UNITED STATES

NUCLEAR WASTE TECHNICAL REVIEW BOARD

1995 FALL BOARD MEETING

STRATEGIC CONCERNS, TOTAL
SYSTEM PERFORMANCE ASSESSMENT

October 17, 1995

Arlington Renaissance Hotel
920 N. Stafford Street
Arlington, Virginia 22203

BOARD MEMBERS PRESENT

Dr. John E. Cantlon, Chairman, NWTRB
Dr. Clarence Allen
Mr. John W. Arendt
Dr. Jared L. Cohon
Dr. Edward J. Cording
Dr. Donald Langmuir
Dr. John J. McKetta
Dr. Jeffrey J. Wong

CONSULTANTS

Dr. Patrick A. Domenico
Mr. Richard Grundy
Dr. Richard Parazek
Dr. Ellis D. Verink

NWTRB PROFESSIONAL STAFF

Dr. William D. Barnard, Executive Director
Dr. Carl Di Bella
Dr. Sherwood Chu
Dr. Daniel Fehringer
Mr. Russell K. McFarland
Dr. Daniel Metlay
Dr. Leon Reiter
Dr. Victor Palciauskas

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Ms. Paula Alford, Director, External Affairs
Ms. Nancy Derr, Director, Publications
Ms. Karyn Severson, Congressional Liaison
Mr. Frank Randall, Assistant, External Affairs
Ms. Helen Einersen, Executive Assistant
Ms. Linda Hiatt, Management Assistant

I N D E X

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P R O C E E D I N G S

[8:30 a.m.]

CANTLON: My name is John Cantlon. I'm Chairman of the Nuclear Waste Technical Review Board. It is my pleasure to welcome you to our fall meeting in Arlington. We have an interesting two days ahead of us, but before I outline what is planned, I would like to introduce some new members of our Board appointed by the President on June 29.

Jared Cohon is dean of the School of Forestry and Environmental Studies at Yale University where he is also a professor of environmental systems analysis and mechanical engineering. Dr. Cohon's areas of expertise include environmental systems analysis and hydrology.

Jeffrey Wong, on the corner, is science advisor to the director of the Department of Toxic Substances Control in the California Environmental Protection Agency. Dr. Wong's areas of expertise include risk assessment and scientific team management.

Jerry and Jeff, it's a pleasure to welcome both of you here to our fall meeting.

Also present as a new member is John Arendt, back here in the middle of the table. John started out as a chemical engineer working on the Manhattan Project in Chicago. He is now a consultant and living in Oak Ridge, Tennessee. His areas of expertise include nuclear fuels

facilities, quality assurance, and the handling and transport of nuclear materials.

Other members with us are Clarence Allen, professor emeritus of geology and geophysics at Cal Tech. We are proud to announce that Clarence has just been awarded the 1955 Medal of Seismology from the Society of American Seismologists for outstanding contributions to seismology and earthquake engineering.

Also on the Board, Ed Cording, professor of civil engineering, University of Illinois; Don Langmuir, professor emeritus of geochemistry at the Colorado School of Mines; John McKetta, Joe C. Walter Professor emeritus of chemical engineering at the University of Texas.

Unable to be with us this morning is Gary Brewer, professor of resource policy and management at the University of Michigan.

Past Board members who now are serving as consultants pending their reappointment or replacement are Ellis Verink, Distinguished Service Professor emeritus of Metallurgy at the University of Florida, and Pat Domenico, David B. Harris Professor of Geology at Texas A&M. Pat is a hydrogeologist.

Richard Parazek, professor of geohydrology at Penn State, is here also as a Board consultant.

In addition, I would like to introduce some of our

staff. Bill Barnard, over on my far left, your right, Executive Director; Paula Alford, Woody Chu, Carl DiBella, Dan Fehringer, Russ McFarland, Dan Metlay, Victor Palciauskas, Leon Reiter, Mike Carroll, Karyn Severson, Richard Grundy, Nancy Derr, Frank Randall, Helen Einersen, and Linda Hiatt.

Today's agenda will focus on what we call strategic issues, while tomorrow we will be examining the current status of repository performance assessment. You will hear more about the performance assessment agenda tomorrow from Don Langmuir, who will chair the session.

We certainly are entering a very important and some might even say traumatic time for the nation's high level waste management program. Congress, in keeping with the move toward greater fiscal constraint, has signaled that less money will be available from the nuclear waste fund to work on radioactive waste than in the past.

Perhaps more important, there are several bills pending that could greatly alter priorities and the way things are done in this arena. These bills signal an interest in establishing an interim waste storage facility. The question arises as to whether both a viable disposal program in siting, construction and operation of an interim storage facility can be accomplished under the projected reduced funding.

Another area signaling change is that of the applicable repository safety criteria and standards. The National Academy of Sciences, charged by the Congress in 1992 with evaluating the technical bases for a Yucca Mountain standard, has now issued its report. Among other things, it recommends replacing the former 10,000 year radioactive release standard with one based on the risk of adverse health effects that could apply out to a million years from now. It also downplays the present subsystem requirements found in the NRC regulations. We must now contemplate what the impact on the program would be if the EPA promulgates and the NRC implements such a standard.

Some of the pending bills in Congress also stipulate a still different standard, seemingly not willing to wait for EPA to evaluate the NAS report.

Finally, the DOE and its contractors are wrestling with detailing a waste isolation strategy, that is, a clear and coherent vision of how the proposed Yucca Mountain repository will contain and isolate waste for tens of thousands of years. Such a strategy, when adopted, could be a guide for prioritizing its efforts in site characterization and program assessment. We are going to address these topics and more today.

We will start of by hearing from Dan Dreyfus, director of DOE's Office of Civilian Radioactive Waste

Management, on his perception of where the program stands today in the face of these challenges.

Following that, we will hear updates from Rick Craun and Bill Boyle of the DOE on progress in constructing the exploratory studies facility and in pursuing the scientific investigations above and below ground.

In spite of these significant potential changes, we have to remember that real progress continues to be made in understanding Yucca Mountain's repository potential from a scientific and technical perspective, which is clearly what the Board feels is the nation's primary high level waste management challenge.

Following these presentations, we have asked congressional staff from both parties to share their views on the program's outlook in Congress.

Robert Fri, the chair of the National Academy of Sciences Committee on the Technical Bases for Yucca Mountain Standards, will then discuss the NAS report. Steve Brocoum of the DOE will supply us with DOE's reaction to that report.

We have also asked John Kessler of the Electric Power Research Institute to provide us with some of the results of their analysis of the impact of the different standards being considered upon performance assessment, the way we measure the ability of the repository to protect the

public's health.

The last presentation today will be that of Jean Younker of the DOE's management and operating contractor on an updated version of a proposed waste containment and isolation strategy. We have been informed by the M&O and by DOE that this is being provided to the Board for our information only, as this work is currently being reviewed by the Department of Energy.

At the end of the day we will have a round-table discussion open to all of these issues raised. We have asked some additional individuals and organizational representatives to join us for this discussion. We will hear more about that later in the day from Jared Cohon, our new Board member, who will serve as the moderator.

We have also allotted time on both days for public comments.

Needless to say, we are looking forward to a very interesting two days.

Our first speaker is Dan Dreyfus, director of OCRWM. Dan, we certainly do appreciate your willingness to come to the Board in what must be very busy times. Thank you.

DREYFUS: Thank you, Chairman Cantlon. I am pleased to brief the new Board members in these interesting times. This should be an interesting kind of a meeting for them to

get their first view of the program.

I always appreciate an opportunity to speak to the Board and particularly now to give you some of my views about the status and outlook for the program.

You will be hearing from several members of my staff on the topics you have asked us to address at this meeting, including the progress that we are making in the scientific program and the construction of the exploratory studies facility.

In view of the significant impacts on the program that the recent congressional budgetary action has had, which Chairman Cantlon alluded to, I think the best use of my agenda time would be to put our current situation in perspective for you and also to the best of my ability at this point to share with you our contingency planning for the future of the program.

As you know, in fiscal year 1995 the Congress gave us a 40 percent increase in funding to initiate a new program approach that we had outlined to the Congress with our budget request. Almost all of that increase was allocated to the Yucca Mountain project, and we utilized the entire increase, completing the fiscal year with very little carryover.

We also had accomplishments that often exceeded our targets for the year. As an example, we have dealt with

the problems that impacted the startup of the tunneling activity. TBM is nearly a mile and a half into Yucca Mountain and well ahead of the planned schedule. We are now making the turn into the repository formation. We have completed three test alcoves and there is another soon to be under construction.

I was especially pleased that the House Commerce Committee in its report on the bill that it is considering recognized our progress for the fiscal year 1995, a rare recognition that things are indeed happening in the program.

Unfortunately, the debate in Congress on radioactive waste management issues has intensified over the past five months. The House of Representatives has focused its efforts on redirecting the program through a comprehensive authorization bill. That bill, H.R. 1020, has been reported by the Commerce Committee in the House with an overwhelming bipartisan majority.

The bill responds to a sense of urgency expressed by the reactor states and the nuclear industry by authorizing immediate construction of an interim storage facility in Nevada near the Yucca Mountain site. It also continues to place strong emphasis on the continuation of work towards a geologic repository. It endorses the current program approach and its target dates.

Now, if H.R. 1020 were enacted as currently

drafted, it has budgetary provisions that would probably permit adequate funding for both the geologic repository and an aggressive concurrent interim storage initiative. Unfortunately, there is not a prospect of that bill to be enacted in the near future. There is no action on a similar measure begun in the Senate, and the House bill is not yet out of the multiple committee referrals that it has had.

The principal difficulty with the debate in the Congress is that the appropriation process, which is focused on deficit reduction, has run ahead of and is in conflict with the policy legislation. At this point, like most federal programs, we are relying on a continuing resolution for our funding.

The House appropriation bill would give us $425 million for 1996 and the Senate only $400 million. Both bills intend to curtail the Yucca Mountain project. The Senate specifically capped that activity at $250 million for fiscal 1996. That is compared with a level of funding for Yucca Mountain of $375 million which was spent in fiscal 1995. Both measures also contemplate the initiation of some sort of interim storage program using a portion of the 1996 funds.

Our budget request for 1996 included $630 million for the second year of our program approach. Of that amount, Yucca Mountain would have gotten $475 million in

1996 to continue our planned activities. That would have supported submittal of the license application for the construction of the repository in 2001.

Obviously we are not going to be able to carry that plan forward. The outcome of all these conflicting actions remains unclear. We do not yet have valid policy guidance governing the structure of the program for 1996 and future years. We don't know when we will get it. Indeed, it looks now as though we might be in a continuing resolution mode for several more months.

At the moment we are in the process of reducing our expenditure level to the degree necessary to conform to an annual budget of about $400 million. That action is required by law and is a simple fiduciary responsibility. We have already taken action that will result in the elimination of about 875 contractor jobs, primarily within the Yucca Mountain project structure. Those separations are taking place as we speak, although they will take several months to be entirely consummated.

We had to act to achieve a path to a lower expenditure rate to minimize the total number of layoffs and to conserve resources for whatever the future programmatic priorities turn out to be. As we took these actions, we tried within our existing authority to preserve the vital program activities and retain some flexibility to respond to

eventual new directions.

Despite the obvious uncertainties about the future of the program, there are some fundamental truisms that we have to confront.

First, in the current political setting it is going to be difficult to regain the level of funding that we previously sought to support our program approach. For all practical purposes, I see no hope of doing so in fiscal 1996 or even in 1997.

Second, it is likely that some initiative on interim storage will be directed by the Congress and that it will be given precedence over the repository in the competition for whatever limited funding is available. In the early stages of the policy debate Congress considered quite seriously and in fact passed a budget resolution in the House that totally terminated the geologic disposal program. As the debate has evolved, however, political and practical considerations about the consequences of abandoning geologic disposal have led to a more rational and prudent approach. I think the sense of Congress now is much more towards the necessity to maintain a geologic disposal program.

I testified earlier this year before the Congress that the cost to commence work on interim storage in fiscal 1996 would fall somewhere between $70 million and $80

million founded essentially on the amount of useful work that could done in an initial year.

I also testified that no worthwhile geologic disposal program can be supported in fiscal 1996 at a funding level below $250 million even if program management costs are reduced severely. I think the House and Senate appropriation bills and the accompanying report language reflect this kind of a concept.

The signals clearly show that Congress is prepared to assign geologic disposal a lower priority than interim storage. Congress seems complacent to delay a decision on geologic disposal if an interim storage initiative is in progress.

The issue confronting us is whether the program can sustain meaningful progress towards a future decision on geologic disposal with a funding level that is significantly below that that was required for our current program approach. We inside the program gave serious consideration to this question, and we believe, albeit tentatively, that it can. We must, however convince the Congress that continued pursuit of geologic disposal is, first of all, worth at least $250 million in the coming fiscal year, and second of all, that it will have meaningful results. To do this, we have to ensure that scientific investigation can produce results within a reasonable time frame despite the

fact that the dates for a license application and the dates to construct a repository and emplace waste will be indefinitely deferred.

Given a likely funding scenario for fiscal 1996 and the foreseeable future, the only practical approach is to concentrate the repository effort on the major unresolved technical questions concerning the conceptual design of the repository and its expected performance in the geologic setting.

In order to manage the program as well as to explain and justify the continuing activity to stakeholders, we will have to set forth a specific set of deliverables that can be accomplished within a few years. I do not believe you can manage a program towards convergences that are ten to 12 years out in the future. There has to be a near term target of some consequence.

That cannot be simply scientific work in progress or interim technical reports. We must arrive at a measurably improved judgment of the viability of this venture at some time that is meaningful to management and to the Congress.

With these criteria in mind, we have defined the following set of deliverables. This, of course, is all tentative contingency planning.

First, a package of more specific design work on

the critical elements of the repository concept and the waste package, a design that demonstrates that the technologies are available to accomplish the objectives of geologic disposal. This will require resolution of critical unresolved issues in the waste isolation strategy and criticality control and in thermal loading.

Second, a total system performance assessment keyed to those design concepts and based on analysis of the data available at that time which will describe the probable behavior of the repository.

Third, an estimate of the cost to construct and operate the repository based on much more solid concepts of what we intend to do than we now have.

Last, a plan and cost estimate for the remaining work that would be required to complete a license application.

All of this, of course, assuming that we do not find a fatal flaw that leads us to decide that the repository should not go forward.

I think the topics you have selected for us to address at this meeting, which include the National Academy's report, status of the waste isolation strategy, and our latest performance assessment, all remain very relevant to the outlook for the program and to the concepts and objectives that we think we need to continue to adhere

to.

I think I will forego commenting on the agenda items because we have relatively complete reports on each of those that will be given to you by others that are closer to them. At this point, probably the best use of my remaining time to the extent your agenda permits would be to take questions. I am prepared to do that.

CANTLON: Thank you, Dan.

Questions from the Board?

COHON: You mentioned the competition for funds that would result from a storage requirement as well as the repository. Assuming that funds were sufficient, could you say something about added management burdens that would be placed on your program?

DREYFUS: There certainly would be an added management burden in the sense that the interim storage concept, especially as it is outlined in the bills before the Congress, is a pretty complicated affair. It involves, first of all, very, very tight, probably unrealistic targets, which would mean that one would have to construct a receiving facility. If indeed it were in Nevada, one would have to be working on railroads and bimodal transfer facilities. There would have to be mounted a nationwide transportation system utilizing hardware that doesn't exist, capabilities that do not exist, and a management structure

that does not exist.

So there are at least three major undertakings associated with that that we don't have in the program at the present time. We didn't expect to be moving waste before 2010, so we have not brought the transportation capability forward in that kind of a time scale. So, yes, there would be a sizable management involvement. Should there be a Nevada site, there is some symbiosis, but that, of course, is another imponderable as to whether there will be any site at all, and if so, where it will be.

CANTLON: Don.

LANGMUIR: Dan, looking at your list of deliverables, I don't see anything that looks like anything I could call site characterization related. My understanding from preliminary discussions around is that surface-based testing has been disconnected, or will be, and from talking to other folks on my side of the program, geochemistry and hydrology are being cut back severely. My cynical sense is that you have been forced to go towards things that are conspicuous and obvious to Congress, like the tunnel, and that because of this, the science and technology that would support the decisions on suitability are being disconnected and cut way back.

I guess I would ask you to comment.

DREYFUS: I have several comments. First of all, there

is a massive amount of material that has been collected. We probably would do well to digest it and get it into the modeling and the performance assessment.

We would continue to tunnel so far as is necessary. Not necessarily the amount of tunneling we have in the program approach. It is indeterminate how much tunneling we would do. I think we need to get it to the repository formation; we need to get heater tests and in situ tests in place in the repository formation; we need to see the Ghost Dance fault. Beyond that, it is a question of what is possible.

You are right. Everything is being curtailed. We would not go forward with an environmental impact statement, which is a necessary prerequisite to making a recommendation to the President. That would be left aside. We would not go forward with the complete preparation for license application that is implied in the annotated outline. We would just simply not be able to do that. I don't think it is necessarily that the science is curtailed; it is that everything is curtailed. The tunnel is a big ticket item so long as we are tunneling. How long we would do that is, of course, up for grabs. It depends on what we can expect for out-year funding.

CANTLON: Ed.

CORDING: I was interested in your comment on the

necessity of having some near term target that would show not just some scientific process but some step, I presume, in the decision-making process on the site. Of course one of the things that has been in the present regulation is a site suitability decision at some point ahead of license application. How do you see this near term target? Would it be different than a full site suitability decision that is in the present plan?

DREYFUS: I think it would have to be a good deal less elaborate because we simply would not have the funding to carry it out. Understand, we don't know where this is going. We are making a basic assumption that in 1996 we will be operating with about $250 million. A prudent person would assume it will go down thereafter.

Why? Because if there is indeed any interim storage program initiated in 1996 or even in 1997, and everybody that is involved in the Congress insists there will be, then that effort will begin to take more funding as it grows. The first year of interim storage is a limited amount of funding. When you start buying transportation, canister fleets and storage canisters, that is a fast growing demand for funds, and there is obviously a cap.

The ultimate cap on this program probably is the collections. The collections are about $600 million a year and another $200 million for the defense. So there is sort

of a philosophical cap at $800 million a year, which is what is the future expectation of revenue. The user fees, so to speak. But the mental cap that I have seen in 1996 is closer to $400 million to $500 million. If you take that kind of a funding profile and start to schedule interim storage into it, you don't see a whole lot of money between now and the end of the decade for the repository.

You can simply take the amount and look at the amount we had intended to spend in the program approach, which I would point out to you is considerably less than the earlier plan intended to spend and has been looked at by several members of this Board and others as being pretty tight to begin with. The answer is we have to decide whether there is anything to do then. I would point out to you that that was the first question. I did go back to the people on the program and said, what do you want to do? Because one option is go tell the Congress you can't do anything useful for this amount of money and we should bag it.

We concluded that we are not at that point, that there was something relevant that could be done, that there were unanswered questions that we could illuminate between now and the end of the decade. This Board has cited several of them.

We don't have the thermal loading strategy in

hand. Absent that, we don't have a clear notion of the costs and technologies that we would be using. We have not disposed of the waste isolation strategy. We have got work that we can do on that. We have a lot of data that has not been fully integrated into the modeling effort. We have the ability probably in 1996 to get the tunnel into the repository level.

So we concluded that there were meaningful things that one could do that could make the discussion of this venture, if you will, a more meaningful, more rational discussion in future years and we ought to do it. On the other hand, there is always the option of saying there is nothing useful that can be done; we should stop. I have to tell you that in the January-February-March time frame we were stopped. That was the outlook for 1996: wrap it up, put the reports on the shelf, and go to interim storage.

CANTLON: Don.

LANGMUIR: All of us, of course, have been listening very carefully and with anticipation, hopefully with you, that suitability could be decided or determined within a few years. Obviously this does a lot to that option, that possibility. If you disconnect the science and characterization which is fundamental to determining suitability, you clearly have put this decision off into some distant future time. Have you talked to the Congress

at all about what your thoughts are on the possibility of getting there given that you have cut half your program off?

DREYFUS: I have been talking to the Congress everywhere I can find them, at every forum that they have got and privately about various options. Of course, what I would prefer is to have had the year coming of the program approach. We managed to get the program approach launched. We managed to get reasonable, if guarded, acquiescence from the Board and the NRC that this was a viable program. I would like to have finished it, but I know what it cost in 1996 to do 1996.

What I will not do -- the staff may -- is go back to where I came in. When I came into this program it was pursuing a program that had built in to the published plans about a $700 million expenditure need with about a $250 million budget and a bow wave that always said that next year we will get $1 billion. I don't sit through bad movies twice. I'll tell you that right now.

[Laughter.]

DREYFUS: I am not going back to that program. One option is to say, oh, well, we'll do everything we were going to do. We'll just take a little longer and we'll hope for $1 billion in 1997. It killed the program, damn near. That is the reason why this program got the reputation that all the money went into infrastructure, nothing went into

science, no progress perceptibly was being made, and we weren't going anywhere. I would sooner see the program stopped than see it go back to that kind of a situation.

What we have to do is look at whether there is something useful we can do within the funding profile that advances the thought process and that advances it over the next few years. Armed with that better ability to explain what we are doing, we may aspire to get the money to finish the job all the way to licensing. But we can't get it this year.

That is really the question. Is there something that we can do that is meaningful, that represents progress, that doesn't put all the money into infrastructure and not much into science, that doesn't have an imperceptible pace forward? I think that means focusing on the principal issues and trying to do something with them.

CANTLON: Dan, in some of the conversations or written material we have seen the phrase -- I'm not sure I have it exactly right -- "a management investment decision on site suitability." You didn't use it in your presentation today. Could you sort of outline for us what that is and when with this lower rate of funding you expect to reach something like that?

DREYFUS: Yes and no. You will be getting detailed discussions from the scientific people as well the

performance assessment people, and maybe they can tell you a little more about what science they expect to be able to get.

I can't tell you that because I don't know what the out-year funding profile is and I don't know for sure what the congressional instructions will be. It is less than the site suitability determination that we hoped to make in 1998.

I can remember this Board telling me that they really didn't think I could get there from here with the money that I then was going to spend. So I'm sure I can't get there with the money that I am now going to be able to spend. It is less than that. To what extent it is less science as opposed to less procedure is something we need to talk about. That is where we are now, trying to figure out what those ingredients have got to be.

We would hope to have a package of deliverables that represents a better description of what we propose to do in the setting it's in. I think the sort of psychological date is before the end of the decade. Managing against targets, that's about as far out as one wants to manage.

ALLEN: I was going to ask the same question. Do I understand that the words "investment decision" are no longer part of the vocabulary, so to speak, that is being

used?

DREYFUS: I don't think it ever was. We have done contingency planning for more contingencies than I care to think about as the kaleidoscopic congressional activity took place. For a couple of reasons.

First of all, I have been testifying before committees that keep asking me questions about what would I do with varying amounts of money and how long would it take to do varying kinds of interim storage and that sort of thing. We have done multiple contingency planning. We don't have a set date; we don't have a set target; we don't have a set of deliverables. We will not be able to have that until the Congress drops the other shoe and tells us what it wants.

H.R. 1020 says we are going to do the program approach and we are going to file an application in 2001. It also says we are going to move waste to Nevada in 1998. So if it funds it, we will have to try to do the best we can to get back on track with the old program approach. If the Congress should relent and give us more money, we will do more. The Senate bill gives us a set of requirements that are not very different from what I just recited.

So no, this is not locked in. It's a shorthand way of talking about things.

CANTLON: Jerry.

COHON: I would like to pursue a little bit further the storage disposal tension. I thought you said before that you believe that H.R. 1020 provides sufficient funding authority to do both. Did I hear that right?

DREYFUS: H.R. 1020 aspires to take us out of the normal budget process. Of course that is one of the reasons H.R. 1020 is apt to have difficulty getting enacted, because it is before the Budget Committee and it has a big problem. If it were enacted the way it is, it would give us access to about $800 million annually without competition for most of it with other programs. So yes, one could under H.R. 1020 aspire to $800 million a year average, and it even has provisions for it to go above that in some years if it averages out.

COHON: That would be sufficient to continue to pursue the program approach in the repository and to deal with the various transportation and other support issues in storage?

DREYFUS: If one were to simply take the program approach funding profile as we had it in our five-year plan and added to that a reasonable estimate of the interim storage that is in that bill, you would get a bigger number. On the other hand, that is a lot of billions of dollars. I am confident that given that kind of a funding profile you could accomplish the purposes of the bill, yes.

COHON: Thanks.

CANTLON: Don.

LANGMUIR: We have the impression from some conversations recently with our staff that the Congress is fairly comfortable with the program approach idea. At least as it is embodied in 1020. It sounds like that is your in track to educate them basically in terms of what the program needs to do and what it might cost to do the important things in getting to suitability and licensing. I am just wondering if that is what you are doing, if that is kind of the strategy you have in mind right now in terms of bringing them up to where you are and trying to maintain some support for this program and with some continuity.

DREYFUS: Mr. Cantlon shared the table with me at several hearings in which I attempted to do that, but the facts of the budget are the facts of the budget. They didn't buy it. You can't argue with that.

The language of the House appropriation report says either terminate, defer or reduce the work at Yucca Mountain. That's what it says.

The Senate bill very specifically says it's capped in 1996 at $250 million and the effort should be on design and performance assessment. The Senate report says very specifically it recognizes that the licensing will be indefinitely deferred.

There is no mystery in the appropriations bills as

to what they have in mind. Both of them contemplate some start in 1996 on interim storage, although they haven't quite authorized it. H.R. 1020 is a whole different game, but as I say, there is a bill introduced in the Senate that is similar but there has been no action in the Senate.

CANTLON: Thank you, Dan.

Our next speaker is Richard Craun from the Department of Energy with an update on exploratory studies facility.

[Slide.]

CRAUN: I'm Richard Craun, assistant manager of engineering and field operations. I have a brief status report for you on the progress we are making in the ESF.

[Slide.]

CRAUN: I will give you a brief status of the ESF, a little bit of information on some of the design process changes we are making, and some information our 1996 budget and our 1996 goals.

[Slide.]

CRAUN: When we created this chart we were down here, so I am not sure I wanted to keep the chart, but now that we are finished the year and we are up here and our plan was here, I use this chart as one of the first charts.

[Laughter.]

CRAUN: As you can see here, we completed FY-95 at

station 2002, and our plan in accordance with the program plan was 1280.

You can see some of the changes we have made throughout the year in decreasing the outage times and increasing the production of the machine, and thus the result.

[Slide.]

CRAUN: Since we have completed 1995, we are starting to look at 1996. This is the program plan and it is for FY-96. This is our current station. I think as of this morning, about an hour ago, we were stationed at 2257. This is a projection forward as to what we are planning on doing over the next few days. I believe in accordance with our current planning documentation we are going to go to approximately station 38 and 39, which would be right up in here, 3940.

[Slide.]

CRAUN: I have another chart that shows the south portal of that station at 78. The red line down here is where we currently are. Our 1996 planning effort started obviously before the end of the year, so we weren't sure if we were going to break the 1800 meters. So if you see information in the future that refers to 1800 meters, it was our best target at the time the planning documentation started as to where we might end up being. Again, we did

exceed that and made it to station 2002.

This one represents the first Ghost Dance fault access, and that would be currently where we are planning on shutting the machine down.

[Slide.]

CRAUN: I have some photographs of some of the surface construction. This is just one photograph here. Obviously the portal. The change house is nearing completion. And this is the conveyor system. Once we installed the conveyor system our tunneling rates were improved substantially. That is working quite well.

[Slide.]

CRAUN: Back to the TBM. This is another one of those fun charts that you see in the textbooks but you don't get to have on your project very often. This is our tunneling advance rate on a daily basis starting back in January of 1995 to September of 1995.

[Slide.]

CRAUN: A few quick statistics. Again, as of this morning at 8:00, our current station was 2257.

Our best week is about 500 feet, or 149 meters.

Our best day is about 50 meters. That was the end of the last day of the year. The crews wanted to break the 2000-foot mark, and they were able to produce 50 meters in one day. That's our best day.

The best shift was 23 and our best month was about 587 meters.

Accomplishments in 1995. We were ahead of schedule on the first station milestone. We obviously completed the end of the year ahead of schedule, and we completed alcove 3. Alcove 3 was a new technique we used. We used an Alpine miner. It took us approximately a week to construct that alcove. So it was much faster than the drill and blast techniques that we used on alcove 2.

We are about to start the construction of alcove 4, and again we will use the Alpine miner. Then we did start a conveyor up in July of 1995.

[Slide.]

CRAUN: I don't know how well this will turn out. It's our muck pile. It is getting bigger. We are using that to expand the ESF. We are building the base now. If we go forward with the repository, this will be the foundation for all the repository facilities. So we are expanding that at this point in time.

[Slide.]

CRAUN: I tried to take four photographs. It's kind of fun to be in the tunnel. I took them from alcove 1, alcove 2, alcove 3, and the end of the machine.

[Slide.]

CRAUN: This is what the machine looked like for a long

time when we first got started and it wasn't moving very quickly. That's about alcove 1, looking out of the tunnel.

[Slide.]

CRAUN: This is now a photograph of the view from alcove 2. You can see the conveyor system installed here. This is a fairly current photograph. The ventilation systems. Separate ventilation system for alcove 3.

[Slide.]

CRAUN: This is now the view from alcove 3. You can see the heavy use of the ribs.

[Slide.]

CRAUN: We put in a photograph of the Alpine miner in alcove 3 just to give you a view. A lot of rock bolt construction and wire mesh were used to construct alcove 3.

[Slide.]

CRAUN: I guess the next view I will enjoy is not being able to see around the corner. This is a view of the machine. It was photographed approximately last week. You can't really see the daylight anymore. That's at about station 2150.

[Slide.]

CRAUN: As we are constructing the tunnel, periodically, as several of you are aware, we are having to install booster stations for the conveyor, ventilation booster stations. We will have auxiliary fans that we will

put in line to increase the airflow or maintain the airflow requirements. This is the second booster station that is nearing completion. In fact it may be completed at this time. I didn't get a status this morning, but it is nearing completion. That will allow us to extend the conveyor system farther into the mountain.

[Slide.]

CRAUN: Some of the items that we have completed on the machine as far as modifications to the machine to help it perform a little better in poorer ground. We have got what I call a ski nose. It's on the bottom gripper. We have a little inclined plate so that as the gripper is brought forward it has a tendency to not want to dig in. If it's a square edge, it has a tendency to cut in and pull down. That gives us some steering problems with the machine. So we put a ski nose on the front of it, and that has helped us. If we are in blocky ground, it will kind of ride up over the blocks.

We have completed a three shoe gripper. On three or four occasions we have actually over excavated. The crown of the tunnel has actually been excavated out. So you lose gripper action on the top gripper. So we have now completed a modification that will allow us to set the horizontal gripper and then push off the bottom gripper. That will allow us to maintain steering capability without

the top and bottom gripper combination functioning properly.

Then we have installed some slot shields. Along the top between the side grippers and the top gripper there are some slots. We have installed some shields that we can actually hydraulically put in place in real poor ground. That will help us control the rock falling in on the miners.

[Slide.]

CRAUN: There was supposed to be a slide here that said what we have done to the design control process. Somehow that slide is not in my presentation. This is the answer to the problem.

[Slide.]

CRAUN: We have gone from a very large package, the 2C package. We had a lot of difficulty getting that package approved on time and issued. In fact, it was even pulled back into the design control process and reworked and then issued. We have taken those large packages and broken them into much smaller packages. As a result of that, we were able to get designs out on time. We are giving the reviewers a little more time to do the review, a more focused review, and our products are coming out in a little better format.

As a part of that discussion there have been a couple of questions on who are the reviewers, so I thought I would share with you who the reviewers of our design

products are.

We have internal reviewers and then we have our external reviewers. We get comments or observations from either set and we comment or resolve or respond to all those comments.

