries.

So this is a series of calculations using logic trees. As a result of that you could take any one of these scenarios--the ends of these trees--which must all sum to one in terms of their probability; and the probability of each one is the multiplication of the products along the particular pathways.

With regard to those, in each case there is a set of parameters that describes the scenario that takes place as you proceed. So that is simply a rather elementary description of what we mean by a logic tree diagram and how it all fits together.

If you take that, then, you go through any of the scenarios and it gives you a release of radioisotopes as a function of time. Therefore, for each of these scenarios you do a calculation and end up with this kind of a curve.

The information on here, of course, is concentration versus time; and it gives you no information as to the probabilities of these particular events occurring: each of these scenarios.

The presentation the Environmental Protection Agency has selected is called a complementary cumulative distribution function, and it is constructed by taking any particular time, T-zero, and looking at that graph and starting at the top because this probability says, "What is the probability that you will not exceed a particular value?"

You start at the top and come down to the highest value. That would be scenario three, here; and that would give you the first blip up from zero on your particular graph. Then as you went to curve one that would give you the second increase and so on: three, four and so forth as you move up.

So as you moved down in concentration you move up upon this graph and it indicates what is the probability that you will not exceed a particular concentration of the radioisotopes that have been released.

We proceeded to construct those kinds of curves for this system. The logic diagram that we produced as a result of our deliberations is presented here.

Let me first caution you that at every one of our meetings this logic diagram changed. I am sure if we met again it would change again.

So we see it as an iterative moving process in which you develop this. The more you know, the more you are familiar with it, the more you say, "Well, we ought to fine tune that a little bit"; but the process we ended up with was to say that we start off with the first step, which is flux infiltration: that is to say, the change in rainfall or other properties regarding the input of water from the surface down into the depths beneath the surface at Yucca Mountain.

Our second event was an earthquake-caused canister failure. I think it is obvious an earthquake has occurred and it has caused an actual rupture of the canister itself. In addition, earthquakes could cause a change in the water table. This is another consideration we took into account.

We could have volcanic activity that had direct implications on the canisters, themselves, and cause releases of radioisotopes. We could also have a change in water table as a result of these volcanos.

Then there is the question of bore-hole stability where you get the release of rock structure from the bore hole itself as a result of possibly volcanos, possibly earthquakes, possibly just stress with time in which you can get canister failure.

This leads us to the general question of canister failure. This leads us to the general question of mean canister lifetime. Of course, in a sense, this collection together produces our source term.

This says, as a result of these particular interactions you can have a loss of canister lifetime or a loss of integrity of the canister; and, as a result of that, you get a source of radioactivity. Then the remainder of these logic diagrams, 8 through 11, indicate the processes in which transport takes place.

The first one has to do with the solubility: the release of material which, in our case, is governed by the solubility of uranium oxide, most probably U_3IO_8 rather than the UO_2 for in which it is disposed of; and then the question of the rock fracture model, which influences the hydrology of the whole system.

We have approximately seven different pathways in which water could proceed from the canister to the release; the question of the porosity of the soil and the extent to which that allows the water to move directly; and retardation: that is, the geochemical properties of the system which cause retardation of the particular radioisotopes and, therefore, have some influence on its transport.

There is a question forthcoming.

DR. NORTH: Could you give me a sense of the scale of that? My quick calculation is you have about 10,000 N points.

Is that about right?

MR. SHAW: No, not quite.

If you had two steps on each one of these, 2 to the 10th is 1,000.

DR. NORTH: I am picking up the branches as you have drawn them.

MR. SHAW: That is just illustrative. We ended up with a little over 1,000 N points.

DR. NORTH: So you pared it down.

MR. SHAW: We have pared it down. Also, a very important part of our calculation is that we have looked at techniques for reducing the number of calculations you carry out. We will have opportunity tomorrow to talk in a little more detail, when we get into that with the subcommittee, about how we did that.

We used calculational techniques to reduce the number of actual tree calculations we carried out.

DR. NORTH: I am presuming you used influence diagrams.

MR. SHAW: Yes, we did use influence diagrams. That is right.

I am not going to go through all of these today, and we will not even go through all of these tomorrow, but to take one example. In the first node, which was the infiltration node, our climatologist said there is a fairly significant likelihood of substantial increase in the future of infiltration because there is a substantial probability that we are in an inter-glacial time now which, over the period of 10,000 years, will certainly proceed toward a glacial time.

As a result of that the expectation is that the precipitation will significantly increase at Yucca Mountain.

Therefore, we looked at what many people regard as the current net flux of .5 millimeters per year as having a probability of about 8 percent, a probability of 90 percent with regard to the net flux being 1.6 millimeters per year, and a net flux of 4 millimeters per year having a probability of 2 percent.

So that gives you a sense of the kinds of things we did as we carried this out.

Tomorrow, by the way, in our somewhat more detailed presentation I will have three other people here to assist me. One is the person who did most of the calculations using the logic tree. He will be able to discuss the question you just raised about how we reduce the number of calculations.

In addition, I will have two experts here on two areas who will cover, in some detail, just how they went through their calculation. The first will be Mike Sheridan from the State University of New York at Buffalo who will talk about volcanism.

Mr. Sheridan will present to you the model that he used to look at volcanic probabilities and how they could potentially impact Yucca Mountain.

The other one will be our seismicity expert, who is Kevin Coopersmith from Geomatrix in San Francisco. He will talk to you about the details of the node with regard to the seismic activity.

The attempt tomorrow will be to give you a better picture of the detail to which we went in each of these nodes by giving you the illustration of those two particular technical areas.

DR. NORTH: Off your last example, what was the form of the communication? Was it simply those probability numbers or did you develop a base of information supporting those judgments?

MR. SHAW: We have a base of information which we feel and hope is supportive of those judgments. That will be presented in the report. We have a report from the Electric Power Research Institute which we expect to be available by the end of this month. It has currently been approved for publication.

The format of the report very much parallels the previous diagram I put up there. In that, almost section by section, is a technology. For example, when you consider climate one of the early sections in the report is on climatology. It lays the ground work for saying what the history has been of climate; what does it look like at the Yucca Mountain area; what is the evidence that, in the past, there was heavy rainfall compared to where we are now; and what is a reasonable judgment, and how did we come to that judgment with regard to these kinds of numbers.

DR. NORTH: There are to dimensions to that I hope we can explore subsequently. One is the methodology for assessing the expert judgment, such as did you assess a continuous distribution and then represent it with these three scenarios?

The other dimension is: To what extent did you use some, I will call it, formal methods for assembling the information on climate change or were you simply getting an assessment of one expert's judgment? For example, did you integrate in any formal way runs on general circulation models or something of that sort?

MR. SHAW: I can respond to that right now.

DR. NORTH: Okay.

MR. SHAW: We did not attempt to gain a wide range of expert judgment as we participated in this process because our emphasis here was in developing a methodology. We did not want to take so much time in saying, "Hey, these are the best numbers that we can come with right now."

We wanted to say, "If these are reasonable numbers how does it all fit together? How does it integrate? Can you get results? Is it a reasonable process?" And so on.

So our strong emphasis was on the methodology, not on the details of the input as long as we felt they were reasonable. To take this example in climatology, Austin Long, our expert from Arizona, went to the literature and would make phone calls and do things like that; but there was not the attempt to do what I think you are hinting at: getting a room full of people or a set of people around a table who are experts in this area, have them come up with some continuous distribution, and then refine that into some discretized numbers.

We did not attempt to do that.

DR. NORTH: The dimension I want to have you describe to us is how deeply did your expert go. One extreme is you get an expert, sit him down for an hour or so and assess a distribution as represented by these three numbers or as a continuous cumulative distribution for which these three numbers are an approximation.

Essentially you got probability numbers from him, but not much of a sense of where did those numbers come from.

Another extreme might be: This individual writes a book for you on all that is known at present about climate change and describes various competing models, summarizes the analysis that has been published in the literature, compiles all the data that is available, and comes up with a probablistic model out of which you develop a probability distribution which then becomes the basis for these three numbers.

MR. SHAW: Of course, my answer was it was somewhere in between the two; but it was closer to the former rather than the latter. In this sense, climate is not a good example because we really had one expert on climate and most of the rest of the people did not know too much about climate.

DR. NORTH: Right.

MR. SHAW: But in most of the other areas that is not true. There was a lot of overlap in the understanding of technology; but in the area of climate or any of the other areas, as we went through our meetings the procedure would be that the expert would get up and give a presentation, and say "This is where I see things are. This is how it applies to Yucca Mountain"; and it would be, in a sense, a seminar given to us, and we would challenge and ask questions.

That happened three different times with the series of work shops we held. Each time the person would come back with a refinement.

When you talk about geochemistry or geology and the fractures and things of that nature, there were enough experts there--usually two or three--who understood those areas that there were some significant technical challenges given to those people.

In addition, the presence of Russ Dyer meant there were situations where we knew the Department of Energy had worked on this particular area, and we would have our expert make telephone calls and maybe even have meetings with the appropriate experts there to see what we could garner from their results as well.

It is certainly not a book defense, but it is a chapter defense which is presented in, hopefully, a formalized technical fashion that convinces people these are reasonable first-cut numbers.

I would now like to proceed to some slides which will show some of the results as we have presented these. This goes back, in essence, to the presentations I showed before: the radioisotopes as a function of time.

I will remind you that this process does result in 1,000 scenarios as a function of time. One the first slide we will put up you will see a whole bunch of traces going across the screen.

We are going to show here that each of the scenarios give you a different trace for the function of the output of the radioisotopes as a function of time.

This example happens to be for neptunium-237. Here we have calculated the curies that are released as a function of time. This goes out to 100,000 years. We are not modest.

We did calculations for approximately six different radioisotopes. We did not try to cover the whole spectrum. We tried to find those that were particular examples of different chemical processes so they would be typical of particular transports.

What you see here is that any one particular curve--and sometimes I realize it is difficult for the eye to pick out any one particular curve--is one of those N branches and is a calculation of radioisotope release of neptunium-237 as a function of time.

You will see two different colors. Actually there are three different colors. It is difficult to pick up the third one because it is down here somewhere.

The fact is that the different colors have to do with the different values of infiltration. In particular the red value is our infiltration value of .5 that you saw on that previous diagram I showed you; 1.6 is the blue; and 4 millimeters per year is the dashed green.

So the red one, which is down here off scale, actually gave zero releases. At .5 millimeters per year, we found essentially no instances where there was any release of radioisotopes over 100,000 years. So it is only where you have increases of infiltration that you begin to get changes.