The process is working much better for us. We have been able to produce about eight or nine design revisions within the last three months. Most of those were on schedule and have had an average of maybe one or two comments that required some work on each of the design packages. So our process is improving.

[Slide.]

CRAUN: Some of the examples of the new process have been applied to what is called the 8A revision to the 2C package. That is the main design of the loop. We have issued our new plan and profile drawings for the main drift, and we used it for alcove 2 and alcove 3.

[Slide.]

CRAUN: I put one quick slide in here. I have discussed with the Board several times the Board of Consultants that we are using. We have our first meeting scheduled for October 24 and 25. The Board members that will be participating are listed there. Their focus will be continued improvement of the machine and cost control.

[Slide.]

CRAUN: Our 1996 budget and planning information. Our current plan is to proceed to station 39 plus 40. We should reach that station by about March of 1996 or sooner, depending on whether or not we maintain our current rate. The planning basis for that construction point is about 22 meters a day. We have been tunneling at rates of 35 meters a day. So if we continue at those rates, we should exceed that and be there prior to March.

We want to complete the design efforts for alcove 4. There are two Ghost Dance fault alcoves, one at about station 39 and the other one at about 49. For the first Ghost Dance fault, that would be approximately where we stop the machine. And the heater test alcove, which is going to be located at about 28.

Then we want to complete the work on some of our surface facilities. I showed you earlier a photograph of the change house. That needs to be completed in this fiscal year.

[Slide.]

CRAUN: Some of the items that we have deferred from our 1996 planning. We have deferred Calico Hills and we have deferred the design of the ramp extensions.

I will explain that in just a moment. I've got another slide that will help explain it.

And deferred the second Ghost Dance fault.

We are focusing our design on what I call an "investment analysis." That would be the design necessary to support TSPA, necessary to support an estimate for completion and a schedule for completion for licensing.

[Slide.]

CRAUN: I said I would come back and talk to you briefly about the areas that have been removed. As defined in the original program plan, this is the north ramp extension, the south ramp extension, and the remainder of the ESF loop. Current intentions are to proceed to station 39. We currently are at station 2250-some. We have started into the corner, into the turn. At approximately station 28 we will be out of the turn and we will be in TSw2 at that point.

[Slide.]

CRAUN: It looks like I may have already covered this. Our design efforts are being focused on those activities necessary to support the TSPA analysis, to develop a defendable life cycle cost, and design sufficient to support a construction schedule and identify any significant risks or issues associated with the designs.

[Slide.]

CRAUN: For the basis for our estimate of 1996, we assumed an increase in our production rate. In 1995, our average tunneling rate was 11 meters per day. For our 1996

basis, our performance is obviously much better than predicted in 1995. For 1996 we used 22 meters per day, and right now we are exceeding that.

Our goal is to excavate to station 49 by March and excavate the heater test alcove.

That concludes my presentation. If there are any questions, I would be happy to answer them.

CANTLON: Don.

LANGMUIR: Dan Dreyfus has described to us the serious problems with budget and reductions in budget and you have indicated you have $61.46 million for your budget. I gather that does not cover some of the originally planned activities in the ESF. But what I am most concerned about, and perhaps we will hear about this from William Boyle, is, what is this doing, if anything, to reduce and significantly cut back, if it is, the science and engineering which was originally the intent of the ESF that would take place as you proceed through the tunneling? What is going on there? Are we still seeing the same tests and measurements that were planned originally, which was the reason for having an ESF?

CRAUN: I can let Bill address part of that. I know we are continuing on the design for the heater test. The information that will be gathered for that, we are proceeding with that. We are still doing all of the mapping

associated with TBM operations. So we are gathering all of that.

The change from 1995 to 1996, we had initially planned a significant ramp-up in the design activities associated with repository subsurface. That will remain fairly stable with a minor decrease in funding profile from 1995 to 1996, so there will be less design activities supported in the 1996 funding profile. That is why we are trying to focus on those items that directly affect the total system performance.

For example, we are slowing down appreciably the surface facility design. Those activities will be cut way back. Our efforts in criticality, thermal retrieval, drift layout for emplacement, those issues will proceed in 1996 under the current funding strategy that we have.

We are trying to focus in on those items that are essential to the layout or the design of the repository.

CANTLON: Ed.

CORDING: Rick, I am really pleased. Over the past several months I have been getting the reports on the progress. It really is good to see the rates that you are achieving. To do that requires a real integrated system, as you obviously know. I just wanted to comment on that.

In order to have a machine operating at good rates of progress it requires support being installed in an

efficient manner; it requires the muck being removed efficiently, and that is what you have with your conveyor belt system now; it requires supplies and materials ready and being brought up so that they are there to be put in. The quality control and all the other things that go to approving the design that is there has to be in place. That is quite an accomplishment and you are to be congratulated on what you have been achieving.

Certainly ground conditions have an effect. You have been in perhaps better quality ground. But it is not just ground conditions. It's a system that can respond to the ground conditions. So I am very pleased to see what you have achieved. I can see you are trying to take advantage of that progress and to maximize your exploration capabilities, and I see somewhat the conflict that comes with the fact that the funding is such that you may be limited in being able to take maximum advantage of what you have achieved in getting access underground.

I wanted to go on to a couple of points on your progress. On one of the pages you describe in March getting to station 39 plus 40. That is current planning.

CRAUN: Yes.

CORDING: Then in your last overhead you describe being at station 49 in March.

CRAUN: That might be a typo.

CORDING: That says an additional 16,000 feet rather than 12,000.

CRAUN: That would be the second Ghost Dance fault. Excavation to station 49 would be the second Ghost Dance fault.

CORDING: Would that be by March of 1996?

CRAUN: That's what I would like to do. Our current planning document is 39.

CORDING: If you keep up 22 meters a day advance, you would be able to get to 49; is that correct?

CRAUN: Right now we are at station 21. In the current baseline we have got about 2,000 meters of production to do to reach the goal of 39 plus 40. So 2,000 meters divided by 22 will give you the number of days of production. That should be around March.

CORDING: To get to 49?

CRAUN: To get to 39.

CORDING: The goal on 49, is that really a goal for 39?

CRAUN: Yes. It's a typo. It should be more correctly stated that station 39 would be the goal.

CORDING: In March, at that point the question of whether you would advance the machine beyond that point is something you are still considering; is that right?

CRAUN: We are looking at ways to alter the infrastructure associated with the TBM operation. Currently

with our budget of $60 million, under the current processes that money will be exhausted in the March time period. We are looking at making some management changes and some changes in our infrastructure that may allow us to decrease the consumption rate of that money. If we are successful, and we should know by the December-January time period, that will make a decision available to the program as to whether or not to continue with the machine's operations and/or to return some of that $60 million to the program to be used in science or engineering. But we are looking at ways to try to improve it.

From 1995 to 1996 we have improved our cost per meter by 40 percent. Our goal, though, is to try to improve that by 80 percent. What we are looking at is all the infrastructure support necessary to keep the machine running. We are not sure that we can make the 80 percent. We may make 70 percent; we may make 60 percent; but we are looking at different ways of structuring the infrastructure to keep the TBM running that will allow us to significantly reduce the cost. If we can, then the program has the option of keeping the $60 million and putting it into tunneling or to take part of that $60 million back and put it into science or engineering.

CORDING: Most of that infrastructure is in support for the operation?

CRAUN: It's support for the operation. You have got an entire procurement process in place. For example, we have been able to issue blanket purchase orders to most of our vendors. We were able to accomplish that in FY-95, so that in fact all of our steel sets are on order or we have set orders in place so we have less paperwork that we may have to accomplish in 1996. We are looking at ways in which we can issue blankets so that the processes of physically procuring material in the government are simpler and easier for us. That is what we are looking at doing.

CORDING: One other comment. As soon as you get around the bend to where you can do the heater alcove, is it the intent to start that construction, or are you going to wait on that to continue the TBM beyond that point?

CRAUN: We will get the TBM past it. I know the design was just released for alcove 3. We have initiated a modification to our design control process that allows us to issue the alcove designs faster and cheaper. Shortly after passing the heater test alcove we will be hopefully ready with that design. So we would have a design out and then construction could come in.

I don't know, Ed, that I remember exactly the date to start mining the alcove for the heater test. I don't remember that off the top of my head.

CORDING: Will they be able to take advantage of your

improved progress and getting that started earlier?

CRAUN: Yes. We are interfacing very closely with science. As we increase the tunneling rate and the TBM is ahead of schedule we are working very carefully and closely with the science side to make sure that we don't get out of step with them.

CANTLON: Pat.

DOMENICO: You will actually be out of money in March. Is that what I heard? And in January you will make a decision whether you will continue beyond March, but how can you do that if you realize you are going to be out of money?

CRAUN: If we are successful in reducing our cost per meter, then I will not be out of money in March. If I spend at the baseline rate, then we will exhaust our funds in March. That's correct.

DOMENICO: In the event that you do stop, when would you anticipate starting up again?

CRAUN: I'm not aware of any plans to restart the machine.

DOMENICO: Not even next year's budget?

CRAUN: It's not in the out-year plan. It has to do with any information that we would get from Congress on the guidance for our program, but at this point in time it is not in our planning documentation.

CANTLON: Don.

LANGMUIR: I would presume there would be an intent in the program with available funding to initiate the thermal testing which is so critical to the long-term predictions of performance. If you have to shut down, there is already test work that has been initiated in alcoves before you stopped, including the heater tests. What is the intent of the program in terms of supporting those activities?

CRAUN: Let me speak for Susan Jones or Bill Boyle. Their first priority and our priority as a management team at the Department of Energy was to ensure adequate funding to continue those critical tests. The in-drift heater tests would continue. That aspect is currently funded and in our planning documentation along with continuing the gathering of the data from the existing alcoves that we have now.

LANGMUIR: In connection with the shutdown, I gather you are finding you can do things for less because you are in sounder rock. Your baseline costs involved a lot of things, including not having the conveyor system perhaps and being in more difficult rock which costs more to get through and took longer. Is it a reasonable presumption that you can do it for less given that the rock seems to be sounder at this point?

CRAUN: Good rock doesn't hurt me. That helps a lot. I'm not a miner, so I don't have a lot of experience in that, but I have found out that if I'm in competent ground

the machine does perform better.

The change in our philosophy on the infrastructure is more than just good rock. We are really looking at ways to eliminate redundancy throughout the program, throughout our management structure in the area of the TBM operation. If I run into poor ground or faulted ground, I'm going to have a hard time improving the efficiency of the operation. If I remain in fairly competent ground, it would be more of the management systems that we would be trying to improve on the efficiency in order to obtain the extra money to either go forward with the machine or increase funding to both science and engineering. It is both an efficiency in the tunneling operation due to good ground and also the infrastructure.

CANTLON: Clarence.

ALLEN: Because of budgetary restrictions you necessarily had to defer a number of these things you had hoped to do. As you currently visualize the situation, are those things, such as the north and south ramp extensions, eventually going to have to be done to support licensing?

CRAUN: That's why we use the word "deferred" versus "cancel." In my mind, many of those activities for a license will be needed. So we just deferred them from our current planning. If in fact the funding is restored and we head back toward a license application, many of those

activities will have to be rescheduled, at least in my mind.

ALLEN: Including the Calico Hills?

CRAUN: Well, there is an opportunity here for success. As it pertains to the current waste isolation strategy, I think our understanding of the the role of Calico Hills may mature over the next year or two. It may not be a key player in our strategy. If it is a key player, then in fact access would be, in my mind, warranted. If in fact it is not, then deferral of that activity would be appropriate.

ALLEN: So there is some indecision on the Calico Hills, but as far as you are aware the north and south ramp extensions are an absolutely necessary part of the licensing requirements.

CRAUN: Let me speak from my perspective. An east-west drift would be very important from a licensing perspective at some point.

CANTLON: Ed.

CORDING: Will you be starting in fiscal 1996 the east-west drift at the Ghost Dance? Will you start that drift at Ghost Dance, which is alcove 4?

CRAUN: No. The heater test alcove will be located at station 28, approximately, and the north ramp extension is not in our planning base for 1996.

CORDING: You are going to be getting down to station 39, which would be right at the first Ghost Dance fault.

CRAUN: Yes.

CORDING: In fiscal 1996 would you then go into the Ghost Dance?

CRAUN: I misunderstood your question. Yes. The current plans are to keep the labor force there. If we terminate operation of the TBM, then the miners would continue to the excavation for the alcove for the Ghost Dance fault. That's correct.

CORDING: Even if you decided to go on, I presume that there would be some --

CRAUN: We have sufficient labor resources to do concurrent mining operation in the alcove and also to continue excavation. That's correct.

CORDING: Presently what is the length of that alcove, or is there a length that is set for that?

CRAUN: I don't have that today.

CORDING: As far as you can go?

CRAUN: It would be to the Ghost Dance fault. Are you asking how long the alcove is?

CORDING: Yes, the east-west. That's a first attempt at going east-west in the center of the repository, or it's the first opportunity, let's say, to go east-west in the center of the repository.

CRAUN: I don't have the dimension of that alcove at the top of my mind.

CORDING: It seems to me that that is one to look at. First of all, Ghost Dance, from what we have seen so far, is not one surface, one plane; it's a zone. Trying to get a good cross section through there even a little bit to the east would be, I think, very beneficial to the program at this point, particularly if you don't have the funding to pursue a true crossing all the way to the Solitario. I would like to hear more of what your plan is on that.

CRAUN: I think I should point out the heater test alcove would have a portion of it that is in an east-west orientation also. That would give us some east-west data.

CANTLON: Russ.

McFARLAND: Rick, you mentioned the Board of Consultants will have the first meeting on October 24 and 25. You have a rather broad group of people, a construction manager, a geotechnical engineer, a major program manager, and a machine specialist. How do you intend to make use of these people under the present budgetary constraints?

CRAUN: I'm hoping to give them as much information as I can so that with their knowledge and experience and with our knowledge from the M&O and the DOE we can get some synergism going, so that we can try to come up with maybe some more thoughts on how we might further improve the efficiency of the machine in both tunneling rate and also from a financial cost control standpoint.

In many ways my expectations are just to kind of have a free form. How I want the board to kind of work in the first few sessions is to have a free form for getting the ideas on the table so that we can kick them around. If I can get ideas from them, then we can work them after they depart and then get back together with them and give them more information. I really just want to have some think-tanking. I think that combination of board members has the background and experience to help us kind of free form like that. I think it will be a good meeting. I'm looking forward to it.

McFARLAND: Thank you.

CANTLON: Early on when you were having trouble in the blocky ground there was a whole array of machine modifications. Are you through now with all of the machine modifications you intend?

CRAUN: Yes and no. I want to get some experience under my belt on tunneling in TSw2, find out how it's working. We do have a canopy design that we have proceeded with. We are far enough on the design so that we know that it's about a six to eight week outage of the machine in order to install the canopy. It was the same canopy design that they installed on the machine down at Magma Copper, I believe. We do have some other modifications that we have sketched out in a fairly detailed manner, so that if we do

get into poor ground and the machine's performance is very poor, we do have some other modifications that we could come in and install.

Typically those modifications are fairly intrusive to the machine. I believe the invert thrust design would require us to disassemble the entire bottom portion of the machine around the bottom gripper. So that entire gripper assembly would have to be dismantled, which would be a fairly substantial modification.

CANTLON: Other questions? Board? Staff?

[No response.]

CANTLON: Thank you, Richard.

The next speaker is William Boyle, DOE, update on the scientific investigations.

BOYLE: Good morning. Thank you for being here.

[Slide.]

BOYLE: As you might see, the title on this sheet differs slightly from that in the agenda handed out today. I will try and explain what it is I hope to talk about today. You will hear over the next two days some people talking about a million year waste site selection strategy and a million year regulatory period, but I'm just talking about the last three months.

[Laughter.]

BOYLE: It's my understanding that the Board wants an

update on the ESF effort and also what progress has been made in the scientific program. The presentation was made in Salt Lake City in July, so I'm mainly covering what we have learned in that time period, but it's apparent some of the Board members want to hear about what we are going to do in FY-96, and I will try to briefly address that at the end.

I also realize that many people in the room are not geologists and geologists have their own jargon. I will try and explain the terms as I go along, but if a word like "lithophysal" creeps in and you don't know what it means, just raise your hand and I'll stop.

[Slide.]

BOYLE: Before we stared the ESF we had done investigations from the surface, including mapping, drilling and geophysical investigations. We had an idea what the geology would be, but now as we excavate the ESF we are mapping it. We are recording the geologic conditions encountered as we go. What we have mainly found out so far is that what we are finding underground agrees pretty much with what we thought we would see.

[Slide.]

BOYLE: Here is a picture. This may show better here than in your photocopies. These are two of the geologists mapping the geology in the tunnel in between the steel sets used for support. Here is a picture of the mapping gantry

that moves back and forth on rails to allow the geologists to record the conditions. Even when the excavation proceeds at a rate of 50 meters a day the mappers have been able to keep up.

[Slide.]

BOYLE: This is a cross section covering the first 1400 meters. This cross section is a vertical slice along the tunnel alignment recording the geologic conditions. This is largely based on the conditions seen along the tunnel alignment shown here. We obviously don't have information on all these, but a lot of this is interpreted based on the condition seen here and also from drill holes. This is the best idea we have today as to what is really out there.

I said earlier what we are finding underground is pretty much what we thought we would see. The very next slide I am going to show you will have similar faults but different.

The most noticeable difference is in this slide you will see that these faults are nowhere near vertical. They are shallower than the faults you will see on the next slide. It is not because the faults are shallower as encountered. Where the faults are different is that from surface mapping people thought that their orientation in space was north to northeast, but as encountered in the tunnel they are actually north and northwest. So the faults

are becoming more in line with the line of the section. It just a geometry problem. When you draw the cross section, the more the cross section is in line with your feature, the flatter steep features become.

That is your geology lesson for today. It's called an apparent dip problem.

[Slide.]

BOYLE: This is what we thought we would see. As you can see, the faults here are more nearly vertical. Like I say, based upon what we found in the tunnel and actually a reinterpretation later of the faults at the surface, they really are striking more northwesterly and not northeasterly. This was the prediction through 1,400 meters.

[Slide.]

BOYLE: Here is the prediction from 1,400 meters through 2,800 meters. I will stop here for a second.

At the July meeting in Salt Lake City there was some confusion about this numbering system with the plus in the middle. It's a surveyor's convention. To know where you are, just remove the plus sign and look at the numbers as the number of meters. Surveyors keep track of things in terms of stationing every hundred meters or hundred feet. So 26 plus 00 represents 2,600 meters.

As Rick mentioned, we are out here at 2,250 or so.

As you will see in this predicted cross section, there is this major feature, the drill hole wash fault. As it turns out, it didn't turn out to be a major feature yet. Why is that?

As shown on this cross section, the drill hole wash is largely based on inferred information that in the presence of drill hole wash is covered with alluvium. If we were to look at a geologic map, I'm sure the fault would be shown as a dashed line with question marks, which to a geologist means he's not really sure but he thinks there might be a fault there.

ALLEN: I hope it would be a dotted line, not a dashed line.

BOYLE: It's certainly not a thick black line. At least I hope not.

There are faults nearby. The drainages are parallel. So people were inferring that perhaps the drainages were parallel due to faulting. As it turns out, there was a fault encountered back here in 1,900 with the same strike as would be thought should be present for the drill hole wash structure, a northwest feature. So it's arguable that perhaps it was just misplaced or it's a fault of the same system. The bottom line is the drill hole wash structure was nowhere near this large.

[Slide.]

BOYLE: This is not in your packet of materials. I can give the staff a copy later. This is a prediction from 2,800 coming out of the curve. This is the north-south traverse across the repository block out to about 5,800 meters. The tunnel is right here. It hugs the contact between the Topopah Spring welded unit number 1 and Topopah Spring welded unit number 2. Because once we get out of the turn we are more nearly parallel to the structure, there aren't as many faults crossing the alignment. There is the one fault down here at the end of the wash.

[Slide.]

BOYLE: There is a table of symbols. There is no information really to be gathered. It is just to let you know you can look at that.

[Slide.]

BOYLE: A subject that has already come up a couple of times today is the rock mass quality. TSw1 is the rock unit we are in now, Topopah Spring welded unit number 1. It is measurably better, as I will show you with the next viewgraph, than the Tiva Canyon welded units that caused a lot of problem early on in the construction. The way in which it is probably most significantly better is there is a lot less fracturing in the TSw1. That is not really what we expected from drilling. This is a case to prove that you can't always tell what you are going to hit underground

based solely on drilling.

I mentioned this word "lithophysal." In some of the rocks out at Yucca Mountain there are fossil gas bubbles, if you will, in the volcanic units of various sizes. In TSw1 they are frequently an inch, two inches, three inches in diameter. When we do our surface drilling we take a core that is two to four inches in diameter. So as you are coring through these holes that are roughly the same size as your core, the core falls apart. When you come through later with a 25-foot diameter tunnel boring machine, a one or two inch hole makes no difference at all. Our drilling had indicated that the TSw1 actually might not be that good based on core recovery, but in actuality, based on tunneling, it is better.

[Slide.]

BOYLE: When things are going much better people wonder why. So we went back and we looked at the drilling records and the lab tests. A lot of the increase in rock mass quality is reasonably attributable to much lower strength and Young's modulus for the TSw1 rocks in comparison to the Tiva Canyon rocks. The Tiva Canyon rocks have an unconfined compression strength of about 30,000 psi. Typical concrete is about 3,000 psi. The TSw1 rocks are about 9,000 psi. So they are strong enough to take whatever loads exist in the earth but they are so much weaker than the Tiva Canyon rocks

that the tunnel boring machine can excavate through them much more easily. It's quite probable that the lower Young's modulus is over geologic time the rocks have not been as fractured as much as in the Tiva Canyon rocks, which have a greater Young's modulus.

Based on everything we know today about the repository horizon rocks, based on what we know from surface outcrops and drillings, we may have a decrease in rock mass quality when we get to the repository horizon. Again, based on our drilling, we know that the fracturing seems to increase, the RQD is lower, the rock quality designation is lower in the TSw2 unit. Also, from lab tests we know that its strength is higher, much more comparable to the Tiva Canyon strengths rather than the Topopah Spring welded unit number 1.

It is clear that the geology has cooperated. That is partly responsible for the increase in tunneling rates, but I think this chart would show that geology is not completely responsible and that the constructor is doing things differently and better.

[Slide.]

BOYLE: What we have here is just a plot of distance along the tunnel with the geologic contacts. Here is a Tiva Canyon rock, Tiva Canyon rock, bedded tuff. Here is TSw1. On the vertical axis we have a measure of rock mass quality

designed by Q with a logarithmic scale. We have these various ground support classes shown on the right.

Ground class I is good to very good rock; II is fair rock; III is poor to extremely poor; IV is extremely poor; and V is even worse than that.

[Laughter.]

BOYLE: As you can see, back in Tiva Canyon we were generally in poor to extremely poor rock, and that was slow going. I would argue that these dots in general plot higher than these, so the rock mass quality has increased, but we still have a very significant number of readings in ground classes II and III compared to IV, and yet the tunnelers have still been making very good progress. So it is arguably not just the geology that has improved but the tunnelers are getting better.

[Slide.]

BOYLE: Here is a feature I am going to talk about, switching from tunneling. It is something we saw in the ESF. For want of a better name, it is called a fumarole. Some people get in a discussion: is it a fumarole or not?

The bottom line to many people is it doesn't matter what you call it. Some people think it's a fumarole deposit. Others think it's a weathered fossil soil. What we do know is that there is some evidence of elevated groundwater temperatures. When these rocks were deposited

over 12 million years ago they were still hot; they were still giving off gases, including water vapor. You can go to Yellowstone or Hawaii or anyplace where there are hot rocks underneath the ground surface, and both the heat and the gases do not come out uniformly. They come out preferentially in places. What we intersected in the ESF was one of those places. It's a fossil fumarole.

What I would like to make clear is that this feature was formed roughly 12 million years ago and it's not a new feature or anything like that. Tests have been done and the specimens have shown to be elevated in lead, zinc, and also tin, but far below any economic values. More recent tests results have shown there are no anomalous levels of gold, silver and mercury.

ALLEN: Excuse me. How do you know these originated roughly at the same time as the rocks were deposited?

BOYLE: I have a memo from Rick Spengler and Zell Peterman dated August 1st where they interpret them as being that old.

ALLEN: Is this not a very important question?

BOYLE: I would say so, but I will also say that there are similar zones observed on the west slope of Yucca Mountain. They are not some new thing. What you will see on the next slide is that the faulting clearly postdates the formation of this feature. People who know far more of the

details have determined that these really were created shortly after the units were emplaced and it is related to the cooling of the underlying rocks and not some more recent phenomena. I don't know all the details on that, but I'm reasonably sure that that is what they are convinced of.

[Slide.]

BOYLE: Here is a picture of this fumarole. This really doesn't do it justice. You actually have to go out in the ESF and see it yourself. It shows up on both sides of the tunnel. It is easily seen in the tunnel because of the distinctive coloration, the oxidation of iron for the red color.

Here is the fault. When you go in there you can see that the fault clearly postdated the formation of this feature, and the fluids apparently were not going up and down the fault either, that there aren't zones of alteration on either side of the fault.

[Slide.]

BOYLE: The presentation isn't only related to the ESF. Here are some results on pneumatic monitoring to find out how the mountain breathes, if you will.

Preliminary results of the pneumatic monitoring during the TBM advancement. The Paintbrush tuff non-welded unit retards gas flow but it is not a complete barrier to gas flow.

The Topopah Spring welded unit has a pneumatic time lag that is affected by the TBM. If you will, the excavation itself is short-circuiting the flow path through the mountain.

Tiva Canyon welded and Topopah Spring welded units, which are hard fractured rocks, their fracture network is highly interconnected and covers a large areal extent.

[Slide.]

BOYLE: Also, some stratigraphic studies were done last year and they are being summarized now. One thing that we found out is the Sundance fault zone is apparently much shorter than people had originally thought. This was re-mapped and interpreted, and it is apparently only 20 percent as long as originally thought. It is narrow and discontinuous. A maximum vertical offset of 12 meters, which is not that much as far as faults are concerned.

ALLEN: Excuse me. This has nothing to do with the tunnel observation?

BOYLE: No. As I mentioned early on, not all of this talk deals with the tunnel. This is based on a re-mapping at the ground surface, a reinterpretation. Different geologists from the USGS went out there and re-mapped it.

[Slide.]

BOYLE: Here is a map of their interpretation of what

they had seen. A few years ago people thought that Sundance fault was this long. Now, based on more recent mapping, they think it's only this long. I think with the progress through March we will go to or through this point. So we will get to see for ourselves soon enough, which is another argument for excavating underground. You can't always tell from the ground surface.

[Slide.]

BOYLE: From trenching and earthquake records we know that the Rock Valley fault should be considered active. It has a significant potential for future earthquakes and possible strong ground motion at Yucca Mountain.

[Slide.]

BOYLE: I'll show you a map locating Rock Valley and where some of the earthquakes are. This is labeled Rock Valley Fault Epicenters. It should be Rock Valley Earthquake Epicenters since May 16, 1993. As shown in red here, labeled 1,2,3,4,5, these epicenters, to locate you on the map, here is US 95. Las Vegas is off to the southeast; Reno and Tonopah to the northwest. Here is US 95 again. Here is Mercury. When you drive out to Yucca Mountain you typically drive out this road right through Rock Valley.

LANGMUIR: How far from Yucca Mountain are we here?

BOYLE: That's on the next slide.

[Slide.]

BOYLE: On September 7, 1995, 20 miles south of Yucca Mountain there was an earthquake. It's magnitude was 3.5. Slight ground shaking was felt at the field operations center, which is five miles north of the epicenter.

The focal depth has now been determined to be four kilometers. It was initially thought to be shallower. When you look on that map I have just shown you, some of the historical earthquakes were quite shallow.

Indications are that it had a strike-slip focal mechanism, which is consistent with the fault zone strike and also the other earthquakes that have happened over the last two or three years.

[Slide.]

BOYLE: Consolidated sampling in the ESF. As of September 22, 919 samples have been collected. For those of you who have been in the ESF, you will see these little cards all over where samples are collected. So the ESF does serve a scientific purpose. It's not just a hole in the ground. It does give us an opportunity to map but also to collect samples for all these other studies.

That was the end of my presentation. I can address FY-96 planning now or I can wait for Professor Langmuir's questions.

[Laughter.]

CANTLON: Let's do the 1996 planning and then we will

do the questions.

BOYLE: I really didn't come prepared to talk about that, but Dr. Dreyfus had mentioned earlier that essentially what we would be doing this year largely was digesting. We have 13 synthesis reports due in FY-1996 dealing with all sorts of topics and scientific programs: hydrology, geochemistry, geology, and geophysics.

Instead of spending our reduced funding on gathering new data and at the end of the year not knowing where we are, we are going to spend a lot of our money looking at the data we've already gathered and try to determine what it all means.

At the end of the year we will be left with some questions about, well, we really ought to answer this. Those items will be addressed in FY-97 or in out years.

We will gather some new information this year, but it is largely based on the ESF. The surface-based testing is largely going away. The new data gathering, as I mentioned, would be in the ESF. Mapping information from alcoves, the thermal testing, planning and implementation would proceed ahead, and the gathering of irretrievable information like weather records and things like that.

To give you a rough idea, I think our budget is somewhere around $36 million to $37 million for the fiscal year we are in now, 1996, whereas last year we spent some

$90 million, plus or minus. So it is a significant cut, and many things had to go.

LANGMUIR: A related question. I presume that as part of the irretrievable maintenance of information collection you are going to stay with the water level recording effort that has gone on historically in terms of groundwater at the site.

BOYLE: That would be my impression, yes, that we would. It's actually a low cost item.

LANGMUIR: But you have got to pay the salaries of the folks who do it.

BOYLE: Right. They will probably be the same individuals who are working on synthesis reports, making them do two things during the course of the year.

CANTLON: Clarence.

ALLEN: Thus far in the tunnel, to what degree do you see stratigraphy or micro-stratigraphy on the walls of the tunnel and the exposures that would allow you to, say, talk about displacement across faults, or are the faults recognizable only because they are fractured in the wall?

BOYLE: Oh, no. It depends on the units you are in. The bedded units, the faults are typically much more well defined. Very frequently just a single plane. Particularly when they have a smaller displacement, you can map distinctive beds either side of the fault even down to the

inch level for some of the faults, if you want to call them that. It becomes more problematic when you get in the more massive welded units. It's just monotonous and it's difficult at times to figure out what the displacement is.

ALLEN: In the repository horizon it is going to be particularly critical when we encounter faults to say something about the displacement. Are you optimistic we will be able to do that?

BOYLE: Yes. In some ways they are telling how much displacement there is. I think there was a whole series of faults at around 1,100, 1,200, 1,300 that continued to drop the bedded units back down towards the tunnel section. They had displacements of a foot or less, but they were able to figure it out. There are partings parallel to the bedding. There is some strike parallel to the bedding of the units, and if those features are crossed, sometimes you can get an idea.

ALLEN: I guess to be consistent with formal geological jargon here, we should be saying separation and not displacement, because we really, at least as yet, don't know much about the possible strike-slip components.

CANTLON: Don.

LANGMUIR: One of the more obvious questions you will get every time you stand up there and describe what has happened for three more months is going to be, have you

found water? Has water been identified in the fracture zones or faults or anything of this kind? What is the status of those kinds of observations?

BOYLE: Essentially, no. That is the quick answer. Of those 900-plus specimens I'm sure some went off for determinations of water content.

LANGMUIR: Certainly there is water content, but have you seen a dripping into the tunnel?

BOYLE: No. Absolutely not.

LANGMUIR: Any wet surfaces?

BOYLE: The only place that I know of that was significantly wetter and obviously so was near the base of the Tiva Canyon and the top of the Paintbrush tuff non-welded units. We knew from drilling that that contact area typically did have a higher water content.

We haven't seen dripping or anything like that, but there are areas in the tunnel, one very close to the first booster station, where you can see clays in place that have now become desiccated and cracked. So obviously they had some water. They are still there, but there are no drips or weeps or seeps or anything like that in the tunnel so far.