As you look at this what is important is the colors give you a sense of whether or not there is a strong dependency of this particular function. You can see there is a strong dependency here.

As you change from an infiltration rate of 1.5 to an infiltration rate of 4 you do get a significant increase in the release of radioisotopes. That is one of the features we wanted to look at: What are the particular parameters that are sensitive, to which the results are sensitive?

One comment we could make here is that high infiltration is a necessary but not sufficient condition to get very high isotope releases, at least in this case: for neptunium.

This is the same curve only we have now reduced the time scale so it is only 10,000 years. This, of course, is the Environmental Protection Agency standard over 10,000 years; and we have put in here the standard for neptunium-237, which is 100 curies.

In addition, you will see two of these squared-off release diagrams. These are simply to show that there is presumably a release due to volcanism in two different legs of our channel, two different scenarios, that we did not calculate. We simply threw in times arbitrary and values arbitrary, but we did no calculations with respect to volcanism release of radioisotopes.

In addition, we did no calculations of gaseous transport so the carbon-14 release is not a part of our process; and those are, in our minds, not significant restrictions because the framework we have developed would easily allow those particular processes to be adopted.

We chose not to have expertise in the gaseous transport area as a part of out team and, therefore, it was not there; and the volcanic calculation is more complicated and we decided not to proceed any further with that.

This is again, for the differences in infiltration, the results we get.

The next process, of course, is to develop a histogram so you can see some of the probabilities of these particular curves. There are little blips here that you can hardly see. It only become significant here. Of course, most of the probabilities are way down here on the extreme.

This is a 10^{-2} curies and this, of course, gets very low. So it is very low activity down here where you see most of these probabilities; but that is not very meaningful because it is all stretched over here on the left side of the curve.

So then one can go to the complementary distribution function, which we plotted here, which is all down here on the left corner. That is not very meaningful either. The reason it is not is because it is on a linear scale; but on the linear scale it allows you to go all the way to zero, which capability you don't have on the next curve, which is the semi-log and very typical curve that we see presented for the release of these radioisotopes as a function of various scenarios.

Therefore, in a sense, this does directly show you the probability of these various scenarios as a function. In this case, we have put it normalized releases. So we divided all those releases by 100 curies in order to normalize it so the value out here, 1, represents 100 curies for neptunium-235; and we have drawn here the Environmental Protection Agency standard along the upper right hand corner.

We make no claims to saying that neptunium-235 does not exceed the standard. We are simply showing how this framework can be used to develop this particular process.

In this case, I am showing the sensitivity or functional dependence of these releases on the infiltration. This is the average curve you saw a moment ago.

In this case, the red one is the base case; the blue one is for the lower flux, only; and the green one is for the higher flux only. This shows you another case where doing this in terms of a logic diagram is very useful and meaningful.

One can set the probabilities to zero for two of the three infiltration cases. Then you get a calculation directly that shows what is the result of that particular scenario: the one in which you, in this case, have a flux that is .5 and, in this case, have a flux that is 4 millimeters per year.

So it does show what we already saw with the colored diagram: that there is a sensitivity, a fairly significant sensitivity in this case, to flux infiltration.

This is another example--I think this is the second case--where we look at the effect of flow paths, the effects of fractures in the rock as the result of earthquakes. For the three scenarios we developed you can see it is not very sensitive.

This shows you that in some cases you have relative insensitivity and, in others, very significant sensitivity to these kinds of results. In this case, the different fracture calculations were for higher fractures and lower fractures in the base case.

We can also, of course, do these various calculations for different radioisotopes. This shows neptunium-237, the base case, in green; technesium-99 calculated in red; and cesium-135 calculated in blue. By normalizing those together--this shows them without the label put on there--you then have the capability of summing those.

In this case, we have a dotted green, which is the full, with the other curves shown as they were before; and this shows it without the diagram.

This also brings your eye, I think, into better synchronization here because we normally think, "Well, you add these up and they must make significant contributions"; but remembering this is on a logarithmic diagram that unless these curves are very close to each other they obviously make very little contribution to the total.

So the total here is almost completely dominated by the neptunium because it is presented on a logarithmic diagram.

That shows how we present the result of the performance assessment. I now have a particular illustration in which I want to show how you can use these results to determine priorities.

We have put together here a little situation where we say a researcher comes forward from some national laboratory and says, "I have an exciting research program that I want to carry out and I am going to work on flux. As a result of that flux I am going to take this base case probability that you have, and I predict that the result of my research will be that we will be able to eliminate one of those N points. I am not sure which one, but I think it is roughly a 50/50 chance that we may end up with only two cases in which the flux is lower than we suggested before or the other two cases: that the flux is higher than it was before."

So we are saying, at the beginning of this research, here is what we expect the end result to be. It is either going to be this distribution or this distribution, and there is equal likelihood between the two.

Meanwhile somebody else comes along and says, "Okay, I have some research I want to do with regard to the fractures, in the area of the fractures; and the base case probability is that there is 50/50 between low and high. And I believe that, at the end of this work," which roughly costs the same amount of money as the original one, "I am going to have either 80/20 in either one direction or the other."

Now, going back to the slides, we can show what the results are of those calculations. We put those in our model and, let me tell you, that as you went through that result what came clear is: there was a significant result or sensitivity for the flux values; and a rather insignificant change when you made the calculation for the fracture areas.