LANGMUIR: I'm going to ask a loaded question. I think all of us involved in hydrology or chemistry are concerned that we have now become one dimensional in the sense that we

are now doing a tunnel and we're not looking at the surface anymore. Many of the questions that remain to be answered and perhaps could only be answered from the surface are no longer going to be answered. One of those questions had to do with the very steep groundwater table that apparently plunged towards the repository from the north.

BOYLE: Right.

LANGMUIR: What is your sense of where we are with that and our understanding of what causes that and how that might impact performance of the repository?

BOYLE: I don't know that we are any farther along than whoever talked to you last about that, because I don't think we have really done anything up there in terms of new holes. I am trying to see if I have any notes.

There is one new borehole -- no, no new boreholes.

LANGMUIR: There were going to be a couple, were there not?

BOYLE: Right.

LANGMUIR: There were two planned?

BOYLE: For the program plan there were, but not now. At least not in FY-96. The way the planning was done is for FY-96 we've cut back on a lot of the data gathering and will do data synthesis. Our budget is supposed to go up in FY-97 for scientific programming and the percentage of money spent

on data gathering will also go up.

DOMENICO: Is there any semblance of the surface-based program remaining?

BOYLE: By the end of the calendar year?

DOMENICO: Yes.

BOYLE: Very low. I think there is some mapping that will continue. There are certain specific activities.

DOMENICO: No drilling?

BOYLE: Essentially by the end of the calendar year, I think it will pretty much stop, yes.

CANTLON: Russ.

McFARLAND: Bill, you started your first or second viewgraph with the statement that geological mapping data agreed with surface-based activities. Then you proceeded to show where it didn't.

BOYLE: That's right.

McFARLAND: Over the last year since I have been on staff there have been continual arguments by some of our good friends on the importance of underground exploration versus surface-based drilling exploration. After going through this 7,000 feet of tunneling, and particularly with the questions the Board has consistently stated with the east-west drifting, how important do you feel this east-west drifting is? Can we get the information needed by other means such as geophysics or surface-based drilling?

BOYLE: I know people have made that argument at times, but I would think there are any number of things that I've shown today that would argue nothing replaces going down there and seeing for yourself for constructibility issues, for structural issues. Apparently Don Langmuir believes there are other questions related to hydrology or geochemistry that are best addressed with surface drilling.

I would like to address the issue on the fault. People thought they were north-northeast based on Scott and Bonk's map, which is in a large scale, if you will. People that have gone back and looked at it say, well, no, it really should have been interpreted north-northwest to begin with, which is what we found in the tunnel. So it is arguable whether that is really that much different from what is really there. There may have been a misinterpretation or a legitimate difference of opinion between various people on what the strikes of the faults were.

CANTLON: Ed.

CORDING: I was interested in the change of the quality of the rock. One of the things that was pretty obvious in the first part of the tunneling when you are at very shallow depth was that not only were there a fair amount of joints, many of them crossing in that north-northwest orientation, but joints as well as small faults. But there were a lot of

very open surfaces going in there. Particularly alcove 2. It was actually driven in some of that. In the tunnel and in the alcove it looked like a bunch of loose teeth in there. That is very difficult to support. When the rock doesn't interlock and comes apart you are losing a lot of capability of the rock to support itself because those irregularities on the surface just aren't interlocked. That was one of my questions.

How much are we seeing of the conditions now? Perhaps the joints may not be as frequent, but it seems to me that one of the major changes one would expect as you get deeper is that the joints are tighter, the ones that you do see, and that there is more interlocking, and therefore it is much easier to support it with a light support system like rock bolts. What is your reading on that?

BOYLE: I would agree with that. Based on a visit last week in the TSw1 unit, even in the lithophysal unit, major through-going fractures are few and far between, but there are more minor fractures that caused us problems in the Tiva Canyon at shallow depth. In the TSw1, even in the lithophysal unit, the surfaces are so much rougher and they really are tightly interlocked, but you can work it.

CORDING: One other question on the alcoves. What are you learning from the alcoves, or have you gotten to the point yet that you are getting information out of alcove 2

and 3?

BOYLE: I asked the hydrology team leader that. They haven't found anything surprising. Whatever they thought about the mountain going in, the results that they have confirm that.

CORDING: I'm not sure whether they are surprised or we would be surprised, but it would be interesting to hear what they are accomplishing.

If you don't see water, it doesn't mean that there isn't seepage. It depends on the humidity. In a dry tunnel with low seepage rates you can get evaporation before you see it.

BOYLE: We have a thermal-hydrologic peer review committee of which Paul Witherspoon is the chairman. He mentioned at STRIPA they can tell that there was evaporating seepage by using some sort of a thermal measure, an infrared device, if you will. Although they couldn't see dripping or seeping water, they knew from the heat loss that it was occurring. As far as I know we haven't done anything like that.

CORDING: I think it would be interesting to hear how you are looking at that and what the water contents are telling you and those sorts of things. I've seen in some of those other sites where they try to seal the surface. They cover the surface of the fractures and then try to observe

things in places like STRIPA and Switzerland. That would seem to me to be an important part of this.

CANTLON: One more question, Don. Then we'll take our break.

LANGMUIR: More water-related questions. We just recently received the perched water report indicating that a lot had been done with perched water in identifying zones that apparently were below the repository horizon. I gather you have not seen any and don't expect to see any given the stratigraphies for these rocks.

BOYLE: I meant to talk about that. All the perched water found to date has been below the repository horizon. They've even done pump tests on some of the zones encountered in the boreholes and all the test results to date show that they are of limited extent and volume. I don't think we saw any perched water in any of the drill holes. Although, as I mentioned before, it was apparent at the base of the Tiva Canyon that there would be an increase in water content, there wasn't.

LANGMUIR: I had a suggestion about eight months ago on that perched water, looking at its chemistries and from inferred chemistries of infiltration waters backing out proportions of perched water that could be infiltration or water from other sources. Has anybody gotten anywhere with that idea?

BOYLE: I know they have looked at dates, which might get at some of what you are getting at from the carbon 14 dates and that sort of thing, but I'm not that familiar with the results.

LANGMUIR: One last quick one related to infiltration. We heard also yesterday that infiltration rates may be as high as 22 millimeters per year as opposed to the .1 kind of figures that often have been used. What is the status of these kinds of ideas in the program right now? These are kind of critical.

BOYLE: That I don't know, but I think I saw a similar presentation a few months ago from the Calico Hills report.

LANGMUIR: This is also a surface-based testing and modeling sort of activity assessing this, isn't it, which we are disconnecting?

BOYLE: I would say the modeling and synthesis of the results would go ahead, but new data gathering would not really go ahead this fiscal year.

CANTLON: Thank you, Bill.

We will take a break. We will reconvene at 10:40.

[Recess.]

CANTLON: The next phase of our program is to give this audience and our Board an inkling of what is going on in Congress. I am sure that this audience is aware that we were created by Congress to oversee the DOE's activity. So

we are very anxious to hear what Congress has in mind for the continuation of this program and the management of high level waste.

Our next panel consists of key congressional staff members who have been asked to discuss, first, the status of introduced amendments to the Nuclear Waste Policy Act and how, if passed, they are expected to restructure the DOE's overall radioactive waste management program.

Second, how this restructuring might be influenced by fiscal year 1996 appropriations and budget concerns.

Third, possible congressional responses to the issues accompanying incorporation of an interim spent fuel storage capability into the DOE's radioactive waste management program.

The panel also has been asked for their views on what the Board's priorities should be to fully be responsive to congressional concerns.

Although the Senate Energy and Natural Resources Committee held the first hearing in this Congress, the House Commerce Committee was the first to report legislation. If agreeable with the panel, I would propose to begin in the House with Troy Timmons and Sue Sheridan and then proceed to the Senate with Alex Flint and Sam Fowler. I will introduce each panel member in turn, who will make a short presentation, and then we will have the whole panel ready

for discussion later.

Troy Timmons serves on the staff of the House Commerce Subcommittee on Energy and Power, which is chaired by Representative Dan Schaefer of Colorado. Troy is the principal Republican staff member responsible for nuclear waste issues. Because of his labors, the House Commerce Committee was able to report amendments to the Nuclear Waste Policy Act before the August recess.

Troy.

TIMMONS: Thank you. I'm just going to talk for a couple of minutes about some of the thought processes that went behind what the Commerce Committee did as we evaluated what to do with the Nuclear Waste Policy Act.

I think our committee was concerned about two primary things when we began looking at the possibility of changing the Act.

The first one was the time lag that had developed between the original proposed dates for completion of activities at Yucca Mountain and what was actually happening at the site. Originally it was anticipated that two repositories would be in operation by 1998. Clearly that was not going to happen by 1998.

And that 1998 date was important to a lot of members. The contracts DOE has signed with utilities assume that DOE will begin accepting waste in 1998. I think our

Commerce members felt that the obligation to begin accepting waste at that time was important to our members.

It was also very clear that the Commerce members in looking at the work that had been done at Yucca Mountain felt that under Dr. Dreyfus things had gotten back on track pretty well and that Dr. Dreyfus was doing a great job out at Yucca Mountain. But when you have still got a 12-year time lag in the activities at the site, we needed to take what actions we could to spur activities along at a quicker rate.

Because of the 1998 date and the feeling that we had to provide some outlet for DOE to begin accepting fuel at that time, the other main thing was the establishment of an interim storage facility at Yucca Mountain. I know there is a lot of controversy about that decision and its impact on site suitability determinations at Yucca Mountain.

I don't want to prejudice this, but from the testimony that was received in front of our subcommittee, our members were fairly comfortable with the fact that a positive site suitability determination would be made at Yucca Mountain and that, given that as a basic assumption, an interim storage facility could be constructed at Yucca Mountain and fit in with the general activities that were going on at the site.

The size of the interim storage facility is

limited in two phases to 40,000 metric tons. The reason that is kept fairly small is because our members also wanted to keep the pressure on a permanent repository. Probably one of the key things in the whole Commerce Committee's consideration of the bill was the importance of keeping the focus on a permanent repository. By limiting the size of an interim facility and ensuring that you couldn't get all of your fuel into that interim facility, you keep the hammer on the need to continue site suitability work at Yucca instead of just going solely to an interim solution.

That's important, because many of our members have problems with defense waste. As you guys know, defense waste vastly outnumbers the amount of commercial waste which is currently anticipated to be generated. A permanent repository is the place we anticipate to put a lot of that defense waste. So that was an important consideration in the Commerce Committee's work.

We also anticipate that the interim facility would be able to take some defense waste, again just reinforcing the importance of dealing with both the utilities' problem and the government's problem with its high level waste as well and keeping that linkage.

The interim facility is exactly that. It's an interim facility, and we wanted to keep the focus on an affirmative program. We anticipate the infrastructure that

is created with building that interim facility will be incorporated into what is needed at Yucca Mountain should that site prove suitable. So the infrastructure that is being completed as part of the interim facility just folds right in with what should happen at Yucca Mountain.

Let me hit on timing of activities in Congress. The Commerce Committee has already completed its action. The Committee on Transportation had a referral. It discharged, and so they don't have to mark up the bill.

Resources Committee, which is probably the other major committee that will be taking action, they had their hearing last week and plan to mark up on Thursday, I believe.

The Committee on Budget also has a referral. I have no idea what they are going to do. The budget issue is clearly one of the biggest congressional problems in dealing with getting legislation accomplished.

I will end it at that point and turn it over to Sue. Then I think we are going to be available for questions.

CANTLON: Sue is minority counsel to the House Commerce Committee whose ranking member is Representative John Dingell of Michigan. She is an expert on nuclear issues. In 1987, as majority counsel to the House Subcommittee on Energy and Power, she worked on the legislation that

established this Board. Ms. Sheridan is a graduate of Duke University and Vanderbilt Law School and previously served on the White House Domestic Policy Council and the Department of Energy.

SHERIDAN: Thank you for having us today. I feel like I'm almost as much a hardy perennial as the issue itself before Congress. I don't know that anybody should be allowed to do nuclear waste more than once.

[Laughter.]

SHERIDAN: Maybe it's helpful to have some long-term memory, because we keep facing the same issues again and again.

One thing I would like to say so I don't forget it at the end. The invitation for us to speak today asked what we might be able to offer by way of suggestions for what the Board could do. From our narrow viewpoint, I would just say that the testimony the Board gave the Commerce Committee earlier this year was extremely helpful.

We, as Troy said, were interested in the relationship and perhaps the tension between proposals for interim storage and permanent storage. As other witnesses did, we got candid views about the value of interim storage and then also candid views from the Board about what risks we might be taking and the considerations we should bear in mind if we added interim storage to the program and what it

might mean for DOE's ability to handle the work load of a dual track program. Again, from our narrow vantage point, I think that was very helpful.

We need the Board to be steady. That's why you were created. I know that sometimes the questions are not solely technical and that that gives an added dimension to your job, but we need to be able to have Congress get candid views about what would happen if we went charging off in a different direction, particularly with so many members on both sides of the aisle who are new. I think something like 60 percent of the House has served for four years or less. We all benefit from a longer view and a nonpolitical view.

I'm going to talk briefly about some of the political aspects of what we are doing in the House. We had a very good bipartisan relationship in the Commerce Committee, which is unusual, and I think it's helpful in terms of our being able to maybe step up to the plate and meet the Senate on legislation, however that may come up, to confirm or redirect, or whatever we decide to do, the directions we've already given DOE under the Nuclear Waste Policy Act amendments. I will note in passing that the vote coming out of the Committee was 30 to 4. That's extraordinary at this point. We don't find the House functioning very smoothly, as you may have noticed.

There are a lot of things that we have to be

grateful for with Chairman Schaefer giving us both the go to act quickly and also a way to work together. We felt strongly that bipartisan support had to be very strong coming out of what is now the exclusive committee of jurisdiction over the whole range of issues on nuclear waste.

As Troy mentioned, there are other committees with other pieces of it in the House, but we have the whole range of the basic issues before us. So as we tried to figure out what was the proper balance within this bill we were keeping in mind the long range. We knew at the time and we now know even more clearly that the President has concerns about Nevada. I will go into that as best I can in a minute. But we knew we had to have a very strong bipartisan bill if it was to have any legs going forward in the process both in the House and ultimately in a conference, if we get to a conference with the Senate. In terms of clarity and kind of doing our job, I feel good about that to date. It wouldn't help if we were sort of flailing, and so far we have avoided that.

Perhaps the biggest thing I can talk about that is on our horizon aside from hoping to go forward in regular order on H.R. 1020 is another avenue of activity which you are aware of, which is that the Appropriations Committee is meeting on the energy and water bill, and there is some

language, I think, on both sides about the waste program, of course, because decisions about annual appropriations have to be made.

I think we all know that there is also in the air a consideration of giving far more than just simple money and funding but a far more detailed redirection perhaps of the program in the appropriations bill. My impression is favoring interim storage is a more primary element of the program than maybe we have in the House bill 1020. That bothers our committee.

Without speaking out of school, I think it's okay to say that the Commerce Committee has had conversations at the member level with the Appropriations Committee members and said, look, we've done our job; we've done it in a timely fashion; we know you have a tough job; but this isn't the time for you to make that decision; at a minimum, let us go forward and try to do our work.

Whether or not there has been evident Senate activity in the authorizing committees over there so far, it's really early enough in the congressional session for us to have a good prospect of a bill by regular order. I don't know what the Appropriations Committee's decision will be, but that has been our appeal to them: we've done our job; we feel we've done a coherent job; we have strong bipartisan support; give us a chance to go forward. That is very much

in process.

I will bring what may be some news to you, because it was news to me. On Friday evening we got a pretty clear signal from Alice Rivlin at OMB that there would be a veto of the energy and water bill if certain things appeared in it, including any site-specific language with respect to an interim storage facility. The language in this letter says that the President's senior advisers would recommend a veto if site-specific language having to do with an interim storage facility were included in this energy and water appropriations bill. I take that as a fairly clear veto threat.

All I can say about that is that I think it ups the stakes. Alex may know more about that because he moves more in the money and funding world than I do. You never know what will happen with vetoes in this current atmosphere, but I think we had that in mind. We knew the President had reservations about interim storage in Nevada being made more specific. He will have those reservations about our bill, but in the context of an appropriations bill, I think this veto threat sort of ups the stakes, and we will just have to see how it plays out. Your meeting happens to be scheduled just as this news breaks.

We hope that will help people be more patient about watching our bill go forward and give us a chance to

finish it.

Two short remarks. My impression about where the Appropriations Committee is in the House is that they are dubious about giving more money to DOE. I think that is unfair, because I think DOE and Dr. Dreyfus and Secretary O'Leary have done an enormously good job to get this program pulled together, coherently give directions, give answers to Congress that are clear.

There is reason for confidence, and I think that is where our committee is. The Appropriations Committee, my impression is that they are a little bit more dubious that DOE really has its hands around the program, and so they are less patient with letting DOE have a good amount of appropriations for another year to see if they can sort of prove out the redirected program.

That is one point of difference between the Commerce Committee in the House and the Appropriations Committee in the House, from what we can tell.

The other major thing Troy alluded to was the funding issue. I don't have to explain to you all where the nuclear waste fund contributions come from. They come from ratepayers. The fact that these funds have been essentially expropriated by the vagaries of the budget scoring process in the Congress, that is a bipartisan sin, a longstanding sin.

Senator Johnston had proposed one way of addressing the problem of continued diversion of nuclear waste fund contributions toward other processes. We have another approach to it in our bill. I don't think there is any approach that is trouble free when you are essentially taking money away from the Budget Committee's money that they can use for their purposes.

Not to throw any slings and arrows, but I think our feeling is it's about time that stopped. I have the Appropriations Committee staff saying to us, well, you know, we just can't keep appropriating money to this program, because we don't see progress. Then we have DOE saying to us, for God's sake, we are on the brink of progress; give us adequate funding; we can't prove it if you don't really make a substantial investment for a couple of heavy years of work so we can get you an answer.

That's pretty silly. We think we have come up with a reasonable way of ending this process of draining off the fund so that we can adequately fund Dr. Dreyfus' work, and, if appropriate, adequately fund an integrated program that maintains work on the permanent repository and also permits an interim storage facility of the limited nature Troy described to go forward without there being a terrible tension and competition for funding.

But you can't do interim storage and permanent

storage at the same time in any proportions if you don't have adequate funding. I really feel you will end up with two bad programs and that the funding bullet just has to be bit. That is just plain big time politics, and we don't know how that will play out. We will face that as we go to the floor.

Not to be overly dramatic about politics, but right now anything that involves money is just awfully high stakes, and unfortunately, we have got this program kind of wrapped up in all those issues. We know there are doubts about the bill that we have crafted, but we feel at least we have stood up and taken a shot at it, and if someone has a better idea, we will look forward to going to conference with them.

One of the nice things about being in the minority is you can be kind of candid about these things.

[Laughter.]

SHERIDAN: Because nobody thinks you have the power to affect it. I am enjoying this new role.

[Laughter.]

CANTLON: Thank you, Sue.

We will now hear from the Senate. Alex Flint is legislative assistant to Senator Pete Domenici. Senator Domenici is intimately familiar with the DOE's radioactive management program as chair of the Appropriations

Subcommittee on Energy and Water Development and the Subcommittee on Energy, Research and Development of the Senate Committee on Energy and Natural Resources. He also chairs the Senate Budget Committee.

Mr. Flint is responsible for the Senator's legislative agenda in areas of energy, science and commerce. Perhaps he can provide us with an insight into what the Senate is likely to do with regard to the DOE's nuclear waste program and whether or not the fiscal year 1996 appropriations bill will specify an interim storage site.

FLINT: I've come to enough of these sorts of forums that it is getting more and more difficult to say something new and interesting in each one, which is my objective when I do speak at such forums. We have hashed through nuclear waste issues so many different ways and so many different times over the years that really it is beginning to be a bit of a jumble.

I'm fortunate that I'm the only staffer you will hear from who only has to worry about the views of one member of Congress, so let me approach it from his perspective.

Pete Domenici has three problems with the current nuclear waste program.

The first is political, that the current nuclear waste program is not solving a political problem, and that

is resolving the end of the nuclear fuel cycle. Domenici wants to get the program directed in such a manner that it does solve that problem and thinks that the first step to do that will be to provide a storage facility for 1998 or thereabouts. The second step is to get the permanent program on a track in which it solves the problem within a more reasonable time frame.

The second is a related problem, and that is a scientific problem. Domenici has read the NAS report from 1990 Rethinking Nuclear Waste numerous times and continues to refer to the scientific trap that that report identified and very firmly thinks that that is still very much in place and that the regulatory environment at Yucca Mountain needs to be changed. That is a very difficult path to walk down, because you don't want to change regulations to fit the problem, but at the same time you do want the regulations to be responsible and reasonable. So that problem will also have to be addressed.

The third problem is budgetary. This is one where we can have some very interesting meetings between our budget staff and our appropriations staff sometimes.

Domenici would at times like to free up more money for the Yucca Mountain program, at other times feels that the program isn't working and doesn't want to spend any more money, at all times wants to preserve the integrity of the

budget process in which we don't have off-budget mechanisms or mechanisms where revenues and receipts for a certain program are scored at certain levels regardless of how much money comes in or goes out for those program, and we have never been able to find a clever solution to this problem with the nuclear waste program.

Very fundamentally, every time we have sat down to do it, if you want to direct that all or more of the waste fee goes to the nuclear waste program, you've got to take that money from the places it's currently going to. That either requires that you change the budget caps of overall spending or that you identify other programs within energy and water appropriations, or that you direct the Energy Committee to identify other savings in the discretionary accounts.

We've never been able to resolve that problem. We fundamentally think that the only way you can really change the problem is already allowed for, and that is that you have to give the nuclear waste program higher priority. Bennett Johnston has done that for years through the appropriations process where he has in essence taken money from other energy and water programs and put it into nuclear waste, and we think that is going to have to continue for the foreseeable future. It can either happen when the President's budget is submitted to Congress or it will

happen when energy and water appropriations committees deal with the legislation.

Now let me spend a couple of minutes talking about how we want to solve the problem.

There is a great deal of discussion about an interim storage facility. Unfortunately, that is still simply discussion. I think it is fair to say that there has not been a consensus as to the best way to go about implementing an interim storage program. I think it is fair to say that in Congress there is a consensus that an interim storage program is necessary. The question becomes, how do you go about doing that? That's something that we are supposed to have solved by now but haven't, and we will see where we go in the next couple of weeks.

Our considerations of that issue are influenced by our consideration of the larger issues. The Senate Energy Committee has not yet reported a new authorizing bill. I think the Senate Energy Committee is very close to being able to do it or it could do it on very short notice if a markup was scheduled and if a commitment was given by the leadership that there was going to be floor time for what is surely going to be a contentious piece of legislation.

I think the Energy Committee has traditionally been very bipartisan, and I think there is a consensus there and that that legislation could be brought out very quickly.

Not only that, while it would differ from the House legislation in some areas to a great extent, I think it would be a similar framework and it's the sort of thing where when it went to conference something would emerge from conference.

The problem is that the Senate Energy Committee has not moved to report that legislation. And in the last couple of weeks, as somebody who works for an appropriator, we've had a lot of discussions with the White House about what their intentions are if we were to select an interim storage facility on the energy and water appropriations bill, and while we have gotten lots of mixed signals, it's clear that they are contemplating a veto.

That makes it very troubling not just for the energy and water appropriations but for any legislation that would select a site and provide for interim storage. That shadow falls across an authorization bill as well, particularly because I think any meaningful authorization bill is really going to have to have some muscle behind it and it is going to have to address NEPA issues and permitting issues and location issues and transportation issues.

I am of the basic philosophy, after talking to these White House people for the last couple of weeks, that no nuclear waste legislation can be enacted in a

presidential election year. It may be a bit of a reach, but be it appropriations legislation or authorization legislation, it involves a lot of difficult choices. This White House has not demonstrated a willingness to take a stand on those issues, at least in the last couple of weeks.

So we have got a lot of different considerations that we are working on. In the energy and water appropriations bill in the next couple of weeks you will see what our level of commitment will be to either the current program or a modified program. It will be a very interesting time, and I couldn't tell you what it's going to look like.

CANTLON: Thank you.

Now we will hear from Sam Fowler, the Democratic chief counsel for the Senate Committee on Energy and Natural Resources. Over the years Mr. Fowler has provided both Senate and House members with counsel on nuclear issues. His expertise is widely recognized within the Congress.

He began his congressional service with Representative Mo Udall on the House Interior and Insular Affairs Committee. He now counsels Senator Bennett Johnston on some of the toughest issues before the Energy and Natural Resources Committee. It was through Mr. Fowler's efforts that Senator Johnston introduced the first nuclear waste bill in this Congress. Therefore it is appropriate that he

have the last word. Perhaps he can forecast for us the outcome of the current congressional deliberations and tell us what the near-term picture is for the DOE's radioactive waste management program.

FOWLER: I wish I could. I will say that I come here today as a friend of the repository program and a friend of this Board. I cannot claim to have authored the legislation that created this Board. Sue Sheridan did that for Phil Sharp back in 1987, but as I recall, it was a Sharp amendment to a Udall bill.

[Laughter.]

FOWLER: Mr. Udall championed the creation of the Board and Sue and I worked very closely on seeing that the Senate went along and put the Board in the 1987 amendments.

As a friend of the Board and the repository program, I must say that the program is in very serious trouble right now. You've heard the reasons from the previous speaker.

There is a belief that the program is not making sufficient progress. I happen to think that that is a very outdated view, that there has been a great deal of progress made under Dan Dreyfus in the last year or two.

There is a belief that this program has an insatiable appetite for money and that that money is not being well spent. There is a resignation that we will never

be able to find our way through the budget labyrinth to find a way to get the program the money it needs on an annual basis.

Finally, there is this concern that regardless of how good a site Yucca Mountain ultimately proves to be, no matter how much money we have spent, how much time we have spent doing all of the scientific tests, it will never be enough for the critics and the overseers.

Lord Salisbury, who was one of the last prime ministers during Queen Victoria's reign, observed to Parliament about a century ago that "no lesson seems to be so deeply inculcated by experience of life as that you can ever trust experts. If you believe doctors, nothing is wholesome. If you believe theologians, nothing is innocent. If you believe soldiers, nothing is safe." There is a concern among the National Academy, this Board, the NRC that regardless of how many tests have been performed critics will never be satisfied.

Because of that concern, there is an enormous frustration among many members of Congress. Back in 1982 and 1987 there was a consensus in Congress that people would do whatever was necessary to demonstrate the feasibility of the repository; any amount of tests, any amount of money within reason would be made available to this program. That is no longer the case. I think there is a great impatience,

a great sense of austerity setting in.

The 1998 date has no intrinsic merit but nonetheless has become one of the controlling forces governing this program. The budget picture, which has already been described to you, is making it extremely difficult to get the program the funds that it needs.

During the previous administration the program just sort of limped along, receiving $300 million to $400 million a year. When Dan Dreyfus first testified before the Energy Committee he basically said that we'll never get there from here, that with that level of funding it would be impossible to have a repository by 2010, as had previously been advertised, and that it would be necessary to both seriously reduce the number of studies that were being contemplated and at the same time dramatically increase the amount of money that would be made available to the program.

Senator Johnston managed to do that last year, increasing the funds for the program by about $100 million, but there is no mood for doing that this year. The program is actually going to take a $100 million cut compared to last year and a $200 million cut compared to the amount of money that Dan Dreyfus said would be necessary.

As Sue said, one of the privileges of being in the minority is being candid, and I'm being very candid with you when I tell you this program is in very serious trouble.

You asked about specific bills. In view of the framework that I've just described, I think we are very fortunate that H.R. 1020 has come out of the House Commerce Committee as well as it did.

Senator Johnston introduced legislation back in January. The utilities at that time would not endorse it because it didn't guarantee the January 31, 1998, date; it didn't impose penalties upon the Department to force the Department to try to meet that date. Senator Johnston viewed the date as an impossibility and the penalties as being counterproductive and politically impossible. So I think Senator Johnston, were he here, would probably commend the House for bringing the House legislation closer to where he was back in January than the industry was proposing.

The appropriations picture. I will let Alex's comments on that stand. Again, I think the appropriations bill reflects the impatience and the austerity that I've talked about.

My own view is that the sort of legislation that Senator Johnston would like to see attached to the appropriations bill is not inconsistent with H.R. 1020. It would simply try to provide the funds in fiscal year 1996 that the program that is embodied in H.R. 1020 contemplates.

I think the White House letter that Sue Sheridan shared with you earlier is rather unfortunate. The

administration has not come forward with a program of its own despite Chairman Murkowski's repeated requests. The level of funding that the administration now claims it has endorsed was not really part of the budget that they submitted earlier this year, and it is going to be extremely difficult to continue the repository program as Dan Dreyfus has laid it out in his program approach with the kind of funding that is actually available.

I'm afraid I have not answered your questions. I'm afraid I have simply given you more bad news, but as a friend of the program and of the Board, I think you deserve to know exactly how serious these matters are at this moment. Thank you.

CANTLON: Thank you.

Questions from the Board? Don?

LANGMUIR: I gather from several of you that the subcommittees have, at least in some instances, been very supportive of the new legislation. Majority and bipartisan support have come out of those committees. What are the chances that you could override a Clinton veto on some of these issues?

TIMMONS: It hopefully wouldn't get to that point. I think what we have put together in H.R. 1020 is a pretty balanced piece of legislation no matter which party you belong to. Our hope is that it has enough bipartisan

support and is seen as a balanced enough piece of legislation even at the point we would emerge from a conference that we would avoid that veto.

Hopefully we wouldn't get to that point. At the point that a veto occurred, I don't know what that does in terms of the bipartisan nature of the bill we have passed. I'm sure there would be a great deal of pressure on those members that had supported us through the process to turn and support the President. Our hope is that we have enough of a bipartisan support for the bill going through that that doesn't become an issue.

LANGMUIR: Does Clinton not have any evident policy on nuclear waste that he has articulated at all?

FLINT: I would have too much fun answering that question.

[Laughter.]

SHERIDAN: This may be a burden that falls to a Democratic. The evidence we have of a policy is spotty. We had a letter earlier this year and then we had this veto letter. Although it is from Alice Rivlin, I think it tells us something. The substantive concerns focus on site-specific location. I don't know why anyone would undertake non-site-specific interim storage for either political reasons or technical reasons.

[Laughter.]

SHERIDAN: What we can glean is that anything that names Nevada is going to run into trouble.

The thoughts I can offer on a veto are two. First, I don't think the bipartisan support will melt away. You can never tell going forward, but the lack of a strong presidential position when we were negotiating within the committee was something we were aware of, and we also were aware of the hints about wanting to protect Nevada that were coming out of the White House. We have made our pact, and in the absence of strong guidance from the White House we have done our best, and I would predict we will stick by it. That was the whole point.

It may depend on how it comes up. Any time you have an authorizing action in an appropriations bill, there is a gray area in between where you are giving money and guidance and then there is an area that we sort of feel we know when we see it when you are really reorienting a program. If you end up reorienting a program against the authorizing committee's wishes in an appropriations bill, you can get a different sort of dynamic than you would if you were just voting on a straight bill.

I am not trying to be hinting. I'm just saying an appropriations bill is in a more explosive circumstance for a veto threat to play out in because you've got other considerations. You've got partisan considerations and

you've got authorizing versus appropriators sorts of tension. So it depends on where it comes up.

I know that Alex's boss may pursue a different path, and Sam's boss may, for that matter, but our hope would be that we don't have a showdown on appropriations.

One lesson over time that always proves out is that you can't tell what will happen on nuclear votes, at least on the House floor. We recently saw a strange circumstance occur where a low level waste compact went down on the House floor. That has never happened before.

It had, I think, more to do with planning and inexperience than anything else. Not Troy's. But, boy, no one predicted that, and I don't think it's a good place to roll the dice. I think a serious approach toward it sort of mitigates against rolling the dice. But a veto threat in an appropriations context in a House bill is pretty dicey, and I hate to see that kind of discredit brought on the program.