The whole determination is that using a framework of this nature, one can come to significant conclusions as to where the value is of particular research. As you fine tune on the parameter measurements you make, one can make determinations as to what value that has on the total performance assessment.

I like to put this warning in front. Keeping in mind that these calculations are strictly illustrative; and, therefore, we don't attach a lot of significance to the numerical results; we have still concluded, based on the calculations we have made, that the following topics are found to be more influential on site performance than the other topics.

There are no surprises here: hydrology, the question of infiltration; the water flow pathways, the extent to which they are influenced by rock fracture and porosity; and any significant rise in the water table. Each of those functions can give us significant changes, and in fact increases, in the release of radioisotopes.

The second major area was geochemistry, particular the uranium solubility as influenced by dissolution chemistry and temperature, and the chemical retardation of released radioisotopes.

These are what we referred to as our necessary, but not necessarily sufficient, conditions to get high releases from the site.

We have some conclusions. Our conclusions, in a rather general way, speak to the framework.

They are: that the use of a multi-disciplinary scientific and engineering expertise to conduct a risk-based evaluation of a high-level waste repository is achievable with current knowledge and technology; that the structured approach is required; and that the work shop format is very well suited to this particular approach? The use of logic trees is a convenient and credible format, although there are certainly many others which one could use to describe the analysis for performance assessment.

The results of the methodology should be obtained during the process of model development: that is, the process should be iterative--one needs to put the model together, run it, say "Does it make sense or does it not?", then rerun it and revise it; and so on.

A methodology of this type can be applied on a larger scale in which a larger body of expertise participates. The application will lead to realistic rather than simple demonstrative results.

Let me expand on that a bit. As one goes back to the tree diagrams we put together--and I think I will put two of them up here simultaneously--and you look at what is included in these diagrams, people usually think strictly in terms of a single model.

Let's take the example of rock fracture. We think of a single model that describes the hydrology in the pathways; but we also know there is a multiplicity of models that are out there: that there is more than one model that describes the hydrology in particular.

A framework of this nature allows one to use a multiplicity of models. For example, each one of these particular pathways at Node 9 could be a different model. One could have a one-dimensional model; one could have a two-dimensional model; one could have a model that says there are direct pathways as well as the very tortuous pathways; and one could get a group of experts together and say "What is the likelihood that the first model best describes Yucca Mountain? What is the likelihood the second model best describes . . .", et cetera.

You could put probabilities on these trees and have them attached to the model so you not only get different scenarios, you actually use different models in order to carry out your calculations. Then as time proceeds, of course, one gets either refined models or better confidence in a particular model that it really describes the particular circumstances.

In that sense, we feel that by expanding and going further with this particular process one can get a better description and an iterative description as one goes along.

As we move ahead, our near-term plans are: to prepare the working version of the methodology development team; perform its assessments and its reports, and I put dates on there of September 1990. We have completed both of those.

The report is not published yet, but it is in publication and will be out by the end of this month.

Our view is that the appropriate phase two is to join with the Department of Energy in sponsorship of work shops on performance assessment methodologies to identify crucial technical topics for work shops. I will come back and give you a little more detail on what I have in mind here.

Phase three would be for us to support--not financially, but technologically--the Department of Energy in conducting expert work shops on the crucial technical topics that were identified in phase two.

In a little more detail, what we would presently say is the way to go about phase two is to have a series of work shops, somewhat akin to what we did in developing the methodology, where we get together the participants who have been involved in performance assessment development.

As far as we know, there are only four bodies right now who have done performance assessment methodologies. I don't mean to exclude the State of Nevada. I believe they have developed scenarios, but do not have a methodology. We would certainly want to include the State in our deliberations.

But, as far as I know, these are the only four groups which have developed performance assessment methodologies: that is the Department of Energy, Yucca Mountain Project Office contractors; the Department of Energy headquarters contractor, which is Boulder Associates; the Nuclear Regulatory Commission; and, of course, our own that I have just described to you here.

I would see as the objective of this particular set of work shops: to exchange detailed explanations of each performance assessment methodology: to revise these methodologies where appropriate: and to obtain some consensus on the highest-priority technical areas; and I would see a schedule of a series of three work shops starting in late 1990 with completion in late 1991.

To focus a little in on what I would see us doing for three work shops, I would see the first work shop being one where we get these parties together and, in maybe a half a day each, presentations are given for: What is your methodology? How does it work? What is the basis for it? What are your calculations? What are you results and so on? Really an interchange, an exchange of methodologies.

The focus is not to say which one is the best, which one should we select; but rather that each party would have a clear understanding of what the other one has done.

As a result of that any of the four of us may choose to go back and say, "Whoops, I forgot that," or some other element that would make our methodology better so there could be some revisions; then come back at a second work shop and talk about revisions, maybe come back and defend some things that were questioned at the first work shop and so on; and then proceed to have each group say, "If you were to select a particular technical area to convene a set of experts to discuss that particular technical area, what would that technical area be? What is the highest priority, the most crucial technical area?"

I laid out some of our considerations, particularly with regard to hydrology and geochemistry. Then to drive towards the consensus--and I would presume you would do it in a preliminary fashion at the second work shop--at a third work shop as to what are the technical areas that most plead for technical consensus; and do more than that: define the particular crucial questions. What are the models that might be there? Who are the kinds of experts we would like to bring together? What might be the best format for those?

This would be to drive sort some particular details as to how you would conduct and who would be the participants in these particular work shops as you would proceed.

Then I would see that moving to phase three, which is the conduct of those particular detailed work shops on the various technical areas. I would choose to have that sponsored by the Department of Energy. We would use it to update and revise our performance assessment methodology.

I could perceive that one to three work shops--that is, technical areas--might be handled in a year, although I think at the beginning it is pretty difficult to get through much more than one.

Any of you who have participated in any of these things know they can be pretty detailed and fairly extensive kinds of work shops in order to drive a group of independent thinkers toward some consensus about their particular area, particularly when you are going out 10,000 to 100,000 years.

We would see one of our roles as being significant independent technical expert input to the Department of Energy.

My last slide is one I have used with my utilities to indicate the various roles we see for the principals that have been involved in this arrangement thus far. You should read across relative to each other, not up and down.

We have seen ourselves as a major player in each of the two phases, and a more minor player in phase three. U Waste has worked with us. This is the EEI group. I would say the Department of Energy has certainly been supportive in this area. We see them as playing a minor but participating role here and taking over the major role in phase three.

This speaks a lot to the utility atmosphere which I think prevails right now, which I think is a significant part of this whole discussion as well.

Right from the start as I have moved into this work the utilities have questioned us as to why we are doing this. My response to that is, generally, we are doing it because we think we have valuable tool here that could be useful to the Department of Energy.

We are also doing it in a sense to prime the pump: to enable them to move ahead on what we see as an important integrative message. We also think we have some technical expertise here that could be useful to the Department of Energy.

Of course, the utilities' question generally is: Why does the Electric Power Research Institute have to be doing this? We are already paying the Department of Energy to do it.

That question continues to prevail. They basically have said to me, "Okay, you have generated your tool. You have done your job. Now get out of the business. Turn it back over to the Department of Energy."

That is the prevailing general mood within the utilities. Although they have been supportive of the work we have conducted so far, they are reluctant to continue on as strong a basis in the future.

That describes what we have done so for. As I mentioned to you before, we will have more detail for the subcommittee tomorrow afternoon.

CHAIRMAN DEERE: Fine. Thank you very much.

MR. SHAW: You are welcome.

CHAIRMAN DEERE: We will now open the session to questions from the Board.

Questions and Discussion

DR. NORTH: I would like to commend you for taking this effort on to prime the pump, and a very good demonstration that is quite responsive to the two recommendations we had in our first report to Congress with respect to development of methodology for performance assessment and getting on with the process of using that.

One of my concerns as this goes forward--and I am going back to your slide with regard to near-term plans--is I think we need to distinguish methodology in two areas. One is the chapter or the book in the analogy I was drawing: the substantive expertise that is getting summarized into one stage of your logic tree. The other area of methodology is the process of going from the chapter to a simple representation with, for example, three probability numbers attached to three scenarios so you really have it down to two probabilities plus descriptions of the three scenarios.

I think it is very important that as the methodology is refined we have an appropriate balance.

I sit here as essentially somebody who has had experience in how one does this kind of analysis and it is easy for me to see a lot of fine points in terms of the use of influence diagrams or Monte Carlo analysis as an alternative.

For those who have not been acquainted with this kind of analysis it may be very easy to seize on those details. I think it would be a terrific mistake if too much emphasis were to go into those details. I think those of us who have practiced in the area of decision analysis could rapidly convince ourselves and each other that those details are not very important: there are lot of ways to make these calculations, lots of ways to do the summary once you have this kind of information into this quantitative form.

The problem is going to be to get the chapter level right so we have what appears to be either consensus or defined areas of disagreement in the material that is being summarized into the various stages of this.

I think it will be extraordinarily important and useful to have the work shops you describe as a way of getting a sense of where our various groups--who have studied this problem in this framework, looking stage by stage at this kind of a logic tree and determining either that there is fairly broad agreement as to what the uncertainty looks like or there is disagreement, and having a numerical representation such as you demonstrated on the infiltration issue may be very useful to get us off the basis of saying, "Well, gee, future climate change is uncertain--get to an assessment about how much disagreement is there about that uncertainty.

MR. SHAW: A few comments in response to that.

We, of course, focused at this stage on doing a simple integrated methodology so we could present a framework within which such calculations could be made for the two purposes I mentioned: performance assessment, and identifying and prioritizing crucial issues. We feel we have done that.

The next step is clearly the kind of thing that you have described. I might even ask Clarence Allen to participate a bit in this discussion.

We are going to reflect back on what we did in the seismicity owners' group where we successfully brought together technical experts, described these sorts of things, and I think did the sorts of things you are talking about: talked about extremes.

Clarence, would you like to make a point or two on that process?

DR. ALLEN: Yes. I participated in that and found it exceedingly valuable. That is why I was a bit surprised that with only one climatologist, and that being such an important issue, that at least at this stage of the game you were certainly depending on one man's advice whereas in the case of eastern seismicity we must have had at least 50 seismologists in that room, most of whom disagreed with one another.

[Laughter]

DR. ALLEN: However, some interesting results came out of it. No question.

MR. SHAW: Let me respond to that particular point.

We felt that before you get to the stage of collecting 50 climatologists together in a room and having them come to some agreement or disagreement on what was going to be the future, they had to understand the framework within which they were supposed to carry out this assignment.