CANTLON: Alex.

FLINT: If I may for just a moment. Sue has raised a very important issue, and that is that a distinction needs to be made between a veto threat on an authorization bill and a veto threat on an appropriations bill, because the appropriations bill introduces this dynamic, particularly in the House. I only say that because in the Senate Senator Johnson and Senator Domenici are both authorizers and

appropriators. It raises the dynamic of the authorizers of supporting the President's veto on the ground that it was authorization in an appropriations bill.

But there are other factors to consider. Secretary O'Leary has indicated that the President would intend to pick an interim storage site by December 31, 1996. That raises the specter of non-Nevada sites being selected. Unfortunately, it puts us in a predicament of if we move on the energy and water appropriations bill to support interim storage with 1996 funding, we have to consider the interest of those other sites who feel that they might otherwise be selected.

Two sites being considered are Hanford in Washington and Savannah River in South Carolina. We have a South Carolinian and two Senators from Washington on the Energy and Water Appropriations Committee in the Senate. From their perspective it becomes a question of, could you move towards interim storage and not pick a site?

There are lots of things to think about.

LANGMUIR: Are you wrestling with a concern at all about the perception if you pick a site before a repository is licensed that you have de facto -- especially if it's Yucca Mountain -- provided tremendous momentum to decide the site as suitable whether it is or not?

FLINT: I break both ways on that issue. I understand

the people who suggest that, yes, after you deal with transportation and other issues related to an interim storage facility next to the Yucca Mountain site you may have prejudice towards the waste staying nearby.

On the other hand, I also make the argument that if you can create an interim storage capability so that you do not necessarily have to utilize your permanent facility that you are in the process of characterizing that you now have the flexibility to make a truly scientific decision about your permanent facility.

I recognize the strength of argument on both sides of that issue.

CANTLON: Jerry.

COHON: I'd like to follow up on this last point, especially addressed to the House staff. If I understood you correctly, there was a presumption by your members that Yucca Mountain would be found acceptable, that that was a premise for H.R. 1020. Did you talk about and what kind of issues came up related to the question of whether a finding later on of suitability would be found to be credible by stakeholders in the process?

TIMMONS: In questioning, NRC and DOE both made pretty clear statements that in their opinions the Yucca Mountain site would be found suitable according to the information they had at that time.

LANGMUIR: Did they testify to that effect?

TIMMONS: Yes. I know NRC did. I don't want to put words in Dr. Dreyfus' mouth, but I think DOE did testify to that effect as well. They were asked, based on what you know right now, what do you think? That was the answer we got back. That was just their best guess at the time.

Using that as an assumption, our members looked at, okay, with that as a basis, how do you best maximize your resources in constructing and operating an interim storage facility?

Our people came to the conclusion that the smartest thing to do since you are going to be using a lot of the infrastructure associated with an interim facility at your permanent site anyway, like a hot cell capability, it's going to be used at a permanent repository, it's going to be used at an interim facility, why not try to minimize the amount of times you are going to spend money on that activity.

In terms of transportation, you are going to have to transport stuff to Yucca Mountain eventually. As you are constructing your interim facility, why not put the infrastructure in place? Based on the assumption our members made, if you are going to be using that site for the repository anyway, it makes sense that that is where you put the interim site.

I know there is a lot of different thinking on that issue. Our members were well aware of the difficulties that that decision has created for the site suitability work at Yucca Mountain, but that was a judgment call. Given the set of facts that we had, that's the call they made.

Do you want to add to that?

SHERIDAN: I don't disagree with anything that Troy said, but I think there was a little more restiveness on the Democratic side about it, partly because some of the members have been down this path a couple of times. Among members who oppose the bill coming out of our committee and those who supported it there was a fear somehow that authorizing interim clearly for the first time in Nevada would somehow send a signal that Congress was prejudging it.

I would hate to see this all sort of land in the NRC's lap with inadequate preparation by DOE, but the fact is there does have to be a license for the repository as well as for every piece of our interim storage facility. I think we sort of took a chance that it would work out.

I'm pretty confident as long as we don't start undermining the licensing provisions that we will get a proper answer from the NRC. They are pretty independent and need to be, and they've got a reputation to protect. So I think all the proper institutional forces are in play.

If our bill worked out perfectly, if it were

enacted as is in time and the time lines ran as we thought, we would not have built very much interim storage in the first stage before we got a site suitability determination so at least we could find out whether DOE's hope that it is on the right track and that it will be suitable will pan out, and we would get some information as we went along. But I think it's a bit of a leap of faith, and some people were more comfortable about it than others. We are taking a little bit of a risk.

At this point the political pressure frankly to assure industry that there was some hope was so strong that it was almost an irrelevant question. I know, because I asked it: why do we have to do a bill? Isn't it dangerous? I realize I was nowhere near the center of where all the members were in asking that question. There was going to be a bill.

CANTLON: Jerry.

COHON: A general question. What kind of advice, if any, or on what topics would you like to hear from this Board given the current state of play and the issues that we are facing?

While you are chewing on that one, can I ask you a more specific one?

I just want to confirm something. It seemed to me the collective message was that given limited funds,

especially if they are as limited as the kinds of numbers we have been hearing this morning, and having to choose that, Congress overall would opt for interim storage at this time even if that jeopardized the long-term disposal program. Is that a fair assessment?

SHERIDAN: Let me hop in. Speaking for one Democrat, Chairman Dingell -- former Chairman Dingell -- boy, I did it again -- would not have supported a bill including any element of interim storage if he felt it was seriously undercutting permanent. So speaking for that member, and I think for most of the Democrats, gosh, if we had to choose, we would have taken our lead from the testimony of DOE, the NRC, and I believe from the Board, that in a world where you had to choose, if you had to choose between interim and permanent, the unanimous testimony we got was choose permanent, for God sake. That was our watchword.

In fact, seeing the concerns in the Appropriations Committee begin to arise, I think one of the reasons we threw ourselves into the bill that came out of our committee was for fear that if we didn't, what we would get was interim alone with whatever level of instructions the Appropriations Committee were able to come up with. So in a sense we were trying to forestall the possibility of an exclusive interim approach; we were trying to very much shore up the fact that the goal is permanent.

Without permanence, I think many of my members wouldn't even want to go down the interim path because it would be seen as raising the questions you asked earlier about why would you sort of front load interim storage in Nevada.

When the transportation issues are felt fully in this political debate, the idea of trucking a lot of waste from the East through the West to sit there for a while may be less attractive than it seems now. I've started to see articles about that. You'd have to have a darn good reason to put it out there.

I don't think we are there. The Senate may be in a different position.

TIMMONS: I will echo what Sue said for the Republicans as well. Chairman Bliley and Chairman Schaefer both strongly felt that if we were going to do a bill, then we had to do a bill that kept the focus on the permanent program and the repository. The interim, if we can do that without detracting from the work at the permanent program, great, because it does address an issue which is important to a number of folks, but that should not be the priority in terms of changes to the program. Certainly from both the chairmen's perspective the importance and the focus should be on permanent.

FOWLER: The first nuclear waste bill that the Senate

ever considered or acted upon was Senator Johnston's MRS bill back in 1979 or 1980. Senator Johnston started off believing that the best solution to nuclear waste was monitored retrievable storage rather than the deep geologic disposal. He lost that battle. He is now one of the forces trying to keep the repository program going.

My personal view is that you probably would not be able to have interim storage, that you would not politically be able to get it through Congress if there wasn't a viable repository program out there.

On the other hand, in answer to your earlier question about the danger of prejudging the Yucca Mountain decision, I think the greater concern is that having once enacted interim storage and facing the continuing demanding funding needs for the repository program and the inability of science to disprove every negative for the next million years, that at some point Congress will throw up its hands in frustration with the repository program.

The greater danger is not in prejudging the decision to put nuclear waste in the deep geologic repository at Yucca Mountain. The greater danger is that Congress in its frustration will let the repository program whither, and that, in answer to your earlier question, is something that if the Board has some thoughts on the Board could usefully advise Congress about.

FLINT: That's a good point. When the Congress first dealt with nuclear waste issues they were going to solve the problem 16 years from that date. Here we are, 12 years later, and the solution is still 16 years away. I think to a lot of members of Congress the issue is that simple.

What that means is that the current program doesn't work. So when you talk about the commitment to deep geologic disposal, I think you need to break it into the concept of deep geologic disposal, for which I think there is a lot of support, and I think that is why people are willing to say that we will presume that deep geologic disposal works.

But if you talk about the current program, all the accolades to Dan Dreyfus are well deserved. I think the fact that the solution is still 16 years off means that support for the current program is questionable. I think that is the reason that this question about if we go to interim storage will we obviate support for deep geologic disposal is real.

I think that if you are able to transform the deep geologic disposal program so that it is effective, so that the time lines become more meaningful, so that the time lines are kept, then I think the question may change. I think there is quite a political consensus that deep geologic disposal is a solution that works, that should be

pursued, but it has got to be not just pursuable; it has got to be achievable. If you get the ability to achieve your objective, I think the support will stay.

CANTLON: I think we are running over our time. Are there any questions over there?

GRUNDY: I had two questions and one of them has been answered.

A couple of you have raised the question about the tension that is going on with the decision on site suitability. This is a technical board and you are concerned, obviously, about the technical questions. What do you think is the consensus on the Hill in terms of the need to modify the regulatory regime to address the scientific and technical issues associated with site suitability? Or is there a consensus?

FLINT: I don't mean to represent a consensus. I'm fortunate that I don't have to.

[Laughter.]

FLINT: I alluded to this when I was speaking up at the podium. I think there is a very strong desire to change the regulatory regime. It would be very beneficial if a reputable technical entity could make recommendations to that.

It is very frustrating for a member of Congress to propose a regulatory environment and then have regulators

come and testify and say, Mr. Congressman, or Mr. Senator, we are doing what you told us to do, when the real question is, was what we told you to do the right thing?

It would be very beneficial if we could get that: Are the technical requirements imposed by the statute the correct technical requirements? Is the way in which those requirements are being implemented the correct way?

Somebody needs to make a very difficult judgment on that, because there is a big gap between political people trying to solve political problems and technical people trying to solve technical problems. We would hope that the solution that we have proposed is the correct solution, but if it's not, would somebody please raise their hand?

SHERIDAN: Richard, on the House side we did address one regulatory issue with respect to setting the release standard for the permanent repository in statute. I am not a fan of that process, because I know we don't know enough to set technical standards in many cases. This one was a pretty noncontroversial step if you are going to go down that road.

Beyond that, I don't know that we had much of a technical program to bring forward because we just weren't focused on that. I think that may be a difference between the House and the Senate. That just wasn't one of our focuses. We don't deal very well with those issues. That

is one reason we need the Board. But that doesn't mean that those who are well acquainted with it may not have that concern. Senator Johnston did back in the 1992 Energy Policy Act, but that hasn't been a focus in the House to date.

TIMMONS: I think one of the frustrations with our members is if you just look at it and say go build a repository and don't worry about anything else, our members, a lot of our Republican guys anyway, would say, cool, we'll have a repository in 1997. There is a tension between the desire to get things done as quick as you can and get things up and running and to make sure that those things are done right and properly and with a proper standard.

I think when our members looked at H.R. 1020, one of the things that we wanted to do, recognizing the need to have proper and responsible standards, was to weed out and eliminate all the duplication that a lot of our guys felt was in there. So you are going through the process and you are doing it the right way, but you are only doing it once rather than several times. That was one key for our members.

Especially in dealing with our side, folks wanted to eliminate much more of the regulatory regime than we did. We just tried to narrow the focus so that we are doing activities once rather than several times.

CANTLON: One last question from Richard.

GRUNDY: I just had an observation. I spent a number of years in their position advising how you get scientific input. I can just tell you when you are on this side advising scientists on how to input the process it is just as complicated for me.

[Laughter.]

FOWLER: I should probably take the opportunity not to respond. Richard's question goes to what I was trying to say throughout my comments. There has been a decision that deep geologic is preferable to indefinite aboveground interim storage. Whether that was a right decision or not, that was the decision that Congress made years ago. The concern that a number of Senators seem to have is if we have set up our repository program so that it can never succeed, if we have created standards that no site anywhere can ever meet.

Nobody is talking about cooking the books or backing off on necessary health and safety standards to see that Yucca Mountain or any other site will be licensable, but there is a real concern whether we have created a regime that dooms to failure the licensing of any site anywhere. We saw that a couple of years ago with EPA's carbon-14 standard, when John Bartlett came in and testified before the committee that he was spending $3.2 billion to try to

meet a carbon-14 standard that represented a minute fraction of background levels, or indeed the amount of carbon-14 that occurs naturally in the human body.

There is a great frustration on Senator Johnston's part that the regulators never came in and told him that this is what they were demanding that the Department of Energy meet; the Department of Energy never came in and told him that that is what they were spending $3.2 billion on.

One of the things that he believes is necessary to do in any legislation that reforms the nuclear waste program is to make sure that we have a regulatory system that will work. If we are trying to design a system that can't work because we can never disprove all of these negatives for a million years, then I think Congress needs to know about that. The only thing that is left to do then is to go with interim storage, aboveground storage, so people can monitor it for all time.

CANTLON: Thank you very much. We certainly appreciate your taking time off from what must be a very busy time of the year for you people. Thanks again.

The last speaker before lunch is Dr. Robert Fri, chair of the National Academy committee that was charged by Congress to look at the technical bases for the Yucca Mountain standard. So we ended up our discussion on exactly that topic.

FRI: Thank you very much for inviting me to join you today. As you say, the discussion just passed is a good setup for what I have to say, which I will probably say in 20 or 25 minutes, if that is acceptable, and then take your questions.

There are two people who are here from the committee, Myron Yooman, who is our staff director, and Chris Whipple, who is a member of the committee who can probably answer more of the questions in fact than I.

[Slide.]

FRI: The genesis of this project at the National Research Council was very much what you just heard, an attempt to try and clarify the regulatory regime. The Congress directed in the Energy Policy Act of 1992 that the EPA set a standard for Yucca Mountain based on dose to an individual and then turned to the Academy complex and asked whether that individual dose standard would protect the health and safety of the general public, asked whether institutional controls would deflect human intrusion over a period of 10,000 years, and asked whether there was a scientific basis for predicting whether the repository would be breached because of human intrusion over that period of time. They asked then for the Academy to establish a committee to answer those questions and to do whatever else was necessary to establish the scientific basis for the

standard.

One is always tempted at this point in his presentation to say that the answers to these questions are yes, no and no, and sit down.

[Laughter.]

FRI: I sometimes think that probably would be the wise course for me. What I will do is not do that, but rather give you some sense of how we thought about this, first to describe the nature of the standard that we envision, which is one that is based on individual risk rather than dose, a small but important difference, and one which does in fact conclude that there needs to be a different basis for human intrusion rather than incorporating it into the basis of the standard itself or into the probabilistic performance assessment.

[Slide.]

FRI: Just so you know, this is the membership of the committee. While you are looking at that, let me say that the committee tried very carefully to describe the scope of its work. We felt that we were not limited in what we wanted to do, but we did feel there were some things we shouldn't do. So let me put the study in context briefly by citing three of those.

First of all, what you are about to hear of the report itself is confined simply to the technical basis for

the standard. It expresses no opinion on the program or the licensability of Yucca Mountain as a site. We did not do studies of whether this site could or could not pass whatever standard is set.

Secondly, you should understand, and this is crucially important, that the conclusions are specific to the Yucca Mountain site. This is not what EPA set out to do in the existing standard, which by law is required to be one that is of general applicability, and therefore there are some important differences that arise which we can talk about as we go through the discussion.

Finally, harking back to the discussion we just heard, we tried very carefully to define the line between science and policy and to stop talking when scientists stopped having anything to say.

For this purpose, policy, I suppose, I could define as a conclusion, a regulatory decision that needs to be reached through broad public participation in order to ensure its credibility. That may not be a very good scientific definition, but it's a very good public policy definition. When we ran into those issues, you will find that what we attempted to do was not to impose our own judgment but at best to offer a scientifically defensible starting point for a public rulemaking.

Let me begin by discussing how we view the

standard, how it's made up, and how one could establish compliance with it and then cycle back to the questions that need to be answered.

[Slide.]

FRI: Essentially, we concluded that the standard should be designed to protect the persons at highest risk at the time the risk is the highest, and therefore the standard should be in the form of a risk statement rather than a dose statement. Obviously the goal of a standard is to put some kind of a cap on the number of additional adverse health effects that would be experienced by the public. A dose standard does that because there is a reasonable understanding of the dose-response relationship, but a risk standard seemed to us to be a preferable form of standard for a couple of reasons.

One, there is some evidence that our understanding of the dose-response relationship has changed over time, and therefore a risk standard would tend to be a more stable standard even if that understanding did change. You wouldn't have to go back and adjust the standard, and since this isn't an easy standard to set in the first place, that seemed to us to be a virtue.

Secondly, because the public is involved in a number of decisions here, it is important that what we are talking about be reasonably intuitively accessible to the

average nontechnical person. A risk standard stated in the form of one in a million chance of additional health effects per year or one additional cancer per year is kind of more understandable, I think, to people than a statement like the dose is .02 millisieverts per year, which is sort of mathematically the same thing given the current dose-response relationship. So we prefer the risk approach.

Given that, we tried to describe a standard or the major elements of a standard that would have to be developed in order to actually write down a standard against which one could measure compliance, and there are essentially three parts of that.

[Slide.]

FRI: First of all, one has to establish a level of protection to be provided by the standard; that is, what risk will you tolerate, one in a million chance of an additional fatality per year or one in a hundred thousand, or whatever. That is a nonscientific issue. It needs to be established by rulemaking.

We do note that looking at the other nuclear risks regulated in this country and how other countries tend to regulate high level waste risk that a risk number on the order of ten to the minus five or ten to the minus six would fall in the range that people tend to use and therefore may be a good place to start, but that doesn't necessarily mean

that's where it comes out.

The second thing you have to establish in the standard is who is to be protected. We suggest that the critical group concept be applied here. A critical group is essentially a small group of persons at the highest risk as defined by cautious but reasonable assumptions and present knowledge, words drawn from the ICRP discussion of the critical group.

The critical group is not an unfamiliar concept in international radiation circles. It is ordinarily defined in terms of dose. We have taken it over into a risk format, which seems to us to be a fairly reasonable thing to do. The idea is to avoid focusing on a maximally exposed individual but rather on a small group of people that contains the person at highest risk and look at the average risk for that group of people.

Finally, you have to decide when the standard should be applied. As you know, the present EPA standard goes out for 10,000 years. We essentially suggested that there not be a time limit, or actually more appropriately, that the compliance assessment be done at the time that the risk is the highest. In the case of Yucca Mountain that is likely to be considerably more than 10,000 years. As I understand it from the scientific people on the panel, that could, of course, be quite different at some other site.

This approach, it seemed to us, at Yucca Mountain had a couple of advantages over the current standard in that it focuses on people who are at highest risk, which at this site is important, because it's the people who live near the repository. Given the more or less closed hydrogeologic nature of this site, that seemed to us to be particularly important.

Secondly, as I already said, it applies to the time of highest risk, which is likely to be much later than the 10,000 year time limit in the current standard.

The question then is, if this is the kind of standard you are going to set, can you measure compliance with it? How do you do a performance assessment? Here is how we thought about that.

[Slide.]

FRI: It is important, as you know, first of all, to separate two ideas here. One is the modeling of the concentrations of the plume that migrates away from the site given certain engineering and geophysical properties over a very long period of time.

The second step is to establish the exposure pathways by which humans are exposed to that concentration of radionuclides in that migrating plume in order to calculate the actual risk.

Our view of these two processes is really quite

different in that we believe that there is an adequate scientific basis for modeling concentrations in the plume over a very long period of time, probably on the order of ten to the sixth years. That is because the underlying geologic regime at Yucca Mountain appears to be sufficiently stable in order to model its behavior over something like that time frame. That is not to say that the geology is passive, only that the forces that determine its behavior ought to be around for something like that period of time.

This may be a point to which you want to come back. Let me just make a couple of points about it, because it is intuitively sort of strange to say, gee, you can predict something out over a million years.

First of all, we are not suggesting that you are predicting in some sense over a million years. What you are trying to do is to find the time at which the concentration is the highest and then do some exposure calculations at that point.

[Slide.]

FRI: Secondly, not everything becomes more uncertain over long periods of time as opposed to the present standard. The degradation of the casks, for example, can be a very important question if you are trying to define what happens up to a 10,000 year point, but if you are looking out ten times longer than that, it may be a matter of

indifference because all the casks will have failed anyhow.

And some events, in particular seismic events, volcanism or climate change, we think can be incorporated into a performance assessment over a very long period of time because you can say something about the probability of such events occurring where with a time limited standard you are forced to say when those events will occur, and that is really very difficult. So having a longer time frame for compliance assessment or at least for calculation of concentrations is not necessarily an untenable situation, and we think it's quite doable.

Now let me turn to the exposure scenario issue. That is more difficult. Science can help put bounds, of course, on the exposure scenario. People only eat and drink so much, and we know that. So there is a lot we can say about the pathways by which we get exposed to the highest concentration in the plume whenever that concentration occurs. However, one must also make assumptions in an exposure scenario about human behavior, and in particular where people are going to be in the vicinity of the site and where they are going to drill wells to take up water from the aquifer and thereby get exposed to it.

We are of the conviction on the committee that there is no scientific basis for predicting human behavior, and therefore these behavioral assumptions need to be made

essentially in a rulemaking as a policy question, coming up with a reasonable structure for evaluating performance and not something that can be scientifically resolved.

In the case of determining the assumptions behind the exposure scenario, however, the committee was not even able to agree on a starting point for discussion. There were two views on the committee.

The majority view was to deal with these questions as probabilistic. In effect, giving some weight to the fact that the site is not highly populated, that there are some places on the site where farming is not likely to take place or wells likely to be drilled, and that therefore the location of people and wells could be handled probabilistically and intersected with the probabilistic migration of the concentrations in the plume.

A couple of our people worked out in Appendix C of the report a methodology for making this calculation. We treated that essentially as an existence proof. We wanted to believe that it was possible to make a probabilistic calculation of this sort. We are not suggested it's necessarily the best way to do the arithmetic, but we were able to convince ourselves that there was a way to do it.

As you probably know, one member of the committee felt that this was not the way to go about it and strongly recommended using essentially a bounding case that ensures

that someone is exposed to the highest concentration that ever exists in the plume.

As I said, choosing between these two approaches is not really a scientific matter, in my judgment. It is more a matter of regulatory philosophy. It is a matter on which regulatory philosophy differs in other settings.

The Environmental Protection Agency has taken up this issue between what they call the theoretical upper bound exposure and sort of a high end exposure estimate, which to me is kind of the difference between the critical group and the maximally exposed individual. That's my opinion, but it's roughly the same thing. They pointed out there is a difference and they tried to figure out how to deal with a high end exposure estimate, although without great success. The National Research Council published a report on risk assessment a couple of years ago now which deals with the same issue and on which there are also two points of view on the committee.

In any case, the object of the exercise is that there needs to be a public process or rulemaking to decide on what is a reasonable set of assumptions for the exposure scenario so that the performance assessment that then takes place is politically and publicly acceptable. It is that kind of a question, not, in my judgment, a scientific issue.

Having laid that background, you will be delighted

to know that I can turn to the questions that the Congress asked us.

[Slide.]

FRI: The first was, will a dose standard or a risk standard protect the health and safety of the general public? In one sense the answer to this question is trivial, because by definition we have said that the people at the highest risk are protected at the time the risk is the highest, and therefore everybody else's risk at every other time is less. So in that sense the answer is, yes, because we set it up that way.

However, there is the question of cumulative effects. There could be large populations exposed to slightly smaller risks than this highest risk, which by multiplying together this very small risk by a large number of people gets you an expected number of fatalities, which is a large number, and it might be kind of politically or publicly an unpleasant number to deal with.

There are, of course, some serious scientific problems in doing this kind of arithmetic. First of all, multiplying together the very small probability times the very large number of potential exposures if you want to come up with a scenario is kind of an arbitrary thing to do. It needs a time limit. The time limit would have to be arbitrary, because if it's infinite, you get an infinite

number of people automatically. It is really hard to set up a priori the appropriate time limit for this and therefore very hard to come up with a standard against which to compare this cumulative dose or population number.

Furthermore, at very low incremental doses over background one is dealing with a linear hypothesis in the dose-response relationship. The data are collected at a much higher level of dose than we are talking about with these very small doses. It is prudent to assume for regulatory purposes that there is a linear relationship, and we certainly don't dispute that.

But if you read the BEIR report carefully, it points out that at these very low doses or dose rates there is a large degree of uncertainty as to the number of health effects and that band of uncertainty may include zero, which means, according to BEIR, there could be a threshold; you just simply can't disprove it.

So it is a very tricky area in which to try to do very precise calculations. Therefore we were unable to come up with a way of dealing with this problem of population or cumulative effects in what seemed to us to be a rigorous or sensible sort of a way and believed therefore that the appropriate approach was to go back to the individual basis for discussion.

What we suggested in order to place a lower bound

on the consideration of these cumulative effects was that the regulators consider the concept of negligible incremental risk, a level of incremental risk below which the health effects are considered negligible and therefore should just simply be excluded and not considered in a compliance assessment. This is a practice that is used in other countries and is documented in some ICRP work.

Again, we don't suggest what that number should be. We do note that if one were to take all of the carbon-14 in the repository, release it, trace it globally and calculate the individual risk number, it is like ten to the minus five less than ten to the minus six, or ten to the minus 11, or something like that. It's a very, very small number. We are not saying that is acceptable, but we are saying that that is the kind of situation that the regulator has got to deal with.

Given that background, we conclude that the individual risk standard will protect persons at greatest risk as well as the health of the general public provided the policymakers and the public are prepared to accept that very low radiation doses pose a negligibly small risk.

[Slide.]

FRI: I can now make reasonably short work of questions two and three. I have already told you that essentially the answers about the two human intrusion questions are no and

no. That's because human intrusion requires the prediction of human behavior for which we do not believe there is a scientific basis. But you still have to handle it. So in addition to saying you can't predict it and you can't rely on institutional controls, here is what we said should happen.

First of all, because you can't predict the probability of an intrusive event occurring, it should not be part of a probabilistic performance assessment; it has to be handled separately. We think that institutional controls, passive or active, are probably not a bad thing, and if the regulator wants to require them anyway, that's probably fine. It's just that you can't say with any certainty that they are going to be effective over a particular period of time.

As to evaluating the impact of an intrusive event, what we believe is useful here would be to conduct a consequence-only analysis. That is, to assume an intrusive event of some stylized form and conduct an analysis to see what happens to the repository and whether it is robust with respect to an intrusion.

I should probably make the point that this question usually gets asked: Does this kind of double up on the standard? The answer is, not really, because in the undisturbed case the highest concentrations and the highest

risks tend to occur at a very long period in the future, presumably after most of the casks are long gone.

What we are looking at here is an intrusive event that would penetrate a cask and go on down to the groundwater. It's a very different time frame and a very different situation. We do suggest that the same basic standard be used to evaluate this consequence-only analysis, that that's the way we see being able to handle human intrusion.

There is a fair amount more in the report, but you are probably tired of hearing about it by now, so I will end this part of the presentation and will be happy to take any questions that you have.

CANTLON: Thank you very much.

Pat.

DOMENICO: I'm surprised that you can't predict the effectiveness of institutional controls on human intrusion but you can predict the movement of radionuclides over a million years.

FRI: That is an important question. We are saying that human intrusion involves the prediction of human behavior and the modeling of geology does not. We have more faith in geology than people, I guess.

CANTLON: Don.

LANGMUIR: I'm another earth scientist type with doubts

about some of the statements there. We were briefed on this a little bit yesterday afternoon, so we wrote a lot of notes on our materials for you.

Under the compliance assessment statements, the first bullet to me is the absolutely critical one for this program as it has been conducted and might be conducted if there were any more money. The statement is "adequate scientific basis exists for modeling concentrations of radionuclides." Picking up on Pat's earlier comment, if you take that without any caveats, it says forget site characterization, forget modeling, forget testing at the site; we know enough now to comfortably predict and defend. Correct me if that's a wrong interpretation.

FRI: I apologize if that is what came across in the slides. The longest chapter of the report itself, Chapter 3, is a detailed discussion of the modeling requirements, the parameter characterization requirements, treatment of uncertainty, and then a sort of step by step analysis of the geologic pathways from the disintegration of the casks through transport to the vadose zone to the groundwater and the dynamics of getting the groundwater off the site, and so on, evaluating the state of play for each of those steps and noting in some cases that enough is known but in many cases that not enough is yet known and it's up to the site characterization program to get the data. I hope that those

discussions are helpful in describing what data and information are in fact required in order to carry compliance assessment out effectively.

Chris, I don't know whether you want to add something to that.

LANGMUIR: Chris could also respond to it. Do you have more confidence than Congress does that the DOE can get the right answers in a reasonable length of time, the kind of answers you are concerned about?

FRI: This is my own characterization of the situation, but the threshold question is, is there any point in doing all of this geology at all? If there isn't, if you are not going to learn enough to improve the quality of the decision that is going to be made, then you might as well forget it right now.

I think what the report says is not that there can be some precise kind of prediction made over long periods of time but that enough can be known about the geology to characterize the concentrations in the plume at some future date so that the distributions and other useful, important parameters, concentration, spatial and temporal parameters, you can say something about them that gives you a better understanding of what concentrations are likely to exist at the time of highest risk than you would if you did nothing. On the other side of the line, we think that doing this

work will improve the quality of the decision. There is nothing in the report that suggests that it is going to be a precise, tight, accurate prediction.

CANTLON: Jerry.

COHON: Following up on that and putting you on the spot and recognizing that you may very well refuse to answer this, and I would understand why, what I am appealing to here is your experience in science-based policy making at the federal level. Do you think there is a basis for arriving at a site suitability determination avoiding the way it is characterized by the congressional staff, the propensity to continually ask for more proof that negatives will not apply?

FRI: Yes, I do. What is required, in my judgment, is a decision by the regulator, through probably a very complicated public rulemaking process, about a framework that is suitable for compliance assessment; what are the set of assumptions and whatever other things you need to know that this is a publicly acceptable framework for judgment. It has got to be a framework that doesn't allow you to go off and cook up any old scenario you want. It can't be a framework that forces the scientist to chase down proving every negative that anybody can think of. I think, conceptually at least, a reasonable framework can be constructed. Whether in the real world of political

hurly-burly it can be or not, I don't know.

Absent that, yes, the folks in the Congress are right. You spend a lot of time chasing negatives and that employs a lot of people, but it doesn't get a whole lot accomplished.

CANTLON: Don.

LANGMUIR: One of the ways that we have encouraged this to go as sort of a scheme of dealing with things is total system performance analysis, which we will hear about tomorrow. I am just wondering if the thought process you went through in any way resembles that. In other words, subsystem to system, and with all of these uncertainties along the way we conclude that this is where we are in terms of an answer with an uncertainty, but it's a satisfactory answer perhaps or could be if we did these things, and are these things then determined based upon a systems analysis, or TSPA, as we would call it, view of the program.

FRI: I don't know enough about what is going to be discussed tomorrow to answer that question.

LANGMUIR: Maybe Chris Whipple or someone else could speak to that.

WHIPPLE: The quick answer is yes.

FRI: Chris says the quick answer is yes, and I will abide by his answer.

[Laughter.]

CANTLON: Victor.

PALCIAUSKAS: Presumably we can calculate the concentrations for half a million years. Given that assumption, we have to know what the conversion from concentrations to risk are half a million years in the future. Is that conversion factor really stable? I'm not familiar with this. Could you elaborate a little bit on that?

FRI: Are you thinking of the dose-response relationship?

PALCIAUSKAS: Yes. Or any risk conversion. It sort of assumes we know everything in the future.

FRI: I used a line earlier on that the assumptions have to be cautious but reasonable and based on present-day knowledge. That really goes to Dr. Cohon's question. No, we do not know what our understanding of the dose-response relationship will be half a million years from now. One can come up with all kinds of scenarios about what might happen at the site over half a million year period.

In order to avoid a whole series of speculations over the course of technology and human behavior, and so forth, the only thing you really can do is to use cautious but reasonable assumptions based on present-day knowledge. That's the starting point for a reasonable framework for evaluation. That's about the best you can do.

PALCIAUSKAS: Would it be fair to say that that introduces probably the biggest uncertainty in that calculation?

FRI: In the sense that you are absolutely sure that the world in the future will not look like the world of today, of course it introduces an uncertainty, but there are an infinite number of those futures. So your choice is between taking something where there is some reasonable basis for evaluation, where you have something to hang on to, knowing that it is only one of a number of possible cases, or to say I'm going to deal with an infinite number of cases, in which case you get nowhere.

CANTLON: Ed.

CORDING: I was interested in your committee's thoughts on the time frame or your thinking on the 1 million year type time frame, recognizing that the neptunium is coming out beyond that period certainly and you are changing now by two orders of magnitude what had previously been considered by the DOE. I was interested in your thoughts on the process of deciding or looking at the 1 million years.

FRI: Here I will ask for a little help. It's the realization that the half lives of the material you are starting with and its daughters extend out over a very long period of time so that the source term is very long and it changes over time. You've got then the movement of the

material geologically to some point where people can intersect it. You have got those two phenomena going on, the dispersion and the change in the source term going on at the same time. The trick is to calculate where those two intersect to create the highest concentrations. We say in the report that's likely to be 100,000 to 200,000 years.

Chris.

WHIPPLE: I will take a stab at what led us to recommend a significant change in the time period of applicability. I think there were several factors, but perhaps the most important one was looking at results of past performance assessment work for Yucca Mountain and seeing the concentration or individual dose curves rising as you hit the edge of the page in 10,000 years. The question of a standard that stops applying while the risk is increasing caused some real discomfort on the committee.

As we got inside further and looked at what contributed to the uncertainty out around 10,000 years, they were things that I think are not particularly uncertain or important by a longer standard, that is, assumptions about groundwater travel times and assumptions about waste package life.

Over the longer haul where you know that the package eventually will corrode and the waste will eventually transport, you could do calculations, and if you

don't care when the concentrations peak but are more interested in roughly at what level they peak, some of the processes actually become unimportant and the calculations become somewhat similar.

Again, the primary factor was a discomfort on the committee with a standard that could be defeated by simply postponing the date at which exposures could be reasonably expected to occur beyond its period of applicability, and also some discomfort that the rationale used by EPA to defend their 10,000 year cutoff was that the scientific uncertainties become too great. EPA never really explained that. They just sort of set it over 20 years, and the committee frankly didn't believe it.

I will note there is a comment in that report that says if EPA or NRC or anyone else in a policymaking position decides that for policy purposes we should cut off the period of applicability since typically EPA is not so arrogant to believe it can regulate to the end of time in other arenas, then that is their business. But you shouldn't argue that this is science when it's policy. You should come out and say it's a policy.

CORDING: One more point on that. Chris, if you were able to come up with some sort of mechanism to delay releases into the several hundred thousand year range, then you are pushing it further out. In a way, that is not

satisfying what your requirement would be. Or say you could push it out beyond a million years, you could delay those releases, and so you push your peak out beyond that period. If one could engineer the system that way or look at the barriers that way, are you pushing yourself into the same problem, those two orders of magnitude difference?

WHIPPLE: I think so. Our take on the performance assessment work that had been done was that long before you reach a million years you tend to reach very stable plateaus in concentration, very slow processes. The decay rates of neptunium, this is not something that comes and goes in a hurry.

The million years was not a policy choice on our committee. It came from the geologists, of whom I am not one, who said that that is kind of the right order of magnitude for the stability of the underlying structure and processes on which you do calculations. That is, we think the whole system may be stable of that order. It was not meant to be a precise number.

I have one concern I will mention, that this recommendation might be taken by the program to be a disincentive to look at robust waste packages: since the time at which peak dose occurs is so long, why not put it in a brown paper bag since you are going to get no credit anyway? I think that is a difficult problem that the

regulators and the program are going to have to deal with.

CANTLON: Two more questions and then we will break for lunch. Pat and then Leon.

DOMENICO: I just one have one question to anybody. I always thought that the EPA was the socially mandated entity designed to protect the public health. Have they been preempted here?

FRI: The nature of the statute is that they will issue a standard on the basis of individual dose, that they will do so within one year after our report is released, which was August 1, and that they will write a standard that is consistent with the recommendations of the National Research Council or explain why. They are busily working on this problem and have a cadre of people devoted to it. As far as I can tell, they are taking it quite seriously, as they have been asked to by the Congress.

CANTLON: Leon.

REITER: Did the committee consider, and if so, why did it not follow at least one of the recommendations of having a two-level standard, namely, be concerned about the peak dose but put particular emphasis on, say, the next 1,000 or 10,000 years, next 50 or 500 generations, since we are more sure of what is going to happen during that time?

The second question is, am I correct in interpreting the report as saying it's all right to have

subsystem requirements but don't let them get in the way?

FRI: The answer to the second question is basically yes, and the answer to the first question is closely related to the second.

[Laughter.]

FRI: Because the time of highest risk was likely to occur so far in the future, from the standpoint of assessing compliance with the standard what happened in the first thousand or 2 or 3000 years did not have much bearing on assessing compliance with the standard. We did consider and in fact some of the people who talked to us recommended a shorter term standard. The report notes that if the regulator wishes to set a shorter term standard to provide additional confidence to the public on the performance of the repository, that's fine, but remember, the answer to the second question is don't let it get in the way.

CANTLON: Thank you, Dr. Fri.

We will continue this discussion after lunch. DOE and EPRI are both going to comment on this. We will reconvene at 1:35.

[Whereupon, at 12:35 p.m., the meeting was recessed for lunch, to reconvene at 1:35 p.m., this same day.]

AFTERNOON SESSION

[1.35 p.m.]

CANTLON: This afternoon we will start with two responses to the NAS standards report by Steve Brocoum and John Kessler of EPRI.

Then we will have Steve back on to talk about the waste isolation strategy update.

Then we will have a break, after which we will put together a round-table discussion on the major issues that we have covered today, NAS standards report, the waste isolation strategy, and congressional staff perspectives.

Following that, we will have public comments. Jerry Cohon will make some comments just before we get the discussion group started.

Steve, if we can have you with your reactions.

[Slide.]

BROCOUM: I will be talking about DOE's preliminary reaction to the National Academy of Sciences report. The perspective that I will be focusing is the report is completed and it's out and now we expect to go into rulemaking and our major interaction will be in the rulemaking process.

[Slide.]

BROCOUM: We will have an overview; we will have some of our reactions, some potential impacts on our program, and

our future activities.

[Slide.]

BROCOUM: We feel that the National Academy of Sciences recommendations are far-reaching, and depending on how they are implemented -- that is a key word -- they could significantly impact the Yucca Mountain project and geologic disposal in general. So the key thing here is how these recommendations are implemented, and of course we are going to go into rulemaking for that.

The immediate reaction that I had when I saw the report was it struck me that the report was in some ways inconsistent with the 1990 National Academy of Sciences rethinking report. That report, as I recall, de-emphasized the use of model predictions and it stated that putting all the emphasis on models may give you a degree of certainty that doesn't exist or may not exist. The report also recommended using those models for comparative purposes.

In my own mind, I haven't been able to reconcile the two reports, and maybe the National Academy could help us there.

We believe some of the recommendations are consistent with our recommendations we made in April 1994; others are not; and I thought I would go through some of those.

When we gave our recommendations to the National

Academy they were underlined by certain principles. I want to kind of go over the principles.

They must be implementable. In other words, whatever standard comes out has to be usable and it has to be able to make progress in this program.

They ought to be relatively simple so that they in fact could be understandable.

They ought to be to the extent possible consistent with existing radiation standards and regulations, and that the degree of proof that the standards would require would be scientifically supportable.

That was kind of the philosophical approach we took in our recommendations in 1994.

[Slide.]

BROCOUM: Several of the recommendations are consistent with our thinking:

The health-based standard based on risk/dose.

Focusing on protecting people in the vicinity of Yucca Mountain.

The endorsement of a negligible individual risk level, and therefore being able to get over this problem of the carbon-14 release at Yucca Mountain.

Risk determined for an average individual in some sort of a critical group.

Compliance based on a mean of some predicted

results.

And future technology and living habits based on current population characteristics. I think the report says with some reasonable assumptions about the future.

[Slide.]

BROCOUM: Other things that were consistent with our recommendations:

Active postclosure oversight cannot be relied upon to ensure repository performance. I think the report even went further and said passive cannot be either.

It has been our position that it is not possible to predict the probability or the type of human intrusion over a period of 10,000 years.

Subsystem requirements may be unnecessary or possibly counterproductive and one ought to focus on the most important aspects of total system performance assessment.

The standard should not incorporate the principle of ALARA.

These are all similar recommendations we gave the National Academy.

[Slide.]

BROCOUM: These are some of the areas that we have concern. We are particularly concerned with demonstrating compliance for periods beyond 10,000 years, trying to

separate the demonstration to a standard from the evaluations one would do to get insight into system performance. We have always, as we did in TSPA-93 and did in TSPA-95, done these calculations out to a period of greatest risk. We think they are most useful to help optimize the repository, to help evaluate different designs, to help choose among different features, and so on, in the repository.

The recommendation is based on the premise that the geologic system at Yucca Mountain is stable or can be quantified for a million years. We think that it will be very hard to reach consensus among all the project scientists on that issue. We have already debated that in house and we have had people on both sides of the table on that. Honestly, I can't tell you right now that we have a consensus. I believe that will be very difficult to achieve.

We feel that significant uncertainties in such long-term predictions would make it difficult to provide a reasonable assurance in a licensing arena. That is not because one cannot make these calculations and maybe understand to some degree the uncertainties, but the fact is that we may not be able to get the uncertainty bounds down narrow enough so they would be acceptable to our regulators. I am trying to say in the current regulatory regime we

think that it will be very hard to implement a standard that goes out without any time limits.

We feel it's ultimately a policy matter as to what the time limits should be or the time one should take into consideration. We think that we should take into consideration how other standards are regulated. If you look at other hazards, they generally go between 1,000 and 10,000 years for other EPA regulations. As was alluded to this morning, the general standard for disposal of waste which applies to WIPP and would apply to any other geologic repository if there ever was one has a 10,000 year period of performance.

That does not mean one would not do these evaluations for longer time periods for the insights and the design considerations and those kinds of aspects.

[Slide.]

BROCOUM: We are also concerned with the quantitative treatment of human intrusion impacts.

The National Academy recommended comparing these impacts with the limit for the undisturbed repository performance.

We feel that in essence the human intrusion scenario would in effect become the controlling scenario, assuming that this scenario doesn't behave as well as the undisturbed one. So in a hearing or in the process that one

goes through there would be a lot of focus in this area.

We still feel that human intrusion should be handled by some qualitative approach and requirements, such as design requirements and passive institutional controls.

[Slide.]

BROCOUM: The approach to calculation of risk to critical group to some of the people in DOE appears to be very complicated.

Appendix C gives a very prescriptive approach to making those calculations and may not be easily understood or comprehended by the public. Some of us felt the Appendix D approach was more straightforward and is kind of more consistent with the way other radionuclide regulations have been in the past.

[Slide.]

BROCOUM: We think that as we go into rulemaking the critical issues for EPA and the other parties to consider are these:

The level of risk that is considered acceptable.

The time frame for the quantitative compliance demonstrations, the standard that you are held to.

The definition of a reference biosphere, including the critical group and exposure scenarios. This is very important because, depending on the assumptions and how you define these, the site can either pass or fail.

Finally, the treatment of human intrusion, as I mentioned in the early slide.

[Slide.]

BROCOUM: What are some of the potential impacts to the project if these recommendations were implemented in a standard?

There might be increased emphasis on measures that would reduce the long-term dose. It was mentioned earlier today, well, if we are only worried about the period of greatest risk, maybe we could do away with the waste package. As you will hear later from Jean, we are taking almost a two-pronged approach. We are taking a containment approach for the operational and the early preclosure up to several thousand years and a different approach which consists of slow release, diffusion and dilution of the radionuclides for a longer term period.

There was also increased emphasis of a calculation of a long-term dose/risk:

Waste form dissolution.

And, of course, saturated zone hydrology, which in the days we wrote the SEP was not considered very important. That has become more important over the years as we realize we may be going to a dose or a risk-based standard.

[Slide.]

BROCOUM: Other potential impacts.

There might be a decreased emphasis on explicitly demonstrating compliance with any subsystem requirements. You'd still have defense-in-depth because you would have multiple barriers, but the focus would be on how the individual subsystems contribute to the overall and total system performance, not how the subsystems contribute through some arbitrary subsystem requirement.

There would also be a decreased emphasis on site characterization data that do not impact long-term doses. For example, pathways for and release of gaseous radionuclides.

[Slide.]

BROCOUM: We are planning to provide informal comments to the EPA. The EPA did issue a Federal Register notice on September 11. They asked for informal comments by October 26. We have those comments in draft form.

They also had a meeting in Amargosa Valley and several meetings in Las Vegas where they got oral and written public comments.

We anticipate EPA moving out expeditiously on the rulemaking and us working with EPA and the NRC during the rulemaking process.

The resulting standard, in our view, has to be implementable. When I say implementable, it has to lead down one of several paths.

The first path would be it would have to eventually lead to a license application. If it successfully went beyond the license application, then have the ability to construct and operate the closed repository.

The other path is it would have to lead to a decision that Yucca Mountain isn't suitable as opposed to constantly being on a treadmill with no discernible progress.

That's kind of how I'm defining implementable here.

We feel as an agency that we offer a unique perspective because we are the agency that will have to demonstrate compliance with whatever standard is eventually promulgated.

Those are our preliminary comments on the proposed standard.

CANTLON: Thank you, Steve.

Questions from the Board? Ed?

CORDING: Steve, the comment on "NAS recommends comparing these impacts with the limit for undisturbed repository performance," could you clarify what is meant there by the limit?

BROCOUM: Whatever the overall risk that is allowed, whatever that risk limit is, which will be decided in a rulemaking process, they recommended that you have at least

one stylized human intrusion case that you use to compare that limit to how robust the repository design is. I believe that was the point of the recommendation.

What DOE is saying is that might become the controlling case, because most likely the repository system will perform worse with some sort of a stylized intrusion than it would if it was undisturbed. That's the point I was trying to make. The limit would be they would have the same standard for the human intrusion case as they would for the undisturbed case.

I hope I'm saying that correctly. I'm looking back at the National Academy people.

CORDING: You had no note on the increased emphasis on measures that would reduce long-term dose, and there you are talking about the engineered barrier system. The focus on the engineered barrier system, you were commenting, was several thousand years. Are you looking at it to do something for you?

BROCOUM: The point I was trying to make is something Chris Whipple said earlier. He said that, well, we hope that people don't say just because we recommend you focus on the period of greatest risk that we don't just get rid of the robust waste package.

The point I was trying to make is when you hear the presentation on our waste site selection strategy, we do

have a robust waste package strategy to provide containment for the operational period and the early part of the postclosure period for several thousand years. We still have that even though the peak risks don't occur in those times. That was my point.

CANTLON: Don.

LANGMUIR: Steve, you pointed out several differences between what your thoughts are and what how you perceive the NAS to have come out on these issues. In particular you are pointing out that you are going to focus on individual subsystems and how they contribute to TSPA. The sense I got this morning from Bob Fri was that they were trying to avoid subsystem details and look at the larger system performance. Maybe I am not right with that, but that was the perception that they gave, that giving too much detail to subsystem performance was not their intent.

BROCOUM: Maybe I misunderstood what they said. What we interpreted was that we shouldn't focus on some arbitrary subsystem requirements; we ought to be focusing on overall system performance. The point I was trying to make is, to see how the overall system performs, you have to understand how the various subsystems perform. Not because you are trying to reach a thousand year groundwater travel time or some other arbitrary substandard from the NRC, but because you are trying to see how the overall system performs. I

was trying to make that distinction there. Maybe I didn't make it clearly.

LANGMUIR: I would love to hear their reaction to the approach that you are taking here and the DOE would take with the subsystem effort to look at things like EBS, and so on, as key parts of the system, whether that is consistent.

I don't know whether Chris could comment.

WHIPPLE: I think there is a key distinction that needs to be made here. What Steve has said is that the program has to look at individual subsystem performance to understand the behavior of the overall system. We certainly concur. The point in our report was that the regulatory agencies -- we had in mind the prescriptive nature of the NRC Part 60 -- should not tie DOE's hands about the way it achieves the degree of safety recommended, that they should be able to do that however most effectively they can. So I think we are in agreement on these.

CANTLON: Other Board questions?

[No response.]

CANTLON: Staff?

[No response.]

CANTLON: Thank you, Steve.

John Kessler.

[Slide.]

KESSLER: I would like to speak today about some

preliminary technical comparisons that we have done at EPRI on the potential regulatory standards. I realize that when I say standard, for NAS those were recommendations for a standard and that is why I have it in quotes.

[Slide.]

KESSLER: The outline of what I would like to talk about today.

Our quick description of our involvement with the standards process.

A bit about our TSPA code IMARC that we have used to evaluate the standards.

And then some very preliminary comparison of the standards: look at the basic standard form; look at release rate versus dose rate or health risk; look at 10,000 year versus peak dose or health risk sensitivities; look at the critical groups issue; and a little bit about moving the fence post. NAS recommended you do your calculations right at the footprint of the repository whereas 40CFR191 talks about five kilometers and then H.R. 1020 talks about something that may be farther away than that.

[Slide.]

KESSLER: Our involvement with this issue begins with what EPRI does.

We conduct research for the U.S. nuclear utilities, and the U.S. utility view in general is that the

standard must protect the health of present and future generations and the standard must also be licensable. When I say be licensable, I am saying it in the sense that we cannot ask for more than science can deliver. That doesn't mean that we think that Yucca Mountain should be licensable; we think the way the standard is set up should allow for the licensing of some repository somewhere, that we shouldn't exclude that option by asking for basically more than science can deliver.

We have actively participated in the NAS TYMS Committee public meetings. We have conducted an analysis of 40CFR191 in a report; we analyzed alternate standards and recommended another standard in a second report.

We have begun assessment of the NAS recommendations and H.R. 1020. I will report a bit about that today.

[Slide.]

KESSLER: Our primary assessment tool is our TSPA code IMARC.

It was developed by Risk Engineering and a small team of experts.

We use an event tree approach, which I think most of you have heard us talk about before.

We are just completing an upgrade of the code that has additions where we extended the analysis out to one

million years; we have time-varying infiltration rates now. Our hydrology model: we have 3-D in the saturated zone, so we can look more now at this dilution issue; still 1-D in the unsaturated zone; we have got fracture/matrix coupling since that is another important issue; dispersion and daughter ingrowth.

[Slide.]

KESSLER: The next viewgraph. I do not intend to go through all of this. I am putting it up there to show basically what are all the components now that are in TSPAs.

Mostly what I want to feature now with these new recommendations for standards that are dose based is that besides the external components, which I consider climate feeding rainfall and geosphere and the engineered, we have a new kid on the block. That's biosphere components.

Before, we used to be able to stop with 40CFR191 basically with a contaminant flux past the fence post. Believe me, that's what it says here even if you can't quite read it up on the screen. Now we have got to calculate a dose to a critical group. That means we have to worry about the biosphere components:

Relevant human behavior, which means we worry about the withdrawal rate.

Agricultural practices; consumption patterns.

We've got a dose per unit intake model we have to

worry about that feeds into the dose to a maximally exposed individual.

And finally, this last controversial issue, the spatial/statistical distribution of population characteristics that is going to give us our dose to critical group.

The new kid on the block, biosphere components, we haven't heard much about it throughout the years, but I can guarantee you we will in the future.

[Slide.]

KESSLER: This is a quick summary of the event tree branches. Basically all of these are connected in series into a large event tree that we did in this preliminary analysis. We certainly intend to do more sensitivities. We went with a reduced set of branches at this point to get some preliminary analysis done.

We have got infiltration/climate here where we have these different infiltration rates that were assumed for a low and high. The ones in the brackets here repeat for the million year cycle.

We looked at different heat transfer mechanisms, different solubility/dissolution rates.

Fracture/matrix coupling. Either it all flowed in the fracture or it all flowed in the matrix, so there was some coupling between the two.

We looked at matrix sorption values that ranged from high values and low values.

Those were some of the sensitivities we have conducted to date.

COHON: Could you go back. I didn't understand what you said about the cycle.

KESSLER: In this top corner we were taking into account fluvials where we are assuming some change in infiltration depending on where we are in the glaciation events. We are tying in the increased amount of rainfall we would expect at a full glacial maximum into what we would expect for a net infiltration, and that is certainly time dependent.

We assume some sort of Melankovich cycle, I believe, in terms of the repetition of glaciation cycles throughout the full million years, which gives us basically this repetition of infiltration rates out through the full million years as well.

COHON: So the cycle is 100,000 years?

KESSLER: Right. We are assuming roughly 100,000 year cycles.

[Slide.]

KESSLER: The preliminary comparison of the standards.

We try to look at the basic standard form, release rate versus dose rate or health risk.

10,000 year versus peak dose.

Critical groups.

And moving the fence post.

[Slide.]

KESSLER: When we looked at release rate versus dose or health risk criteria we noticed something about the saturated zone flow velocity. We found that higher velocities increase the release past the boundary. That is, they managed to flush things out past the boundary a little quicker.

However, the situation got a bit more complicated when we looked at doses. For later times we found that the higher velocities could cause more dilution and therefore reduce the dose. So again there is one factor that is shown here as to how things change between just release and dose standards.

[Slide.]

KESSLER: This is the classic CCDF that shows two different values of saturated zone flow velocities of one meter or ten meters per year at 10,000 years versus 100,000 years.

At 10,000 years we see that we have a higher CCDF, that is, more dose/risk, so to speak, for the higher flow velocity.

However, at 100,000 years we see a lower dose risk

for the higher flow velocity. That's because we are just getting initial breakthrough here at 10,000 years. Higher velocity manages to push more radionuclides out past that five kilometer boundary, because that is what we are dealing with here in this case.

In the case of the 100,000 years we are actually getting some dilution. Then we see that the higher flow velocity reduces the dose.

[Slide.]

KESSLER: On to the new IMARC. Here are some preliminary results.

We have this transition period where we are going up to a peak. Right now we are showing the primary contribution to that peak due to technetium-99 with iodine-129 down here about a quarter of a magnitude lower. The neptunium-237 contributes the most amounts to the peak at even later times, out in the couple hundred thousand year time frame, with iodine a close second behind it.

What I want to focus on for this talk is not so much what the absolute values are or where exactly these peaks occur, but on the relative effects and just the general shape of the curve and what that means for the forms of the standards.

We have this transition period here that occurs in this 10,000 to 100,000 year time frame. We have this peak

dose here that occurs out at 100,000 years and beyond. That's the thing that I want to focus on here.

COHON: I'm sorry. Can I ask another question? I'm sorry to slow you down.

KESSLER: That's okay.

COHON: You are using a term over and over, and I need some help with it. In what sense is this dose?

KESSLER: For all of our branches on our event tree we calculate dose versus time.

COHON: Dose to whom?

KESSLER: In this case dose to an average individual in a small population. I will describe briefly what I mean.

COHON: Who is assumed to live in a certain location?

KESSLER: Right, around the vicinity of the repository. Not some world average to a maximally exposed, but an individual in a small population.

LANGMUIR: Another clarification, please. Is the assumption that failure occurs at 10,000 years? In other words, everything starts at that point? Is the assumption you have waste isolation to that point?

KESSLER: No. We have containers that begin to fail before 10,000 years.

LANGMUIR: What does the model say about the failure? Is that coming up?

KESSLER: No, but I'll make it come up.

[Slide.]

KESSLER: This is not in your handout package. This is just an example of one set of container failure curves we happened to use that is just sort of indicative of the pattern.

What I am showing you here is for what has been labeled the "NPC-1 Container" where we have got 10 centimeters of mild steel over .95 centimeters of alloy 825.

This is the solid curve, and this is basically what our fraction failed versus time is for these particular set of conditions, 114 kilowatts per acre.

We are assuming a heat-pipe conduction mode and fracture flow. For the thicker containers we get a shallower curve with the mean failure time being much longer. I am just showing you this one.

The point here to notice for later on in the talk is that you have still got the majority of your containers that have failed out here between 50,000 and 100,000 years.

[Slide.]

KESSLER: As we have already heard from several people, we certainly concur that the time period of the standard can significantly impact the waste isolation strategy.

From zero to on the order of ten to the fourth years, this transient period, we've got a whole series of important factors. All of these have significant impacts on

what you are measuring for an expected dose at 10,000 years or on the order of 10,000 years.

[Slide.]

KESSLER: However, for that peak dose period or peak health risk period that is at roughly ten to the fifth years and beyond, the list gets a heck of a lot shorter.

Right now in our preliminary analysis we see two things show up, saturated zone dilution and the biosphere components. That's it.

I have a feeling if we do some more we may show some sensitivity to the number of packages that get wet. Although we haven't done that yet, it certainly fits with some flow diversion barrier engineered features that may be available. That may add that third one to this list.

The point I want to make is that there is are lot less things that affect peak dose when you look out at 100,000 years versus 10,000 years.

[Slide.]

KESSLER: A few examples of that follow. We made some base assumptions just to sort of see what contributions we get from what part of the system.

Again, these are preliminary, so the order of magnitude increase or exactly where these come together probably would change with additional analysis, but I think the general trends won't. I compared two things, the base

case here where we assume we have got the containers with that failure distribution I just showed, and here, where I say "no container," I'm assuming that all my containers fail at 1,000 years.

We chose 1,000 years simply because we couldn't deal with all those short-lived radionuclides in our analysis at the time, and I think that, as Steve just alluded to, and I guess we will hear from Jean next, there are some very good reasons why you would want to probably have a 1,000 year container.

The message here is that if you assume all those containers fail at 1,000 years versus if they don't, eventually they are going to come together at the peak dose. The containers, therefore, have a huge effect at 10,000 years.

DOMENICO: Excuse me. Can you explain why that happens?

KESSLER: Basically all the containers have failed.

DOMENICO: There is a cause and effect. That's the cause. Why do they converge at that time period? What's happening?

KESSLER: All the containers fail out in this range here. After a few more tens of thousands of years all those containers that have failed now have a chance for the release to start reaching your well withdrawal point. Now

they all contribute to dose just as if there were never any containers there to begin with. You have got all the containers releasing in this curve finally at this point. You've got all the containers releasing all the way along here. That's why they are separated.

LANGMUIR: John, is there any backfill considered at all in these analyses?

KESSLER: In this run, no, we haven't done much backfill. That is why I'm saying that looking at flow diversion barrier type backfills is something we haven't looked at in terms of IMARC. We are doing separate analyses on that right now, to show that that may make a big difference.

[Slide.]

KESSLER: Similarly, we could do the same thing with geology. I've got my base case here. When I say "no geology" what I am assuming is, just for the sake of calculation here, mind you, that I don't have anything under the repository, except we arbitrarily chose a one meter thick saturated zone just so our models wouldn't blow up and we could basically flush everything down to our well withdrawal point. So we are losing all the effect of dilution in the saturated zone; we are losing all the effect of all the unsaturated zone components that show up.

What you see is is that geology does you a lot of

good in that it shifts things down at some time period for that transition period. It is still doing you some good out here. What is that due to? Dilution. Dilution only. That's the only thing. Saturated zone dilution is what keeps these curves separated. That is what you have got left from geology out in a long time frame.

[Slide.]

KESSLER: Comparing the health risk limits. I thought it would be sort of interesting to try to put all the different kinds of standards and terms of some average individual risk limit like NAS was recommending.

NAS suggested average individual risk limits in this order of ten to the minus sixth to ten to the minus fifth. Their risk was to this average member of the critical group.

H.R. 1020 has 100 millirem per year standard, and if you use the ICRP conversion, that converts into an average individual risk limit of five times ten to the minus fifth, which is the same order of magnitude. Here the risk is to an average individual in the local population. I will get into the differences in these terms next.

40CFR191. Well, how do we put that in terms of individual risk?

I chose saying I know the basis for the release limit was 1,000 deaths in 10,000 years.

For carbon-14, where they assumed that was due to a world population of 10 billion over 10,000 years, we essentially have affected population average individual risk of less than ten to the minus tenth.

If we assume a drinking water release pathway where it may only affect those that are using the contaminated water of Yucca Mountain, if they only use it for drinking water, the M&O said, well, we think that Yucca Mountain water can support about 10,000 people. If you divide the 1,000 deaths in 10,000 years over those 10,000 people, now your individual risk is on the order of ten to the minus fifth.

If you look at a subsistence community that needs that water also to grow their crops and deal with their livestock, M&O again said we think roughly an order of 100 people can be supported. Now if we are putting those 1,000 deaths in 10,000 years all in that 100 people population, the individual risk is up to ten to the minus three.

[Slide.]

KESSLER: That leads me into critical groups.

We've heard a bit about the NAS approaches to critical groups. I won't go through it again other than to say there are the two, the probabilistic critical group and the subsistence farmer critical group.

[Slide.]

KESSLER: 40CFR191 has this population-based approach, and I say it neglects risk heterogeneity in the sense that EPA didn't say anything about how those 1,000 deaths in the 10,000 years should be distributed. They just said 1,000 deaths in 10,000 years.

Therefore, there is really no special production of those at greatest risk, and I say beyond 1,000 years, because they do have dose standards for the first 1,000 years in 191.

H.R. 1020 talks about an average individual in the local population, where they are making assumptions, I believe, about spatially averaged population distribution and averages of distributions in consumption rates that would go into that average individual concept.

[Slide.]

KESSLER: I thought it would be interesting to try to explore a little of the basis behind the ten to the minus sixth to ten to the minus fifth per year limit that NAS was recommending.

I did not want to go back and look at precedent. There are plenty of reports you can just go and mindlessly take numbers out of. But I thought it might be interesting to look at some involuntary risks or risk limits, an annual individual average. They are U.S. population averages, or in this case it's living in Denver.

This one, being struck by a crashing airplane, is just a crazy example. It's on this order of magnitude. It's sort of an involuntary risk, but the important thing is it's averaged over all of those living in the U.S. There is a lot of heterogeneity in terms of individual risk tied up in this number.

Similarly, extra fatal cancer risk living in Denver is this. There is probably a lot of risk heterogeneity within the population of Denver, which is still pretty large.

U.S. Food and Drug Administration has a food additive regulatory risk floor. That is, if the individual risk from eating some food additive is less than ten to the minus sixth, they don't worry about it anymore. Again, food consumption pattern assumptions are buried in that number. A lot of heterogeneity there.

EPA general risk limit range is on the order of ten to the minus sixth to ten to the minus three.

[Slide.]

KESSLER: If we start with those risks that you could say society broadly tolerates or our regulators tolerate or allow and you look at who those risks were assumed to be for, you begin to very quickly realize that there is a health risk limit-critical group link that is involved.

Involuntary health risks of on the order of ten to

the minus sixth to ten to the minus fifth are broadly tolerated by society, with some exceptions, certainly.

Group sizes are often orders of magnitude larger than these few tens of individuals that the NAS recommended.

Risk heterogeneity exists within these critical groups and it can be very large in terms of what society broadly tolerates.

[Slide.]

KESSLER: So what are the implications of all this for critical groups at Yucca Mountain?

Our take on this is is that applying a ten to the minus sixth per year individual limit to a maximally exposed individual is inconsistent with the fundamental philosophy behind the ten to the minus sixth limit in the first place, and it is certainly very conservative.