Our attempt was first to say, "Let's understand the integration" because until you get to that point you cannot understand how the output is going to be used. I think it is important to understand how the output is used from climatology or any of the other aspects before you bring these people together because then you begin to clearly state the objectives.

That was our purpose: get one expert in each of these areas, make it be illustrated so you pull together the framework, they understand the integration and interaction between these various technologies, then you are ready to get the larger group of experts together in each particular area so you can refine and better define the kinds of things Warner is talking about.

As it was done in the seismicity owners' group, I would view this as being a process in which people were forced to defend the particular positions they took, that you understand the range of positions that people take, that you assign probabilities to these ranges: you come up with either continuous or discrete distribution, and that in an almost book-like fashion you end up with a description of the process, of the technologies, of the references, of the particular positions people took, of the conclusions people came to as best you can, make it as transparent as possible as to how that whole process took place.

Inevitably we are going to have new data, new positions and so on that are going to refine on that particular process.

DR. ALLEN: Perhaps my attitude toward it could best be expressed by the fact that when I was serving as a consultant to the Electric Power Research Institute in their technical advisory group on this at the end of two or three years I should have been paying tuition.

MR. SHAW: I think I have indicated to you, we will be happy to send you a bill for that.

DR. PRICE: You omitted volcanism for the purposes of phase one. Are there any plans it will be included?

Yes, it is on the overall master, but you said you ran into complexities. Can you describe any of that as it might impact the future direction of what you are doing?

MR. SHAW: Our expert, Mike Sheridan,--who had been in Arizona and, more recently, is now at State University of New York at Buffalo--looked at the whole question of volcanic activity in the area of Yucca Mountain.

As a result of that and discussions with some of the experts both for the State of Nevada and for the Department of Energy he has evolved the model that allows him to predict the likelihood of volcanos occurring, and the likelihood of the dikes from the volcanos intersecting with the boundary of Yucca Mountain.

We did not get to the point of saying, "Now, if this occurs, can we describe"--and I presume this would be in a more deterministic fashion--"the interaction of the magma with waste containers; and, from that, talk about the release of radioisotopes?"

We felt that was, in detail, beyond what we were willing to put together in this particular kind of a methodology. So we reached the point of having a model, which Mike is very excited about and is going to be publishing, that talks about the predictions, the probabilities that volcanos occur, that the dikes will intersect and so on, which then could be used to further make calculations with regard to the release of radioisotopes and their subsequent transport.

We feel that could easily be incorporated, but we did not carry it any further than that. Time and resources were both limitations.

DR. NORTH: Did you have a representative volcanic release as part of your set of thousand-plus scenarios? As I recall, you had some--

MR. SHAW: There were two straight lines on there, and they were symbolically represented. None of the others had any volcanic release associated with them. All the scenarios for volcanic release were just in those two rectangular boxes there.

CHAIRMAN DEERE: What did they do with the bore hole stability? Did they take that anyplace?

MR. SHAW: We did a fairly simple process on that. We looked at the whole question of stress on the rocks and rock fracture, and we looked at the likelihood that there would be the release of rock segments from the bore hole so they would intersect with the waste package.

We considered, in a very conservation fashion, that if they intersected with the waste package, if they came out of the bore hole cutting itself and came in contact with the waste package, that was a failure: that was a pathway for water and corrosive events to occur that would not otherwise occur because of the gap between the waste package and the bore hole.

But we wanted to include the whole concept of the stability of the bore hole as an important one with regard to waste package failure. So we did that.

Then, I would have to say, it is important to note that when we got to the question of waste canister and its lifetime, and we looked at the variety of processes that lead to waste canister failure, we said, "This is ridiculous. To try to do this in a deterministic fashion is far too complicated."

So we ended up with three set of Weibull diagrams as being appropriate to describe waste canister lifetime for a moderate package, for a cheap package and for a very expensive package. We have Weibull diagrams as being typical with regard to industry experience for the lifetime of such kinds of equipment; and we simply used those as our descriptor of the waste canister failure.

DR. LANGMUIR: Would you take us through how that impacted the tree process? Did it matter, in terms of the ultimate release, which package you chose?

MR. SHAW: Yes. Very significantly, if you had our super-package--the more expensive package--it had a very significant effect on the release of radioisotopes. You saw the three diagrams for infiltration where there was significant increase between the three. You saw an even more difference in the waste package case.

So, yes, our conclusion was that it was very significant.

I was going to say that the waste package Weibull diagrams were arbitrary. That gives it less credit than it deserves.

They were done in conjunction with Lawrence Livermore and some of the work those people have done in order to get what we felt were reasonable values for the mean, minimum and maximum lifetimes associates with each of those Weibull diagrams. Therefore, there is some feeling of credence with regard to that.

However, we did not take particular materials and say, "How long do we think these materials are going to last?" In other words, we did not do any preliminary waste package design and then, from that, try to get the parameters you would use in a Weibull diagram.

DR. LANGMUIR: It was interestingly lacking from your final little table of critical issues or critical disciplines, waste package design and corrosion. These are not issues that were retained in your final conclusion as issues that needed to be further pursued.

MR. SHAW: I agree with you.

DR. LANGMUIR: You went to hydrology and geochemistry as the issues.

MR. SHAW: That is right. I would say that was an oversight on my part. That would be another significant issue.