Even at ten to the minus fifth per year limit to an average individual in the local population, which is the H.R. 1020 approach, it is still conservative in the sense that the present and future local Yucca Mountain populations are probably much smaller than these huge Denver and U.S.-wide populations that we have risks of the same order of magnitude that society broadly tolerates.

The FDA's risk floor of ten to the minus sixth per year implies the average food consumption habits over large populations and that is somehow acceptable, at least to FDA.

[Slide.]

KESSLER: At EPRI we have developed an average individual concept where we have some statistical components based on present-day behavioral distributions, some probabilistic components, and we think if you go and explore a bit that there is some basis for assigning numbers and values to those probabilities or statistics.

What I want to really focus on for you today is it makes a difference what you assume for population behavior.

[Slide.]

KESSLER: Here's another CCDF of dose in this case at 100,000 years versus probability for a couple different ways you could define your average individual in different kinds of local populations.

Here's the maximally exposed individual out here.

We assumed a single farm family, which is this yellow curve.

Small population, which in our mind was several sets of small farm families.

And a large population which we thought in terms of a more urban population using primarily drinking water.

The idea is you get CCDFs all over the map. Biosphere in the sense of what you assume for exposure pathways is tremendously important.

[Slide.]

KESSLER: Finally, I will quickly go over the fence post. What I mean is the downstream position assumed for licensing calculations.

NAS recommended the edge of the repository footprint.

40CFR191 talks about five kilometers from the edge of the repository.

H.R. 1020, I believe, has language in there about the edge of the withdrawn land.

[Slide.]

KESSLER: For preliminary calculation purposes we just looked at the edge of the repository footprint versus five kilometers downstream and looked at the expected value of dose versus time. What we see is, yes, we do have slightly lower expected doses five kilometers downstream, but there is not a whole heck of a lot of difference here.

I would like to again emphasize these are preliminary conclusions. We haven't done a sensitivity study. Where it shows a vertical transverse dispersivity of five meters we did not do any sensitivities on that, and our results may change, depending on what we choose.

The point is it gets back to the idea that dilution is something that would certainly affect not only the total results, but affect certainly how much difference there would be between where downstream you calculate your

results for.

[Slide.]

KESSLER: Conclusions.

Both the NAS recommendations and H.R. 1020 are a significant improvement over 40CFR191. They both directly regulate health effects, i.e., they are dose or health risk-based. Their limits are based on broadly tolerable individual risk values.

We also think that individual risk limits and critical groups should be consistent. We think that an annual individual risk range of ten to the minus sixth to ten to the minus fifth is probably broadly tolerable. However, we think that there is an inconsistent approach if these limits are applied to a maximally exposed individual, and we think that they are most consistent if they are applied to an average individual in the local population.

[Slide.]

KESSLER: The next major conclusion is certainly the time of the regulatory cutoff affects the amount of work to be done. I showed you my whole list of things that were important to 10,000 years. However, there is a much shorter list if we have a peak dose or health risk line.

And the location of the fence post, at least in our preliminary analysis, is not very critical.

Thanks. Any questions?

CANTLON: Thank you.

Don.

LANGMUIR: John, you mentioned that you hadn't yet considered the effect of backfill.

KESSLER: Not in terms of burying it into IMARC yet. We are doing some side calculations.

LANGMUIR: Perhaps you could comment. I am interested in what you think its effect might be on these differences in doses as a function of time.

KESSLER: I think they could be significant. We have made assumptions here about the numbers of containers that get wet. Backfill, if it's done right in terms of acting as a flow diversion barrier, can significantly reduce the number of containers that get wet. In some of our sensitivity studies that I didn't show today there is certainly a direct correlation. If you can reduce the number of containers that get wet or increase the diffusion time out to where you do have flowing water, you have the potential of permanently reducing or reducing for a much longer time frame the release that would correlate into reduced dose.

LANGMUIR: When do you plan to do that work? When might we hear about it?

KESSLER: We are wrapping it up now. When we want to bury it into IMARC, I'm not sure when we will get around to

it, but we are going to issue in the next couple of months a report on the preliminary assessment or feasibility study of the flow diversion barrier concept in the sense of what it gains us and where the difficulties lie.

LANGMUIR: Will you simply assume a crushed tuff material?

KESSLER: We've got a crushed tuff material with some sort of finer alluvium that still has a good saturated conductivity over it. We look at issues like implacability and, if you have slumping places in it, how much slump can you withstand before you get flow that gets focused and breaks through that capillary barrier.

LANGMUIR: So this is chiefly a diffusion barrier. You are not going to invoke adsorption as a retardation mechanism as part of the performance of backfill?

KESSLER: Certainly that's in there.

LANGMUIR: You wouldn't expect a lot of that in crushed tuff, but you would in bentonite. Will you consider all those options?

KESSLER: We are not considering bentonite because it doesn't have a good saturated conductivity.

CANTLON: Pat.

DOMENICO: Where you apply the standards is confusing me a little bit. I think the only way you can get dilution like this is to have some slight drips from the unsaturated

zone moving into a fast moving saturated zone. The greater the velocity, the more the dilution, and the further you are away from where those drips are taking place, the greater the dilution. So I don't see why there is very little difference between the edge of the repository or five kilometers down. That is one point.

The other point. Did you change these models at all in recent years? Is this the same thing that we heard from you guys a couple years ago?

KESSLER: No. Our hydrology model is significantly different than it was.

DOMENICO: It's a drip model, isn't it?

KESSLER: We still have the drip source in there, yes.

DOMENICO: Did you have that drip source in two years ago?

KESSLER: Yes, we did.

DOMENICO: Then how come your dilution didn't play that much of a role in reducing concentrations two years ago? I always thought you never had enough dilution.

KESSLER: It did. In that early curve I showed where it did, the first one where I showed the CCDF where they switched positions between the one and ten meter velocity. That could be the way we chose to calculate our concentrations.

DOMENICO: So you did have the drip source term always?

KESSLER: Yes.

DOMENICO: I don't see how else you can get dilution to play any role. Dilution is a hard thing to model. Dispersion doesn't do anything for you then. Dilution is doing it all for you.

KESSLER: Correct. We have longitudinal dispersion in the model.

DOMENICO: Did you see anything in the model about whether that drip was distributed or localized?

KESSLER: It's localized in the sense that for certain characteristics of heat loading and infiltration rate and time and temperature curves we assume certain fractions of the repository were under certain kinds of water conditions.

DOMENICO: Would you say that drip was equal to your assigned infiltration rate?

KESSLER: The distribution?

DOMENICO: No, the actual drip rate was equal to what is coming into the repository from above. Is that a fair statement?

KESSLER: I think we did a mass balance so that what came in above went out. It was just how it was distributed within the repository.

DOMENICO: Did you vary that number at all?

KESSLER: We haven't looked at fraction of repository wet too much. This is why I am saying it's preliminary.

When we look at different sensitivities we can see that that probably is a significant factor. This is certainly one of the areas where we need to do some more work.

DOMENICO: Thank you.

CANTLON: Don.

LANGMUIR: It has been quite a learning process for Board members in the last couple days here. One of the things we have been hearing among a lot of things is that perhaps the unsaturated zone is not relevant anymore to the issues, particularly on the long-term performance of the repository.

I was looking back at your overhead number 10 which lists processes considered or important factors in the short-term performance of the repository, and you do mention matrix alteration/dissolution rate, but I don't see anything at all here which directly argues for the unsat zone performance of Yucca Mountain as an important barrier to release. Do you consider it, or is it sort of subsumed in things like fast flow paths?

KESSLER: Yes. Certainly fast flow paths, both unsaturated and saturated.

LANGMUIR: I guess the bottom line is, are we done? Do we need to look at any more unsat zone characteristics?

KESSLER: It depends on what your standard is.

LANGMUIR: Only in terms of the short-term repository

performance?

KESSLER: In terms of short-term repository performance.

LANGMUIR: It won't matter when we get to the long-term, the neptunium kind of issues?

KESSLER: For the long term I am showing the only geology effect you get is dilution in the saturated zone. For the short term there are a lot more things that have to do with the unsaturated zone. I caveated that by saying certainly there is a feedback on the unsaturated zone in terms of the number of containers that get wet but it's a sensitivity we haven't really looked into yet. Flow diversion barrier you could also say is an unsaturated zone phenomena, and those things remain to be explored.

CANTLON: Jerry.

COHON: I'd like to go back to the one before slide 22.

KESSLER: The different critical groups?

COHON: Right. Maximally exposed individual, single farm family, et cetera.

KESSLER: Here it is.

[Slide.]

COHON: You put it aside because you knew I was going to ask about it.

KESSLER: Yes. That's why I couldn't find it.

COHON: I just need help in understanding this. First

of all, could you explain why the small population and large population curves differ in the way they do?

KESSLER: It's what we assigned in the previous viewgraph. I didn't want to get into it because I could spend an hour just talking about how we generated all of these numbers. This is a preliminary assessment.

We assigned probabilities or statistical distributions to things like what's the probability that a large population is going to use local water, what's the probability that they are going to use a certain portion of the water, what kind of dilution factors would be involved in a large population, how much water do they need versus the contaminant size versus a small population or a single farm family. That kind of stuff is in there.

COHON: Staying with the small and large population, is it that there are certain assumptions about the spatial distribution of these two populations and therefore there are these differences we see in the average person in those two populations?

KESSLER: There are some spatial distribution differences in there, yes.

COHON: Because a large population is larger, the variation is greater within the population; is that a factor?

KESSLER: I'm not sure about how much variation is

within the population, because we averaged everything.

LANGMUIR: Excuse me. Are you simply dividing the size of the population by a fixed amount of dosage that is available from the repository?

KESSLER: No. In this case we said large population. In a sense we didn't have a number in mind. We had what we thought the large population behavior was. We assumed, for instance, without identifying the size of the large population, things like, if they are going to use the local groundwater source, first of all, they probably extract the majority of the water running by for their use. Then we made the assumption, well, that probably feeds into a central water distribution system. In that sense you are diluting the dose where maybe one of the wells in your whole feeder system is the one that is extracting from the plume. There's our dilution factor. It is distributed then among the urban water supply in that way.

COHON: A very basic question. Is the one number we have been talking about the mean average person annual dose in a particular year?

KESSLER: These represent the average.

COHON: That's the average person.

KESSLER: Average individual in the population.

COHON: It's a cumulative distribution function, right?

KESSLER: Yes, but this is a cumulative distribution

function over primarily distributions in geologic and natural parameters. That's what gives us our CCDF here. I don't really have a full probabilistic model in terms of CCDF here. I basically added on a post-processor where I have averaged.

This gets back to one of your questions about what does that mean in terms of distribution between small and large populations. Do we have more risk heterogeneity in the large versus the small? The answer is we haven't looked at that because we've just looked at the averages for population. I think that the NAS recommendations say you should look at that.

COHON: Thanks.

CANTLON: Leon.

REITER: John, I believe EPRI originally proposed dual criteria.

KESSLER: Yes.

REITER: Would you sort of repeat what it was, why you thought that was good and whether you still think it's a good idea?

KESSLER: I think we've heard rationalizations very similar to what we had coming from Steve just a few moments ago. When I think of licensing and I think of trying to license calculations that go out to these very long time frames, I think of the poor scientist who is putting his

hand up on the witness stand during the very litigious public hearing process that is going to be involved in the licensing process and defending their calculations at that time.

I don't see that as a winning strategy, or at least a strategy that has any real merit. We all understand these are projections. At least I understand they are projections. They are not predictions.

Therefore, we propose that from a licensing standpoint you go with the two time frame approach, that is, for some shorter time period where you have a lot of confidence, you feel you can make it through. You are still looking at the same basic processes that occur out of these longer time frames but for a shorter time frame when you really have a lot of confidence. That is your licensing basis period.

Then we recommend for the longer time frames you do these calculations to do projections as to what your behavior is so you have an understanding as to what things are important out there at the long time frame so you can prioritize and manage your resources properly.

REITER: So specifically, the short-term criterion was a criterion of acceptability and the long-term one was more as a guide for priority assessment?

KESSLER: And regulatory insight. We still think that

it has a role in providing information to the regulator and some level of confidence or robustness in the regulatory decision for the licensing basis period, which is the shorter period.

REITER: How do you counteract the idea of the National Academy which says we really should regulate or concentrate on the time of maximum dose?

KESSLER: Based on what I heard Bob Fri say this morning, I don't think I am particularly counteracting that.

Maybe Bob or Chris, you can comment.

I think what I heard was they are recommending that you do calculations out there, that you have some sort of risk criterion out there. They may say it's a smaller uncertainty, but there is still that uncertainty out there.

It's the difference between making calculations for the time period of maximum health risk and making licensing basis calculations where you have this process and all your scientists have to defend everything. That's where I've got some concern.

CANTLON: Dr. Fri.

FRI: If I understand our report --

[Laughter.]

FRI: I should make two comments. The longer time period is the licensing calculation. That's the basis on which you license, not the shorter term basis. In fact the

report considers the recommendation that it be a shorter term standard and says we don't think that is particularly significant in terms of protecting public health, but if it makes you feel good -- I don't mean that as a flip comment -- the regulator could go ahead and do it.

To go back to Dr. Cohon's point, I don't want anybody to be confused that what is shown on this chart or the set of assumptions on the previous chart is the same thing as the Academy report recommended, because it's an entirely different approach to calculating exposure.

REITER: It is the same or isn't the same?

FRI: It is not the same. For example, one of the factors that is considered on the previous slide has to do with the detection or remediation of dose. That is a consideration that in the Academy report we explicitly say you shouldn't consider.

KESSLER: We could certainly agree to disagree on some of the factors that we used in our illustrative calculation. I don't say this is any more than illustrative. We threw out some examples of factors that could be considered in probability. If some of those factors are generally considered to be inappropriate, then let's take those out. But I still think there is a lot of reasonable basis for some of the factors that were in the calculations we made.

CANTLON: I think we ought to go to keep on schedule.

We have a discussion period at the end. So we are probably not through with these issues.

The next talk will deal with the waste isolation strategy update. Both Steve Brocoum and Jean Younker will be participating.

[Slide.]

BROCOUM: We will be talking about the waste containment and isolation strategy. We starting thinking about the waste isolation and containment strategy in preparing for the program plan based on recommendations we had gotten several times from the Board and other groups that we needed to think about it.

[Slide.]

BROCOUM: Last January in Beatty we had the four-part, almost panel presentation to the Board. We also made presentations to the NRC and to the ACNW covering the key elements and linking it to the testing plan.

The feedback from the NRC and others said we needed to have a written document.

[Slide.]

BROCOUM: We wrote a letter of direction to the M&O on May 15. We directed the M&O to provide more detail in a white paper on the elements of the strategy and our current understanding and to identify the information needed to evaluate the hypotheses in the strategy, which is described

as a series of five hypotheses.

[Slide.]

BROCOUM: The program right now, of course, is in a difficult period of management challenge. It's a time of change. Standards most likely will be changing. The whole regulatory framework may change over the next several years. We are having reductions in budgets, forcing us to focus our activities.

Many of the changes are attended by controversy and provide new challenges and perhaps opportunity as these new approaches are proposed and debated.

[Slide.]

BROCOUM: The M&O delivered its draft strategy to DOE on October 10, a few days ago. We have started a formal review to determine if it's acceptable. We are expected to complete the review by December 1995.

After we complete the review and assuming the M&O can accommodate the comments we make, we would probably put a DOE cover on that strategy and release it as a DOE document and use it to help guide our program.

On the table outside we have put out the draft document from the M&O.

At this point, Jean is kind of going to go through the strategy for you over the next half hour or so.

YOUNKER: Thank you for the introduction, Steve. It is

always pleasant to be here speaking with you.

This is a difficult topic and there has been a lot of controversy. I want to make sure that I pay credit to a number of individuals who have helped with this. Some of them are in the room, and if there are questions and I need to call on them, I would like to do that. It has been a joint effort from a number of people and gone through reviews within the M&O before it went to the DOE. So we have had a lot of thought and debate behind this.

[Slide.]

YOUNKER: You will notice some things that are quite similar to the preliminary strategy we presented in January.

[Slide.]

YOUNKER: I want to put just a little background and perspective on where we are.

[Slide.]

YOUNKER: Some of this is now starting to sound like a broken record.

Certainly I think times are changing, they have changed, and they are going to change some more, very clearly from some of the comments you heard from Dr. Dreyfus this morning as well as the Senate and House staffers that joined us.

You have heard just now from John Kessler that long-term effects are more important perhaps if you look at

the kind of standard we may be moving toward, depending, of course, on exactly what EPA does with recommendations from the Academy. But it certainly appears, as we have just talked about, that it may diminish our ability to rely on delay in transport time in the way we thought about it back when we conceived of the site characterization plan, for example.

Looking at doses drives us to need more information on the saturated transport system. Although we knew we needed to understand it for other reasons, perhaps for calculations for an environmental impact statement type of analysis, I think where we see ourselves heading now is clearly going to require a better understanding and better credibility of our ability to get at dilution factors that you can rely on in the saturated zone.

As you heard from Dr. Dreyfus this morning, we spent an awful lot of time a couple years ago putting together that program approach that you have had briefed as it evolved. A lot of the thinking went into that.

What we have been forced to do now, of course, is look at the new climate that we seem to be heading into and the changes involved, and how do we take that program approach and focus it even more.

I have always viewed what we did then as focusing in from where we had started on the site characterization

plan. Now we are going through yet another change and another focusing to really put ourselves in a position, I believe, where what I am going to present to you today is probably unlike any presentation that I've made, and that is that we are really directing ourselves toward what is the best case we make for the site, what is kind of the minimum amount of work we believe may be able to be accomplished in the near term in order to test the hypotheses that are the guts of that argument. So I think what you will hear today is kind of a different tone and a different offering than what you have had from us before.

As Steve already said, they did direct us to put together the elements of a strategy, do the best job we could based on current understanding, and then to define the hypotheses to be evaluated as a way to really focus in on what work has to be done.

[Slide.]

YOUNKER: The strategy does focus in on two major objectives.

Limit the annual dose to members of the general public. We talk about how the amount of water contacting the waste in the emplacement drift is one of the most essential parameters that we have to get a handle on. Then the containment time that you get given that water in the flux, the mobilization rates that you get within that

environment, and the effectiveness of the components of the engineered barriers together with the dilution factors will be tested.

We then have the second objective, which is containment of the waste for thousands of years during that period when you have the high inventory/high temperature.

Several comments about this have already been noted. I think Chris Whipple said we hoped that our recommendations wouldn't lead to a situation where you might move away from this kind of an objective for a geologic repository. I think the authors of the strategy and many of the commenters have debated this a lot. I think pretty much all of us believe this is still something that is wise for this country to do from the standpoint of a geologic repository needing to provide a high confidence of safety in that shorter or nearer term period.

[Slide.]

YOUNKER: We describe how the dry conditions in the repository and the expected low container corrosion rates that you get in those dry conditions can be tested in a focused manner.

[Slide.]

YOUNKER: As we were putting this strategy document together and going through some of the early briefings we found some feedback from Dr. Dreyfus. He said, you know,

one of the things we really need to be able to tell people is what is the problem that we are dealing with here.

So one of the things that the team that put this together came up with was, look at the total radionuclide burden. There are different ways you could do this and this is just one way to characterize it. Kind of get a sense of what the total potential dose inventory is in rem through the time frame that we conceivably can do our modeling and do our calculations.

You can notice that when we get out into that long time frame, as I think Don Kessler already showed in one of his charts, the neptunium-237 is certainly your major isotope giving you your potential for high doses out in the plus-100,000 year time frame.

You can also see another reason why we think this many thousand year objective for as complete containment as you can get makes sense, because you do get rid of some of those high heat producers and the high inventory radionuclides in that first couple of thousand years.

Clearly the message here is that a geologic repository to be successful and acceptable to a regulator, and I think to this country, has to be able to deal with this in a comprehensive manner and reduce that inventory to safe levels through controlled release.

[Slide.]

YOUNKER: What does the picture look like? If we look at the kinds of releases, the predictions we have from our TSPA-95 calculations that you will hear about all day tomorrow, I think you can see that it looked pretty good from the standpoint of that inventory that I just showed you, being up at the ten to the 16th, over ten to the 16th potential rem per year, and here we are talking on the order of ten to the minus one rem per year for the neptunium peak coming out in the couple hundred thousand year time frame.

Tomorrow the presenters will go through this in a lot of detail. The message here for you is just that it looks reasonably good in terms of the total isolation characteristics at the site in the way we have represented them in the TSPA-95.

[Slide.]

YOUNKER: If you take that and previous performance assessment results, and in fact take what John Kessler presented just previously, and you put it together in a chart, you start to see a pattern that we thought would be helpful for people to think about. John already talked about this period and this period. We just put it in a picture form for you to visualize it.

You begin to see that there are key attributes of the system. Sometimes they are kind of processes; sometimes they are elements of the system that lead to the same basic

pattern of performance in an awful lot of predictions and calculations that have been done, and that is now looking at the dose rate through time. During that early containment in our environment with the very low humidity leading to low corrosion rate, and, of course, the question of if you had early release, then the travel times would be important. The strategy aims you toward as complete containment during a many thousand year time frame as what is feasible or reasonable.

The transition period you see for the calculations is, of course, controlled by rates of mobilization of radionuclides and then transport properties of those radionuclides.

The shape of this can change, but I think really what is controlling the peak dose period we have tried to list up here. Seepage rate or total influx into the system, of course, is important all the way across. We did just show that here. But the mobilization rates, the EBS transport rate, we thought a little bit more about it. You will see some sensitivities in the TSPA-95 looking at what various forms of engineered backfills perhaps with capillary barrier type of functions could do for you in terms of reducing the peak doses.

And then dilution, which is, of course, the big player.

DOMENICO: Excuse me. What is the length of the containment period you are taking?

YOUNKER: The regulatory length is 300 to 1,000 years as specified in Part 60. What we say in the strategy is that we think in this environment you have a good chance of having pretty much complete containment for several thousand years.

DOMENICO: What is several?

YOUNKER: I would love to see us be able to be confident about more than 5,000.

DOMENICO: Then what is the length of the transition period? Does that take us to a million?

YOUNKER: It depends. This is just a schematic, Pat. For some radionuclides, for technetium, for example --

DOMENICO: The slide before was not a schematic.

YOUNKER: No, it was not. It's going to vary depending on the radionuclide. It depends on the half-life of the radionuclide. If you look at the previous one, technetium, you usually see it at around 100,000 years.

[Slide.]

YOUNKER: Given that we have this kind of background and framework, then how do we cast this strategy in a set of testable hypotheses?

These will sound pretty similar to the ones we presented to you almost a year ago.

Seepage. The amount of flux contacting the waste in this environment will be low. This is testable, we think, in a reasonable amount of time. There are good observations already about that that you heard about this morning in the ESF.

We think that those dry conditions will lead to containment for thousands of years. How many thousands remains to be observed and tested.

The mobilization rates in that environment will be low.

We think some additional engineered barriers will limit the rate of release to a low value.

For those radionuclides that are released the concentrations will be strongly diluted during transport in the natural barriers.

[Slide.]

YOUNKER: You have to look at the cross-cutting issues as well.

Of course the impact of climate change on the hydrology is covered as you test hypotheses or as you think them through and lay out the testing. In this case, clearly what you are concerned about is what is the impact on the hydrology, how much can this change the amount of influx into my waste package environment and the amount of transport potentially out.

Effects of heat similarly are addressed by looking across the system to set up hypotheses that kind of represent the isolation system and asking the question, what are the thermal effects on the way we are expecting the various processes to operate.

Then the potential effects of disruptive processes and events also have to be looked at, and they are in the strategy document, as you will see: tectonics and seismicity, volcanism, and human interference. These are kind of looked at as kind of failure scenarios, if you will, or how processes and events can make this total system not work as we have posed that it will.

[Slide.]

YOUNKER: For the rest of the talk the format will be I'll give you kind of the basis for the hypotheses along with a schematic to get you thinking about the sort of information we have used to formulate the hypotheses, and then we believe a focused set of observations and tests for modeling that will lead you to a good rational test of that hypothesis.

[Slide.]

YOUNKER: For this purpose, I am going to see if I can do a dual machine show here.

[Slide.]

YOUNKER: There is a picture for you to think about in

terms of what current information tells us about the amount of water coming into the waste package environment.

Clearly, seepage rates or influx affects everything: containment, mobilization, transport, and the degree of dilution because just volumetrically you get an effect.

We had a model. This one is very, very similar to the one that we actually used as a basis for the environmental assessment back in the mid-1980s, and I think this one is pretty close to the one in the site characterization plan. This basic conceptual model of the way the unsaturated system works with potential for lateral diversion, potential for some perching at impermeable zones, and perhaps along fault contacts is the basic model that we still have today.

On the smaller scale conceptual model in TSPA-93, you all probably remember the WEEPS model that we used to get at the heterogeneity or potential for fast paths in the unsaturated zone fault system. We have the conceptual and the mathematical representations to help us understand and represent the flow into the drifts.

The average flux at the repository horizon, given everything that we know, is likely to be low, less than a millimeter per year. I think Dr. Langmuir mentioned the observations of the higher infiltration rates. Those are,

as far as I know, relatively shallow, ten meters or so depth so far. I'm not saying that we won't see higher infiltration rates in localized areas, but as far as I know, we don't have any impression yet that we would see those at repository depths.

We certainly do believe and have evidence from some of the isotope data that localization or this type of flow system could be at work in some places. We also found in our TSPA-93, to our surprise, that we in fact got lower releases under that type of characterization of the system. One reason was we had less contact time with the waste.

And, as we pointed out this morning, I think in response to a question, we really haven't seen any dripping. There have been some zones that were a little bit wetter and they were where we expected them based on that kind of conceptual model of the site. So we have some confidence that we are looking at this thing about right.

LANGMUIR: Has anybody collected any water from those areas where there was moisture and gotten any age dates on it?

YOUNKER: I'm quite certain that they are doing that. I haven't heard any dates back yet from anyone. Maybe Bill Boyle can comment.

BOYLE: My answer is essentially the same as yours. I show where we took over, I would say by now, a thousand

specimens in there, and I am sure for some of those people are looking at those sorts of things, but I haven't seen any results yet.

[Slide.]

YOUNKER: What do we need to do to test this hypothesis as we posed it?

Getting the existing borehole data synthesized. You heard Bill comment that is one of the major focuses of the FY-96 program on the site side of the house, and that is to take what we have, put it together and see what kind of an information base we really have.

I can give you one piece of feedback on that. In the climate program we have a preliminary report coming in. The USGS authors who had worked on this were pleasantly surprised to find the variety of information that was there that just had not been pulled together. That gave them some insights as they began to put this report together.

I guess I have an impression that in a lot of the areas, as people begin to take the information from similar areas and pull it together, we are probably going to find out that we do have some increase in understanding in this next year as these synthesis reports are written.

We also have to make the observations in the ESF. As it stands now, we are going to get an opportunity to do that at least as far south as that second Ghost Dance access

and be able to go over in the alcove to the Ghost Dance and get some observations there to see whether we see any evidence of current or ancient flow systems through the Ghost Dance.

Moisture content of the near-field rock and humidity in the drift and in the host rock are important pieces of information to help us get at that whole question of what kind of humidity, moisture conditions will exist in the near-field environment.

The modeling needs to focus clearly on both the large-scale and small-scale. Some of this is already underway. We think some of it can be enhanced fairly efficiently.

Effects of heterogeneity. I think you had a presentation not long ago on what kinds of difficulties you can run into and how important it is to consider the effects of heterogeneities in the rock material and the hydrologic properties when you do the small-scale modeling.

Climate effects and thermal effects, of course, are going to be important as we look at these cross-cutting type of issues.

Again, a way to look at this is to do your modeling to determine the conditions under which the influx or the amount of moisture contacting the waste package would be too high such that it would give you a problem with

containment, mobilization of radionuclides, transport through the EBS, or with dilution, trying to get at what are the critical parameters, what are the critical amounts that would give us a problem with the hypothesis as we put it together.

[Slide.]

YOUNKER: Stepping to the second hypothesis, this one is our containment one.

I have two schematics for you for this one before we go to the word slides.

[Slide.]

YOUNKER: I think most people who have looked at corrosion know that you tend to see this kind of a change in corrosion rates under relative humidity conditions around 50 or 60 percent generally observed in metals. I think this one is for steel.

[Slide.]

YOUNKER: Another chart here just shows you the relative humidities with different salt solutions present, when you see no corrosion versus when you see some attack of the steel. Just to give you an impression of the kind of database that is out there that we are drawing our conclusions from. Not that this is specific for Yucca Mountain environment, but that the observations are out there and that we need this kind of information so that we

can make the case that the kind of material we put in this environment will behave like that.

LANGMUIR: Jean, in passing, there has been, at least starting last year, a rather substantial Livermore effort in corrosion.

YOUNKER: Correct.

LANGMUIR: How is that relevant to this, and are they learning anything that is relevant to what you are concerned with now?

YOUNKER: Absolutely.

LANGMUIR: Any disagreements, any additions, or is it pretty much the same thing? They are looking at lots of different metals.

YOUNKER: The people that are working on this have reviewed this; they have contributed to it; they have no concern that we are heading in the wrong direction with the strategy.

LANGMUIR: Will their funding be continuing?

YOUNKER: Let's get right down to the point.

[Laughter.]

LANGMUIR: Yes.

YOUNKER: I certainly don't know the answer to that except to tell you that I believe that our goal in terms of FY-96 planning is to make sure that the waste package and the corrosion work is prioritized. That's the goal that

I've seen being played out in all of our FY-96 planning. If you want a better answer, I'll defer to DOE on that.

In terms of what kind of relative humidity we might expect in the near-field environment at Yucca Mountain, this one now takes you one step closer to Yucca Mountain, goes over to Livermore to pick up some of Tom Buscheck's work, looks at the calculated temperatures that he sees and the relative humidities.

If you look at something like 4,000 years, you can see that for something in the 60 to 80 MTU per acre you are in that 50 percent humidity environment. So this gives me my several thousands of years that I am hoping for if around a 50 percent relative humidity is going to work out in this environment.

CANTLON: Jean, before you take that off, when you were looking at that feature did the whole question of refluxing come into the calculation, the groundwater table refluxing up?

YOUNKER: Not so much refluxing up from the groundwater table but certainly the question of what will the relative humidity be in the drift through the period of time when you will start to see some water returning or water moving around.

LANGMUIR: This does not assume backfill? This is airspace around the waste?

YOUNKER: I suspect this one does not assume backfill. I'm looking out in the audience to have somebody give me a nod. No backfill in that calculation, I don't believe.

In the presentations that the performance assessment team will be giving tomorrow you will see that some different values for relative humidities were used in the calculations there. We have had TSPA-95 operating in parallel with the development of a strategy and we have used some different analytical bases in some cases.

The difference doesn't bother me, but I think it's important because it points out that a difference in the modeling assumptions gives us a very different predicted relative humidity. So it looks like, okay, here's a really key piece of information that I need to try to get a handle on in terms of how sensitive my relative humidity predictions are to the way I represent in my modeling.

[Slide.]

YOUNKER: The word slide now is over here and the work needed to test the hypothesis we will put up here.

[Slide.]

YOUNKER: Limited corrosion at low humidity.

We believe modeling indicates our humidity may be low for thousands of years. How many thousands of years will have to be further evaluated as we use these models and get some of the differences understood better.

The low humidity conditions may be enhanced by backfill. I will talk about that more two or three viewgraphs later. So if you will hold the thought on backfill for a minute, we are going to spend some time on it.

One of the things you will see tomorrow is that in our interface between the waste package, corrosion people, people who have been working the waste package design and our performance assessment, it turns out that they were considering and were very much aware of cathodic protection type of process that happens when you put two different metals together, like the corrosion-resistant inner barrier/ corrosion-allowance outer barrier.

Apparently by the way we have captured that in the TSPA, you will see some results that show it could conceivably give you some very large effects, very much lengthening the lifetime of the waste package or the time to the first failure. If it turns out to be valid for this environment, it can be a major addition to our waste package life.