The issue of uranium solubility is, in a sense, tied to that; but the issues of waste package lifetime and uranium solubility are really, in a sense, tied together. It is the release that comes from those two processes that is certainly an important aspect: one of the real technical issues.

So under technical issues you would have a third category, really.

MR. SHAW: I agree with you. Either that or I would tie it together with uranium solubility and say release in one case and the other one is transport, which would be the retardation.

CHAIRMAN DEERE: I saw that a number of your curves seemed to start at 1,000 years. Could you show some of those that might show the difference in the lifetime length of the canister? Or was I mis-reading something there?

MR. SHAW: Do you mean the complementary distribution curves?

CHAIRMAN DEERE: Yes.

MR. SHAW: You are saying they come down to zero at 1,000 years?

CHAIRMAN DEERE: Yes. Some seem to start at 1,000 years and then work up. I wondered, were those where you assumed the package was becoming soluble by the year 1,000, 500 or whatever?

MR. SHAW: The complementary distribution functions don't show time. It is curie release along this axis and probability along this axis.

CHAIRMAN DEERE: It may have been another diagram.

MR. SHAW: All of those curves are at 10,000 years. All of the complementary cumulative distribution functions have to be taken at a particular time in order to plot those; and we selected the 10,000-year Environmental Protection Agency limit for the curves.

CHAIRMAN DEERE: I think I was talking about your earlier curves that had 1,000 things plotted on them in three colors.

MR. SHAW: You are saying that we essentially saw zero release or that zero was 10^{-3} or something like that: you saw very little release up to 1,000 years. That is correct.

That is because we have spaced our calculation in 1,000-year intervals. So that is an artifact of the calculational process.

CHAIRMAN DEERE: When did the so-called super package start releasing?

MR. SHAW: I think it was 5,000 years. I think we assumed there was no release from that package for 5,000 years.

Please don't quote me on that.

CHAIRMAN DEERE: Could you show a couple of those early diagrams.

MR. SHAW: I don't have those in the projector.

CHAIRMAN DEERE: The ones you start with.

MR. SHAW: You mean the ones we had up?

MR. SHAW: Sure.

You want the radioisotope concentration as a function of time? Is that the one to which you are referring?

CHAIRMAN DEERE: Yes, way at the beginning.

[Pause]

CHAIRMAN DEERE: Is see the blue starts at 3,000 years. Am I right?

MR. SHAW: Start is 10^{-3}. Start is relative.

There is no zero in this curve so one has to reflect on that.

The other point I would make is you can see the straight line segments of each of these. That just simply means we made a calculation here and here, and here and here. They are obviously continuous curves, but we only make calculations in those intervals of time: every 1,000 years.

So there is no zero. It keeps on coming down here.

CHAIRMAN DEERE: Right, but I see some of them start at 1. So you must have forced it there, at 1,000.

MR. SHAW: That is right. Over the first 1,000 years we just made a cumulative calculation and we said, "Okay, what is the value of the 1,000?"; and we plotted that point. Then we said, "Okay, what is the value of 2,000?"; and we plotted that point.

So don't take a lot of significance in what is down here. It is simply the cumulative amounts that are being calculated.

CHAIRMAN DEERE: So if I see a straight line that comes down to 1,000, then I look over here and I see another one that comes down to 4,000, something seems to be starting at--

MR. SHAW: That is right. That does indicate there are delays in some of these processes: that some of the scenarios have early releases and some of them have much later releases.

In particular you can see here, certainly, the blue versus the green: that as you get less infiltration, you get delays in the releases; as you have infiltration you tend to have earlier releases in the scenarios.

As I mentioned before, the .5 millimeters is down here somewhere.

DR. MACEDO: Why is it such a strong function of infiltration?

MR. SHAW: Because of the corrosion process on the waste package; because of the transport process for the hydrology itself. Those are the two key areas in which increased infiltration produces more rapid transport.

CHAIRMAN DEERE: Any other questions from the audience? Max Blanchard?

DR. BLANCHARD: If the centralization issue referred to this concept where you tried for some previous utility licensing processes, under those conditions did staff from the Nuclear Regulatory Commission participate in any of those?

MR. SHAW: Let me repeat the question: I made reference to work we had done previously on the seismicity group; and was there Nuclear Regulatory Commission participation in that particular activity?

The answer is yes. We felt right from the start it was not only important, it was vital that key Nuclear

Regulatory Commission people would participate in this program so the results would be acceptable and appropriate. We wanted to get their input right from the start.

So they did participate throughout the whole process of developing the analysis for the seismicity on the east coast.

DR. ALLEN: Leon Reiter was a very active participant in those.

CHAIRMAN DEERE: He is right here.

DR. REITER: I want to differentiate between the participation and input, and observers. The Nuclear Regulatory Commission was there as observers. The Nuclear Regulatory Commission did not approve or disapprove of the input.

In fact, there was a very strong position taken by the Nuclear Regulatory Commission that that was not the purpose. The idea was to see whether or not the methodology was consistent and was a workable methodology.

That is what we commented on. That is a big difference from approving or disapproving the input.

MR. SHAW: Thank you.

DR. ALLEN: You may have been there as an observer, but you were a very active participant.

[Laughter]

MR. SHAW: Thank you both for the clarification.

DR. BLANCHARD: My second point is: In your phase two of your proposed future activities you have identified a number of groups or organizations that you think should be represented to make this an effective process.