LANGMUIR: We had a consultant at the previous Board meeting who was an expert in cathodic protection issues. He was concerned that your three-layer design was not going to work and that in fact it would accelerate, because of different effects which I don't understand fully, the

corrosion rates and may reduce in fact the lifetimes, with your proposed designs.

YOUNKER: Hopefully we had some people here. Was this one when you had the engineered barrier system people here? I would think they would probably have that feedback. I hope. We can make a note to make sure that they do.

[Slide.]

YOUNKER: In terms of the work needed to test the hypothesis for containment under low relative humidities, we can get a better representation of the environment; we can observe the amounts and chemistry of the water in the ESF; measure the possible effect of backfills on humidity.

My punch line coming up is going to be that we think by putting the right kind of backfill in there that that will help you to control that relative humidity because it will keep you hotter longer.

I know that story may sound like a change, but it really looks like there is some real potential for keeping your relative humidity lower using the backfill as kind of an increased insulator. Keeping the temperatures higher longer could give you a real advantage.

And then, of course, some thermo-hydrologic testing and modeling.

Get at corrosion rates and mechanisms under low humidities.

And then get at this cathodic protection.

[Slide.]

YOUNKER: Moving to the next hypothesis, which is waste mobilization.

[Slide.]

YOUNKER: A picture for you, which is just a schematic showing the types of alteration that is observed on spent fuel. This is just an SEM photograph and something suggesting a change in the dissolution rate that you get based on whether you have straight uranium oxide or the uranium U-307.

Over here the dissolution rates that we think are reasonable under saturated conditions, something like ten to the minus four per year, ten to the minus six for unsaturated conditions, just to give you kind of a sense of where we are.

Elemental solubilities give you even lower mobilization rates. In the report you will see there are some references for where this information is being obtained.

We know given current information that there are some questions about neptunium solubility, questions about the effect of waste form alteration. If we can keep containment complete for several thousand years, keep this waste form alteration to a minimum, then we have the

potential for reducing the solubilities, keeping the spent fuel in a less soluble state. That could be very useful.

Then, of course, questions about the role of colloids.

CANTLON: Before you leave that, Jean, was there any look at fillers as a way of slowing up mobilization? Was there any of that in your models?

YOUNKER: Within the waste package itself?

CANTLON: Right.

YOUNKER: We haven't looked at anything like that.

[Slide.]

YOUNKER: In terms of testing the waste mobilization hypothesis, some refinement of the neptunium solubility data may help because there is some suggestion at least that the values we are using are too high for the unsaturated type conditions at Yucca Mountain.

The effect of radiation and chemistry on waste form dissolution as well as the effect of this containment for several thousand years on waste form alteration. If we can prevent that oxidation of the uranium oxide, we will have a more stable waste form.

And then stability of colloids.

[Slide.]

YOUNKER: As promised, let's talk about engineered barriers.

Our schematic is just for backfill, but bear in mind that we are always conservative in this area because we don't talk about the waste package components that will still be there. We know that the waste package won't magically disappear; there will be pieces of it there. Under unsaturated conditions you know that the way in which the waste would become mobile is going to be very slow, and certainly some of these same effects will work in the components of the waste package. And also in the invert below the waste package, if the invert is still there. Or whatever is there.

[Slide.]

YOUNKER: This one is just to get you thinking about some of the results that Conca has shown, and that is you get this thin film under the unsaturated conditions; you get the thin film type of diffusion. If you look at the diffusion coefficients for backfills, at something like 30 percent or 40 percent the rate comes down exponentially, so you know that under those kinds of unsaturated conditions diffusion rates are going to be very, very slow.

[Slide.]

YOUNKER: Very slow transport through the waste package, as I indicated, because of the low water content. So it isn't just the backfill like the picture.

And these films will very likely be discontinuous.

If they are discontinuous, you essentially get no transport in that situation.

Backfill -- this is just a crushed tuff or pieces like a tuff gravel type of backfill -- could further limit transport.

One of the effects that we have thought about a lot since the last time we talked with you is that evaporation effect may limit the amount of water contacting the waste. So it is keeping the water off the waste. If there was any effective flow at all, the presence of the backfill could be very helpful.

In addition to this film effect that has been shown in Conca's work, we may not even get them under repository conditions. Depending on the relative humidity, at least for several thousand years you may not.

Then, if transport does occur, you may get evaporation and trap the radionuclides in pores of the backfill.

So there are several different aspects of backfill that as you start to look at it further begin to look very promising.

[Slide.]

YOUNKER: What do you need to do to test it?

We don't think it's an exhaustive effort. You can get at some transport characteristics of the waste package,

some focused experiments and modeling; determine flow and evaporation characteristics of the backfill. Some simple designs. We are not looking for a fancy capillary barrier. Nothing that would have to be engineered and maintained for the kinds of time frames we are talking about, but something simpler like a crushed tuff or a tuff gravel.

And then look at transport properties of that material.

[Slide.]

YOUNKER: This is the time when I bet Pat Domenico is going to give me trouble. I just have this feeling.

Just as an example, Pat, we wanted to show a picture of a plume to get people thinking about dilution. Given some of the things that Pat has already said, I figure I'm in trouble on this one.

There are a lot of examples in the literature. However, you don't get a lot of good pictures. When the team was out looking for these pictures, we figured it was because people don't like to show these pictures, the migrating contaminant plume.

This is one of a radium-226 concentration in a uranium mill site up in Wyoming. The only reason why we wanted to show you this was to get you thinking about the kinds of dilution that has been observed. In this case over just a 200 meter distance, with the source being up here,

you see basically three orders of magnitude drop in concentration over a couple hundred meters.

It is different lithologies and there are lots of reasons why it may work exactly like that in the flow paths at Yucca Mountain in the saturated zone, but this one is just for comparison. It's layers of alluvium conglomerates and then some sandstone. If you are talking on the order of several orders of magnitude in that kind of an environment, there is no reason to expect that we won't get something similar to that at Yucca Mountain.

[Slide.]

YOUNKER: We know that you do get dispersion of concentrations in heterogeneous systems, and we certainly know we have heterogeneous transport flow systems in the unsaturated and saturated zone.

Textbook solutions will give you large dilution factors in the kinds of environments that we are going to be modeling.

You get some mixing during withdrawal as well, and there are debates about how large that may be.

Then we certainly know from current information that we have uncertainties in our transport models at the site and in how to scale the test results.

[Slide.]

YOUNKER: Work needed to test the hypothesis.

Get some information on dispersiveness of the local flow system; continue the modeling of the saturated zone flow system. There is a deliverable this year coming in that should probably get us a large percentage of the way there for this one.

Then get at the scaling effect by analyses using different transport models, because you do get very different results, depending on how you represent the transport.

[Slide.]

YOUNKER: This looks like a long list, but it really isn't. If we can get some bounds on the amount of water contacting the waste coming into the emplacement drifts, we can bound the processes that produce the low humidity at the waste package.

If we can get a handle on that, that will then get us those bounds on waste package breach rates, how many thousands of years we may expect the waste packages to provide complete containment in this environment.

That then allows us to get the waste mobilization rates given that environment.

And then bounds on flow and transport properties of the EBS.

Add to that the bounds on the dilution factors

[Slide.]

YOUNKER: We think that the strategy is based on the work conducted to date. We have taken in a lot of input during the review process and I think we have represented the best approach we can for Yucca Mountain.

We have identified the critical issues. We think we have a good handle on how to resolve them.

As I said earlier, this does call for a significant change in emphasis. This is not the old site characterization program and it is not even the program plan's program anymore. It gives us a basis, though, for defining what needs to be done.

I think the authors of the strategy believe that if you could focus on those key uncertainties or key issues that we could at a reasonable cost to support near-term milestones make some real progress toward determining whether this strategy in fact is a viable strategy for Yucca Mountain.

CANTLON: Thank you, Jean.

Don.

LANGMUIR: I'm not going to make friends of the geochemists in the audience. Looking at hypothesis 3, you have listed work needed to test hypothesis. I thought we had killed colloids a year ago as an issue. My sense is with a diffusion barrier they are not going to get through it; they're not an issue.

The neptunium solubility data, once you get away from the fuel itself you're never going to be at saturation with anything made out of neptunium. So you are really only concerned about the release rate.

YOUNKER: I think in both cases there were comments on the initial versions of the strategy that led us to believe there may be something you need to do. I don't know the details, but in one case it was some modeling that Tom Woolery has done that questions whether those neptunium solubility data, the ones all of our work are based on, may be too high.

LANGMUIR: He argued that you had redox control on releases, and he was concerned about the solubility of neptunium as a reduced species, which it is not going to be. So I think it was an inappropriate concern.

YOUNKER: So you are saying there is probably much you can do with it.

LANGMUIR: It's going to be oxidized over the long term.

YOUNKER: The colloid issue. We just talked about this. I think there is a question of whether the colloids are stable in the environment, as you point out, but also once the material has been attached to the colloid and moved and it is somewhere else, then what happens to it?

LANGMUIR: It has got to get out of the backfill. We

can argue about this. It gets quite detailed. But you are going to redistribute anything absorbed on a colloid on the country rock, which will dilute it and eliminate it. So I don't see it going anywhere.

YOUNKER: That would be very helpful to our strategy.

[Laughter.]

LANGMUIR: I've been saying this quite a long while.

DOMENICO: Jean, with regard to backfill three things come to mind. The first is retrievability, which now may be difficult.

Second is thermal testing if you are going to incorporate backfill. When you do the thermal testing, you will have to incorporate backfill and that will not be an easy task getting measurements there.

Third, if you are going to trap radionuclides in the pores, the issues of criticality raise their head once more.

YOUNKER: The first one was retrievability. The backfill would not be put in until you were ready to permanently close the facility. I'm saying this is kind of the way we have been thinking about it. You wouldn't put it in until after you were past the point of where you thought you were going to have to retrieve. Not that future generations might not decide to retrieve and have to do something about what we gave them, but that's the concept

right now, I think.

The second one was thermal?

DOMENICO: Thermal testing.

YOUNKER: We would have to test with backfill present to see the effect of it?

DOMENICO: You are going to do some thermal tests to see the effects of the heat on the mountain, I presume.

YOUNKER: Yes.

DOMENICO: If you are going to have backfill, it's going to have a different effect.

YOUNKER: Yes. So we would want to do some tests with the backfill.

DOMENICO: Which would not be very easy.

YOUNKER: Yes. We suspect that you could do some of those even in scale, do laboratory scale tests. I think that is one of the areas that is going to take some real attention.

DOMENICO: The third one is the trapping of radionuclides in the pores.

YOUNKER: Another one that will need some experiments.

The last one was criticality?

DOMENICO: No. That's it. That's the last one. That's three.

CANTLON: Ed.

CORDING: Jean, you are talking about the backfill

operating. What time periods is that supposed to be working? Just order of magnitude.

YOUNKER: We would like to think that the right backfill put in would just kind of continue to be a part of the system throughout the entire release period.

CORDING: You are really looking at this to do something for you to the very long-lived radionuclides. With respect to characteristics of the medium as well as characteristics of the backfill, certainly you can engineer the backfill and put what you want there to some extent, but I see some of the same sorts of problems with backfill as we would have with the medium: things getting short-circuited, collection of deposition materials that would ultimately change permeability, flow paths, local concentrations of flow.

I certainly would agree with the statement that you expect that materials can be provided with transport properties that are at least as good as those of the host rock. Of course, we have uncertainties because we don't engineer it, but if we can't count on the host rock, I am wondering how well we can engineer our backfill for hundreds of thousands of years. There is deterioration of the backfill as well under those environmental conditions: slaking, swelling, those sorts of features, things that change it.

I am not quite sure I understand how one expects that to work in that time frame. You are talking about the barrier below perhaps delaying it, but ultimately it all comes out. If it comes out at 200,000 or 400,000 or 600,000 years, so what?

YOUNKER: I think the strategy really looks to the backfill for the largest help in keeping the temperatures high and keeping the relative humidity low as long as you can to give you that high confidence of safety over the short term. When I answered your question the fact that it is still going to be there, I think it will have some effect over the longer term.

CORDING: We are talking more about the period in that first 10,000 years or perhaps somewhere into the transition period. Are we basically saying that no matter what happens with the neptunium, once it gets out, it's gone, there is nothing one can do about it? Or do you really think the backfill could do something with that long lag?

YOUNKER: I think bringing the concentrations down through dilution for neptunium looks like it's the major effect.

CORDING: In the saturated zone.

YOUNKER: And some during unsaturated zone transport.

LANGMUIR: You've forgotten your own arguments that it's a diffusion barrier.

YOUNKER: Although it certainly will add something on the diffusive side, I think the big point is keeping the relative humidity lower and helping to avoid drips, potentially catching drips if they do get on the package.

LANGMUIR: If there are fracture zones where you could get quick releases, it limits that to some extent and buffers the process to minimize access to fractures.

YOUNKER: I probably kind of led you in the wrong direction by saying over the long term, because I was thinking it will still be there. If it's there, it can only help, I think.

CORDING: I have a feeling that if you have a drip or local concentration flow that is hitting the waste package and it is carrying material out, as it gets into the backfill it is going to also be changing humidity there and affecting all that. You are getting into a situation that maybe you can't control as well as you would like.

CANTLON: Jean, has any thought been given to the interplay of the backfill placement and the pitting of the containers accelerating the corrosion problem? When you put that backfill in, unless somebody has got a magic way of doing it, you are going to get a lot of pitting on the surface of the containers.

YOUNKER: We certainly had comments from the people on the corrosion side of the house on the first couple versions

of the strategy.

Larry.

RICKERTSEN: The view we had in doing this is that it looks like there are several properties of the backfill that are very promising. It also looks like they are easily testable. We think we can test the heat transfer properties, the flow properties, the evaporation characteristics, the diffusion characteristics, in the laboratory at least, and do that fairly quickly. The work by Conca suggests that his experiments run fairly quickly, and that we can do those and that they would be beneficial.

Our view is the backfill may help us. It's meant to enhance performance. If we find in those tests it doesn't work, we're certainly not going to use it. We want to see if it performs as well as the promise shows. The idea of the strategy is to map out what we are going to do, and this looks like a very profitable direction for us to go.

CANTLON: Yes, but has anybody looked at the pitting?

RICKERTSEN: Similarly, we will look at that. That will be one of the things listed explicitly in the report. There are other things also that we are concerned about. We will look at all of those. The evaluation of backfill will be a comprehensive evaluation of all of these effects, including the feasibility of putting it in.

CANTLON: Victor.

PALCIAUSKAS: The waste isolation strategy really gives you a nice way, as you stated, of looking at the various contributions of various parts of the program. If you have the problem of a ten to the 17 rem in the initial graph, time buys you about three orders of magnitude. Then you have at least about ten to the sixth of the waste mobilization rate. My question is on the tail end, because then it becomes a little bit more uncertain because you have dispersion, dilution, actual mixing, and then you have the actual collection.

There is a question there.

In your performance assessment, what kind of values are you getting on the tail end of those calculations?

YOUNKER: In terms of dilution?

PALCIAUSKAS: In terms of all three processes, unless you can separate them out even more. This would really help to gauge where the problem is.

YOUNKER: You are saying what does the total --

PALCIAUSKAS: In other words, if I were releasing a certain rate from the EBS, and obviously in your performance assessment somewhere you come out and you get a final dose, what are those values that you are getting? Perhaps I should ask Bob Andrews.

YOUNKER: Yes. Somebody will put that in perspective tomorrow.

PALCIAUSKAS: I was wondering if I could have it right now.

YOUNKER: Dr. Andrews.

ANDREWS: If I understand your question correctly, there is a lot of dispersive kind of effects that happen in the natural system and in the engineered system. All of those dispersive effects, whether it be in the EBS itself --

PALCIAUSKAS: No. After the EBS. We are looking at the geologic system now.

ANDREWS: There are still large dispersive effects in the unsaturated zone. There are variabilities of advective transport velocities which have a dispersive kind of effect on releases to the saturated zone. Within the saturated zone there are also dispersive effects, and the dispersive mixing kinds of effects of different groundwaters even as you go further down gradient from either the fence, as John Kessler had, or the five kilometer boundary, which is the old EPA boundary, and you continue down gradient you have additional dispersive mixing effects.

In the results you will see tomorrow we don't break out the contributions of each of those dispersive effects, if you will, on the total reduction in the peak. We could do that, but we just don't do that. You end up

seeing a cumulative effect of all of those dispersive phenomena, both the unsaturated zone and the saturated zone, and you can't break out which one is dominating.

PALCIAUSKAS: At least maybe I can get the total sum of those. Is it ten to the minus eight, ten to the minus nine, ten to the minus seven?

ANDREWS: I would rather have some charts in front of me and probably wait until tomorrow.

YOUNKER: For TSPA-93, I think we pulled the number out, and we thought it was on the order of five orders of magnitude. That was for TSPA-93, though. A slightly different modeling approach was used.

CANTLON: Jean, I take you back to the question on refluxing. Have you given up that under the high thermal load you are going to get refluxing?

YOUNKER: I don't think so. The focus in this is on relative humidity and on maintaining that low relative humidity in the short term. How the refluxing will affect that is kind of part of the argument that we are putting together here, I believe. In terms of the upper limit on flux and whether you would get some additional flux through the refluxing, that is something we are going to have to get a better handle on through the thermo-hydrologic testing and modeling.

CANTLON: Other questions?

Leon.

REITER: Jean, I have two questions. It's obvious that a tremendous shift from the SCP has to do with how you look at the Calico Hills versus the saturated zone. One of the bills that is being considered is H.R. 1020. As you know, their standard is a 100 millirem dose over 10,000 years.

At the last Board meeting and subsequently we've heard the results of a systems analysis which indicates that if you have a 10,000 year dose the impact of the Calico Hills could be very large. In other words, in terms of reducing the dose.

If such a bill is passed, would that cause you then to say let's go take a look at the Calico Hills to see how it can contribute to reducing that dose by retardation and delay?

YOUNKER: If DOE adopts this strategy and goes forward, I don't think so. That's just my own opinion, and I haven't thought about this a lot. It seems to me that if we believe that the low relative humidity controls the corrosion to the extent that we are heading in that direction, then for a 10,000 year type standard you really wouldn't. The Calico Hills would only be a backup for you in case you had early releases, in case a few canisters failed and you had potential for transport during that early time frame.

In a 10,000 year standard, my opinion is the site

will look quite good. I don't think you will have to do very much more for investigating the Calico Hills and relying on it very much.

REITER: It's just that we saw these very pessimistic models of the Calico Hills that resulted in 10,000 year doses that were extremely high.

YOUNKER: Those were sensitivity calculations specifically designed to question the value of the Calico Hills.

REITER: Right, and the impact of that was that it really doesn't matter for 10,000 year releases; it doesn't matter for million year doses; but it could be very important for 10,000 year doses.

The second question is, those nights when you wake up at 3:00 in the morning and say, oh my God, what can go wrong here, looking at the hypotheses or the things you are trying to prove, what are the top one or two things you think are really the most vulnerable in terms of proving the waste isolation strategy?

YOUNKER: He asked the question differently this time anyway, didn't he?

[Laughter.[

YOUNKER: He always asks this question.

REITER: Larry or Jan or Ed can answer that.

YOUNKER: In terms of the places where there is the

greatest risk would be a way of looking at it. I suppose the one that a lot of questions have been asked about, which is, can we really get a handle on a functional backfill, a backfill could be emplaced that would have the kind of characteristics that we are talking about and would give us that kind of performance. That is probably one that to me is important. Even if it turned out later that you don't need to rely on it, it would still be a very nice defense-in-depth. I would feel confident or would feel better if I had a backfill that I thought I could get some performance out of.

Being a geologist, Leon, you know I'm going to say my uncertainties are over in the engineering stuff.

[Laughter.]

YOUNKER: So it's going to be corrosion rates. I'm really worried about corrosion rates under low humidity.

REITER: You are not worried about dilution? You think that that is a robust estimate?

YOUNKER: I think we should be able to test it. We should be able to get some handle on it.

CANTLON: Don.

LANGMUIR: In your answer to Leon's question you raised another one, in my view. We've heard, but not recently, that corrosion waste package failure is going to be a statistical distribution, and you just said that if you have

early failures you may have to worry about this early time arrival stuff. You are going to have some failures right off the bat, right? There will be a certain percentage of canisters that will fail. How does that affect what you are going to do?

YOUNKER: I was saying it probably in a rather naive way. It was just the idea that if I did have early failures, then it would be nice to know I had some zeolites in the unsaturated zone that might help me, but I wasn't saying it from the standpoint of really thinking about what the failure distributions look like. I wasn't being that specific in my thinking.

CANTLON: Let's take our break. We will reconvene with a round-table and pursue these items.

[Recess.]

COHON: Good afternoon. My name is Jerry Cohon. I'm a member of the Nuclear Waste Technical Review Board and I am moderator of today's round-table discussion. I am in fact a rookie to this Board, as you know. This is my first meeting, but our manager, John Cantlon, believes in throwing rookies into game situations right away. So here I am.

I come from Cleveland. So baseball is very much on my mind right at the moment.

We have heard presentations today on a wide range of strategic issues, including pending action by the

Congress, changes in the criteria that govern the disposal of high level radioactive waste, and a proposed strategy for containing and isolating the waste in Yucca Mountain.

This really is a time of change potentially affecting virtually every element of the program, and not only the DOE, but all those involved or concerned with high level waste.

In light of this, we would like the round-table to address the following questions. I should warn the panelists these questions were put together by Leon Reiter who asks people questions like, which problems do you count when you are trying to fall asleep at night?

1. In this time of change, how firm a grasp do we have of the range of possible outcomes in key items such as funding and the applicable standards and regulations?

2. What outcomes in the legislative and regulatory arenas appear to be most likely?

3. Is it possible to define a waste site selection strategy and site characterization strategy when the regulatory criteria are in a state of flux?

4. How good is the proposed waste isolation strategy? Leon would like to know.

5. What effect does reducing funding have upon the NRC and State of Nevada programs?

6. How can the viability of the geologic disposal

program be maintained?

7. What are the resultant priorities that we should be pursuing?

In addition to some of the speakers who have already participated in today's meeting, we are joined by Margaret Federline, deputy director of water management at the NRC; Bob Loux, director of the Nuclear Waste Project Office of the State of Nevada; Larry Weinstock, director of the Radiation Protection Division at the Environmental Protection Agency; Bill Magavern, director of the Critical Mass Energy Project at Public Citizen; and Steve Kraft, director of high level waste at the Nuclear Energy Institute.

We have also asked some old hands in this business, Chris Whipple of ICF Kaiser, former chair of the board of Radioactive Waste Management of the National Research Council, and Bob Williams, formerly at EPRI, to provide us with their views.

As you can see, I am joined here by fellow Board members Don Langmuir and Pat Domenico.

We have allotted several minutes each to those participants who have not made presentations yet today to make a few short comments if they so desire. We have asked Margaret Federline to start off.

Margaret.

FEDERLINE: Thank you. We really appreciate the opportunity to be here and provide some of our perspectives. I wanted to briefly outline for you some of the changes that we are undergoing at NRC in the beginning.

First, I just wanted to clarify a statement this morning that was made by Troy Timmons. I think he said that some of the Commission's testimony had indicated that NRC had made a determination that Yucca Mountain was suitable. To clarify that, I think what our testimony actually said was that we have not identified any fatal flaws to this point in time, and if a complete license application is submitted, we believe that a reasonable assurance finding can be made; it's not impossible to license a repository site. Just for the record, I wanted to clarify that.

As you are probably aware, the Commission has supported an integrated solution to waste disposal and storage but with the strong emphasis that geologic disposal needs to be well funded and needs to be a significant aspect of this strategy. The Commission is already on record with indicating that they believe geologic disposal is still feasible and that it can be licensed.

Our preliminary staff views, after looking at H.R. 1020 and the National Academy of Sciences recommendations, are that both provide concepts under which a safe repository could be licensed. They are different approaches to public

protection. One emphasizes early protection with a high degree of certainty when things can be known to a high degree. The other is a more predictive standard. But we believe that either could be used and that it is a matter of public policy to select one approach or another.

We do intend to work closely with the Environmental Protection Agency. In fact we have already established a liaison relationship and we are going to have a task force that is going to support EPA in the development of its standard. So we are going to be working hard on developing a standard.

In answer to the one question of how viability of a program can best be maintained, we believe that improved integration and focus of the program in addressing key technical issues is the key to maintaining viability. We heard this morning from the congressional staff and I think we have heard in previous interactions with the congressional staff their perceived frustration on the lack of focus, not only at DOE, but also at NRC as well. So we have recognized this.

About six or eight months ago we made some changes in our program which we think will still be viable as DOE implements the budgetary limitations that both DOE and NRC have received. We are going to be focusing on what we believe the key issues are for repository performance, and

we are going to be interacting with DOE to the limited extent that we are allowed with the budgetary limitation we have on those key issues. We believe what Jean said in the waste isolation strategy is very pertinent, because we can best resolve issues if we tackle those issues that are most significant to repository performance.

Just a couple of comments. We are encouraged by DOE's improved focus in the program approach. Reluctantly, budget limitations won't permit them to continue this, but I think we saw progress with the ESF as was described this morning as well as progress in the surface-based programs. We hope that this will continue in a very focused way.

We endorse their continuing to develop the waste isolation strategy. We are going to be interacting with them on the waste isolation strategy. The main thrust of our new approach is to try and raise any concerns that NRC has early such that if data needs to be collected it can be collected in the most cost-effective time frame, not two or three years down the pike.

We are glad to see DOE incorporating the multi-barrier concept. Of course, on the tip of our tongue is how does NRC feel about the National Academy's recommendation on subsystems? I think we still feel that a multi-barrier concept and defense-in-depth provides a great enhancement of confidence for licensing a repository. I

think what the National Academy said on the quantitative subsystems measures we will have to take under consideration at the agency, because there are diverse views on that at this time.

We at the NRC are also suffering budget limitations. You are probably aware the House has looked at an $11 million budget; the Senate is looking at a $17 million budget. We are concerned about being able to retain technical expertise during this period of time not only at NRC but also at DOE. Particularly for us is the aspect of retaining our Center for Nuclear Waste Regulatory Analysis, which represents our only conflict-free assistance in the repository program.

Our interactions with DOE will be limited. We understand that. They have a very limited budget and they must focus on the scientific work. We feel that an enhanced role for our onsite representatives will allow us to access the information that we need, and we are also implementing videoconference facilities so that interactions with DOE and other parties can minimize impact on all three groups.

We are also improving the focus of our own performance assessment. We are going to be focusing on what we believe the key issues are for repository performance and doing total system analysis to confirm the significance of the key issues.

All in all, although we have great management challenges and we have budgetary limitations to face, we think there are ways that we can obtain the information that we need, and our prime emphasis is going to be on the resolution of issues, hopefully demonstrating to the nation that we can make some progress on the repository program.

Thank you.

COHON: Thank you.

One question of fact. How do the House and Senate numbers for the current year compare to last year?

FEDERLINE: We are currently operating under a $22 million program.

COHON: Thank you.

The way we are going to proceed, with everybody's agreement, is we will skip over the old hands and let them go last and hear from the other federal party representative at the table, and then we will go to Bob and then William Magavern and Steven Kraft.

Larry Weinstock.

WEINSTOCK: Hi, everyone. First of all, I would like to thank the Nuclear Waste Technical Review Board for inviting me to come and speak at this meeting.

The Yucca Mountain rulemaking represents one of the top priorities of my division and our office. We are aware of the importance of these standards and the need to

move quickly.

Our goal in these standards is not only to set standards that are going to be protective for the long term for the public and for the environment, but also to set standards that can be implemented and can actually be put into place. We believe that a standard has to meet both of these tests. It has to provide public safety and it has to be implementable for it to meet the test of acceptance and credibility to the public.

We believe that this acceptance by the public is crucial, because without it, not only Yucca Mountain but the nation's nuclear waste programs in general are really going to be doomed to failure. What we mean by public credibility is in the broadest sense of national credibility that we have something that we are doing that makes sense, that is protective, and that we are not rushing to judgment and not acting irresponsibly.

How are we going to go about this?

The first thing from our agency's point of view is to realize that there is a vital role played by the stakeholders, and those stakeholders are many and varied interests. They include DOE, the nuclear industry, the State of Nevada, local and tribal governments, environmental groups. Also they include the states and localities around the waste generator sites and the places where the waste is

being stored.

Many people have told me I don't have a very enviable job in this task. I kind of think that there is a certain symbolism about the way I have been seated at this table with Bill on one side and Steve on the other.

[Laughter.]

WEINSTOCK: We really do want to bring all the parties together and get all the parties who are involved in this talking to us and hopefully also talking to each other so that we can have a standard that at least as many people as possible can find agreement with.

To that end of getting stakeholder involvement, we just completed a series of meetings in Nye County and Las Vegas, Nevada, and Washington, D.C. The purpose behind these meetings was to explain our role to the public and to stakeholders, but also, and most importantly, was to get their feedback: What do they think of the NAS report and how we should interpret it and how we should use it to go ahead and move forward with our standards. Meetings like this also provide a forum for us to get ideas.

We've made a Federal Register notice requesting comments on that report and how people think we should go forward and interpret it. We've requested those comments by October 26. Obviously it may be more difficult because it will be further on in that rulemaking, but we will accept

them at any time.

Also, at this point I would like to take this opportunity to invite the Board if they have any comments or suggestions to submit them. We recognize the expertise and knowledge in this Board, and we would certainly like them to play a role in our rulemaking process as well.

Once we get this stakeholder input, we also have a special partner in this process, and that is the NRC. As Margaret said, we are going to be working closely together. We feel that that is crucial, because no matter how good our standards are, if they can't be implemented, we have failed in our role. So it's crucial that we work with NRC.

Not only has NRC been working on this particular project longer and in more depth than we have, but we need to better understand their processes. An NRC licensing process is very different from the EPA rulemaking process that we are using at WIPP. There are a lot of things that we could put into a rule that we could very easily implement. It would not cause us any problem because of the nature of our process and the type of decision-making that we do.

At WIPP we are not going to have court-like hearings and proceedings. It's really a straightforward rulemaking with the administrator of EPA making a final judgment. The NRC process is very different, and that does

lead to differences in how standards can operate and need to be able to operate.

So I am pleased to announce kind of behind Margaret that we have created these liaisons. I am grateful that the NRC has created a task force to work with our staff, and I am confident that this interaction will work, that it will also build on what I see as an ever improving EPA-NRC relationship and serve as an example of interagency cooperation which both agencies can be proud of and build on and even improve in other areas of joint activity the cooperation between the agencies.

Our current plan is to go forward with the rulemaking by incorporating public comments as we develop our proposed standards. We recognize that there are very strict time limits that Congress has placed on us, and we are going to do everything we can to ensure the proposal will come out as quickly as possible. After that proposal we will be taking comments and having hearings in Nevada and Washington before developing a final rule.

I know that a lot of people are interested in what EPA's reaction to the NAS report is. I am just going to have to skim over that briefly.

First of all, we believe the NAS has done a very good job with a very difficult task. We appreciate the fact that they went out of their way to make distinctions between

policy and scientific judgments. We feel that certainly the Academy has some useful policy advice, and it is not that whatever policy advice we would just dismiss, but we do feel that it is appropriate for them to make those distinctions and that it does help us and helps the public understand areas where ultimately we may differ or may not differ with the Academy.

We also believe that they did a good job of answering the most important questions that were faced but left open many important questions but the ones that really are appropriate for a regulatory agency to answer with input from the public.

Certainly the depth of analysis and discussion is going to also greatly aid the agency.

I know many people would like me to come up and tell you all the portions of the report the agency agrees with and those it disagrees with. While I'm at it, you would probably like me to tell you what the standard is going to look like and what the form is. But I can't do that. It is not just that this wouldn't be the best time or place to do that, although it really wouldn't be the best time or place to that.

The fact is I honestly don't know what the standard is going to be, and EPA has a lot of different reactions to it, almost as many as there are different

staff. We are still working through the many issues involved. There are a lot of issues we really, as I said, want public input on, and that input is going to matter. It would be actually much easier if were going to do this in a vacuum; we would be much further along in our decision-making; but because we do want to get this input, we are kind of still holding back and trying to weigh all the things and balance all the information that people will bring to us.

In addition, we are doing some analyses ourselves. We are going to be working with the NRC. We expect to get more information from them.

We have a team developed and put together to work on the Yucca Mountain standards. The staff has been organized and assignments have been laid out to address the many issues presented, including things such as the level and the form of the standard, how much licensing analysis should we define for the ultimate licensing.

In addition, we are going to do some analyses ourselves on a large number of technical questions, one of which would be the level of geologic stability that is required for purposes of standard setting and compliance demonstrations, and how long we believe that that level of geologic stability exists.

Only after we get these public comments and

complete these analyses will we be able to decide how we want to resolve these issues. Clearly our starting point is the NAS report, and certainly that report does give us a great deal of flexibility. Our goal is to use that flexibility to set standards that will ensure that Yucca Mountain will not open unless it's safe, but also to ensure that if Yucca Mountain is safe that it can open.

We recognize that as a nation we have created this waste and we have a responsibility to do something with it. While we cannot let it be placed in a location that is not safe, prudent stewardship of the environment says we also cannot miss an opportunity to place it in a safe location.

So with your help and the help of members of the audience and many stakeholders, I am confident that we will be able to reach those goals, and we certainly look forward to the effort.

COHON: Thank you.

Bob Loux.

LOUX: Thank you. I also would like to thank the Board and the staff for inviting us here. I'm not sure that we have a lot to add to what has already been said. Given our role in oversight, I suspect we, like many others, are sitting tight and watching what is going on and we will all find out down the road about some of the major policy things that you referred to earlier.

I can tell you one thing. We don't know what kind of funding we are going to have in FY-96. Dan hasn't told us. He doesn't know yet, I guess. Because our funding is now up to the DOE's discretion as opposed to a line item, at least under the language we have seen most recently, and may or may not require some appropriation committee approval. So we don't know what kind of program we are going to run, we don't know what kind of funding we are going to get, and Dan will tell us that.

Because of that, Dan, I wanted to echo the praise of the other congressional staff people.

[Laughter.]

LOUX: I really think you are doing a marvelous job, Dan. I really do.

[Laughter.]

LOUX: If you devoted most of your funding to SUB-SEABED, I think you would have a really fine program.

[Laughter.]

LOUX: In all seriousness, though, looking back at some of the presentation earlier today, I can't help but think of the paradigm that has been created by the congressional staffers on one hand and many others praising the turnaround of the program but at the same time going to bind your hands on the repository effort if they have their way.

I see the congressional actions and those by their

supporters framed around the need to balance the budget by 2002, at least as stated by Congress, which sort of appears to put you at about $400 million or $425 million per year. Certainly no one in the industry or those paying the fee want to see more than one mill paid into the program.

It appears unlikely, according to Alex Flint, that taking the program off budget looks viable. So it seems to me the answer is clear in answer to your question relative to how do you preserve geologic disposal. You certainly have to take the interim storage issue off the table. Otherwise you just don't get there. It seems relatively straightforward.

I do believe, Dan, that there is an administration policy, contrary to some earlier comment. You have a program in place. You have a law in front of you looking at geologic disposal and Yucca Mountain investigations. I think that represents the administration's policy. I think it's clear from the correspondence from OMB and other statements that the administration feels comfortable with the current program of geologic disposal, does not see necessarily the need for interim storage in the short run, and it seems to me, though, that given their actions and the other constraints on the system, that there is the proverbial wreck ahead in some sense.

Given that, Dan, think of us well in 1996 and

we'll be looking forward to working with you.

COHON: Thank you.

Bill Magavern.

MAGAVERN: Thanks. I think this time of change offers both dangers and opportunities for radioactive waste policy. The biggest danger is that the nuclear industry's big lobbying and advertising campaign will convince Congress that there is some sort of crisis, which I think is being artificially created, and convinced to fixate on the date of 1998, which is somewhat mythical when it comes to radioactive waste; to try to rush to open a so-called interim storage dump and to try to ram that down the throat of the State of Nevada or another unwilling community and at the same time to slash environmental standards for the permanent repository.

The danger really comes from the fact that there is not the money to pay for both a program that is allegedly interim and a permanent program; that if you put this so-called interim dump in Nevada that you seriously jeopardize the credibility of the site characterization program at Yucca Mountain; and the fact that there is no safety or economic rationale for moving waste away from operating reactors. It's simply to solve a public relations problem that the nuclear utilities have.

I don't think this would come about because of any

effort, but the opportunity is that it is possible that budgetary problems could force a rethinking of high level waste policy when people realize that we don't have the money to do interim and permanent. We should take another look at our overall policy, realize that the waste can stay at the point of generation for the interim period, and then I would hope we would go beyond that and take another look at alternatives to geologic disposal, because I don't think that the geologic disposal program has been going well either scientifically or politically since its inception.

What I hope the government and the industry would learn from all these years of contentious battling on radioactive waste is that if you try to take the waste and just ram it down the throat of an unwilling state, unwilling community, it's not going to be quick. We've seen that. And it's certainly not going to be cheap. So let's try to do it another way; let's try to do it with some democracy and some science instead of cutting political deals in the back room.

COHON: Thank you.

Steve Kraft.

KRAFT: Let me add my thanks to the Board for inviting me here. I always enjoy meeting with the Board.

I don't have any prepared remarks. Of course what Bill Magavern just said about the money is interesting.

There most certainly is enough money available to do interim storage repositories and any other little thing you'd like to do several times over. The problem is that the money is locked up in the federal budget smoke and mirrors process. Until we get that fixed Congress has it in their power to say that there is not enough money.

Off the top of my head, I think I can recall that the amount of money that has been committed to date for this program is $11 billion. Dan and his predecessors have spent $5 billion, in that range?

DREYFUS: It depends on where you cut it off.

KRAFT: It depends on when you cut off the calculation. There is a paper balance in the fund, something over $5 billion, and there is interest earned on the fund and there is almost $2 billion owed by utilities for what we call prior fuel, fuel discharge prior to April 7, 1983. If all that money was available, I don't think Dan would have a problem running the program.

That is just locked up in the way the Congress determines scoring scores the various pieces of legislation, how the money gets spent. The fact is that over the coming ten years in the budget calculations OMB has already assumed the income from the one mill fee coming into the program. So any adjustment looks like it is hurting the deficit, and it's not. I won't go through all the details, but this gets

into metaphysical accounting things that ultimately hurt the nation whether or not it looks that way.

With regard to some of the other points, and we will get into this in some discussion, I continue to listen very carefully to NRC, EPA and DOE in their implementation of the parts of the federal program that they are responsible for. It all leads me to wonder.

I'm not picking on Margaret. She said some things very clearly. For example, multi-barrier subsystems and subsystem performance standards may give confidence, and you've advised Congress of that. But if Congress chooses to direct you to do nothing but total system performance standards, NRC is still going to do that.

I say that not to pick on Margaret but to point to where the decisionmakers and the policymakers are. They are not at this table, and they are faced with many other competing interests in many other areas having nothing to do with nuclear waste and how they make their decisions. How that in fact is going to ultimately come out this year, next year, whenever, if you think predicting future geologic events is tough, sit where we sit sometimes predicting future political events.

Again, thank you for the invitation, and I stand ready to participate in discussion.

COHON: Thank you.

Let me turn to the old hands and see if they have anything they would like to add at this time before we open it up.

Bob Williams?

WILLIAMS: Thank you. Just a few brief comments first. My thanks to the Board for the invitation to this meeting.

About 25 years ago I had a title of strategic planner. One of the questions was, how do you tell a strategic plan from a strategic dream?

The short answer is to do real work. So I would urge the program to find a way to continue the work of the tunnel boring machine for six months or a year longer. I would urge the program to try to maintain the continuity of long-term experiments and to start some heater tests. I could recite the litany of the heater tests, and G tunnel, and so on and so forth, but in the interest of brevity I won't.

Secondly, I think the scientific and technical trap is too bland a term, so I propose a new term. It's the technological equivalence of the O.J. Simpson trial. The hearing that we are going to have ten or 15 years from now has every chance of being as big a debacle as the O.J. Simpson trial. There are going to be allegations of errors of omission and commission on the part of the program. The Johnny Cochran of this activity is probably still in junior

high school. He will be there wanting to send big government or big industry or big utilities a message.

One of the things that he will send the message over is all of the things we promised to do in some site characterization plan drafted in 1986, reissued in 1988, hemmed and hawed about from 1990 to 1995. So a very crucial thing is to kill some of these earlier program documents and make an overt finding as to why they are superfluous or not being pursued. Otherwise the errors of omission and commission will be rampant.

As far as the MRS and storage are concerned, throwing somebody an MRS without a site is like throwing them a life ring with a hole in it. It is not going to be very helpful.

I won't try to invent a new MRS program here on this podium at this point, but I would observe that if some structuring of the Mescalero initiative were done, if one other private sector initiative were involved, then you could pay these fellows later. You could buy them out five or ten years down the road and you would have the cash flow for the present program.

So I think some creative privatization is a possibility. I realize that politics always gets in the way of creative and productive activities. I thought there was a lot of receptivity on the part of the congressional

delegation to maintain the viability of the technical program on long-term disposal. So hopefully that might provide the impetus to do that sort of thing.

I will stop at this point.

COHON: Thank you.

Chris Whipple.

WHIPPLE: Thank you, Jared. Since my early comments have been made with the NAS committee hat on, I will stay in that role with a few more stray thoughts that occurred to me as I listened to the talks today and to talk about the regulatory process as it affects the program.

I have been doing some thinking about Steve Brocoum's comment on the inconsistencies between the Yucca Mountain standard report recommendations and the earlier NAS Rethinking High Level waste report which Clarence Allen and I and a cast of others worked on. The continued currency of that report I find very satisfying.

The flip answer is the rethinking was the generic report and Yucca Mountain standards were site specific. I don't think that goes to the heart of trying to get your hands around how to do you regulate something for which the risks extend so far out in the future as it is beyond any experience to have done something like that.

I will get to a little bit of how you do that. There is no quick answer, of course.

One of the things I hope was recognized in Bob Fri's presentation earlier today is a key goal the committee backed into was to try to defend science from the regulatory process. Don't ask people to do calculations that can't be done and then call them scientifically valid calculations. That's why we threw out assessing probabilities of human intrusion. That's why we threw out pretending that you knew what the future biosphere would look like. In some ways it's why we were given real heartburn by the previous rationale for the time limit.

As short a time has passed since that report came out, I've already seen in the two technical presentations today by Jean and John insights emerging as you look further out in time. Key sensitivities turned out to be time dependent, whether you are looking at short term or long term, and a lot of insight emerges from that work. So I see some good coming.

To get back to what you do to make this thing feasible, I think the hardest piece of this is going to fall on Margaret and Larry to accept that the standard of proof over the time scales that we are dealing with have to be well below those that we are used to dealing with in the regulatory arenas.

I had the cause to go back and reread the 1982 high level waste standard a few weeks ago. There is some

really elegant language in there about how we can't know about stuff in the far future, about how we can only have a reasonable expectation to believe this will perform. I think we need to get away from the concept that the words "reasonable assurance" and "high level waste repositories" can ever be put in the same paragraph and have a program that succeeds.

A final comment that goes to the isolation strategy. I was in England last week in technical meetings. The British have a way of doing things that involves using what is called the safety case. The proponent of a big, complicated, potentially hazardous project is given a blank check to defend to the regulator why they should be allowed to go ahead, and the burden is on them to say this thing is safe because, and they get to fill in the blanks their own way. Again, it puts a burden on the regulators to be flexible, to be thoughtful in saying, well, wait a minute, I think you are wrong over here.

As I understand its operation, it does not result in a multiyear process of having to chase down all the "what ifs" if the central case is plausible and the evidence behind it is presented properly.

I see the isolation strategy as taking advantage of the insights from that approach and I commend you all for adopting it.

COHON: Thank you.

Let me invite anybody else currently sitting at the table to say something if they want, or I'll start asking Leon's questions again.

[Laughter.]

COHON: Steve.

BROCOUM: I would like to second that we have to rethink the whole regulatory process. The whole Nuclear Waste Policy Act encouraged pre-licensing interactions to help resolve issues before you got into the actual licensing hearing. If you look back at the last seven or eight years and you try to list how many significant calculations have actually been resolved, that list essentially does not exist. That whole process up to now has not worked from our perspective, from the DOE perspective, and we have not been able to demonstrate real progress on resolving issues with our regulator.

We have been able to demonstrate some procedural progress on a more procedural level, but in terms of solving significant issues, those have been on the program for a long time know that many of the issues that we talked about today have been on the table five or ten years ago. Countless meetings have been held on these issues and very little resolution has taken place that one can actually point to and say here is something that we have resolved and

then move forward on it.

COHON: Chris, in the case in England that you just described, who defines safe? Who answers the question how safe is safe enough?

WHIPPLE: In the end it's the regulator, just like here.

COHON: Does the applicant start out with the definition or does the applicant operate under some previous definition of safety?

WHIPPLE: It depends, but actually the Europeans are probably more prone than we are to use quantitative measures of performance. For any number of industrial facilities they have adopted quantitative standards that look a lot like the old safety goals adopted by NRC whenever it was, the mid-1980s, late 1980s.

So there is an underlying quantitative goal but there is not a blind evaluation of which side of this bright line you are on. It's guidance rather than something of a higher level requirement.

COHON: I can't believe this group has nothing to say. John Kessler.

KESSLER: Just a point of clarification, Chris. If you were trying to be consistent with the European approach, would the regulatory situation there say it's up to DOE to decide, for instance, what it wants to do for form of the

human intrusion or for the kinds of exposure scenarios it wants to bring forward? Would the regulators there say that it is up to the applicant to make those kinds of decisions or recommendations?

WHIPPLE: It's up to the applicant, and if the regulator doesn't believe the applicant has done a good job, it gets sent back.

KESSLER: So then it would be an interaction. For instance, for Yucca Mountain, it would be between DOE and NRC to come to some sort of mutual decision on things like human intrusion, the nature of it, and the types of exposure scenarios?

WHIPPLE: The Europeans do a number of things that are unthinkable by American standards. They send their technical people from different agencies out of the room to go talk to each other and presume that nobody is cutting a deal to stab somebody else in the back when they are doing that. As a consequence, they save a lot of time and money, and compared to us on this program they have less of those. Less money, anyway. The general presumption is of individual honesty and technical competence, which makes this whole thing easier if you could have those as underlying premises.

The other thing they do is when they go through a big one of these things, they do a public inquiry, which is

much like a licensing hearing here. Intervenors are given significant opportunities to punch holes in the safety case, and typically there is a judge with technically trained assistants running this hearing. When the day is done the judge says you made your case or you didn't.

COHON: Steve Kraft.

KRAFT: Chris, you may have answered the question. I'm no student of the processes in other countries, but a point that John Kessler made throughout his presentation that I thought is worth repeating is that when you say that you believe you can prove that Yucca Mountain is going to be successfully licensed, you are really making two separate statements.

One, you are saying I know I got the science right. Jean is going to say we know to a scientific certainty, whatever that is, however you want to define it, we know we got it right.

But then you are also saying I can take that information through the labyrinth of an NRC licensing proceeding which has all the trappings of a very rigorous courtroom atmosphere. Then, of course, once you are done with that you get into the courtroom,. Fortunately, the Waste Policy Act says first court of jurisdiction is the appeals court. So we skip the circuit courts and get into appeals court. We will go to the Supreme Court. And there

are other processes that are built into the Act that get Congress involved.

My impression of the European processes is, while they do have those public inquiries, they do not have the courtroom cross-examination and interrogation quality to it of an O.J. Simpson trial, as Bob pointed out.

There is another aspect to it too. Quoting something that I heard said at another NAS meeting, which is that in many countries, perhaps in all the European countries, a decision was made to take advantage of the good things that ionizing radiation can do for us, whereas in this country we have never really made that decision. Every time we are faced with a licensing decision of any type in front of NRC it becomes a pseudo-litigation over the future of the use of ionizing radiation.

So the attitudes are different and the processes are different, and you cannot draw direct analogies. You are going to have scientists who probably, as Bob has said, are in high school now defending science that was done ten years ago today in front of a licensing board whose judges haven't graduated law school yet. You are not going to meet any of those standards in that circumstance.

COHON: Chris.

WHIPPLE: I hope the point I was making was not that we should become English overnight. I think that would be

difficult. The logic behind their approach through the use of a safety case in terms of what it does to the people running the program and forcing them to focus on what they do and don't know and what's important, I find that to be very effective. In the context of what benefits it would bring to the technical program, I think there are some regulatory ones as well, but there so many other cultural differences that I think that is probably not a particularly feasible thing to do.

COHON: Margaret, Bob, and then Don.

Margaret Federline.

FEDERLINE: Let me just add to what Chris was saying. NRC actively participates in the International Forum. We have seen internationally the iterative performance assessment process work quite constructively, and in fact in the site characterization analysis we urged DOE to focus on achieving integration through focusing on their performance assessment. We are encouraged, although it has been a number of years since we recommended that, that we do see a prominent role of performance assessment in the program.

We also are conducting a performance assessment as they do in Europe between the regulators and the developers and we do in fact have opportunities to sit down with DOE. We have a number of types of interactions defined. We have Appendix 7 visits where we can go and actually interact with

the technical people. No management decisions or program decisions are made at those meetings, but it's an exchange of technical ideas.

I am not sure that we are as different from the Europeans as people might characterize, and I would remind you that we have successfully licensed over 100 nuclear power plants. The process can work if we are constructive about it and move through it with good conviction.

COHON: Bob Williams.

WILLIAMS: I wanted to turn this discussion to answer one of Leon's questions so I get at least a passing grade on the exam.

The answer to question three is, yes, the program can proceed without a standard being issued. The existing standard has in fact been in remand for eight years and that hasn't stopped us.

The interesting thing that I took great heart from today was to see DOE in the face of the recommendations of the National Academy stand up and say, nevertheless, we would pursue a robust waste package because it has some benefits other than those that are statutorily hypothetically prescribed if the EPA were somehow to adopt the NAS report.

I thought there was a lot of good, a lot of innovation, a lot of potential in the new waste isolation

strategy. I would encourage that.

COHON: Don Langmuir.

LANGMUIR: I'm not sure this is in the flow, but I was intrigued by Chris Whipple's comments on the European program. It struck me that those of us who have been aware of the European program for a long time have been aware that they have been looking at millions of years for their horizons for many, many years now. I would like his comments.

My sense is we are headed that way; we are finally beginning to adopt some of the thinking that the Europeans have had in terms of how to deal with a long-term program.

I guess I would like to have him comment on that and maybe what the differences are in the NAS report relative to what the Europeans are thinking and have been proposing to do.

WHIPPLE: You are getting into an issue on which the committee spent a fair amount of time. I don't claim to be an expert here, but my understanding is that the way the approach works is they do performance assessment using fairly conservative bounding assumptions, including subsistence farmers and million year time periods, and if they can live with it, they are satisfied and they quit. If they can't, they go back and say, all right, how much do we have to relax this or that assumption or this or that rule?

Maybe it's not a subsistence farmer; maybe it's a farm family, and maybe they are not two kilometers away; maybe they are eight.

They do these analyses to get an overall sense of what kind of ballpark they are in in terms of safety versus their standard, and then they make a licensing decision. I think that was part of the reason why we had the dissent in the last six or eight months on the study of fighting over biosphere assumptions about where people are and how one does that analysis.

I think in the end it was the collective view of the committee that the U.S. process does not have the flexibility to do some iterative regulatory compliance calculations and durations; complaints, with some justification, that the rules were being modified to fit the site would be made.

In the European process, where they don't perceive that they have enough money to go out and dig four or five different sites before they get one that works, it's considered strictly intelligent and proper to iterate on the rules as well as everything else to make this investment go. But that is not our approach here.

COHON: Let's focus on Leon's questions. I think they are very good ones and pertinent ones. We have touched on parts of many of them already. Since many of you have

mentioned already the waste isolation strategy we heard about today and your comments so far have been very favorable, let's focus on two questions in particular:

Is it possible to define a waste site selection strategy? Bob Williams says yes.

Is it possible to do so in the face of the uncertainty that we have identified already?

And how good is the strategy that you heard today?

We had some responses. Does anyone else want to say something about that?

Margaret.

FEDERLINE: I believe it's going to be important to have an understanding of the key processes that underlie repository performance no matter what time period. Certainly if 10,000 years or a million years is selected, certain processes will become more key than others. But it's necessary to understand through a systems analysis what the constraints are in each of the processes.

The way the rules are written, even the existing rules, DOE can come in and propose alternative approaches for multiple barriers. So I think a process where you do understand -- and that is what we are heading for; I think that is what Bob Williams was referring to -- that the program should go on and do that.

COHON: Dan.

DREYFUS: I don't see that you need to know the standard to do waste isolation strategizing. I think that basically you can only do what you can do. The object of the exercise is not a mystery; the object of the exercise is to prevent radiation doses that are harmful. One knows that going in. The guidance is there to construe a waste isolation strategy.

If you know the standard, you may change your application of resources with regard to those aspects of the waste isolation strategy you spend the most time on proving. Obviously if you are focused on a peak dose at 300,000 years, you are going to spend your time differently than if all you are trying to do is display total confinement at 10,000 years.

That is not to say that anybody is intent on disregarding what happens in the ten thousandth and first year, but it's a different proof. So while you may do the same things physically, you may spend a whole lot more time on the regulatory arguments if you have a different standard to meet. This, of course, is one of the problems that I think you have been talking about here, about the European approach, and that rather than describing the best thing you can do and then measuring it for its adequacy, you set up a standard a priori and then see if you can jump the hurdle, which I find to be an illogical way to go.

When all else fails, I often resort to country and western music for philosophical guidance.

[Laughter.]

DREYFUS: There is one going around now that says, "Give me one more chance. I'll learn to dance the dance."

[Laughter.]

COHON: Don Langmuir.

LANGMUIR: I had a quite related question but more specific to this meeting. I just learned, as some of you did, about the NAS report recently. My question is this. Have the recommendations in the NAS report, suggestions made by that report shown us a way to simplify our strategy, made it easier, or in fact by adding the additional 10,000 to 1 million years on to the end here, have we made it a more complicated activity and more difficult effort to get the license for a repository?

There are insights and simplifying arguments suggested by the NAS which presumably could help us out to get there and focus more directly on our answers.

I will leave the question I asked as I asked it earlier. Is it easier or is it more difficult now if we adopt that strategy?

WILLIAMS: Let me tackle that. I've agonized over that, Don, and I think it's a wash. I think we have improved things by addressing a fundamental element to look

at the time period where the hazard is greatest. Whatever advantage we have gained by that, we certainly have covered up one Achilles heel. We have introduced the complexity of biosphere and geosphere.

I think we need to go back to a report that Max Blanchard and Tom Isaacs issued. I can't remember the exact title, but the name that is in my head is a step by step or phased licensing approach, where you don't try to make the ultimate finding early on in the process but you recognize that there will be an evolutionary accumulation of data in the course of the regulatory reviews and approvals and monitoring of the repository.

I think without something like that we are embarked on the outcome of the O.J. Simpson trial.

COHON: Pat.

DOMENICO: I would like to say something about the standard and what someone said a little earlier about asking no more than science can deliver. I've always felt from strictly a geologic perspective it's impossible to determine whether a site is suitable or unsuitable unless you define suitability in a very specific way. By suitability or unsuitability, I mean the presence or absence of favorable characteristics. But that's a moving target. We once thought a favorable characteristic was slowly moving groundwater. Today I learned the faster it moves the more

dilution you get.

[Laughter.]

DOMENICO: And dilution is the solution to this pollution, obviously.

[Laughter.]

DOMENICO: That aside, if we define suitability as the presence of favorable conditions, I think Yucca Mountain will be found suitable. Whether or not it's licensable is going to depend upon that standard and whether we can meet that standard with some model calculations. I don't think we are above changing the parameters in the model to meet that standard as opposed to the Brits or the Europeans where it's "let's change the standard."

I think when we start talking about the model calculation, we can get that model to give us anything we want. I know that. That's disturbing.

I think the main point is the connection between what that standard is and not asking science to do more than it can do, and you are asking geology to do a lot here unless we define suitability from that very, very special perspective of presence or absence of favorable characteristics. There is no question in there. That's just a philosophical statement that I picked up when I heard country music the other day.

[Laughter.]

COHON: Don Langmuir.

LANGMUIR: Chris Whipple. Poor Chris. I keep coming back to his very interesting comments. The suggestion you made earlier that maybe the EPA and the NRC ought to be thinking about less stringent standards for longer time periods when you can't predict --

WHIPPLE: Lower hurdle on proof. It's still a stringent standard.

LANGMUIR: I guess I would like to hear the agency's comments and thoughts on that. That's a very different idea than they are probably used to dealing with. I would be interested in Margaret and Larry's comments regarding that.

WEINSTOCK: For us it's not a new concept. It was nice to hear Chris actually quote an EPA preamble talking about it. It is something that we do all the time. We have a number of standards that go out 10,000 years outside of the high level waste area. This is not the only case that this happens in the agency. We treat those differently.

My group has the WIPP program and I have been involved in that for a number of years. We are not expecting the same level of proof for the WIPP that we would for an air and toxic standard that we have people monitor and test every piece of equipment that is controlling every emission point. Indeed we look at a wide variety of things for different kinds of standards of proof.

In the air program we have state implementation plans which have the easy task of only looking ahead ten or 20 years but have to look at every source of air pollution in a major urban center. The standard of proof we put on those for approving SIPs, which we do all the time, is different than the standard of proof we put on a construction permit or an operating permit for a refinery.

It is nothing new and it is certainly something that we've had to consider: are we going to provide guidance on that point beyond what we have done in the past?

One of the things that we are going to look at is whether or not the standard should change if we go further. Is it appropriate to have different types of standards? That's hardly a new idea for different time periods, but it is certainly one that we would look at. It may just be a question of different standards of proof, but it is hardly unique to this problem.

FEDERLINE: I would just add to what Larry said. NRC, in implementing the high level waste standard, adopted EPA's recommendations about reasonable expectation. Proof is not to be had in the ordinary sense of the word. I think we have a fairly common understanding that this is a different threshold because we are talking about time periods that can't be measured.

When the Commission established its subsystem

criteria it was looking for a way to enhance its confidence. Back in the days when Part 60 was adopted a probabilistic standard was very new for the agency. I think there were some concerns as to whether it could be demonstrated with a high degree of certainty. Acknowledging this reasonable assurance concept, the Commission said by using these subsystems we will gain some additional confidence that if one barrier fails that all barriers won't fail.

COHON: It seems to me that we have heard many significant things today. We've talked about two of them primarily in this panel so far. One of those things, of course, is the committee report, which, among other things, has made 10,000 years seem not so distant anymore.

[Laughter.]

COHON: I was struck listening to you all talking about 10,000 years as that period during which we have great confidence compared to the million that the Academy wants us to look at.

In addition, I think clearly defining or trying to clearly define the line which separates science from policy is extraordinarily valuable and important and something we forget very often. Someone said after Bob Fri's comment that in defining that line the attempt is not to keep science and policy apart from that point on -- that's impossible and undesirable -- but making it clear how far

one can go with science and at what point the questions can only be answered in a policy sense, meaning a political sense as well.

The other significant thing, I think, was hearing about the strategy that Jean presented to us and which has gotten very favorable reaction, which I think is very nice to hear.

One of the things I would like to get you to talk more about, the you being DOE generally and others who want to comment, is the impacts this strategy will then have on the rest of the program.

Let me pose a very specific question. Will this then shape all of the science activities that go on at Yucca Mountain?

BROCOUM: I think I said when I introduced Jean that DOE is reviewing that right now starting October 10. We expect that review to be done by the end of the year. Assuming the review is favorable, then I think it will be influencing our detailed planning for this year and following years.

The idea of coming up with the strategy is to help us focus the program. We have less funds. Obviously if we have less funds and we have a lot of pressure to demonstrate progress, we need to focus the program, and we are turning to this strategy and the associated PAs to help focus it.

COHON: Let me put you on the spot a bit more. We recognize this is very current effect and very live discussions that are going on. Assuming you can't do all the science you would like to do, and that's a given, given the budget reductions, what factors from the strategy can you use to determine what is most important now?

BROCOUM: Let me just make another comment. We said the surface-based program is essentially terminated. We are putting a lot of focus this year on synthesizing or understanding information we have or trying to capture the information the principal investigators have gotten. We will see where we are. We will have our strategy and we will decide where to move forward.

The next step is to actually say which specific tests or what specific pieces of information. I think Jean talked in general terms about information needed and that would have to be turned into which existing tests are we doing that supply that information or what new or different tests do we need. That's the intent, assuming again we get a buyer for the strategy.

I keep turning it back to the buyer. We did have some controversy about releasing the strategy at this point in time. There are some people that are worried about it. When they hear a million years, people do get nervous. That is why we are doing the formal review or formal comment and

response process, to see if we can reach a consensus that we want to buy into the strategy and then move from there.

COHON: Bob Williams.

WILLIAMS: About a year ago, which was the last time I looked carefully, the quantity of flow in the groundwater was a very crucial parameter. My recollection is it was going to be determined by roughly ten boreholes that went down to the saturated zone. I thought I heard today that the surface-based drilling, including those boreholes, was being cut back. My limited understanding is that you would need more boreholes to know more about the heterogeneity in the groundwater flow.

At this point we seem to be at odds over the direction of the surface-based characterization versus the needs under this new strategy. Perhaps somebody could straighten me out on that. Jean or Steve.

BOYLE: I'll give it a shot. If you heard something a year ago, it was probably a program that was laid out to get to the license application by 2001.

WILLIAMS: Mr. Lucky of the USGS.

BOYLE: Right. We are not going to do that now. The present program isn't going to look like what you heard a year ago.

WILLIAMS: Where do you get the groundwater flow, though? Out of thin air?

BOYLE: We have deep holes already. There are some drill holes out there. We have a C well complex that we ran pump tests on this year, so we have an idea about the saturated zone response on a large scale with large-scale pump tests, what the permeability is, how quickly the water moves. So it's not as if we have an absence of information; we are just not going to have as much as we would have had if we were going to have a license application by 2001 with all that money that went with the program approach.

WILLIAMS: You know the groundwater flow to within an order of magnitude?

BOYLE: Order of magnitude how? I would guess that they know the permeability on a large scale within an order of magnitude based on pump tests. They certainly know where the water table levels are over parts of the repository within a few feet underneath the repository. So we do have some information.

As I recall, what you may have been referring to was, I think, TSPA-93 said how much water flowing through the repository is one of the critical issues. We are getting measurements of that every day. As you have heard already, we don't see any going by in the ramp right now.

COHON: Given what we just heard, once the strategy is in place with total performance assessment, which we will be hearing about a lot tomorrow, they will then be in a

position to at least come up with credible answers to questions like how important is it to know the groundwater flow with more certainty than we currently know it. That's the idea.

Let me thank the panel very, very much for their participation in this session, which I found useful. I hope everybody else did.

We will turn now to the public comment period. Are there any members of the public who would like to make a comment or ask a question?

[No response.]

COHON: Seeing none, let me first give my own personal appreciation to all of those who participated throughout the day. Not just the members of this panel, but all those who participated. As a new member of the Board I found it extremely valuable and stimulating, and I thank you very much. Let me call on John Cantlon, the Chairman of the Board, now.

CANTLON: My charge is very simple. I'm to recess this program until tomorrow morning at 8:30 a.m., with thanks to the participants, and we look forward to an equally active program tomorrow. Thank you very much.

[Whereupon, at 5:10 p.m., the meeting was recessed, to reconvene at 8:30 a.m., Wednesday, October 18, 1995.]