Would you expect to begin discussing with those organizations the availability of people and the opportunity to make commitments to really support phase two?

MR. SHAW: The question is with regard to phase two and our projected interaction among the various performance acceptance methodology groups, when do I expect to proceed to make contact and get commitments from those various organizations?

I have contacted all those organizations with regard to dates in the first week in December. All of those have indicated a willingness to participate in that.

I now need to get a firm confirmation; but I have at least a preliminary confirmation that, yes, we are interested and, yes, we will participate.

DR. DOBSON: You noted that your solubility appeared to be a significant parameter. I just wanted to know whether or not that was primarily because of the activity of uranium or because of the inter-relationship on the solution of other uranium nuclides and uranium: in other words, whether or not the solution is an adequate representation of solubility.

Did it relate to the release of other aconites and soluble species, or did it relate primarily to the actual activity range?

MR. SHAW: To quickly paraphrase the question, it is: Why is the uranium solubility significant? That is a more relevant question for me to answer.

It is significant because we felt it is not well known, and we chose two values. One was 2×10^{-4} and the other is 2×10^{-6}. That is a very significance, of course, in uranium solubility and will influence very markedly the results of a release of uranium; and because we assumed congruent release of the radioisotopes with regard to the uranium solubility.

We felt that a number of things were sufficiently unknown that we could not predict the solubility any better than that.

For example, the transportation of UO_2 to U_3O_8; the question of: What is the temperature at the time of the solubility?; the question of: What is the chemical pH, the oxide redox conditions and so on are very loosely known, and particularly when you start to talk about the solubility of these constituents of uranium. That is not even very well known, especially as you go to the higher temperatures.

On that basis we said, "We really don't know the uranium solubility very well and, therefore, we are going to choose two values that are two orders to magnitude." When we did so, we found it had a very significant effect on the total results.

In that range, at least, it was a significant parameter.

DR. REITER: I wonder if you could put on the transparency that shows the flux rate?

MR. SHAW: Sure.

DR. REITER: I want to make the point here that reasonable people coming together on an issue may not agree.

There are very critical parameters you pointed out, and you pointed out some other studies that were being conducted.

For instance, in this same parameter the Nuclear Regulatory Commission in their evaluation assumed flux rates of 2 to 8 millimeters a year. The Yucca Mountain Project Office, in their evaluation, thinks that the value will be something like 1 millimeter per year: an order of magnitude less than you indicated. In another study that was carried out by PNL they point this out as being a very critical parameter.

Something we have observed in the Seismic Hazard Group is that different people coming together can get large differences in their conclusions. One of the very first PRAs you did was for Indian Point. Two separate consultant teams for the utility came out with estimates of hazard that not only did not match, but the uncertainties did not overlap; and we have seen that throughout.

[Laughter]

DR. REITER: Are you taking into account that different people might assign different weights such that you would not end up with one cumulative curve, but you might end up with a family of curves as did the Electric Power Research Institute Seismicity Study and the other studies?

MR. SHAW: I think the process you described is certainly supported wholeheartedly. As we moved into phase three, I think it is absolutely essential that you carry along the full range of expert judgment at this particular stage because we are certainly dealing here with highly unknown factors.

When you talk about the global climate model Warner made reference to earlier, it is one that is constantly undergoing major changes as we proceed; and we are learning a lot about how to model these sorts of things and what might happen in the future, and the various glacial and inter-glacial cycles: how long is between them and the differing evidence that people come up with.

I think unless you have a process that allows all of these judgments to be taken into account and defended, and the results compiled so you can fit it into this kind of a fashion, whether it be a Monte Carlo continuous distribution or logic diagrams, whatever that process or framework is I think it has to, at this stage, include the opportunity for all of those opinions to be taken into account.

DR. DOMENICO: I am curious about this diagram here.

In the hydraulic model, was the velocity and the flux coupled by that? I mean, if the flux got to a certain point where it exceeded the matrix conductivity the fractures would take over and give you faster flow?

Was that present in the hydrologic model?

MR. SHAW: Yes.

DR. DOMENICO: So it was, indeed, a fracture type of hydrologic model?

MR. SHAW: We had opportunity for both matrix and fracture flow, and fracture flow would not come into account until we exceeded a certain flow rate. So there was an inter-coupling between them.

DR. DOMENICO: That explains why the higher the flux--that was what was happening?

MR. SHAW: That is correct.

DR. DOMENICO: Will some of the details of that model be in your report?

MR. SHAW: I hope most of the details are in the report, not just some.

DR. DOMENICO: Good.

DR. NORTH: That is the chapter we were promised.

MR. SHAW: That is right.

CHAIRMAN DEERE: Thank you very much. I think we have benefited from this presentation and the various questions from the different people.

I would like to remind everyone that the Board will continue in closed session this afternoon. We will be meeting here, let us say now, at about 1:15.

Then we particularly would like to invite those in the audience to come to the Structural Geology and Geohydrology Panel meeting tomorrow, which is a technical exchange with the Department of Energy, treating structural geology and geoengineering. We will be speaking about the Calico Hills risk benefit analysis, surface-based testing prioritization, dry-drill and core recovery development.

In the afternoon we will return for Bob Shaw's presentation: a continuation of what you have just heard.

Thank you all very much.

[At 12:00 Noon, the meeting recessed to reconvene in closed session at 1:15 p.m., this same day.]